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The Impact of Banning Mobile Phones in Swedish Secondary Schools

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The impact of banning mobile phones in Swedish secondary schools

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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 % compared to 21 % in B&M, 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.

Keywords: Mobile phone ban; Student performance

JEL codes: I21; I28; 033; J24

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E-mail: bjorn.tyrefors@ifn.se Tel.: +46(0)8-665 4500) and Stockholm University. We are grateful for generous funding from Jan Wallanders och Hedelius stiftelse and the Marianne and Marcus Wallenberg Foundation. We thank Torbjorn Ott for helpful comments.

1. Introduction

Upon their return from summer break in 2018, French students in secondary schoolsschool were 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 hamper learning.[4] 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.

The only large-scale quantitative evaluation of mobile phone bans known to us is B&M.[5] Although their study is novel, innovative and well-executed, 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

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

[5] 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 technology, 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 et al. (2009).

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. We focus on increasing the response rate to estimate the main effect, but we acknowledge this tradeoff. In this way, we arrive at a response rate of approximately 75 % compared to 21 % in B&M. Another aspect is that the data used in B&M (2016) covers the years 2001-2013. The development of both digital technologies 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 to 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. 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. First, teachers may in fact already have mobile phone bans in practice in the classroom regardless of school policy. On the other hand, as been documented by Ott (2017), teachers in the upper secondary school may not consider mobile phones in general as particularly useful during lessons, still they may well permit specific schoolwork related usage. Moreover, Swedish schools have since long made large investments in digital technologies and devises, for example laptops and tablets. Both on a national and local level there have been plenty of initiatives to integrate the devices into classroom practice

(Tallvid, 2015). Therefore, the use of digital technology is quite intertwined with practice in Swedish schools. To implement 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 it is up to 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.[6] 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 digitalization of school (Utbildningsdepartementet, 2017). In 2018, several of the Swedish school's course plans were revised, and put into action and focused on the use of digital technologies as tools for learning and work. Since 2006 Swedish teachers have authorization regulated in the school law, to confiscate objects that disturb or threaten the security in the

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

education. That law was designed and put in to action 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 usage 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 school work is decreasing, nevertheless two out of three teachers in secondary and upper secondary school believe that mobile phones distract the work in the classroom every day (IIS, 2018). Recently, the 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.[7]

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.[8] 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

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

[8] 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.

