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Effects of school closures on displaced students and future cohorts in Sweden 2000–2016

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Effects of school closures on displaced students and future cohorts in Sweden 2000–2016[a]

by

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Abstract

The aim of this paper is to investigate the effects of school closures on student achievement among displaced students and future cohorts in Sweden. In contrast to previous studies that only analyze the effects of school closures on displaced students, this study also takes account of the effects on subsequent cohorts of students who never experience the disruption of the move. Furthermore, the study is one of the first q uantitative studies to estimate the effects of school closures outside the US. The effects are analyzed using a quasi-experimental study on all public middle school closures in Sweden 2000–2012, a majority of them located in cities. The study utilizes a design that provides a better control for family background compared to previous studies on school closures and that makes it possible to study the effects on future cohorts. The performance of students who graduated from closed Swedish middle schools are compared with the performance of their 'treated' younger siblings who were expected to graduate from the same schools but as a result of the closures attended other schools. The results suggest that the school closures in Sweden overall had no effects on student achievement, even though displaced students and future cohorts attended slightly higher-performing schools than their siblings as a result of the closures. Contrary to theoretical expectations, the effects on displaced students are very similar to the effects on future cohorts, suggesting that the disruption effects were close to zero. Furthermore, the extensive school voucher system in Sweden does not seem to direct students to higher quality schools after school closures.

Keywords: School closures; school choice; student achievement; disruption effects.
JEL-codes: H44, I21, I28.

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1 Introduction

School closures are an increasingly common reform strategy for school districts in Western countries facing declining enrollments, constricting budgets, or low academic performance. In the United States, thousands of schools have been closed down during the last two decades (Brummet 2014) and the Obama administration even provided federal grants for the closing of low-performing schools (Engberg et al. 2012). In Sweden, hundreds of schools have been closed down during the same period (Taghizadeh 2016), mainly as a result of declining enrollment trends, and closures of under-performing schools are becoming more accepted as a solution to raise student performance and combat school segregation (see e.g. SVT 2016a; SVT 2016b; Dagens Nyheter 2015). Given the prevalence of school closures in Sweden and abroad, understanding their effects on student achievement is crucial for policy-makers. However, so far only the effects of school closures on displaced students have been studied. Little is known about the more long-term effects on subsequent cohorts of students who never experienced the disruption of the move and where the effects of closures on student achievement may be more positive. Furthermore, almost all previous studies focus on the U.S and we do not know whether their results are generalizable to school systems with extensive voucher systems, such as Sweden.

The aim of this paper is therefore to investigate both the effects of school closures on student achievement among displaced students and the effects on future cohorts in Sweden. What are the total effects of school closures and are the American results generalizable? How do the effects on displaced students differ from the effects on subsequent cohorts of students whose education is not disrupted? The effects are analyzed using a quasi-experimental study on all closed public middle schools in Sweden 2000–2012 using registry data. The study utilizes a novel design in the school closure literature that provides a better control for family background compared to previous studies while making it possible to study the effects of closures on future cohorts. The performance of students who graduated from closed Swedish middle schools are compared with the performance of their 'treated' younger siblings who were expected to graduate from the

same schools but as a result of the closures attended other schools. To provide more variation in the dataset for the inclusion of birth order controls, municipality controls and fixed time effects, control students from families not affected by closures are included in the model. The identification is based on the assumption that the student achievement and school quality differentials between the older sibling and the younger sibling on the closed schools would have been the same as for sibling pairs on comparable schools, in the absence of a closure. This assumption is tested using pre-closure placebo tests were the younger siblings are 'placebo treated' before the closure. This quasi-experimental setup increases the possibilities to capture the causal effect of the closures.

As the first quantitative study to my knowledge analyzing the effects of school closures on subsequent cohorts of students who never experienced the disruption and one of the first quantitative studies on the consequences of school closures outside the US, the study contributes to a more comprehensive picture of the consequences of school closures for school researchers. More specifically, the analyses of both short-term effects on displaced students and long-term effects on future cohorts provide better conditions to isolate positive school quality effects from negative disruption effects, and to compare the importance of these two effects, compared to previous studies.

The study is also of substantial societal value to Swedish policymakers since we may expect more school closures in the future as a result of negative demographic trends in rural municipalities (Boverket 2012), growing inequalities in learning outcomes (OECD 2015) and increased school segregation[1] (Böhlmark et al. 2016), if current trends continue. These are now leading items on the political agenda in Sweden. A common argument in the debate about school closures among protesters is that school closures motivated by declining enrollment trends harm students and result in lower academic performance (Taghizadeh 2016; Uba 2016). Is this true? Or could school closures besides saving money be an effective policy to raise student performance? This study will for the first time provide Swedish empirical evidence in relation to these important policy questions.

[1]Closures of highly segregated under-performing schools are becoming more common in Sweden to raise student performance and combat school segregation (SVT 2016a; SVT 2016b) and the new government will make it easier to close schools (Socialdemokraterna 2019).

2 Previous research and theory

There exists a small literature on how school closures affect student achievement among displaced students. Most of the existing quantitative studies are relatively new, indicating the novelty of the research field. The results from the four most cited published research articles (Engberg et al. 2012; Brummet 2014; Carlson & Levertu 2016; Sacerdote 2012) and the two most cited published research reports (Carlson and Levertu 2015; De la Torre & Gwynne 2009) are summarized in *Table 1* below.

Table 1: Previous quantitative studies on elementary school closures

	Engberg et al. 2012	Carlson & Levertu 2016	Brummet 2014	Torre & Gwynne 2009	Sacerdote 2012	Carlson & Levertu 2015
Results						
Math scores after 1 year	−	0	−	0	−	+
Math scores after 3 years	0	+	0	0	0	+
Reading scores after 1 year	−	0	0	0	−	+
Reading scores after 3 year	0	+	0	0	0	+
Study Characteristics						
Location	USA (unknown)	Ohio	Michigan	Illinois	Louisiana	Ohio
Number closed schools	22	18	246	18		120
Number displaced students	1 806	1 597	39 205	5 445	8 500	17 485

− + statistically significant at the 5 percent level or lower, 0 not statistically significant.

As we can see in the table, the results are somewhat contradictory and only half of the studies find statistically significant effects on reading. That said, a pattern that can be found in previous studies is that three of them find an immediate post-closure drop in achievement one year after the closures followed by a steady increase. After three years, student performance tends to match pre-closure achievement levels and trajectories (Engberg et al. 2012; Brummet 2014; Sacerdote 2012). In the cases where substantially and statistically significant student performance gains are reported as a result of closures, they are found among students who are displaced from relatively low performing schools (effects not shown in the table here, for more information see: Brummet 2014; Carlson & Lavertu 2015, 2016; De la Torre & Gwynne 2009).[2] Closures of relatively high-performing schools, in contrast, may cause persistent harm to student achievement that remains several years after the closure (cf. Brummet 2014).

The positive and negative effects found in the literature are widely believed to be a result of two mechanisms: the disruption mechanism and the school quality mechanism.

[2]However, negative spillover effects on students in the receiving schools may mitigate some of these gains (Brummet 2014, Carlson & Lavertu 2015, but see Engberg et al. 2012).

First, school closures may disrupt peer and teacher networks which could affect learning outcomes negatively (*the disruption mechanism*), explaining the short-run negative effects found in previous studies.[3] This mechanism is supported by numerous studies in the related literature on student mobility that find negative effects of school switching on student outcomes (Hanushek et al. 2004; Booker et al. 2007; Xu et al. 2009). The mechanism is also supported by qualitative analyses of school closures (e.g. Lipman & Person 2007; Kirshner et al. 2010). In a survey of 483 displaced students from a closed low-performing US high-school (Kirshner et al. 2010) almost half of the displaced students reported weaker relationships with teachers and peers at the new school compared to the old school and one-quarter of the students even reported feeling mistreated by youth or adults at their new schools. The displaced students also reported differing academic norms, routines, and expectations in the new schools and roughly half of the survey respondents reported that their new classes were harder. Interviews with personnel and parents from a receiving school suggest in turn that the negative disruption effects may not be limited to displaced students. Interviewed teachers, students and parents reported that the closure led to a climate of uncertainty, tension, and stress among both personnel and students at the receiving school. Furthermore, the interviewed teachers reported that the influx of new students negatively affected the performance of students in the receiving school and contributed to increased discipline problems (Lipman and Person 2007).

