Economics of Education Review 77 (2020) 102009

Contents lists available at ScienceDirect

Economics of Education Review

journal homepage: www.elsevier.com/locate/econedurev

The impact of banning mobile phones in Swedish secondary schools

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ARTICLE INFO

JEL codes:
I21
I28
033
J24

Keywords:
Mobile phone ban
Student performance

ABSTRACT

Recently, policy makers worldwide have suggested and passed legislation to ban mobile phone use in schools. The influential (and only quantitative) evaluation by Beland and Murphy (2016), suggests that this is a very low-cost but effective policy to improve student performance. In particular, it suggests that the lowest-achieving students have the most to gain. Using a similar empirical setup but with data from Sweden, we partly replicate their study and thereby add external validity to this policy question. Furthermore, we increase the survey response rate of schools to approximately 75 %, although at the expense of the amount of information collected in the survey. In Sweden, we find no impact of mobile phone bans on student performance and can reject even small-sized gains.

1. Introduction

Technical innovation has changed the education landscape. The cost of information and communication technology (ICT) is no longer trivial, and it holds the promise to deliver significant enhancements to education and learning. However, the rapid increase in the use of ICT also has a backside as it may distract students and hamper learning. Furthermore, there is no consensus among researchers on what type of technology works and in what context, as discussed in Escueta, Quan, Nickow and Oreopoulos (2017). Therefore, it is of little surprise that some countries are now taking steps to hinder and control ICT use in schools. For example, since 2018, French students in secondary school are no longer allowed to use their phones during the school day. This prohibition was a result of new mobile phone legislation. A similar policy discussion is occurring in Denmark and Britain, based on the assumption that mobile phones distract students and hinder learning.[1] The same rationale is behind the Swedish prime and education ministers' recent proposals to implement the same type of ban. In all cases, advocates of the ban refer to the influential study of mobile phone bans in Great Britain by Beland and Murphy (2016), henceforth B&M. Their study suggests that a ban is a very low-cost but effective policy to improve student performance. In particular, it suggests that the lowest-achieving students have the most to gain. Given the low-cost nature of the policy, it is not surprising that both left- and right-wing governments have been eager to suggest and implement mobile phone bans.

Although there are few other studies that causally evaluate mobile phone bans, there are some well-designed experimental studies on the consequences of access to, as well as the flip side of banning, ICT at the K-12 level, as surveyed in Escueta et al. (2017).[2] The provision of one computer per student (so called 1 to 1) is found not to be sufficient for learning gains, and the overall conclusion from the K-12 level suggests that giving a child a computer may have limited impacts on learning outcomes (e.g. Fairlie & Robinson, 2013, Fairlie & Kalil, 2017) but could improve computer proficiency and other cognitive outcomes. Malamud and Pop-Eleches, (2011).[3]

However, when specifically evaluating mobile phones bans, the only large-scale quantitative evaluation of mobile phone bans known to us is B&M.[4] Although their study is novel, innovative and well-executed,

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[1] See for example New York Times Sept. 20, 2018. https://www.nytimes.com/2018/09/20/world/europe/france-smartphones-schools.html.

[2] Regression discontinuity studies are also surveyed.

[3] This conclusion also seems hold in Sweden. See the recent evaluation of the introduction of 1 to 1, Hall et al. (2019).

[4] There are, of course, case and small-sample studies on the use of mobile phones and/or other digital devices in classes and /or schools. Evidence that mobile phone use may hamper learning are, for example, Berry & Westfall (2015), Campbell, (2006), Thomas et al. (2014), Thomas & Muñoz (2016), Lenhart et al. (2010) and Gao et al. (2014). At the same time, there is little consensus on this topic, as discussed at length in Ott (2017). Studies suggesting that mobile phones support rather than hinder learning are, for example, Peck et al. (2015) and Sharples (2013). There is also a related large literature on the adoption of new technologies, which is typically viewed as productivity-enhancing. E.g. see Ding et al. (2009), Malamud and Pop-Eleches (2011), Machin et al. (2007) and Barrow and Rouse, 2009.

https://doi.org/10.1016/j.econedurev.2020.102009
Received 20 June 2019; Received in revised form 15 May 2020; Accepted 19 May 2020
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before it can be taken as general policy advice, scientific evidence must be accumulated by replication both in similar settings and in other countries with different contexts. Given that B&M has also received significant attention from policy makers outside Great Britain and particularly in the Scandinavian countries, we are inspired to provide a replication based on Swedish data. As we also want to improve on the response rate in the B&M survey, we focus on the main analysis in their work, namely, whether a ban on mobile use at the school level improves learning outcomes. Thus, we cannot fully do justice to B&M because they provide much more in-depth surveys by, for example, analyzing the compliance rate and other major factors. They also elaborate on the strength of the ban and are able to define bans as being effective if use is restricted on school premises. Consequences of such restrictions may well vary from strict bans, as in our definition. This is a difference, but we choose to focus on increasing the response rate on behalf of details, although we do acknowledge that this may explain the difference across studies. By keeping the survey simple, we achieve a response rate of approximately 75 % compared to 21 % in B&M. Another large difference between the studies is that the data used in B&M cover the years 2001-2013. The development of both types of digital technology is rapid, and since 2013, both mobile phones and computers have become more advanced and more widespread.

We find that mobile phone bans have no impact on student performance, and we can reject even very small effects of banning mobile phones in the Swedish setting. Based on the evidence in this study, our policy advice is that while introducing a mobile phone ban is tempting due to its low-cost nature, such a ban should not be expected to produce substantial gains in student performance.[5] Thus, this study nicely complements B&M and provides policy guidance to politicians outside Britain. We argue that there are at least two reasons why the results may differ other than the methodological differences discussed above. First, teachers may in fact already have mobile phone bans in practice in the classroom regardless of school policy.[6] On the other hand, as been documented by Ott (2017), although teachers in upper secondary schools may not consider mobile phones in general to be particularly useful during lessons, they may still permit specific schoolwork-related usage. Moreover, Swedish schools have a long history of making large investments in digital technologies and devices, such as laptops and tablets. In fact, Sweden was among the first countries in the world to use computers in schools, a practice that began approximately 45 years ago (Riis, 2000). On both the national and local levels, there have been plenty of initiatives to integrate such devices into classroom practice (Tallvid, 2015). Sweden is the most computer-dense country in Europe, with 70 percent of all students in grade 8 having their own computers (Utbildningsdepartementet, 2017). The other Scandinavian countries are similar with respect to computer density. Therefore, the use of digital technology is quite intertwined with practice in Swedish schools. Implementing new technology or a ban in such a setting may well be ineffectual.