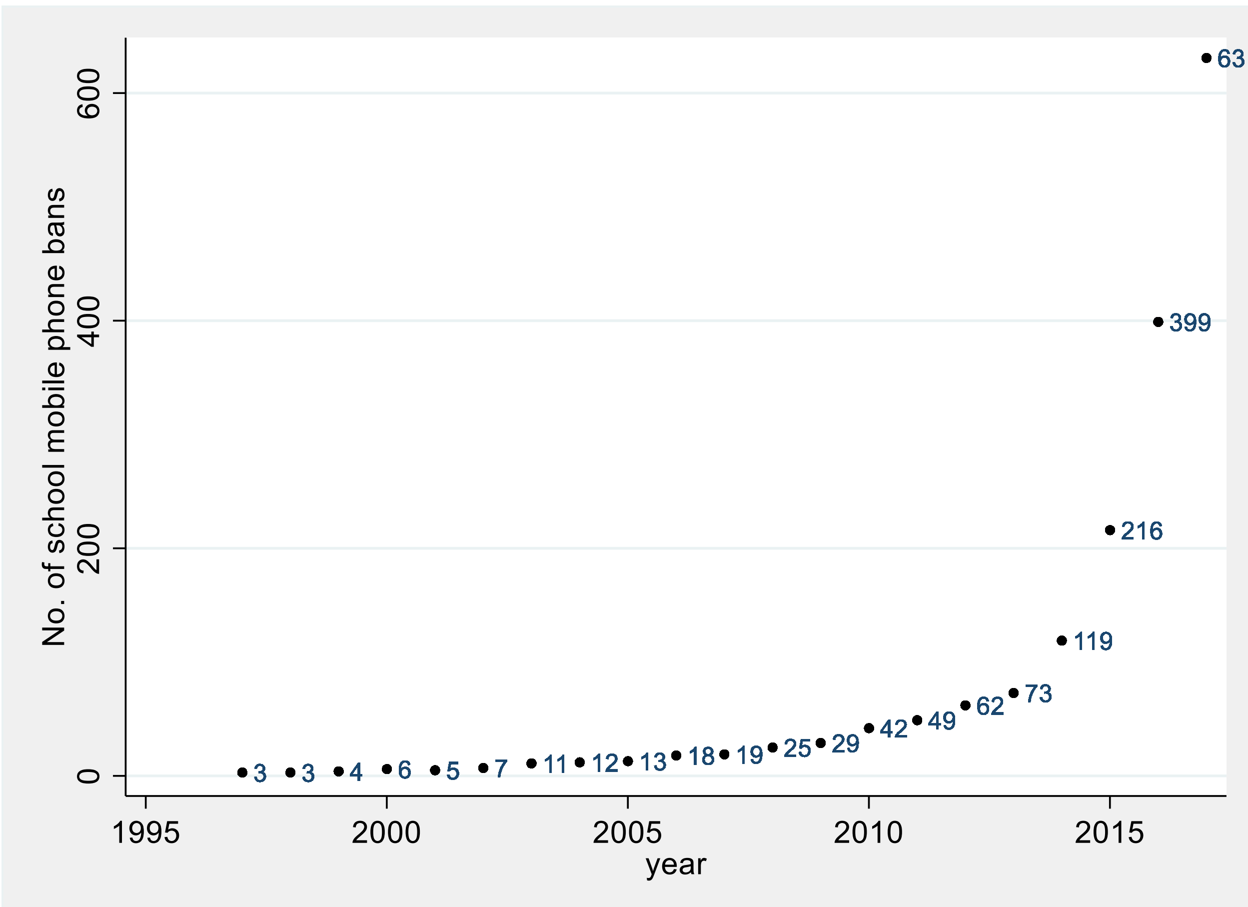
activities in school courses, that is, they use all available material on the pupil's educational performance. These grades should reflect the pupil's knowledge of the course material; they are then summed into a final school-leaving certificate of merit or grade points, where the maximum is 340 based 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 1997-2017, 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 %.

Figure 1 shows the increase in the number of schools with a cell 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.

Figure 1. Increase in the number of schools with phone bans from 1997-2017



Notes: Figure 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.

Our outcomes of student performance are the school's average merit or grade points and the share of students (measured as percentages) failing the standardized national test in mathematics.

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.

The share failing the standardized national test for mathematics complements the main outcome variable. First, this outcome is a function of national test scores, and because B&M use test scores, we also wanted to use a similar measure. As B&M find that the effect is driven by the students at the lower end of the ability distribution, the share of students who fail the national test in mathematics should be an interesting measure complementing the final grades. Second, grades have been found to be inflated in Swedish schools, as they are set by the student teacher (see, for example, Vlachos, 2018 and Berg et al., 2019). However, 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).

With national tests, some students are absent on the test day, so we would expect the school averages to be somewhat inflated.[9] There is no consistent test score metric over the period. Although the share of students who fail is reported, if a school has fewer than 10 students receiving 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 when calculating the share, 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-

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

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. The results from the national test in mathematics in 2017 are not published.

Starting with the grade points in Panel A in table 1, for the school year 2016/17, we have 226 merit points on average for the full school population (N=1423). For our sample, schools responded on the survey (N=1086), and the merit points are economically and statistically indistinguishable at approximately 225 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 percentage failing the national test in mathematics in panel B in table 1. In the following econometric analysis, we follow the same procedure as in B&M and standardize the grade points nationally each year, so that it we have a mean of 0 and a standard deviation of 1.[10]

[10] 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.

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

	(1) All schools 2016/17	(2) Sample with response 2016/17	(3) All schools 1997-2018	(4) Sample with response 1997-2018
	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: Percent failing mathematics national test			
Percent fail math	16.05	16.18	9.811	10.84
	(14.04)	(13.49)	(12.44)	(12.03)
Observations	1,448	1,059	23,306	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.

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 schoolsschool, 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 similar to the population.

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

	(3) All schools 1997-2018	(4) Sample with response 1997-2018
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 (2016). 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 (2016), which can be consulted for more details.