On the other hand, being displaced from low-performance schools may expose students to higher quality peer groups and teachers, which could improve their learning trajectories and vice versa (*the school quality mechanism*). This mechanism could explain the positive effects found in a few previous studies. The mechanism is supported by many studies on peer effects[4] which report relatively small but statistically significant effects

[3] One could imagine a related migration mechanism: families move as a result of the school closures to other neighborhoods or cities, further disrupting peer, friend and teacher networks among affected students. However, this mechanism affects a much smaller number of students compared to the other mechanisms, and are therefore not studied in this paper/or taken account of. In my study, the families affected by the closures were only around 4 % more likely to move (between the older sibling graduated and the younger sibling graduated) than the average family in Sweden (13.3 % of the families affected by closures moved, compared to 9.3 % of the families not affected by closures).

[4] Peer effects are here defined as any externality in which peers backgrounds, current behavior, or outcomes affect an outcome (cf. Sacerdote 2011). In this study, I focus on how peers affect the learning and achievement trajectories among other peers.

of peer background on test scores in American schools (Sacerdote 2011). Hence, while there is a lack of consensus in the peer effect literature, many studies suggest that having high performing peers can have a positive impact on own test scores. However, there are also studies arguing that peer effects are non-linear. According to these studies, students in the bottom decile of the test score distribution benefit more (than the average student) from adding students just above them (deciles 2–3) and large peer effects have also been found on students in deciles 9 and 10 who benefit strongly from adding peers in the highest deciles and who are negatively affected if peers are added from lower deciles (Hoxby and Weingarth 2005; Burke and Sass 2013).

With the disruption and school quality mechanisms in mind, in the case of school closures, we would expect a negative effect on student achievement if the disruption mechanism predominates, and a positive effect if the school quality mechanism predominates, as long as most students are displaced to better schools. Which of these mechanisms dominates and under what circumstances is still an open empirical question.

A limitation with the existing studies is that they only analyze the short-term effects of the school closures i.e. their impact on displaced students. This amplifies the effect of the negative disruption mechanism on the results. School closures will also affect future cohorts of students who would have attended the closed school but are unable to do so as a result of the closure. None of the studies referenced in this paper analyze these students. Here, we could expect more positive effects, as achievement benefits may be larger for subsequent cohorts of students who never experience the disruption of the move, and may begin their education in higher-performing schools (cf. Bummet 2012; Engberg et al. 2012). The focus on the short-term effects of school closures in the literature may, therefore, lead to a misleading picture of the consequences of school closures. To estimate the full effect of school closures, we must both analyze the short-term effects on displaced students and the more long-term effects on future cohorts. Furthermore, by focusing on the performance of subsequent cohorts of students who never experience disruption, it may be possible to isolate the school quality mechanism, i.e. we would expect that the effects of school closures on student achievement are primarily conditioned on whether the closure exposes students to higher quality peer groups and teachers or not.

Furthermore, as illustrated in *Table 1* above, most of the previous studies focus on US states or cities which could make it difficult to generalize the results to other countries.[5] Institutional differences between the school system in the US and in other countries may reduce the value of existing studies to policy-makers and researchers outside the US. In particular, it might be difficult to generalize the results to countries with extensive voucher systems, such as Sweden, in which the state pays the full costs of students who attend both private and public schools and where they are therefore free to choose among all types of schools. In the US, about 10 % of students attend tuition and foundation-funded private schools (Cape 2017). The same figure for Sweden is under 0.1 % (around 1 000 of over 1 000 000 students attend tuition funded schools) even though as many as 15 % of students attend independent (private) schools founded by vouchers (Skolverket 2017). Theoretically speaking, we could expect larger positive school quality effects of school closures in countries with extensive voucher systems such as Sweden if the voucher system provides students with better choice opportunities to move to higher-performing schools following the closure. This is especially true for the group of students that is most likely to gain from school closures: poor students attending low-performing schools who, at least on paper, have the opportunity to move to a high performing private school in Sweden. It is therefore particularly interesting to study the effects of school closures in this setting.

In summary, by analyzing both the short-term effects on displaced students and long-term effects on future cohorts, this study provides better conditions to isolate the school quality mechanism from the disruption mechanism, and to compare the importance of these mechanisms, compared to previous studies. Furthermore, by focusing on a country with an extensive voucher system, it analyzes a context not studied before and the results could have important policy implications.

[5]To my knowledge, four non-US studies focusing on school closures exist. However, two of them focus on special types of primary schools (Izadi 2015 on Finish rural schools and De Witte & Klaveren 2014 on Dutch Islamic schools) and one on school closure threats (Chiu et al. 2016 on Hong Kong schools) which reduce their generalizability. I have only identified one study on school closures outside the US that focus on ordinary schools (Beuchert et al. 2016 on Denmark).

3 Background: the school system and school closures in Sweden

The Swedish school system consists of two different parts: Elementary school (years 1–9, students aged 7 to 16, mandatory) and high school (years 10–12, students aged 16 to 19, non-mandatory). This study focuses on the lower secondary part of elementary schools (years 7–9), referred to as middle school in this paper which is the term used in the US (*högstadium* in Swedish). The study covers all closures of middle schools that took place in Sweden 2000–2012 where sufficient registry data on students was available.[6] Focusing on middle school students makes it possible to compare the results to previously published American studies which also focus on elementary school students. Another reason for focusing on middle schools is that it is likely that the effects on future career chances are larger and more important for these students compared to those in lower elementary school. Middle school is first to introduce a grade based sorting mechanism in the Swedish school system and grades determine, among other things, which high school (*gymnasium*) and which program the students will be able to attend to, which in turn, affect their chances of attending a university.

Middle school is separated from the rest of the elementary school system in Sweden in the sense that a majority of the schools covering years 7–9 only covers these school years (or 6–9). In 2016, there existed 449 public 7–9 schools in Sweden and 152 public 6–9 schools (601 in total), compared to 472 public 1–9 schools (Skolverket 2017). Middle school is also separated from the rest of elementary school in the sense that some students apply and choose middle school during the spring of sixth grade and, in the case of grade 1–9 schools, middle school is usually housed in separate buildings.

When choosing schools, students can apply for both public and independent schools. In 2016, there existed 323 independent 1–9 schools, 60 7–9 independent schools and 66 6–9 schools, in addition to the public schools. Both public and independent schools function under a voucher system, meaning that students do not pay for their education.

[6] To generate accurate school-level variables, registry data for at least 10 students per school was needed. As a result, a few school closures were not included in the study.

If an independent school is oversubscribed, it is allowed to choose students on the basis of three selection criteria: proximity to the school; waiting list (by date of application); and priority for children whose older siblings are already enrolled in the school. The same criteria apply to public schools, although students are always guaranteed a slot in the closest public school to their house.

As a result of demographic change, economic difficulties as well as the competition between public and independent schools, more than hundred public middle schools have been closed down in Sweden during the last decade.[7] As in most countries, the main driver behind public school closures in Sweden have been declining student enrollments. This conclusion is based on three pieces of information. Firstly, around 90 % of the closures in the study (86 of 97 closures) took place 2005–2012, a period where the number of school children aged 7–15 years decreased dramatically in many municipalities (Montin 2008). Secondly, the closed schools had more negative enrollment trends than other schools in Sweden the same period (2000–2012). On average, the closed schools lost 26 % of their students over the four-year period prior to the closure, while comparable schools in Sweden the same period only lost 3 % of their students.[8] The negative enrollment trend does not seem to be strongly related to the establishment of independent schools since the closed schools were located in municipalities with fewer students in independent schools (8.4 %) and with less positive two-year trends in this regard (+.35 %), compared to the average school (9.1 %, +.44 %). If the school closures were driven by independent schools we should see larger differences and a reverse pattern. Thirdly, the interpretation above also finds support in my review of municipal proposals behind the closures. I have access to the municipal documentation behind most of the school closures that took place in the largest municipalities in Sweden (see Taghizadeh 2016),

[7] The school closures were not part of the nation-wide reform effort to create grade 1–9 and 6–9 schools and to reduce the number of 7–9 schools, that took place from 1994 to around 2002 in Sweden (cf. Holmlund and Böhlmark 2019). Almost all of the school closures in this study took place after this period and around 70 % of the affected schools were grade 1–9 and 6–9 schools, the type of school created by the reform.