The rest of the paper is structured as follows. In section two, we present institutional detail, data and empirical design. In section three, the results are presented, and section four concludes.

2. Institutional details, data and empirical design

2.1. Institutional details

The use of mobile phones in Sweden, especially among young adolescents and children, has been increasing in recent years. Ownership rates in the age group under study (15-16) are rather similar when comparing the UK with Sweden. According to The Internet Foundation in Sweden IIS (2016), 98 % of all young people aged 16-25 had their own smartphone in 2016 (in 2018 this reached 100 % according to IIS, 2018). In 2018, 95 % of 16-24-year-olds had their own smartphone in the UK (Ofcom, 2018). In line with UK data, ownership rates in Sweden do not vary considerably across income groups or gender.[7] The use of digital tools and methods is also increasing in schools. In 2016, the Swedish National Agency for Education suggested that each student should have their own computer, and the implementation of this policy began soon after (Swedish National Agency for Education, 2016). In 2017, the Swedish Ministry of Education and Research released a strategy for the digitalization of school (Utbildningsdepartementet, 2017). In 2018, several Swedish schools revised their course plans in order to implement and focus on the use of digital technologies as tools learning and work. Since 2006, Swedish teachers have had the authority, under a regulation in the school law, to confiscate objects that disturb or threaten the security of education. That law was designed and implemented specifically to target mobile phones (Ott, 2014). According to a web-based survey panel with teachers from all over Sweden, 35 % of Swedish schools only allow the use of mobile phones during specific projects, 28 % have a mobile phone ban during recess, 14 % have no mobile phone ban at all, and 8 % reported that they have a complete mobile phone ban in schools (Telenor, 2018). In schools, the use of mobile phones for schoolwork is decreasing; nevertheless, two out of three teachers in secondary and upper secondary school believe that mobile phones distract from daily work in the classroom (IIS, 2018). Recently, Sweden's prime and education ministers have declared that a mobile phone ban will be implemented, and the prime minister explicitly referred to the findings in B&M as justification for the ban.[8]

Schooling in Sweden is compulsory for children aged 7-16 years. Schools are either municipal schools or voucher schools, and in the case of the latter, the provider can be a company, foundation, or an association. Education is free of charge. Three times during their compulsory education, in grades 3, 6 and 9, pupils complete a national test, the goal of which is to ensure equivalent and fair grading and to analyze levels of proficiency in Swedish schools. The final grades on the school-leaving certificate in grade 9 are based on a scale from A-F, where F indicates failure.[9] In addition to these national tests, pupils receive grades in courses from sixth grade onwards, and at the end of ninth grade, which also marks the end of secondary school, they receive a final grade in each course. The teachers determine the final grade based on grades from the national tests and other activities in school courses, that is, they use all available material on the pupil's educational performance. These grades should reflect the pupil's knowledge of the course material; they are then summed into a final school-leaving certificate of merit or grade points, where the maximum is 340 based on 17 subjects. The grade points are the selection mechanism for studies in upper secondary school.

2.2. Data

The data on educational results were obtained from the Swedish National Agency for Education. The data are school-level panel data on the educational performance of 9th grade pupils over the period 1997-

[5] We acknowledge the possibility that other academic and non-academic outcomes outside the scope of this paper may be related to a mobile ban.

[6] Similarly, mobile bans may be less enforced. We have not found evidence of bans not being enforced, but at least in theory, if enforcement is lower in Sweden this could also explain the difference across the two studies.

[7] However, even if ownership is universal, productive use may be correlated with socioeconomic status as discussed in Selwyn, (2004).

[8] See, for example, the interview with the Prime minister on public radio at https://sverigesradio.se/sida/artikel.aspx?programid=83&artikel=6183805

[9] Before 2012, grading was done on a four-level scale: excellently approved (MVG), well approved (VG), approved (G), and not approved (IG), also enumerated from 0 to 20. The final grades are used to calculate GPA, which is used for admission to high schools.

Table 1
Descriptive statistics on the outcome variables for responding vs. nonresponding schools.

	(1) All schools2016/17	(2) Sample withresponse2016/17	(3) All schools1997-2018	(4) Sample withresponse1997-2018
Panel A: grade points				
Grade points	226.0	225.4	214.6	214.7
	(28.46)	(28.62)	(22.88)	(23.04)
Observations	1,423	1,086	22,832	16,724
Panel B: mathematics national test score				
Test score	10.81	10.81	11.18	11.22
	(2.275)	(2.283)	(1.961)	(1.961)
Observations	1,401	1,070	12,599	9,371
Panel C: percent failing mathematics national test				
Percent fail math	15.97	16.18	10.83	10.84
	(13.60)	(2.283)	(12.12)	(12.03)
Observations	1,386	1,059	17,436	12,760

Notes: Table 1 presents descriptive statistics for key variables for all schools and schools responding to the survey. Standard deviations are shown in square parentheses. Sources: Swedish National Agency for Education and author-conducted survey.

2017, where 1997 indicates school year 1997/1998 and 2017 indicates school year 2017/2018, etc. The data cover the universe of all schools with at least 15 pupils. We sample from schools that operated in the school year 2016/2017 ($N = 1423$). Similar to the sample used in B&M, the students in our sample were aged 15-16 years when the grades were received. Although we can observe school performance outcomes for the full population, there is no national policy on mobile phone use in schools, and there are no existing data available on mobile phone policies. To obtain this information, we sent a survey to schools across Sweden during 2018-19 in which we asked if there was a ban on mobile phones at the school level and, if so, when it had been implemented. If, after reminders via email, a school had still not responded, we tried to connect with the respective school principal by phone until the survey was completed. As presented in Table 1, we successfully obtained necessary information on the mobile phone ban for 1,086 out of 1,423 schools, which is approximately 76 %. We differ from B&M because we sample from the full population of grade 9 schools in Sweden instead of using schools from four cities. Second, we received a 76 % response rate from our population instead of a 21 % response rate. When we pool all the years, we have 16,724 observations out of a potential 22,832, or a response rate of 73 %.

Fig. 1 shows the increase in the number of schools with a mobile phone ban over time. In 1997, there were only three schools with a ban. By 2014, there is an increase in the implementation rate, from 73 schools with a ban to 119 within one year, and in 2017, 631 schools have a mobile phone ban, which is approximately 60 % of our sample schools.