Our baseline specification becomes

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

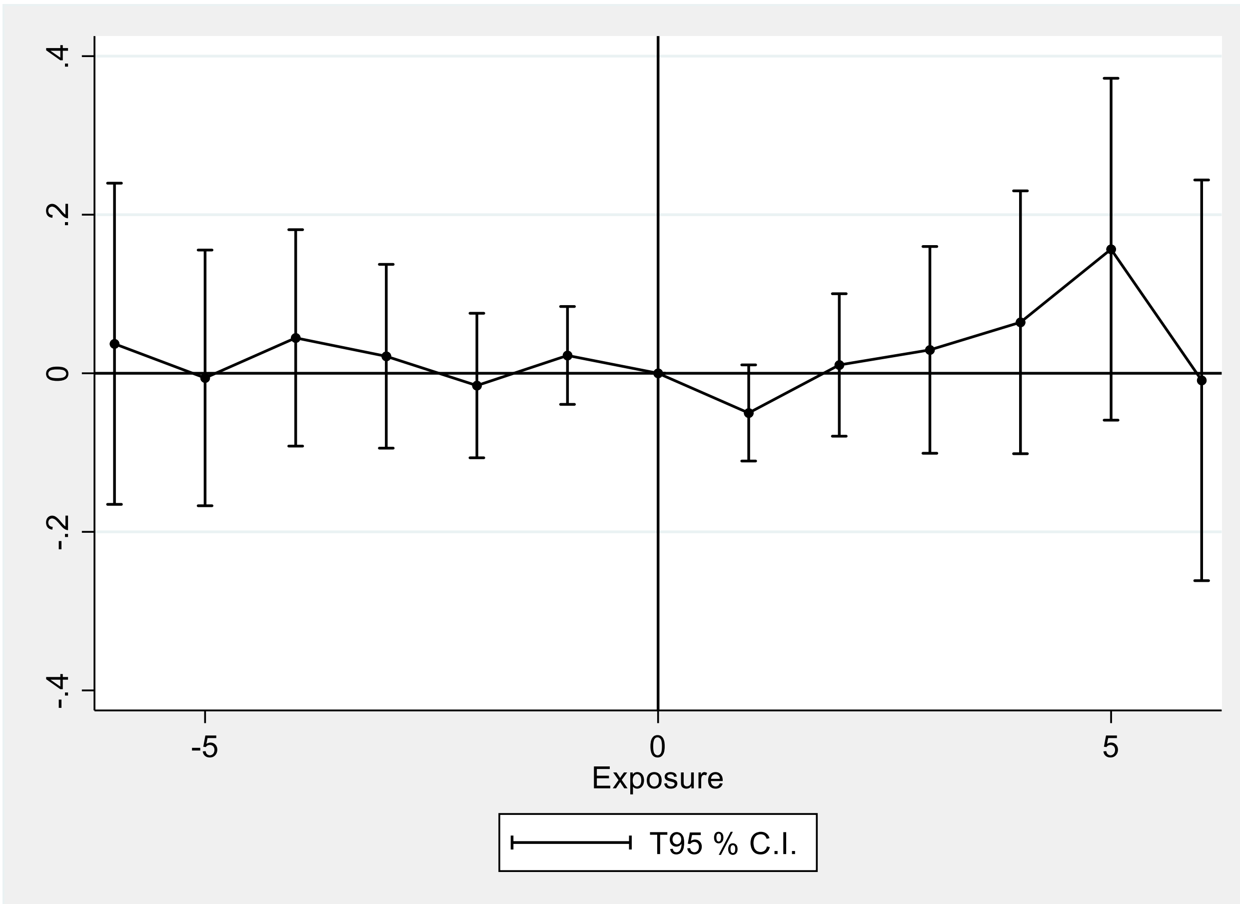
where Y_{st} is student performance in school *s* in year *t*. Our primary measure of student performance is school average grade points, and our secondary is percentage failing the national test in mathematics. Ban is the indicator variable of interest for whether schools ban mobile phones at time *t*, 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 (2016) – 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 equation (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

Starting with an investigation of the common trends assumption, we plot the annual "treatment" effect both before and after the year of mobile phone ban in Figure 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 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 figure A1.[11]