[8] These figures are based on coarsened exact matching (see Iacus et al. 2012) with non-closed schools based on the number of students in the school four years prior, year and number of students with parents with higher education. The average student trend for all non-closed schools during the studied period (2000–2012) was more positive than the matched trend: an increase of + 5 % students compared to four years prior.

covering around one-third of the schools in this study. 35 of the 37 closed middle schools mentioned in the closure documentation were primarily motivated on the basis of negative student enrollment trends in the municipality or for the school, and student results/grades were only mentioned in relation to one of the closures. Independent schools were mentioned in 9 of 37 cases (although it was seldom mentioned explicitly as the main cause behind the closures). While the sample of schools here is not random and does not cover all the closed schools in the mentioned study, my review of the closure documentation suggests that negative demographic trends were the main cause behind the closures and that closures explicitly based on student performance are very rare in Sweden.

Based on the student enrollment trends, one might get the impression that most of the closed schools in Sweden were rural schools. However, this was not the case and most of the closed schools were actually located in cities. Only 27 % of the schools in the study can be classified as rural schools and the rest as urban schools. [9]

As can be seen in *Table 2* below, the closed public schools in Sweden were lower performing than the average Swedish school the same period. Furthermore, displaced students and future cohorts graduated from somewhat better performing but still below average schools as a result of the closures. These three characteristics were also true for the schools included in all past studies reviewed in the last section (comparing *Table 2* with *Table 3* below), which increases the possibilities to compare the results from this study to previous American studies, assuming that these factors influence the overall impact of school closures.

Table 2: How the closed middle schools and the receiving schools in Sweden differ from the national average middle school the same year

	Closed Schools 2000–2012		Receiving Schools 2004–2016	
	(Mean)	(Diff National Average)	(Mean)	(Diff National Average)
Mean grade rank (0–1)	.454	-.028***	.479	-.008*
Prop students higher educ parents (0–1)	.115	-.052***	.172	-.008*
Prop students immigrant parents (0–1)	.188	+.037*	.215	+.040***
Size: Mean number ninth year students	68.2	-3.39	84.4	+13.1***
Number of schools	97		714	

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

[9] A rural school is here defined as a school located outside of the municipal urban population center. A municipal population center is in turn defined as the largest area in the municipality with contiguous buildings with no more than 200 meters between houses (*den största tätorten i kommunen*).

Table 3: How the closed schools and the receiving schools differ from the national average in previous research

School Characteristics	Engberg et al. 2012	Carlson & Levertu 2016	Brummet 2014	Torre & Gwynne 2009	Sacerdote 2012	Carlson & Levertu 2015
Closed lower performing than avg	Yes	Yes	Yes	Yes	Yes	Yes
Receiving perform better than closed	Yes	Yes		Yes	Yes	Yes
Receiving perform better than avg	No			No	No	No
Closures mainly based on stud results	Yes	Yes	No	No	No	No

However, one factor that could decrease the comparability between the Swedish closures and the American school closures (in addition to the institutional differences mentioned in the theory section) is the cause behind the school closures. While most of the school closures covered by previous American studies were based on under-utilization (under-enrollment) according to the authors, just like the closures in Sweden (cf. Brummet 2014; Carlson & Levertu 2015; De la Torre & Gwynne 2009), this was not always the case. The studies referenced above mention that in addition to enrollment, policymakers also examined factors such as school condition, closeness to nearby schools and test scores when choosing which schools to close. Furthermore, there are American studies such as Engberg et al. (2012) and Carlson & Levertu (2016) where all the examined closures were a result of state policies to close down low performing schools. In these cases, we should expect more positive effects on student achievement than in the Swedish case, since policymakers were deliberately displacing students from low-performing schools to higher performing schools to increase student achievement. While *Table 2* and *Table 3* above suggest that closures based on under-enrollment also seem to result in displaced students attending somewhat higher-performing schools, the positive school quality effect may be less significant here (and less likely to neutralize negative disruption effects) since it was a side effect rather than an explicit policy goal behind the closures.

4 Method

4.1 Empirical strategy

The key difficulty when estimating the effects of school closures is that they are not randomly distributed: some types of students, families and/or schools are more likely to be affected than others. As a result, it is not possible to capture the causal effect by simply comparing student performance among displaced students with non-affected students. In-

stead, I rely on a novel empirical strategy for the school closure literature, where I try to estimate the causal effect of school closures by comparing student achievement among students who graduated from schools that would eventually close with the performance of their 'treated' younger siblings who were expected to graduate from the same schools but as a result of closures attended other schools.

I make the assumption that the two siblings share the same likelihood of graduating from the middle school, in the absence of any closures. Swedish registry data suggest that this assumption holds for a majority of the treated students. Looking at students graduating from the closed schools prior to the closure, the proportion of their younger siblings attending the same school was 68.4 % (1–6 year difference between siblings).[10] Hence, a majority of the treated students in the study are affected by the treatment effect: the younger siblings were expected to graduate from the same school as their older siblings but were unable to do so following the closure. This empirical strategy estimates the intent-to-treat (ITT), which will slightly underestimate the treatment-on-the-treated (TOT) effect, since around 30 % of the treated students would not attend the same school as his/her older sibling, regardless of the closure.[11]

Against this background, I estimate the following model:

$$Grade\ rank_{ist} = \beta_1 Treatment_{ist} + \beta_2 B_{ist} + f_s + f_{tm} + \varepsilon_{ist} \quad (1)$$

Where $Grade\ rank_{ist}$ is the student achievement outcome[12] for a student i (unit of analysis) from a sibling pair s, belonging to cohort t. The variable $Treatment_{ist}$ denotes my indicator of a school closure. It is a dichotomous measure indicating whether the individ-

[10] The same figure for all sibling pairs in Sweden 2000–2016 (age difference 1–6 years) was 85 %. Both figures will slightly underestimate the number of siblings attending the same school since they are based on school codes and a few schools changed school codes over time.

[11] Another potential problem with this empirical approach is that it may also capture spillover effects within the affected families, making it more difficult to interpret the treatment effects found. For example, experiencing the months before a school closure could have negative disruptive effects on the older sibling which in turn affect both parents and younger siblings negatively. However, I argue that such spillover effects should be small in comparison to the disruptive effects affecting the treated younger sibling, who (in contrast to his/her older sibling) has to change schools, teachers and peers as a result of the school closure. Such effects should dominate over potential spillover effects from the older siblings and should be the main driver of the treatment effect.

[12] My analysis is not limited to this dependent variable: school quality variables are also used.

ual student was affected by a school closure (1) or not (0). Treated students are students whose older sibling attended a school that later was closed and who graduated after the closure. In addition to their older siblings, the study includes sibling pairs where neither of students was affected by closures to provide a larger variation in the dataset for birth order controls and time fixed effects. Since these additional control sibling pairs do not differ in treatment status and family fixed effects are used, they do not affect β_1 directly. Students attending schools who received students from closed schools were excluded from this group of students since they could be affected by the closures. B_{ist} is a birth order control equal to 1 for the younger siblings and 0 for the older siblings. f_s is family fixed effects for each sibling pair which are included to control for shared genetic and family environmental factors including education level of parents and immigrant background. Furthermore, since family fixed effects control for average differences between treated and untreated students, they also indirectly control for the characteristics of the closed school (e.g. it is not a problem for the model if the closed schools were lower performing than the average school). Since the education level of parents is among the most important factors that influence student achievement according to school research, the possibility to include family fixed effects is one of the main advantages of my empirical design focusing on sibling pairs. f_{tm} is cohort*municipality effects, based on the graduation year of the students and his/her municipality, which are used to control for variables that change over time within municipalities and that may influence student achievement and/or school quality. Examples are grade inflation (i.e. a tendency among schools to increase grades over time in order to attract students) and the increasing number of independent schools over the period studied. Standard errors are clustered at the school level (school of older siblings), allowing for the error terms within schools to be correlated.

If the treated students performed significantly better or worse than their older siblings (i.e. the effect of β_1 is significantly larger or smaller than zero), then we have found a causal effect of school closures on student performance, assuming that the student achievement and school quality differentials between the older sibling and the younger sibling on the closed schools would have been the same as for sibling pairs on comparable schools, in the absence of any intervention (closure). This assumption is tested using

pre-reform placebo tests presented after the main results, where younger students are assumed to be 'treated' in the years before the closure. The results are generalizable to all students assuming that the treatment effects are independent of (1) sibling birth order and (2) whether the students have siblings or not. Unfortunately, is not possible to test whether these assumptions hold using the data available. Yet, since only 10 % of school students in Sweden have no siblings (SCB 2018), the results should be generalizable to most students in the country.