Our main outcome of student performance is the school's average merit or total grade points when students are leaving grade 9. Importantly, Statistics Sweden collects these statistics, and school participation is mandatory. Thus, there should be no attrition bias due to missing individual data on final grades. There is, however, one drawback to using the final grades: they have been found to be inflated in Swedish schools, as they are set by the student teacher (see, Hinnerich & Vlachos, 2017; Vlachos, 2018 and Berg, Palmgren & Tyrefors, 2019). For this reason, we also use two other complementary measures of student performance. The two other outcomes are based on the national standardized test for mathematics, the average test score and the share of students (measured as percentages) failing the test. Mathematics test results are often used to approximate an objective measure of ability, and the grading of high school mathematics tests based on the syllabus has been found to have high reliability, such that those test scores have been used as a reference point for ability when comparing final grades (Vlachos, 2018). However, for the national test score data, there is more missing data, as discussed in detail below. Thus, we acknowledge that none of the performance measures are single-handedly perfect measures of student performance. However, as they suffer from different pros and cons (no attrition but lower reliability vs. higher attrition but high reliability), we argue that if the results are qualitatively the same regardless of outcome measure, then this lends credibility to our study.

With national tests measures, some students are absent on the test day, so we would expect the school averages to be somewhat affected by positive sorting.[10] Moreover, there is no consistent test score metric over the period, although the share of students who fail is reported as long as a school has more than 10 students receiving a fail over the sample period. If fewer than 10 students receive a fail, then the share is coded as missing. In our baseline results, we assign the share to be zero in this case. However, we show in appendix Table A1 that the results are similar when changing definitions by imputing the potential theoretical maximum, or when dropping these observations. In addition, we have little reason to believe that the nature of the attrition changes over time, and hence, a DID-design should be suitable. Last, for the years 1998-2002, the statistics for national tests are based on a random sample of 150 schools. Starting in 2003, the universe is drawn similarly to our other measure, grade points, but with the exception that the results from the national test in mathematics in 2017 are not published. When calculating the average school test score in mathematics, we also lose about 20 % more of the observations in total, as the grade intervals more often contain fewer than 10 students as they are finer intervals than the percentage of fails for the years 1997-2012. For the years 2013-2016, this is not a problem as the raw average test score and not the intervals are reported.

Starting with the grade points in Panel A in Table 1, for the school year 2016/17, the merit points on average for the full school population

[10] In our sample, the participation rate was 91 % on average.

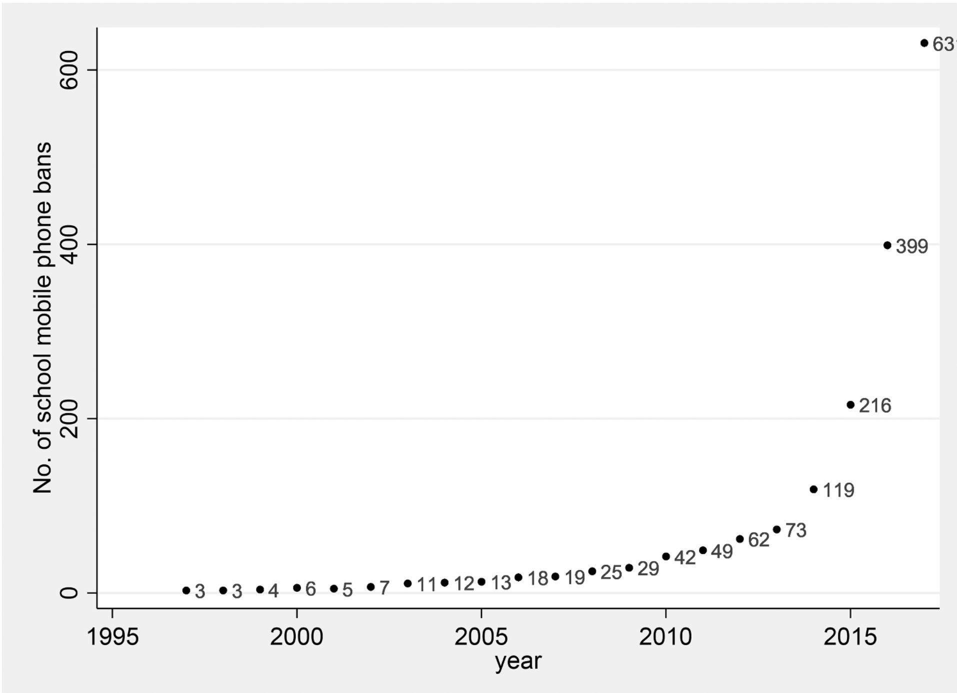

Fig. 1. Increase in the number of schools with phone bans from 1997-2017.
Notes: Fig. 1 depicts the number of schools with mobile phone bans in our sample each year. School administrators were first asked by email if the school had a ban on mobile phone use. If no responses were received, telephone contact was made, with the ultimate goal of reaching the principals of the schools. Source: Author-conducted survey.

($N = 1423$) is 226. For our sample, namely schools that responded to the survey ($N = 1086$), the merit points are statistically indistinguishable at approximately 225.4 points. Turning to the pooled sample, the grade points are 214.6 for the population and 214.7 for our sample of responders. Again, there is no meaningful difference when comparing the population figures with the sample. Similar arguments can be made regarding the test scores on the national test in mathematics in Panel B and the percentage failing the national test in mathematics in Panel C. In the following econometric analysis, we follow the same procedure as in B&M and standardize the grade and the test score points nationally each year, so that it we have a mean of 0 and a standard deviation of 1.[11]

Table 2 provides more descriptive statistics on the pooled population and the sample for school characteristics. The variable percentage boys is the school average of the percentage boys. Percentage foreigners is measured in the following way: before 2011, percentage foreigners was the share born outside Sweden. After changes to this definition, it became the share of newly immigrated individuals. The education level of parents is a school average based on individual measures from 1 to 3, where a parent has education level 1 if s/he has completed secondary school, 2 if high school and 3 if higher education. The socioeconomic index is a combination of the three variables mentioned and is produced from a regression of merit points on these variables. Then, the predicted values serve as the socioeconomic index. Again, we conclude that our sample of responding schools is very similar to the population and hence representative.

Table 2
Descriptive statistics on characteristics for responding vs. nonresponding schools.