[11] 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

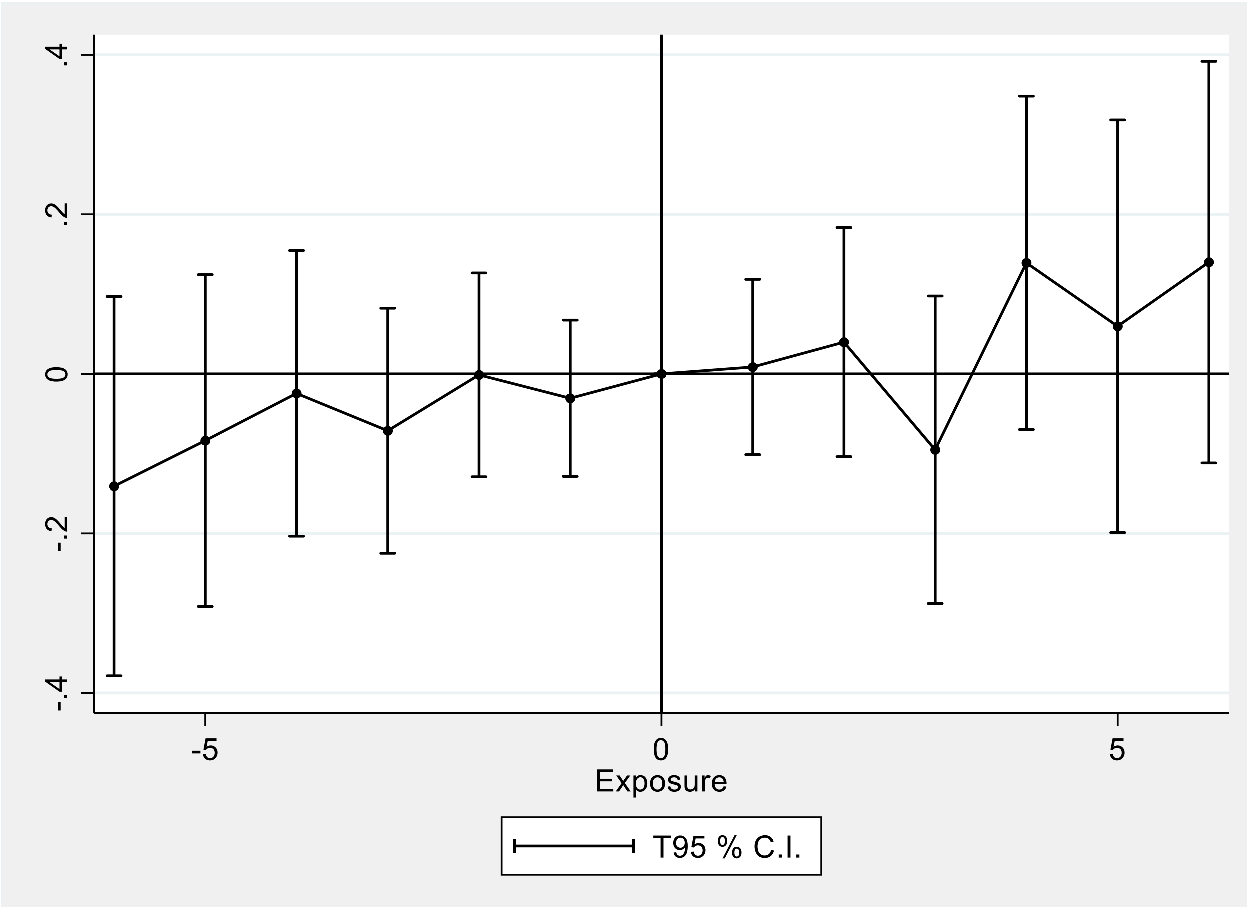
Figure 2. Impact of the phone ban by years of exposure on merit points



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.

Turning to our second outcome, 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.

Figure 3. 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.

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 Figure 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 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.

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 0.024]	[-0.100 0.015]	[-0.094 0.017]	[-0.092 0.008]	[-0.092 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 4 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.

Last, in table 4, we present the estimation results using the same setup as in table 3 but for the other outcome, i.e., percentage failing the national test in mathematics. Again, the effect of a mobile phone ban in Sweden seems to be very small, if any.

Table 4 Effect of mobile bans on percentage failing national test in mathematics

% fail math test	(1)	(2)	(3)	(4)	(5)
Ban	0.003 (0.542) [-1.061 1.067]	0.008 (0.543) [-1.057 1.073]	-0.000 (0.546) [-1.071 1.071]	0.008 (0.536) [-1.044 1.060]	0.027 (0.542) [-1.036 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 4 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.