4.2 Variables and data

Two dependent variables on grades are included in the analysis and they have their own benefits and drawbacks. Both are transformed into cohort percentile ranks to limit the effects of grade inflation on the results. In other words, each student is given a percentile rank based on how their grades or national test scores relate to the scores of all other students in Sweden the same year. *Grade Rank* consists of a 0–320 grade points scale based on all subjects transformed into a grade percentile rank between 0 and 1 where .51 correspond to a percentile rank that is above 50.99 % of all students in Sweden the graduation year. The dependent variable *National Test Rank* consists of a 0–9 points scale where 9 points correspond to the highest possible grades in the national tests on Swedish, math and English: A or MVG, that is transformed into a national test percentile rank. This variable has an advantage over grade rank in the sense that it should at least theoretically be less sensitive to grade inflation since the tests are designed nationally and the teachers are provided with guidelines when correcting them. However, compared to grade rank, the number of observations is lower since all students did not take the tests and the number of missing observations is larger. Furthermore, no data on national tests are available for the first three years of the period studied (2000, 2001 and 2002), making it impossible to conduct reliable pre-closure placebo tests on national test rank.

Four dependent variables on school quality are also included in the analysis. Positive effects of school closures are in the literature primarily assumed to be a result of displaced students and future cohorts attending schools with higher-performing peers and higher quality teachers (the school quality mechanism). By investigating the presence of

such effects, we could get an idea of the causal mechanisms behind the treatment. *Mean Grade Rank School* is my main measure of peer quality. It captures the average student percentile grade rank in the school (total grade rank of all students/number of students in the school). I use a variable based on grades instead of national test score since it generates a much larger number of observations. Assuming that the variable is not affected by grade inflation to a high degree, it should capture the average level of knowledge and skills among students in the school (here defined as peer quality). *Proportion Qualified Teachers* and *Years Experience Teachers* are my two variables on teacher quality. The former captures the proportion of teachers in the school with higher (university) education in teaching whereas the latter captures the average number of years the teachers in the schools have been teaching in their career. Measuring teacher experience is particularly important in the Swedish case since there has been a negative trend over time in the abilities of new teachers who graduate from teacher education (Grönqvist and Vlachos 2016). Finally, *Attends Independent School* captures whether or not the student attends an independent (1) or a public school (0). Unfortunately, it was not possible to include a reliable measure on the number of teachers per middle school student since I am not able to identify which student cohorts the teachers are working with. Furthermore, no data was available on the pedagogic quality of the teachers.

Data for the dependent variables on student achievement and school quality outcomes, and the control variables on graduation year, municipality and birth order, are collected from administrative registers from Statistics Sweden. The register includes data on all students who graduated from a Swedish middle school 2000–2016, including, graduation year, ninth year grades, ninth year national test results, socioeconomic data for their family members as well as data on school characteristics. The school closure data was collected through a triangulation of four main data sources. First, through two school closure projects, I compiled a preliminary list of proposed and implemented school closures in Sweden 2000–2012. The list was based on a thorough review of all newspapers in Sweden (see Uba 2016) and municipal documents from the 29 largest municipalities in Sweden (see Taghizadeh 2016). Second, to make sure that the middle schools really were closed and to capture the graduation years for the last cohorts, the preliminary list

was then compared with registry data from The Swedish National Agency for Education (Skolverket) who have historic data on all schools and how many students they had enrolled each year, and individual student registry data from SCB (Statistics Sweden). In this way, I could make sure that my closure dates (last cohort graduation years) were accurate and that no middle school students were enrolled in the schools after these dates. As a result, the proportion of students attending the same school as their older siblings is zero in the treatment group (see the summary statistics in the appendix).

5 Main results

5.1 Total effects of school closures

Table 4 presents the total effects of middle school closures on school quality and student achievement. The analysis includes treated students who graduated 1–6 years after the closure[13] so that the results are both affected by the disruption and the school quality mechanisms. More than half of the treated pairs graduated one or two years after the closure, meaning that they most likely attended the same middle school as his/her older sibling in seventh or eighth grade and may have been displaced. The rest of the treated students graduated three to six years after the closure, meaning that they were in third, fourth, fifth or sixth grade during the closure and most likely started middle school in a different school.

[13]The 6-year limit was necessary for the placebo analyses.

Table 4: Effects of school closures on school quality and student achievement among displaced students and future cohorts, treated students graduating 1–6 years after closure, age-difference siblings 1–6 years

	Mean Grade Rank School	Attends Independent School	Proportion Qualified Teachers School	Years Experience Teachers School	Grade Rank Student	National Test Rank Student
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	.019***	.137***	-.027**	-1.67***	-.003	.007
	(.007)	(.015)	(.011)	(.391)	(.008)	(.010)
Constant	.434***	.034	.731***	13.0***	.903***	.400***
	(.021)	(.133)	(.033)	(1.70)	(.061)	(.078)
Adj. R-squared	.219	.100	.328	.347	.035	.028
Closed Schools	97	97	97	97	97	94
Treat Obs	4 760	4 760	3 369	3 369	4 760	3 376
Total Obs	325 738	325 738	312 960	312 960	325 738	252 524
Sibling Order Controls	X	X	X	X	X	X
Family F.E.	X	X	X	X	X	X
Municip*Grad Year F.E.	X	X	X	X	X	X

Standard errors in parentheses are clustered at the school level (school of older sibling)
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

We start with the effects on school quality presented in models 1, 3 and 4. The treatment effects are statistically significant below the one percent level in two models and below the five percent level in one. The treated students graduated from schools with a 2 percentile higher mean grade rank than the closed schools. Hence, the school closures had a positive effect on displaced students and future cohorts in terms of peer quality, assuming that grades reflect knowledge and skills among students. However, the treatment effects in terms of school quality were not only positive. The treated students graduated from schools with -2.7 percentage points fewer teachers with higher education in teaching and -1.67 years less experienced teachers, than the closed schools. The treated students were also 14 percentage points more likely to attend independent schools than their older siblings (model 2) and around 8.6 percentage points more likely to do so than similar younger students who graduated from the schools before they were closed (cf. *Table 7*). The higher likelihood of attending an independent school can party explain the negative effects on teacher quality since such schools on average had a lower proportion of educated teachers (.669) and less experienced teachers (9.44 years) compared to the public schools (.837, 14.6).

The effects on student achievement are presented in models 5–6. The treatment effects are statistically insignificant in both models and the coefficients are close to zero. Taken together, the results suggest that the school closures in Sweden overall had no effects on student achievement (displaced students and future cohorts). A potential explanation can be found in the school quality models. Although the school closures resulted in displaced students and future cohorts attending somewhat better schools compared to their older siblings in terms of a higher school mean grade rank, the peer quality increase might have been too small to compensate for negative disruption effects on displaced students. Furthermore, teacher quality, as measured in experience and education, was clearly worse in the receiving schools, also neutralizing potential positive peer effects.

5.2 Effects of school closures on displaced students

In *Table 5* below, I try to isolate the effects of middle school closures on school quality and student achievement among displaced students by only analyzing treated students who

graduated 1–2 years after the closure. This implies that the younger treated siblings were in seventh or eighth grade when their older siblings graduated from the middle school (ninth grade) and it was closed. A large majority of the closed schools had students in seventh grade (79.4%) and eighth grade (90.7%) at the time of closure. Since these students started middle school and were displaced before their graduation, the treatment effects here should mainly be generated by the disruption mechanism.

Table 5: Effects of school closures on school quality and student achievement among displaced students, treated students graduating 1–2 years after closure, age-difference siblings 1–6 years

	Mean Grade Rank School	Attends Independent School	Proportion Qualified Teachers School	Years Experience Teachers School	Grade Rank Student	National Test Rank Student
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	.016**	.136***	-.020*	-1.33***	.002	.004
	(.007)	(.015)	(.010)	(.420)	(.009)	(.011)
Constant	.435***	.075	.731***	13.1***	.912***	.412***
	(.020)	(.140)	(.035)	(1.67)	(.066)	(.085)
Adj. R-squared	.226	.099	.325	.277	.035	.028
Closed Schools	97	97	95	95	97	93
Treat Obs	2 834	2 834	2 210	2 210	2 834	2 012
Total Obs	287 194	287 194	276 282	276 282	287 194	221 512
Sibling Order Controls	X	X	X	X	X	X
Family F.E.	X	X	X	X	X	X
Munic*Year F.E.	X	X	X	X	X	X

Standard errors in parentheses are clustered at the school level (school of older sibling)
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The effects on school quality are presented in models 1, 2 and 4. The results are very similar to those in *Table 4* and the treatment effects are statistically significant below the five percent level with one exception (model 3). Similarly, the treatment effects on grade rank and national test rank are again not statistically significant. The results support the interpretation in the previous section. Again, it seems that the disruption and the school quality mechanisms had very small effects on the students and/or may have neutralized each other. Focusing on students who were more likely to be negatively affected, since many of them were displaced from their middle school as a result the closure, does not change this interpretation.