	(1) All schools1997-2018	(2) Sample withresponse1997-2018
Percentage boys	51.21 (7.934)	51.23 (7.782)
Percentage foreigners	6.615 (8.054)	6.687 (8.218)
Education level parents	2.207 (0.230)	2.209 (0.231)
Socioeconomic index	214.0 (19.00)	214.1 (19.10)
Observations	22,832	16,724

Notes: Table 2 presents descriptive statistics for variables for all schools and schools responding to the survey. Standard deviations are shown in square parentheses. Sources: Swedish National Agency for Education and author-conducted survey.

2.3. Empirical strategy

We estimate the effect of a mobile phone ban on student outcomes in a staggered difference-in-difference setting following B&M. However, our approach differs in that we are using school-level data, but this should come at no cost because treatment is at the school level in both studies and grade data are complete. As the treatment is staggered in time, schools that have not yet or never introduced a ban will serve as control groups. The standard errors are clustered at the level of treatment, the school level. In addition to additivity, the major identification assumption is parallel trends across treatment and control units in the absence of a mobile phone ban. We follow – as closely as our data allow – the same empirical design as B&M, which can be consulted for more details.

Our baseline specification becomes

[11] B&M use a student achievement measure similar to final grades in terms of measurement. The GCSE test scores used are graded from A* to G, with an A* being worth 58 points and decreasing in increments of six down to G grade. Students take GCSEs in different subjects, and B&M use the individual's sum of these GCSE points, standardized in the same manner. Thus, the aggregation of the outcome is similar.

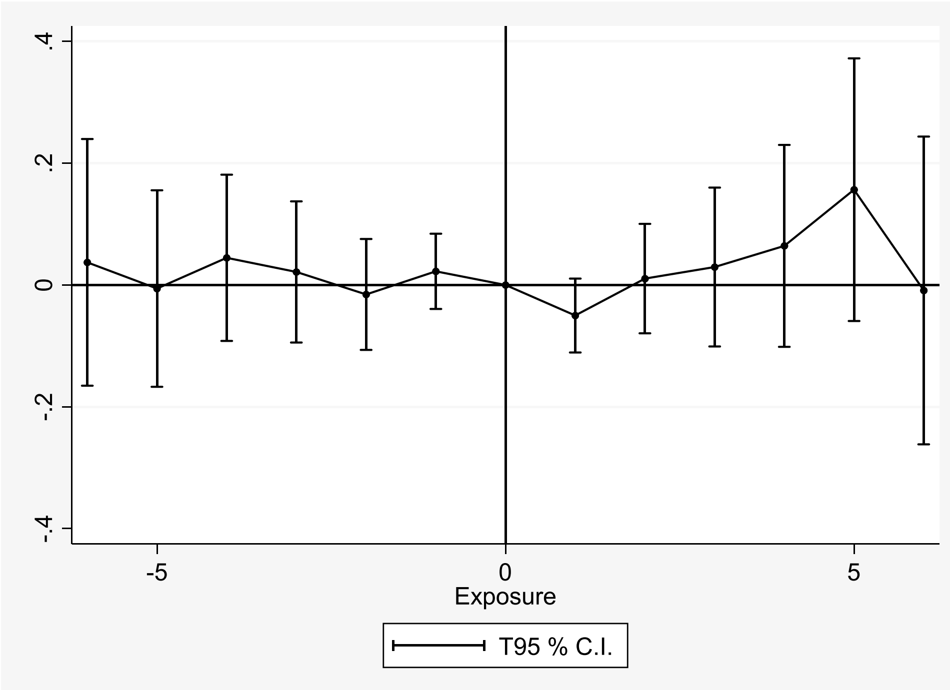

Fig. 2. Impact of the phone ban by years of exposure on merit points.
Notes: Estimated impact of mobile phone ban on age 16 standardized grade point by years of exposure. The baseline year is the year prior to introduction. Error bars represent the 95 % confidence intervals with robust standard errors clustered at the school level. Sources: Swedish National Agency for Education and author-conducted survey.

$$Y_{st} = \beta_0 + \beta_1 Ban_{st} + \mu_s + \gamma_t + \varepsilon_{st}$$

where Y_{st} is student performance in school s in year t. Our measures of student performance are school average grade points, average test score on the national test in mathematics, and percentage failing the national test in mathematics. *Ban* is the indicator variable of interest for whether schools ban mobile phones at time t, and its coefficient, β_1, is the parameter of interest and captures the impact of the introduction of the mobile phone ban on student test scores, μ_s is the school fixed effects, $\gamma_t$are the year fixed effects and ε_{st} is the error term. Note that we could also – in line with B&M – include controls (X_{st}) such as gender. However, again, this will be aggregated to schools, and thus, those types of variables will be in the school-level share, as presented in Table 2. We do not have pretreatment test scores, and thus we cannot replicate the value-added estimate in column 2 Table 4 in B&M. Fortunately, they conclude that the main effect is not affected by including prior performance. This conclusion is not surprising under the assumption of common trends. With respect to parallel trends, we follow B&M and check for potential trends in student achievement before the introduction by expanding Eq. (1) to include a sequence of lags and leads of the treatment indicator. This is typically referred to as an event study in the literature. If there are no effects in the pretreatment period, this is consistent with the parallel trends assumption being fulfilled.

3. Results

3.1. Main results

Starting with an examination of the common trends assumption, we plot the annual "treatment" effect both before and after the year of mobile phone ban in Fig. 2 for the grade points. We show the results for up to 5 periods before and after, and the year before treatment is used of as the baseline. Year 6 before and after the baseline year contains all years before and after year 5, respectively. For a causal interpretation, one should put more weight on the years just before and after the introduction of the ban, as these years will have fewer potential changes in the composition of students. Focusing on the pretreatment periods, we see no violations of the parallel trends assumption. Interestingly, there are no signs of a treatment effect either. The small upward trend in year 5 seems to be consistent with positive student sorting, as shown in appendix Fig. A1.[12] We also note there is little evidence of dynamic treatment effects. In particular, the effects two years after the treatment should be of interest as it will capture classes that have been under treatment for the full three years of secondary school. However, the effects for the first three years after introduction are statistically not distinguishable from each other. Moreover, we have performed another standard validation exercise, which is presented Table A2. We have added schools' specific linear time trends (Angrist and Pischke, 2009). The point estimates are again small and insignificant.

Turning to our second outcome, the average test score on the national test in mathematics, we again see no signs of violation of common trends before the school ban. Corresponding to our main outcome, we see no impact after the ban in Fig. 3.

Turning to our third outcome in Fig. 4, the percentage failing the national test in mathematics, we again see no signs of violation of common trends before the school ban. Corresponding to our main outcome, we see no impact after the ban either.