4 Discussion and conclusive remarks

This paper adds to the almost nonexistent body of studies that use quasi experimental designs to investigate the consequences of banning mobile phones in Swedish schools. Using a new country as our testing ground and the universe of Swedish 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). Schools are also encouraged to develop the pedagogical usage of 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 school work and for example the case study by Olin-Scheller & Tanner (2015) find that mobile phones are mostly used between assignments and the use does not general 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 untransferrable to similar countries such as Denmark and Norway. The policy guidance is therefore: although a national ban is a low-cost policy, small or no learning gains are to be expected in Sweden and countries with similar school systems and similar investments in information and communication technology.

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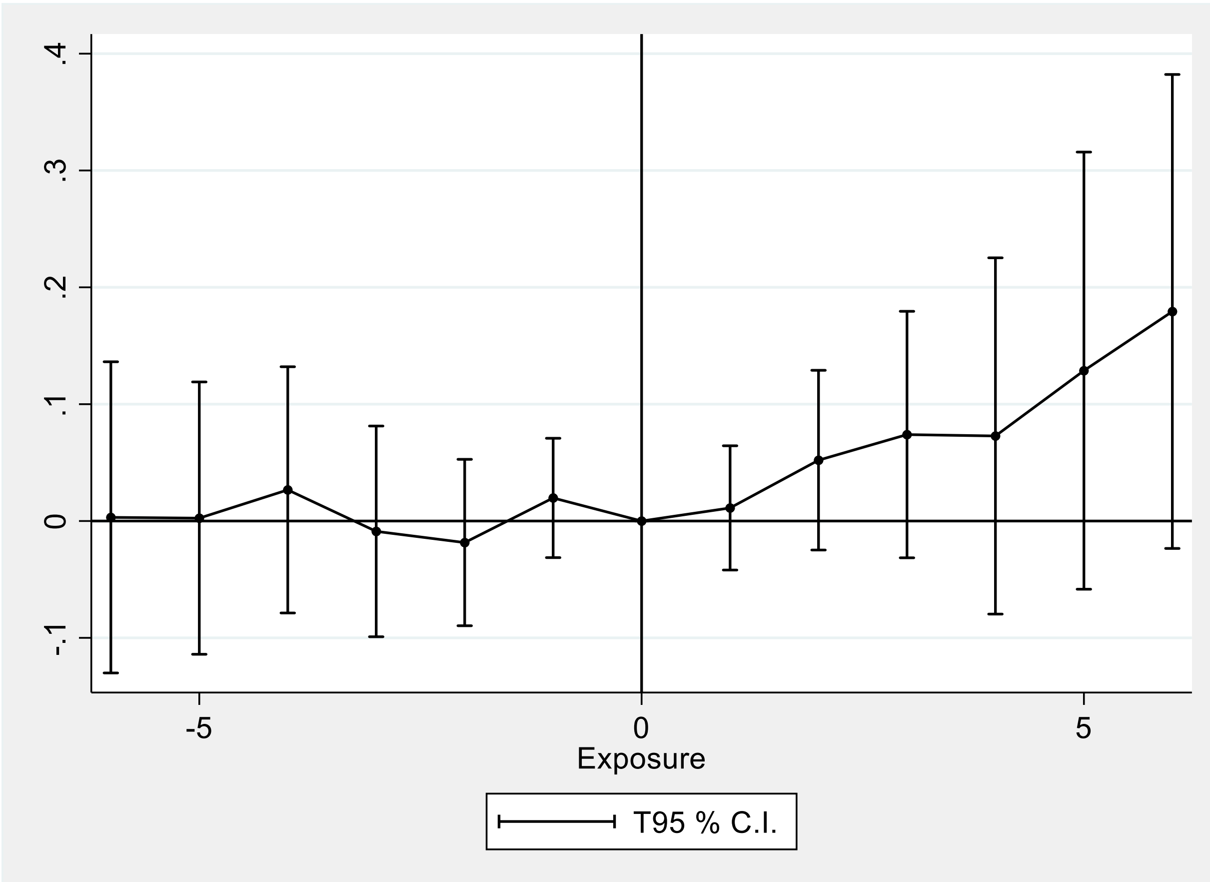
Appendix

Figure 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