5.3 Effects of school closures on future cohorts

In *Table 6* below, I try to isolate the effects of middle school closures on school quality and student achievement among future cohorts by focusing on treated students who graduated 3–6 years after the closure. This implies that the treated younger siblings were in third, fourth, fifth or sixth grade and most likely had not started middle school when it was closed.

I argue that the treatment effects here should mainly be generated by the school quality mechanism and not by the disruption mechanism. While around 60 % of the closed middle schools were part of grade 1–9 schools, and we would expect some disruption on the younger siblings (since they are forced to change elementary school), such schools usually house middle school students in separate buildings and school choice tends to change the student compositions in middle school. In other words, most of the students would change classroom, teachers and/or peers in grade 6 or 7 regardless of the closures. Furthermore, previous research on displaced students suggest that school closures have limited disruptive effects on student achievement after three years (when the younger siblings graduated from middle school and their grades were measured).[14]

[14] Another possible cause of disruption would be the possibility that some families move to other parts of the cities as a result of the closures. However, the families affected by closures were only 4.7 % more likely to move (change neighborhood) compared to the non-affected families (treat siblings: 15.8 % control siblings 11.1 %.)

Table 6: Effects of school closures on school quality and student achievement among future cohorts , treated students graduating 3–6 years after closure, age-difference siblings 3–6 years

	Mean Grade Rank School	Attends Independent School	Proportion Qualified Teachers School	Years Experience Teachers School	Grade Rank Student	National Test Rank Student
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	.022***	.146***	-.040***	-2.46***	-.005	.019
	(.008)	(.020)	(.015)	(.409)	(.014)	(.014)
Constant	.410***	-.027	.720***	13.3***	.338***	.418***
	(.033)	(.104)	(.034)	(1.65)	(.095)	(.074)
Adj. R-squared	.220	.099	.358	.302	.042	.036
Closed Schools	95	95	94	94	95	94
Treat Obs	1 926	1 926	1 159	1 159	1 926	1 364
Total Obs	184 608	184 608	176 164	176 164	184 608	140 534
Sibling Order Controls	X	X	X	X	X	X
Family F.E.	X	X	X	X	X	X
Municip*Grad Year F.E.	X	X	X	X	X	X

Standard errors in parentheses are clustered at the school level (school of older sibling)
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The treatment effects on school quality are statistically significant in all models. However, in this subsample, the positive effects on mean grade rank school and the negative effects on teacher quality are somewhat larger. We also see a slightly larger likelihood of attending an independent school among treated students in this subsample (model 3). Looking at student achievement, we see a relatively large positive effect on national test grade rank compared to the estimates in the other tables. However, none of the two measures on student achievement are statistically significant. Again, it seems that the positive peer quality effects and the negative teacher quality effects may have neutralized each other, resulting in limited effects on student results. Furthermore, since the effects on student achievement among future cohorts were so similar to those on displaced students, it seems that the disruption mechanism overall had a limited effect on students.

5.4 Placebo effects

In tables 7–9, I present pre-closure placebo estimates, where the younger siblings are assumed to be treated 1–6 years before the actual closure, and older siblings are untreated. This provides a contra-factual illustration of what would have happened to students in similar schools in the absence of a closure. If the statistically significant treatment effects found are not reflected in the placebo tests and the placebo estimates are close to zero, we can be more confident to interpret them as causal effects.

Schools threatened by closure are not perfectly comparable to other schools as pre-closure cohorts might have been informed about the upcoming closure or heard rumors about it. Such students may be more likely to choose an independent school to avoid potential disruption of an upcoming closure. Secondly, since the threatened schools saw negative enrollment trends the years before the closure, there might have been school quality concerns over the situation in the school motivating students to choose other schools. [15] The placebo analysis is informative in understanding the trends of cohorts graduating before the closure, and to what extent they pose a problem for a causal interpretation.

[15] Another potential problem is the possibility that families have more information and school experience when they choose a middle school for their younger children compared to his/her older siblings. However, this problem should be handled here through the inclusion of control sibling pairs.

Table 7: Pre-closure placebo effects on displaced students and future cohorts (Table 4), treated students graduating 1–6 year before closure, age-difference siblings 1–6 years

	Mean Grade Rank School	Attends Independent School	Proportion Qualified Teachers School	Years Experience Teachers School	Grade Rank Student
	(1)	(2)	(3)	(4)	(5)
Treatment	.001	.051***	.013**	-.018	.001
	(.004)	(.009)	(.006)	(.204)	(.007)
Constant	.416***	.045	.687***	12.1***	.756***
	(.032)	(.128)	(.034)	(1.58)	(.100)
Adj. R-squared	.245	.089	.362	.284	.038
Closed Schools	95	95	91	91	95
Treat Obs	4 670	4 670	3 308	3 308	4 670
Total Obs	224 236	224 236	216 962	216 962	224 236
Sibling Order Controls	X	X	X	X	X
Family F.E.	X	X	X	X	X
Municip*Grad Year F.E.	X	X	X	X	X

Standard errors in parentheses are clustered at the school level (school of older sibling)
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 8: Pre-closure placebo effects on displaced students (table 5), treated students graduating 1–2 years before closure, age-difference siblings 1–6 years

	Mean Grade Rank School	Attends Independent School	Proportion Qualified Teachers School	Years Experience Teachers School	Grade Rank Student
	(1)	(2)	(3)	(4)	(5)
Treatment	-.004	.063***	.013	.156	-.006
	(.006)	(.012)	(.011)	(.333)	(.011)
Constant	.417***	.063	.687***	12.4***	.791***
	(.035)	(.141)	(.036)	(1.62)	(.039)
Adj. R-squared	.245	.089	.363	.286	.039
Closed Schools	91	91	91	91	91
Treat Obs	2 366	2 366	1 687	1 687	2 366
Total Obs	216 912	216 912	210 114	210 114	216 912
Sibling Order Controls	X	X	X	X	X
Family F.E.	X	X	X	X	X
Municip*Grad year F.E.	X	X	X	X	X

Standard errors in parentheses are clustered at the school level (school of older sibling)
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 9: Pre-closure placebo effects on future cohorts (table 6), treated students graduating 1–4 years before closure, age-difference siblings 3–6 years

	Mean Grade Rank School	Attends Independent School	Proportion Qualified Teachers School	Years Experience Teachers School	Grade Rank Student
	(1)	(2)	(3)	(4)	(5)
Treatment	.001	.073***	.024**	.483	.010
	(.008)	(.019)	(.011)	(.349)	(.016)
Constant	.419***	.090	.702***	12.0***	.778***
	(.034)	(.131)	(.044)	(1.64)	(.154)
Adj. R-squared	.263	.103	.416	.303	.053
Closed Schools	80	80	78	78	80
Treat Obs	1 621	1 621	1 383	1 383	1 621
Total Obs	97 642	97 642	94 700	94 700	97 642
Sibling Order Controls	X	X	X	X	X
Family F.E.	X	X	X	X	X
Municip*Grad Year F.E.	X	X	X	X	X

Standard errors in parentheses are clustered at the school level (school of older sibling)
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Looking at the placebo estimates, statistically significant effects are found on two dependent variables: attending an independent school and qualifications of teachers. The placebo effects are two to three times smaller than the real treatment effects and the effects on qualifications of teachers are positive unlike the negative treatment effects.

The effects on attending an independent school are statistically significant at the one percent level in all three tables and are robust over many different specifications including matching models based on the characteristics of the school attended by the older sibling, its enrollment trends and the number of independent schools in the municipality. It hence seems like even before the closure, students were leaving the closing schools for the independent school sector to a higher extent than in non-closing schools. The effects on the qualifications of teachers, on the other hand, are not as robust. Firstly, statistically significant effects are only found in two of the three tables and they are only significant at the 5 percent level. Secondly, in contrast to the real treatment effects, the placebo effects are not statistically significant at all if matching is used. [16]

What do these statistically significant placebo effects say about the treatment effects found in previous tables? On the one hand, one can argue that the mere existence of significant placebo effects i.e. non-parallel trends, makes it difficult for us to interpret the treatment effects found in the previous tables causally. On the other hand, no robust statistically significant placebo effects on school quality were found and no statistically significant placebo effects on student educational achievement at all, indicating that the pre-closure outflow of students to independent schools did not affect student performance. This speaks in favor of a causal interpretation of the baseline effects on student grades, as the grades of older untreated siblings were not affected by upcoming closures. While one should be careful when interpreting the treatment effects found, the results from the placebo models do not rule out that they can be interpreted as causal effects of the school closures.