Now turning to the estimation, it comes as no surprise that no significant effects are detected. Column 1 in Table 3 presents the equivalent of column 1 Table 4 in B&M. Focusing on the 95 % confidence interval in brackets, we find that effects larger than 0.024 can be rejected. Thus, we can reject effect sizes of the magnitude presented in B&M. In columns 2-5, we continuously add school characteristics. Given the pattern observed in Fig. 2, we should not expect a large difference in the point estimate. However, residual variance could be affected, and estimates could be more precisely estimated. Indeed, this pattern can be observed, and in column 5, when finally adding the socioeconomic index, we can reject positive effect sizes larger than 1 percent of a standard deviation. The treatment effect is never significantly different from zero at a 5 percent significance level.

In Table 4, we present the estimation results using the same setup as in Table 3 but for test scores on the national test in mathematics. Again,

[12] Sorting is more prevalent in the Swedish school system due to free school choice and the voucher system (Malmberg et al. 2013). However, the positive sorting explanation must be taken with a grain of salt as the pooled effect is not significant with an estimate of 0.031 and a standard error of 0.033.

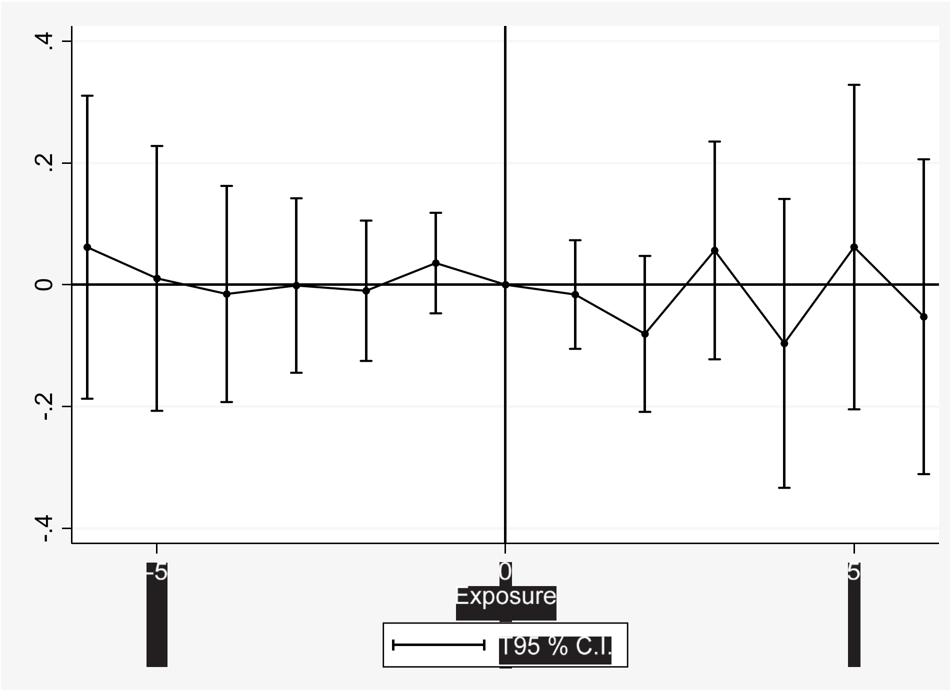

Fig. 3. Impact of phone ban by years of exposure on national test scores in mathematics.
Notes: Estimated impact of mobile phone ban on age 16 standardized test scores in national test in mathematics by years of exposure. The baseline year is the year prior to introduction. Error bars represent the 95 % confidence intervals with robust standard errors clustered at the school level. Sources: Swedish National Agency for Education and author-conducted survey.

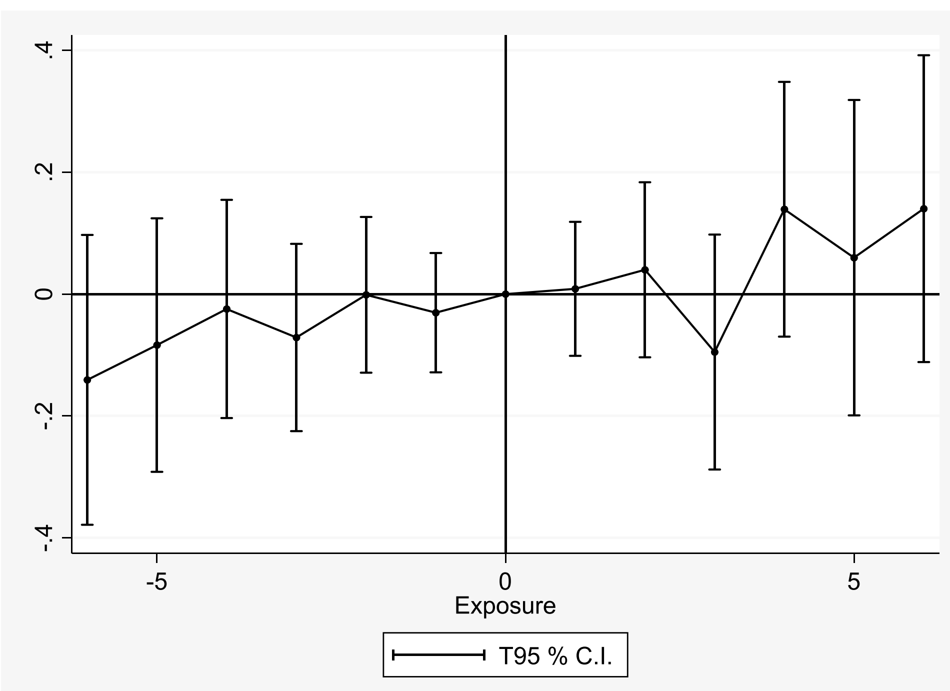

Fig. 4. Impact of phone ban by years of exposure on percentage failing the national test in mathematics.
Notes: Estimated impact of mobile phone ban on age 16 percentage failing standardized national test in mathematics by years of exposure. The baseline year is the year prior to introduction. Error bars represent the 95 % confidence intervals with robust standard errors clustered at the school level. Sources: Swedish National Agency for Education and author-conducted survey.

the effect of a mobile phone ban in Sweden seems to be very small and negative, if any. None of the estimates are significantly different from zero at the 5 % significance level.

In Table 5, we present the estimation for the outcome percentage failing the national test in mathematics. Again, the effect of a mobile phone ban in Sweden seems to be very small, if any.

In Table 6 we perform validity checks by following Pei, Pischke and Schwandt (2019), i.e., relating the introduction of a ban to other school-level policy variables. For example, one could be worried that the introduction of bans is related to other policy changes and thereby invalidate a ceteris paribus evaluation. In columns 1 to 6, school inputs such as the student teacher ratio are used as outcomes. Reassuringly, none of the policy variables are related to the introduction of bans.

We argued that Swedish schools have a long history of making large investments in digital technologies and devices and that that history might help mitigate some of the potential negative impacts of mobile phones (Tallvid 2015). Unfortunately, we have not been able to find data on historical use in Sweden. However, we identified one large-scale ICT reform that occurred during the studied period, and that is the introduction of the one student-one computer program, the so called “1

Table 3
Effect of mobile bans on merit points.

Merit points	(1)	(2)	(3)	(4)	(5)
Ban	-0.034	-0.042	-0.038	-0.042	-0.042*
	(0.029)	(0.029)	(0.028)	(0.026)	(0.025)
	[-0.092	[-0.100	[-0.094	[-0.092	[-0.092
	0.024]	0.015]	0.017]	0.008]	0.008]
Observations	16,724	16,724	16,724	16,724	16,724
R-squared	0.743	0.748	0.757	0.781	0.786
Share boys	no	Yes	yes	yes	no
Share immigrants	no	No	yes	yes	no
Parents education	no	No	no	yes	no
Socioindex	no	No	no	no	yes

Notes: Table 3 presents regression estimates for standardized merit points. All specifications include school effects and year effects. We use robust clustered standard errors at the school level. Robust standard errors are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$ and a 95 % CI in brackets. Sources: Swedish National Agency for Education and author-conducted survey.

Table 5
Effects of mobile bans on the percentage failing the national standardized test in mathematics.

% fail math test	(1)	(2)	(3)	(4)	(5)
Ban	0.003	0.008	-0.000	0.008	0.027
	(0.542)	(0.543)	(0.546)	(0.536)	(0.542)
	[-1.061	[-1.057	[-1.071	[-1.044	[-1.036
	1.067]	1.073]	1.071]	1.060]	1.090]
Observations	12,760	12,760	12,760	12,760	12,760
R-squared	0.501	0.501	0.503	0.511	0.509
Share boys	No	yes	yes	yes	no
Share immigrants	No	no	yes	yes	no
Parents education	No	no	no	yes	no
Socioindex	No	no	no	no	yes

Notes: Table 5 presents regression estimates for percentage failing the national test in mathematics. All specifications include school effects and year effects. We use robust clustered standard errors at the school level. Robust standard errors are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$ and a 95 % CI in brackets. Sources: Swedish National Agency for Education and author-conducted survey.

Table 4
Effects of mobile bans on test scores on the national standardized test in mathematics.

	(1)	(2)	(3)	(4)	(5)
Variables	Test scores	Test scores	Test scores	Test scores	Test scores
Ban	-0.023	-0.025	-0.024	-0.021	-0.025
	(0.038)	(0.038)	(0.038)	(0.037)	(0.038)
	[-0.097	[-0.099	[-0.099	[-0.094	[-0.100
	0.051)]	0.050]	0.050]	0.051]	0.049]
Observations	9,371	9,371	9,371	9,371	9,371
R-squared	0.689	0.690	0.691	0.713	0.710
Share boys	no	yes	yes	yes	no
Share immigrants	no	no	yes	yes	no
Parents education	no	no	no	yes	no
Socioindex	no	no	no	no	yes

Notes: Table 4 presents regression estimates for standardized test scores on national standardized tests in mathematics. All specifications include school effects and year effects. We use robust clustered standard errors at the school level. Robust standard errors are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$ and a 95 % CI in brackets. Sources: Swedish National Agency for Education and author-conducted survey.

to 1" program. Hall, Lundin and Sibbmar (2019) find no relation of the 1-to-1 reform on performance, but for completeness we still want to investigate whether the introduction of mobile phone ban is related to the introduction of a 1-to-1. We use the year of the municipal introduction of 1-to-1 as the starting year.[13] We code a dummy taking the value one for the year of introduction and onwards and zero before. Using the dummy as outcome, we report in column 7 a non-statistically significant relation. Thus, the late 1-to-1 reform seems to be unrelated to the bans on mobile phones. Combined with the findings of Hall et al. (2019) it seems that the two interventions are not affecting student performance and they are not related in time. Again, one explanation for the results could be that Swedish schools have long made large investments in digital technologies, that 70 percent of all students in grade 8 already had their own computer, and that Sweden has one of the highest computer densities in Europe, a feature shared by other Scandinavian countries. This may limit the scope of ICT-investments or bans in Sweden and similar countries in Scandinavia due decreasing returns to scale.

The overall picture for Sweden is that there were no significant gains from introducing bans. However, maybe the difference with B&M is still not country-specific but rather due to different sampling. For example, we acknowledge that we are comparing the universe of schools and not only urban schools, as was done in B&M. Another feature of B&M is that the effects are driven by groups in the lower tail of the achievement distribution. The next section deals further with comparability or external validity.

3.2. Comparability

In Table 7, column 1, we show the results when estimating our model using only the four largest cities: Stockholm, Gothenburg, Malmö, and Uppsala. In the second column, we instead exclude those same cities. We repeat the same procedure for all three outcomes. Comparing the estimate in column 1 with the estimate in 2, we find no statistical differences. Taken at face value, introducing a ban would decrease the merit points for schools in both urban and rural municipalities. When instead evaluating the test-score-based outcomes, we instead see the effects, taken at face value, that are more in line with B&M, as a ban would increase test scores in urban schools (Column 3) and would decrease the share failing in math in urban schools (Column 5). However, none of the point estimates are significantly different from zero, and importantly, none are statistically different from each other when comparing the urban rural municipalities across outcomes.[14] Thus, we find no evidence that our results would be different in urbans

[13] The survey can be found on https://skoldator.wordpress.com/lista-over-en-till-en-skolor-i-sverige/

[14] We used the suest command in Stata to test for the difference in coefficients after estimating the models separately.

Table 6
The effect of the ban on other school policies.