[16] Matching treated sibling pairs with control pairs based on qualifications of teachers in the older siblings school using coarsened exact matching (Iacus et al. 2012) or entropy balancing (Hainmueller 2012). The matching results are available from the author if requested.

6 Heterogeneous effects

6.1 Total effects of closing low-performing and high-performing schools

The analyses presented in the previous sections found small and non-statistically significant effects of the school closures in Sweden on student achievement among the treated students. In this section, I try to isolate positive and negative school quality effects as well as effects on student achievement by analyzing the effects of closing down low-performing and high-performing schools on student achievement. Based on previous studies on school closures (Brummet 2014; Carlson & Lavertu 2015, 2016; De la Torre & Gwynne 2009) and studies on peer effects (Sacerdote 2011), positive effects of school closures on student achievement are most likely to be found when closing down low-performing schools whereas negative effects are most likely to be found when closing down high-performing schools. I therefore focus on sibling pairs where the older sibling graduated from these two types of schools. In *Table 10* and *Table 11* , I present the effects on students whose older siblings graduated from schools which at the time of graduation were among the 25 % worst performing schools in the dataset and the 25 % highest performing schools. The age difference between the siblings is set to 1–6 years to maximize the number of observations and to capture the full effects (displaced+future cohorts) of the school closures.

Table 10: Effects of closing down low-performing schools (bottom 25 %) on school quality and student achievement, treated students graduating 1–6 years after closure, age-difference siblings 1–6 years

	Mean Grade Rank School	Attends Independent School	Proportion Qualified Teachers School	Years Experience Teachers School	Grade Rank Student	National Test Rank Student
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	-.002	.088***	-.038	-3.13***	-.058	.048**
	(.029)	(.032)	(.029)	(.676)	(.039)	(.018)
Constant	.285***	.165	.630***	12.9***	-.282**	.455***
	(.069)	(.113)	(.054)	(1.57)	(.116)	(.117)
Adj. R-squared	.628	.250	.528	.539	.155	115
Closed Schools	52	52	47	47	52	49
Treat Obs	1 098	1 098	752	752	1 098	661
Total Obs	32 692	32 692	30 868	30 868	32 692	22 220
Sibling Order Controls	X	X	X	X	X	X
Family F.E.	X	X	X	X	X	X
Munic*Year F.E.	X	X	X	X	X	X

Standard errors in parentheses are clustered at the school level (school of older sibling)
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 11: Effects of closing down high-performing schools (top 25 %) on school quality and student achievement, treated students graduating 1–6 years after closure, age-difference siblings 1–6 years

	Mean Grade Rank School	Attends Independent School	Proportion Qualified Teachers School	Years Experience Teachers School	Grade Rank Student	National Test Rank Student
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	-.023***	.127***	-.021	-.400	-.010	-.043***
	(.006)	(.022)	(.025)	(1.01)	(.015)	(.017)
Constant	.715***	.178	.683***	11.8***	1.04***	.554***
	(.035)	(.137)	(.030)	(1.75)	(.131)	(.079)
Adj. R-squared	.589	.236	.324	.545	.534	0.113
Closed Schools	57	57	49	49	57	50
Treat Obs	1 024	1 024	709	709	1 024	852
Total Obs	125 604	125 604	127 668	127 668	125 604	100 554
Sibling Order Controls	X	X	X	X	X	X
Family F.E.	X	X	X	X	X	X
Munic*Year F.E.	X	X	X	X	X	X

Standard errors in parentheses are clustered at the school level (school of older sibling)
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

As we can see in the tables, the effects on peer quality are only statistically significant on treated students whose older siblings attended relatively high-performing schools (*Table 11*, model 1). Having an older sibling who graduated from a high-performing school that was closed, resulted in attending a school with a 2.3 percentile lower mean school grade rank. When it comes to teacher qualifications and experience, few effects are statistically significant. The only exception being a rather large negative effect on teacher experience (-3.13 years) among students whose older siblings attended relatively low-performing schools. We also see positive statistically significant effects on attending an independent school for both treated groups however the effect is much larger for students whose older siblings attended relatively high-performing schools.

The effects on grade rank are not statistically significant in any of the tables (model 5). However, the effects on national test rank are statistically significant in both cases (model 6). As expected, the effect on national test rank is positive when it comes to students displaced from low-performing schools and negative when it comes to students displaced from high-performing schools. Since only one of the two student achievement variables is statistically significant in the tables, we cannot draw any safe conclusions when it comes to the academic effects of closing down low and high performing schools. However, the combination of statistically significant negative effects on the one percent level on both mean grade rank school and national test rank, and a negative coefficient on grade rank, suggest that causal effects on student achievement are most likely to be found on students displaced from high-performing schools and that these effects are most likely to be negative. In the case of closing high-performing schools, the disruption mechanism and the school quality mechanism (at least in terms of peer quality) go in the same negative directions and may reinforce each other, resulting in negative effects on student achievement.

Placebo effects for this analysis are presented in tables 19–20 in the appendix and the results are largely similar to those in section 5.4, tables 7–9. The independent school variable remains statistically significant in both models while the statistically significant effect found on qualification on teachers disappears (and is replaced by an effect on teacher experience in *Table 20*, that disappears if matching is used).

7 Effects on receiving schools

To capture the full effect of school closures, we must not only analyze the effects on both displaced students and future cohorts, but also spillover effects on students in receiving schools. Hence, in *Table 12*, I present spillover treatment effects on students in schools who received students from closed middle schools. The treated students here consist of students who graduated from the middle school the year it received students from a closed middle school, which are compared with their older siblings who graduated 1–6 years prior. The age difference between the siblings is here again 1–6 years.

Table 12: Effects on students in receiving schools, treated students graduating from receiving schools 1 year after closure, age-difference siblings 1–6 years

	Mean Grade Rank School	Attends Independent School	Proportion Qualified Teachers School	Years Experience Teachers School	Grade Rank Student	National Test Rank Student
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	-.005	.020**	-.010	-.339	-.007	.001
	(.010)	(.010)	(.008)	(.311)	(.011)	(.011)
Constant	.629***	.189	.646***	9.74***	.743***	.275**
	(.053)	(.155)	(.063)	(2.12)	(.149)	(.117)
Adj. R-squared	.262	.099	.359	.289	.041	.035
Receiving Schools	296	296	289	289	296	279
Treat Obs	7 168	7 168	6 906	6 906	7 168	5 502
Total Obs	178 172	178 172	174 414	174 414	178 172	138 432
Sibling Order Controls	X	X	X	X	X	X
Family F.E.	X	X	X	X	X	X
Municip*Grad Year F.E.	X	X	X	X	X	X

Standard errors in parentheses are clustered at the school level (school of older sibling)
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Only one statistically significant treatment spillover effect is found in *Table 12*, on attending an independent school. The effect is statistically significant on the 0.05 level and is very small, corresponding to a two percentage points increase in the likelihood of attending an independent school. Since the effect is very small and no statistically significant effects are found on school quality or student achievement, it seems that school closures overall had no effects on students in receiving schools. This conclusion is supported by the placebo analysis (see *Table 21* in the appendix), where I find a similarly sized effect on attending an independent school, although it is only statistically significant at the 10 percent level and disappears if matching is used.

8 Conclusions

The purpose of this study was to investigate the effects of school closures on student achievement among displaced students and future cohorts in Sweden. First, since previous quantitative studies to my knowledge only study the effects on displaced students, it was motivated to study how the results differ if we also take account of the effects on subsequent cohorts of students whose education is not disrupted by the closures. Second, since almost all previous studies focus on the US, it was motivated to analyze whether their results were generalizable to other countries. More specifically, this study analyzes whether the results from the US hold for countries with extensive voucher systems, such as Sweden, where there are theoretical reasons to expect more positive peer and school quality effects from school closures.

The effects were analyzed using a quasi-experimental study on all middle school closures in Sweden 2000–2012 focusing on sibling pairs. The performance of students who graduated from closed middle schools was compared with the performance of their younger siblings who were expected to graduate from the same schools but as a result of the closure attended other schools. This design provided me with better possibilities to control for genetic and family environmental factors, including education level of parents and immigrant background, compared to previous studies on school closures. The design was tested using pre-closure placebo analyses where the younger siblings were treated

before the closure. As expected, most of the placebo models generated statistically insignificant effects. However, small but robust statistically significant placebo effects were found on attending independent schools in all placebo analyses. While these effects were expected given that students have strong incentives to move to other schools also before a closure, the reader should have them in mind when interpreting the results (see section 5.4 for a more in-depth discussion on the statistically significant placebo effects and how they could be interpreted).

Assuming that the treatment effects found in my results can be interpreted as causal effects, my results have several implications for the literature on school closures. Firstly, the results suggest that school closures need to generate relatively large peer effects in order to have a positive impact on student achievement among affected students. The school closures in Sweden could have generated positive peer effects since displaced students and future cohorts attended schools with a slightly higher student mean grade rank than the closed schools and the presence of higher-performing peers could have affected their learning trajectories positively. While the positive effects on school mean grade rank were small, they were statistically significant in all models and were not found in the placebo analyses. Hence, they could be interpreted as causal effects of the closures. However, despite this, the school closures had no effects overall on student achievement. These results, in addition to the negative effects on student achievement found in many previous studies, suggest that negative disruption effects can easily neutralize small positive peer effects, assuming such effects existed in the Swedish case.

Secondly, my study suggests that school closure researchers should not only take peer effects into account when analyzing school quality outcomes of school closures. For the first time in this small literature, my study provided estimates on teacher quality outcomes of the school closures. Since the effects on teacher experience and education levels for displaced students and future cohorts were relatively large and mainly negative, they could also have neutralized potential positive peer effects, generating treatment effects on student achievement close to zero.

Thirdly, my results suggest that the effects on school closures on displaced students are very similar to the effects on future cohort who do not experience disruption. This result

is surprising given that, theoretically speaking, achievement benefits should be larger for cohorts of students who never experience the disruption of the move, and may begin their education in higher-performing middle schools. Contrary to theoretical expectations, my results suggest that the disruption mechanism has limited negative effect on students. If such negative effects were substantial, we should see a larger difference between these two groups of students when it comes to student achievement. However, it is clear that we need more studies to draw any safe conclusions regarding the impact on future cohorts since this study is the first of its kind. In particular, we need studies with more students included and where the closures resulted in larger school quality increases than witnessed in this study.

Finally, my study does not rule out that the effects on student achievement found in previous American studies are generalizable to countries with extensive voucher systems, such as Sweden, to some degree. A good comparison here is the largest study in previous research (Brummet 2014) where the school closures (like in Sweden) was caused by declining enrollment trends and where student performance was not a decisive factor when choosing which schools to close. Similar to that study, few statistically significant effects on student achievement were found in the Swedish case and the effects were not more positive, despite the theoretical expectations.[17] My results show that the extensive voucher system found in Sweden does not seem to direct students to higher quality schools after school closures. While many students affected by the closures took the opportunity to leave the public sector for independent schools after the closure, such schools have on average lower educated and less experienced teachers according to my analyses. Furthermore, the system did not result in displaced students and future cohorts attending schools that were above the average Swedish school in terms of student achievement (peer quality). This result could partly be explained by previous studies on school choice in Sweden which suggest that the type students affected by the closures (i.e. students with relatively

[17] If grade inflation is larger in independent schools than in public schools in Sweden, the effects of the school closures on student achievement among affected students who left the public school sector could even be negative. However, if this was the case we should see a larger difference in student achievement between displaced students (who are more negatively affected overall by disruption) and future cohorts (who are less affected).

low educated parents and immigrant parents, see *Table 2*, are less informed, less likely to utilize school choice and less likely to choose independent schools (Anderson et al. 2012; Böhlmark et al. 2016; Holmlund 2016). They may therefore have been less likely to choose a high quality independent school after the closure, even though the school choice system provided them with more choice opportunities. Furthermore, even if they wanted to choose such schools, selection criteria such as waiting list (by date of application) and sibling priority, may have constituted a barrier. Future studies could explore these issues more in depth and analyze under what circumstances school choice could amplify positive academic effects of school closures.

The study also has important implications for policy-makers who close down schools. When schools are mainly closed as a direct result of declining enrollments, like in Sweden and in most other countries, the total effect (displaced students+future cohorts+students in receiving schools) on student achievement may very well be zero. Negative effects are most likely to be found when closing high-performing schools. In those cases, both the school quality mechanism and the disruption mechanism goes in the same negative direction and may reinforce each other. In line with this, previous studies have shown that closures of high performing schools are more likely to have negative effects on student achievement among displaced students (cf. Brummet 2014). This was also true in my study; statistically significant negative effects on national test rank was found when looking at the closures of the 25 % best performing schools, although one should be careful when interpreting this result given the low number of treated students in these analyses. Similarly, it seems that positive academic effects school closure processes are most likely to be found when closing down relatively low-performing schools. In such processes, positive peer and teacher quality effects for displaced students are more likely to beat or at least cancel out the negative disruption effects.

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9 Appendix

9.1 Summary statistics: treatment models

Table 13: Summary statistics for displaced students and future cohorts (Table 4), treated students graduating 1–6 years after closure, 1–6 year age difference between siblings

	Older sibling attend closed sch		Older sibling attend non-closed sch		Total
	Older	Younger	Older	Younger	
Student grade variables					
Grade rank	.466	.438	.516	.487	.500
	(.288)	(.281)	(.284)	(.278)	(.281)
National test rank*	.407	.397	.449	.433	.440
	(.292)	(.286)	(.295)	(.286)	(.291)
School level variables					
Mean grade rank school	.444	.463	.485	.486	.484
	(.082)	(.089)	(.092)	(.092)	(.092)
Attends independent school	0	.155	.073	.088	.081
	(0)	(.362)	(.261)	(.284)	(.273)
Prop Qualified Teachers*	.819	.813	.827	.840	.833
	(.106)	(.120)	(.106)	(.103)	(.105)
Years exp teachers*	15.4	13.7	14.6	14.6	14.6
	(3.70)	(3.71)	(3.36)	(3.20)	(3.30)
Younger sibling	0	1	0	1	.500
	(0)	(0)	(0)	(0)	(.500)
Graduation year	2007.4	2011.1 .	2007.9	2010.9	2009.4
	(2.19)	(2.27)	(2.72)	(2.86)	(3.17)
Number of students	4 760	4 760	158 109	158 109	325 738
Prop siblings attend same school	0	0	.653	.653	.634

Mean coefficients; sd in parentheses; *Subset of main dataset used

Table 14: Summary statistics for displaced students (Table 5), treated students graduating 1–2 years after closure, 1–6 year age difference between siblings

	Older sibling attend closed sch		Older sibling attend non-closed sch		Total
	Older	Younger	Older	Younger	
Student grade variables					
Grade rank	.476	.441	.518	.487	.502
	(.288)	(.281)	(.283)	(.278)	(0.281)
National test rank*	.419	.392	.450	.431	.440
	(.292)	(.283)	(.295)	(.287)	(.291)
School level variables					
Mean grade rank school	.449	.441	.518	.487	.502
	(.081)	(.085)	(.092)	(.091)	(.091)
Attends independent school	0	.145	.070	.083	.076
	(0)	(.352)	(.255)	(.276)	(.266)
Prop Qualified Teachers*	.813	.817	.824	.842	.833
	(.104)	(.118)	(.103)	(.101)	(.103)
Years exp teachers*	15.2	13.9.	14.7	14.7	14.7
	(3.63)	(3.77)	(3.23)	(3.20)	(3.22)
Younger sibling	0	1	0	1	.500
	(0)	(0)	(0)	(0)	(.500)
Graduation year	2007.2	2010.2	2007.5	2010.3	2008.9
	(2.15)	(1.90)	(2.60)	(2.53)	(2.93
Number of students	2 834	2 834	140 763	140 763	287 194
Prop siblings attend same school	0	0	.734	.734	.719

Mean coefficients; sd in parentheses; *Subset of main dataset used

Table 15: Summary statistics for future cohorts (Table 6), treated students graduating 3–6 years after closure, 3–6 year age difference between siblings