Outcome	(1) No. of Head-masters	(2) No. of teachers	(3) Share(%) female teachers	(4) Share(%) teacher: pedagogical education	(5) Share(%) teacher: special pedagog. education	(6) Student teacher ratio	(7) 1to1 computer
Ban	0.022	-0.242	-0.415	0.299	0.193	0.005	-0.022
	(0.043)	(0.493)	(0.462)	(0.487)	(0.191)	(0.143)	(0.023)
Observations	15,148	15,498	15,496	15,317	12,088	15,486	16,724
R-squared	0.602	0.789	0.659	0.682	0.547	0.147	0.619

Notes: Table 6 presents regression estimates for other school level policy variables. All specifications include school effects and year effects and the control for the socioeconomic index. We use robust clustered standard errors at the school level. Robust standard errors are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Sources: Swedish National Agency for Education and author-conducted survey.

Table 7
The effect of the ban on student outcomes for urban and rural schools.

Outcome	(1) Merit points	(2) Merit points	(3) TestScores	(4) Testscores	(5) % failmath test	(6) % failmath test
Ban	-0.078	-0.038	0.097	-0.049	-1.192	0.305
	(0.064)	(0.028)	(0.097)	(0.041)	(1.143)	(0.616)
Observations	2,475	14,249	1,487	7,884	2,000	10,760
R-squared	0.884	0.740	0.792	0.679	0.606	0.490
Urban	yes	no	yes	no	yes	no

Notes: Table 7 presents regression estimates for student outcomes for urban and rural municipalities separately. All specifications include school effects and year effects and the control for the socioeconomic index. We use robust clustered standard errors at the school level. Robust standard errors are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Sources: Swedish National Agency for Education and author-conducted survey.

Table 8
Restricting the sample to 2000-2014 and 2000-2012.

Outcome	(1) Merit points	(2) Merit points	(3) Test scores	(4) Test scores	(5) % fail math test	(6) % fail math test
Ban	-0.064	-0.046	-0.101	-0.090	0.630	0.258
	(0.056)	(0.078)	(0.084)	(0.122)	(1.000)	(1.221)
Observations	11,859	9,944	7,165	5,271	10,437	8,551
R-squared	0.803	0.806	0.718	0.742	0.509	0.497
Sample	2000-14	2000-12	2000-14	2000-12	2000-14	2000-12

Notes: Table 8 presents regression estimates for student outcomes for the time periods 2000-2012 and 2000-2014 separately. All specifications include school effects and year effects and the control for the socioeconomic index. We use robust clustered standard errors at the school level. Robust standard errors are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Sources: Swedish National Agency for Education and author-conducted survey.

Table 9
The effect of mobile phone bans on student performance for different quintiles of the socioeconomic index distribution.

Variables	(1) Merit points	(3) Test scores	(3) % fail math test
ban_index1	-0.063	0.251**	-3.328**
	(0.057)	(0.101)	(1.555)
ban_index2	-0.023	-0.119	0.620
	(0.056)	(0.095)	(1.338)
ban_index3	0.008	-0.045	0.202
	(0.060)	(0.080)	(1.318)
ban_index4	0.009	-0.102	0.748
	(0.059)	(0.078)	(1.058)
ban_index5	-0.076	-0.062	1.211
	(0.051)	(0.062)	(0.911)
Observations	15,877	8,945	12,055
R-squared	0.790	0.709	0.507
Socioeconomic index	yes	yes	yes

Notes: Table 9 presents regression estimates for student outcomes based on the impact of the ban for different quintiles of the socioeconomic index. All specifications include school effects and year effects and quintiles indicators. We use robust clustered standard errors at the school level. Robust standard errors are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Sources: Swedish National Agency for Education and author-conducted survey.

schools in Sweden.

In Table 8, we instead mimic B&M by restricting our sample in time. We acknowledge two differences from B&M. They study the period until 2012. Most of increases in bans in their sample took place between 2005-2010, and by 2010, all but 6 schools in their sample had some type of ban in place. One potential difference between our result and B&M could be due to the time profile. The first iPhone was introduced in 2007 and it could be the case that the UK setting schools were choosing phone bans as a pro-active reaction to a new technology. As most of our bans come later, our results could be driven by a reaction and not pro-actively, particularly because B&M's first working paper is from 2014 and attracted media attention. Hence, the result may be driven by that. Moreover, the timing of treatment is related to the increased use of better and better smartphones. Smartphones could add more distraction, but on the other hand, they are potentially more powerful ICT tools and can be used for many tasks that students would typically use laptops to solve. This may, depending on the size of the two effects, act as a moderating factor when comparing to B&M. For this reason, we first re-estimate our models for the same year as B&M (2000-2012) but also 2000-2014. Table 8 shows the results when imposing these restrictions. Again, the results point to no learning gains from banning mobile phones.

An interesting finding in B&M is that banning mobile phones creates large learning gains for the groups in the lower tail of the achievement distribution. As we do not observe individual test score data, we cannot perfectly replicate their finding in a Swedish setting. However, we can use a school-level variation and the socioeconomic index as a proxy for the achievement distribution. In Table 9, we present results from a

Panel A: Impact of the phone ban by years of exposure on test scores.

Panel B: Impact of phone ban by years of exposure on percentage failing the national test

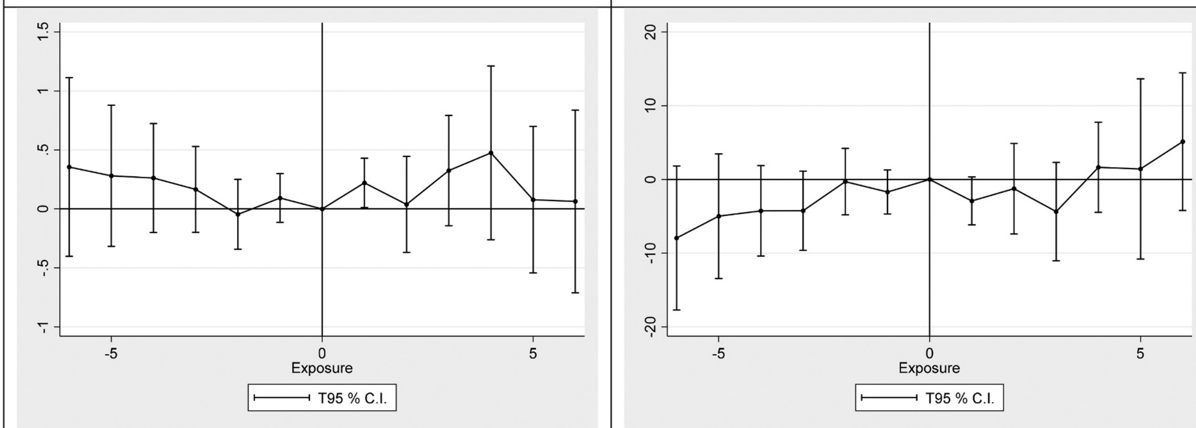

Fig. 5. Impact of the phone ban for the first quintile of the socioeconomic index

regression where we allow the treatment to vary across quintiles of the socioeconomic index. For the first outcome, merit points, we observe no significant negative effects of a ban. When using test scores as the outcome presented in column 2, there is some evidence of a non-linear effect, as in B&M, as the first quintile seems to benefit from a ban. Similarly, the first quintile seems to benefit from the ban when analyzing the percentage failing in column 3; i.e., this group sees a decrease in the failing percentage when the ban is introduced. These results are suggestive and need to be investigated more thoroughly in future work. At the moment, we believe that these effects should be interpreted cautiously since we have estimated quite a substantial number of specifications, and these effects may well be statistically significant just by chance. In fact, when plotting the event graph for the first quintiles for test scores and percentage failing the test in Fig. 5, panel A and B, there is no convincing causal pattern.