	Older sibling attend closed sch		Older sibling attend non-closed sch		Total
	Older	Younger	Older	Younger	
Student grade variables					
Grade rank	.452	.434	.509	.484	.496
	(.286)	(.281)	(.284)	(.277)	(.280)
National test rank*	.388	.403	.440	.433	.436
	(.289)	(.291)	(.294)	(.285)	(.290)
School level variables					
Mean grade rank school	.437	.434	.509	.484	.496
	(.083)	(.094)	(.092)	(.091)	(.092)
Attends independent school	0	.170	.071	.091	.081
	(0)	(.376)	(.256)	(.287)	(.272)
Prop Qualified Teachers*	.831	.804	.826	.841	.833
	(.107)	(.124)	(.103)	(.103)	(.104)
Years exp teachers*	15.9 .	13.2	14.7	14.5	14.6
	(3.80)	(3.53)	(3.23)	(3.17)	(3.21)
Younger sibling	0	1	0	1	.500
	(0)	(0)	(0)	(0)	(.500)
Graduation year	2007.7	2012.4	2007.7	2011.7	2009.7
	(2.22)	(2.18)	(2.82)	(2.83)	(3.44)
Number of students	1 926	1 926	90 378	90 378	184 608
Prop siblings attend same school	0	0	.548	.548	.537

Mean coefficients; sd in parentheses; *Subset of main dataset used

9.2 Summary statistics: placebo models

Table 16: Summary statistics for placebo displaced students and future cohorts (Table 7), treated students graduating 1–6 years before closure, 1–6 year age difference between siblings

	Older sibling attend closed sch		Older sibling attend non-closed sch		Total
	Older	Younger	Older	Younger	
Student grade variables					
Grade rank	.478	.450	.517	.484	.499
	(.284)	(.279)	(.283)	(.278)	(.281)
School level variables					
Mean grade rank school	.451	.456	.485	.483	.483
	(.079)	(.085)	(.091)	(.090)	(.090)
Attends independent school	0	.058	.060	.072	.065
	(0)	(.235)	(.238)	(.260)	(.247)
Prop Qualified Teachers*	.775	.813	.816	.840	.827
	(.107)	(.119)	(.103)	(.099)	(.103)
Years exp teachers*	14.25	14.67	14.69	14.86	14.77
	(2.95)	(3.99)	(3.21)	(3.19)	(3.21)
Younger sibling	0	1	0	1	.500
	(0)	(0)	(0)	(0)	(.500)
Graduation year	2005.2	2007.9	2006.5	2009.2	2007.8
	(2.18)	(1.92)	(2.32)	(2.15)	(2.63)
Number of students	4 670	4 670	107 448	107 448	224 236
Prop siblings attend same school	.684	.684	.851	.851	.844

Mean coefficients; sd in parentheses; *Subset of main dataset used

Table 17: Summary statistics for placebo displaced students (Table 8), treated students graduating 1–2 years before closure, 1–6 year age difference between siblings

	Older sibling attend closed sch		Older sibling attend non-closed sch		Total
	Older	Younger	Older	Younger	
Student grade variables					
Grade rank	.483	.454	.518	.484	.500
	(.286)	(.279)	(.282)	(.277)	(.281)
School level variables					
Mean grade rank school	.453	.459	.485	.483	.484
	(.077)	(.090)	(.091)	(.090)	(.090)
Attends independent school	0	.072	.060	.072	.065
	(0)	(.258)	(.237)	(.258)	(.247)
Prop Qualified Teachers*	.793	.830	.815	.840	.828
	(.103)	(.123)	(.103)	(.099)	(.102)
Years exp teachers*	14.5	15.0	14.7	14.9	14.8
	(2.99)	(4.52)	(3.21)	(3.18)	(3.21)
Younger sibling	0	1	0	1	.500
	(0)	(0)	(0)	(0)	(.500)
Graduation year	2005.9	2008.8	2006.4	2009.2	2007.8
	(1.99)	(1.66)	(2.28)	(2.13)	(2.59)
Number of students	2 366	2 366	106 090	106 090	216 912
Prop siblings attend same school	.634	.634	.851	.851	.846

Mean coefficients; sd in parentheses; *Subset of main dataset used

Table 18: Summary statistics for placebo treated future cohorts (Table 9), treated students graduating 1–4 years before closure, 3–6 year age difference between siblings

	Older sibling attend closed sch		Older sibling attend non-closed sch		Total
	Older	Younger	Older	Younger	
Student grade variables					
Grade rank	.459	.439	.507	.480	.492
	(.282)	(.277)	(.281)	(.276)	(.279)
School level variables					
Mean grade rank school	.451	.455	.484	.483	.483
	(.081)	(.084)	(.090)	(.088)	(.089)
Attends independent school	0	.065	.051	.067	.058
	(0)	(.247)	(.219)	(.250)	(.234)
Prop Qualified Teachers*	.760	.820	.806	.842	.823
	(.109)	(.115)	(.104)	(.097)	(.103)
Years exp teachers*	14.0.	14.9.	14.6	14.9.	14.8
	(2.78)	(3.94)	(3.15)	(3.15)	(3.17)
Younger sibling	0	1	0	1	.500
	(0)	(0)	(0)	(0)	(.500)
Graduation year	2004.1	2008.1	2005.4	2009.2	2007.3
	(1.81)	(1.69)	(2.05)	(2.00)	(2.80)
Number of students	1 621	1 621	47 200	47 200	97 642
Prop siblings attend same school	.632	.632	.817	.817	.811

Mean coefficients; sd in parentheses; *Subset of main dataset used

9.3 Placebo models for high-performing and low-performing schools, and for receiving schools

Table 19: Pre-closure placebo effects of closing down low-performing schools (bottom 25 percentile) on school quality and student achievement, treated students graduating 1–6 years before closure, age-difference siblings 1–6 years

	Mean Grade Rank School	Attends Independent School	Proportion Qualified Teachers School	Years Experience Teachers School	Grade Rank Student
	(1)	(2)	(3)	(4)	(5)
Treatment	.003	.030**	.023	-.389	-.031
	(.017)	(.014)	(.017)	(.296)	(.023)
Constant	.287***	.220**	.704***	11.8***	-.044
	(.041)	(.099)	(.037)	(1.12)	(.101)
Adj. R-squared	.600	.261	.520	.476	.148
Closed Schools	60	60	54	54	60
Treat Obs	1 048	1 048	864	864	1 048
Total Obs	21 884	21 884	19 618	19 618	21 884
Sibling Order Controls	X	X	X	X	X
Family F.E.	X	X	X	X	X
Municip*Grad Year F.E.	X	X	X	X	X

Standard errors in parentheses are clustered at the school level (school of older sibling)
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 20: Pre-closure placebo effects of closing down high-performing schools (top 25 percentile) on school quality and student achievement, treated students graduating 1–6 years before closure, age-difference siblings 1–6 years

	Mean Grade Rank School	Attends Independent School	Proportion Qualified Teachers School	Years Experience Teachers School	Grade Rank Student
	(1)	(2)	(3)	(4)	(5)
Treatment	-.010	.068***	.016	.995***	-.011
	(.010)	(.019)	(.010)	(.368)	(.013)
Constant	.733***	.090	.641***	11.9***	1.07***
	(.042)	(.145)	(.048)	(2.15)	(.135)
Adj. R-squared	.421	.087	.366	.307	.081
Closed Schools	56	56	55	55	56
Treat Obs	954	954	975	975	954
Total Obs	87 304	87 304	85 920	85 920	87 304
Sibling Order Controls	X	X	X	X	X
Family F.E.	X	X	X	X	X
Municip*Grad Year F.E.	X	X	X	X	X

Standard errors in parentheses are clustered at the school level (school of older sibling)
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 21: Placebo effects on students in receiving schools, treated students graduating from receiving schools 1 year before closure, age-difference siblings 1–6 years

	Mean Grade Rank School	Attends Independent School	Proportion Qualified Teachers School	Years Experience Teachers School	Grade Rank Student	National Test Rank Student
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	-.004	.016*	.014*	-.052	-.006	-.004
	(.005)	(.008)	(.007)	(.217)	(.007)	(.008)
Constant	.428***	-.070	.741***	13.6***	.956***	.467***
	(.025)	(.123)	(.040)	(1.60)	(.084)	(.107)
Adj. R-squared	.238	.088	.352	.274	.038	.031
Receiving Schools	249	249	249	249	249	238
Treat Obs	5 359	5 559	4 167	4 167	5 359	4 035
Total Obs	222 276	222 276	214 996	214 996	222 276	167 576
Sibling Order Controls	X	X	X	X	X	X
Family F.E.	X	X	X	X	X	X
Municip*Grad Year F.E.	X	X	X	X	X	X

Standard errors in parentheses are clustered at the school level (school of older sibling)
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$