4. Discussion and concluding remarks

This paper adds to the almost nonexistent body of studies that use quasi experimental designs to investigate the consequences of banning mobile phones. Using a new country, Sweden, as our testing ground and the universe of Swedish secondary schools, we exploit differences in implementation dates and find no improvement in student performance in schools that have introduced a mobile phone ban. We cannot confirm the results of B&M. There are many potential reasons for why our results differ from B&M. We study a different country and school system as well as a somewhat different period. As noted by Ott (2017), Swedish schools have long made large investments in digital technology (e.g. tablets and laptops), and ICT is intertwined in the Swedish educational system. Schools are also encouraged to develop pedagogical uses for digital technology as tools for learning in school practice. Mobile phones have typically not been included. Nevertheless, students have brought their mobile phones to school and used them for schoolwork. For example, the case study by Olin-Scheller and Tanner (2015) finds that mobile phones are mostly used between assignments and that such use does not generally disturb teaching. Taken all together, this indicates a structured use of digital technology. This may well be one important distinguishing feature explaining the difference in results with B&M, as they focus more on unstructured use. Our main conclusion is that the result in B&M is not transferable to Sweden and potentially not transferrable to similar countries such as Denmark and Norway. Lastly, we also acknowledge the possibility that other academic and non-academic outcomes outside the scope of this paper may be related to a mobile ban. Still, the best policy guidance is as follows: although a national ban is a low-cost policy, small or no learning gains are to be expected in Sweden and in countries with similar school systems and similar investments in information and communication technology.

CRediT authorship contribution statement

Dany Kessel: Writing - original draft, Validation, Investigation, Data curation, Methodology. **Hulda Lif Hardardottir:** Writing - original draft, Writing - review & editing, Investigation, Validation, Methodology. **Björn Tyrefors:** Writing - original draft, Writing - review & editing, Validation, Investigation, Methodology, Conceptualization, Data curation, Supervision.

Acknowledgment

We are grateful for funding from Jan Wallanders and Hedelius stiftelse, Marianne and Marcus Wallenberg Foundation, Sciences. We thank Torbjorn Ott, Richard Murphy and two anonymous referees for helpful comments.

Appendix

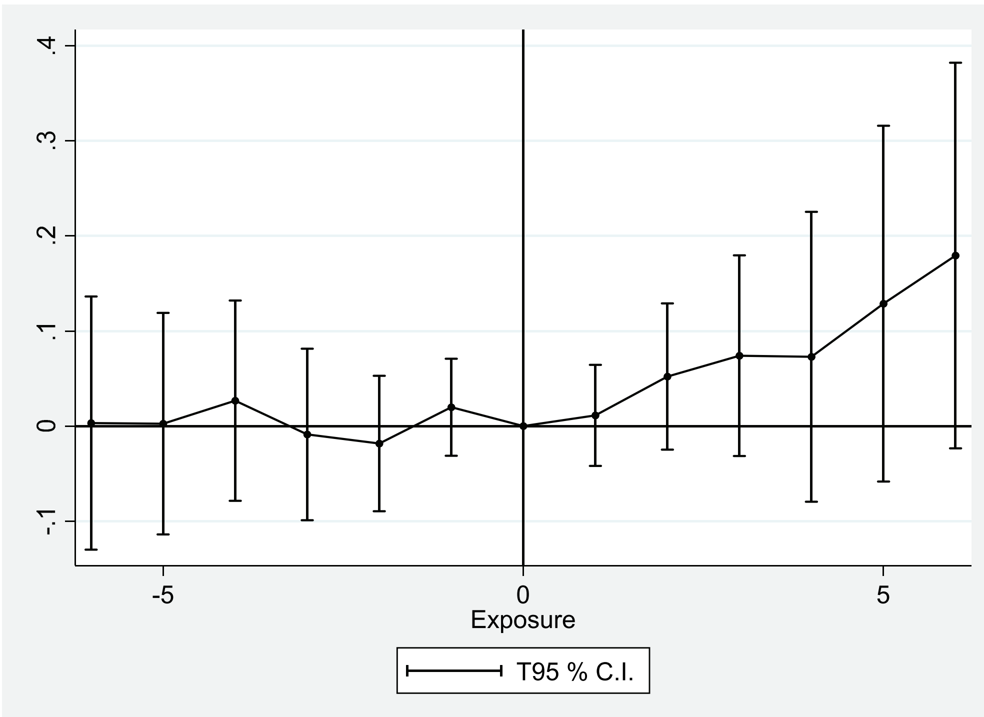

Fig. A1. School choice and posttreatment composition. Notes: Estimated impact on age and socioeconomic index of mobile phone ban by years of exposure. Baseline year is the year prior to introduction. Error bars represent the 95 % confidence intervals with robust standard errors clustered at the school level. Sources: Swedish National Agency for Education and author-conducted survey.

Table A1
Robustness checks: percentage failing national test in mathematics.

Variables	(1) Removing missing	(2) Impute share using max. no.
Ban	0.232	0.027
	(1.676)	(0.542)
	[-3.056	[-1.036
	3.521)	1.090]
Observations	7,514	12,760
R-squared	0.490	0.509
Socioindex	yes	yes

Table A2
Including linear time trends.

Variables	(1) Merit points	(2) Test scores	(3) % failmath test
Ban	-0.016	-0.009	-0.194
	(0.025)	(0.043)	(0.675)
Observations	16,686	9,267	12,669
R-squared	0.835	0.776	0.584
Share boys	no	no	no
Share immigrants	no	no	no
Parents' education	no	no	no
Socioindex	yes	yes	yes

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