Essays on Empirical Labor and Health Economics

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Acknowledgments

I started my PhD with losing about 40% of my hair, including my eyelashes, due to stress during the first year. Now, I am finishing my PhD with my hair grown back but instead addicted to caffeine.

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The best way to make friends when you move to a new city is to start practicing a new sport. Therefore, at the age of 29, I started with a sport you can only become good at if you start during your critical childhood years (i.e. below the age of 5): weightlifting. Although, my development within weightlifting has up to now been a flat curve, it has been uplifted by the number of good friends I have made. I am very happy for all the new friends I made during my time in Bergen. You made these years fun and memorable.

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Contents

Introduction

In the past few decades, many Western societies have experienced a sharp increase in economic and social inequalities along many dimensions, including education, income, and physical and mental health (United Nations Department of Economic and Social Affairs, 2020). In addition to the effects on the current generation, rising inequality may have long-run implications on the distribution of health, human capital, and income of the next generation. Relative to more advantaged children, those born to low-educated, low-earnings parents are at substantially higher risk of growing up to be low-educated and have low earnings themselves (see, e.g., Chetty et al., 2014; Björklund and Salvanes, 2011; Black and Devereux, 2010; Solon, 1999).

This phenomenon is present across countries, as parents' economic circumstances are important determinants of children's income and educational success (Lundborg et al., 2018; Chetty et al., 2014; Mayer, 2010). Furthermore, an expanding literature documents that access to higher educations is an important predictor of better economic success, well-being and health (see, e.g., Dahl et al., 2021, 2020; Oreopoulos and Salvanes, 2011; Aakvik et al., 2010; Kenkel et al., 2006; Li, 2006; Arcidiacono, 2004; Dee, 2004; Card, 1993; Kane and Rouse, 1993). Meanwhile, returns to education have increased during the last few decades. This implies that children born into poverty are at greater risk of not being able to break the link in income and education between their parents and themselves (see, e.g., Psacharopoulos and Patrinos, 2018).

The degree of income mobility varies considerably between countries. While Scandinavian countries are known to have high intergenerational income mobility, countries such as the United States and United Kingdom have substantially lower income mobility. Yet, when considering the intergenerational persistence in educational attainment it is still more or less the same across these countries (see, e.g., Carneiro et al., 2021; Landersø and Heckman, 2017; Björklund and Salvanes, 2011; Hertz et al., 2008). Despite Scandinavian countries promoting equality of opportunity, through heavily investing in children's human and health capital, the achievement gaps, such as reading and writing skills, and health inequalities between disadvantage and advantage children is significant already during childhood (Ribeiro et al., 2022; OECD, 2021; Bütikofer et al., 2021; Huttunen and Lombardi, 2021; Katikireddi et al., 2020; Dahl et al., 2014), and many barriers remain until this gap is closed.

Children's family, school, and neighborhood contexts shape their trajectories and generate correspondence between their parents' outcomes and their own. Ensuring equality of opportunity, in spite of these different circumstances many children faces, is a priority for many public policy decisions. While the desired amount of inequality differs along the political spectrum, the notion that "every child should have the same chance to succeed" seems to be a common denominator across party lines. Upward mobility is not only morally desirable – it also matters for economic growth. Economic growth can suffer when children from poor parents are hampered in living up to their economic potential, because talent becomes misallocated (Bell et al., 2019).

Societal investments in human capital may disrupt the transmission of poverty across generations, e.g., by increasing educational attainment and labor market attachment and decreasing engagement in risky behavior. An extensive literature shows that early childhood is a particularly critical developmental period and also, because of this, an opportunity for effective intervention. Shocks as early as the in-utero environment and during the first years of life, can affect individuals' later life health and labor-market outcomes (see, e.g., Cygan-Rehm and Karbownik, 2022; Karbownik and Wray, 2019b; Bütikofer et al., 2019; Black et al., 2019; Almond et al., 2018; Aizer, 2017; Aizer and Currie, 2014; Almond and Currie, 2011). A possible benefit of these type of interventions is the spillover effect on later generations; having healthier and more educated citizens may have longer-run effects by improving the outcomes of their children (East et al., 2022; East and Page, 2019). Understanding to what extent policy-driven investments are passed through to later generations can shed light on why economic status is persistent across generations. In addition, if policy driven investment spills over onto the next generations, the returns to such investment would be highly undervalued, and suggests a much lower net-cost of such programs than initially thought.

Probably the most accepted and widely used tool for governments to invest in its citizens is by providing free public education. Public education is often considered as "the great equalizer", by which inequality of opportunity and poverty can be reduced. Several studies show that interventions in the preschool and early school years can have substantial effects on schooling attainment, labor market success, and other measures of health and well-being into adulthood (see, e.g., Duncan et al., 2022; Geruso and Royer, 2018; Havnes and Mogstad, 2011; Currie and Moretti, 2003). Parts of this literature have focused on the school environment, showing the importance of the quality of education, such as teacher and peer quality, and the classroom environment, in improving students learning and human capital formation (see, e.g., Rege et al., 2021; Park et al., 2020; Bettinger et al., 2018; Roth, 2017; Figlio et al., 2016; Burke and Sass, 2013; Carrell and Hoekstra, 2010; Banerjee et al., 2007; Darling-Hammond, 2000).

Moreover, a large and growing literature that has gained importance in recent years investigates how early life health endowments and circumstances impact future health and labor market outcomes (see, e.g., Currie, 2020; Bütikofer et al., 2019; Karbownik and Wray, 2019a;

Bharadwaj et al., 2019; Black et al., 2019; Bharadwaj et al., 2018; Currie and Almond, 2011; Currie, 2009). One of the factors that has been linked to both health and school achievement is nutrition and how well a child is fed (see, e.g., Lundborg and Rooth, 2022; Lundborg et al., 2022; Bütikofer et al., 2018; Frisvold, 2015; Fitzsimons and Vera-Hernández, 2013; Hoynes et al., 2011; Victora et al., 2008; Glewwe et al., 2001; Winicki and Jemison, 2003). Childhood and adolescence are believed to be critical periods for diets of high nutritional quality (WHO, 2006). Over the last 30 years, there has been a drastic change in diets and activity patterns, and today childhood obesity has become a concern for both high- and low-income countries (Popkin et al., 2012). Almost one-third of children are overweight or obese in the OECD countries, and childhood obesity is much more prevalent among low socioeconomic families and minority groups (OECD, 2019, 2017).

Extensive research, links obesity during childhood and adolescence to having lower self-esteem and experiencing more discrimination. These factors may in turn interfere with skill acquisition and human capital accumulation, and thereby adversely affect labor market outcomes in adulthood (see, e.g., Lundborg et al., 2014; Cawley and Spiess, 2008; Janssen et al., 2004). Additionally, childhood obesity is particularly concerning as it is a strong predictor of obesity in adulthood, which is in turn linked to adverse health outcomes, such as diabetes, heart disease and certain types of cancer (OECD, 2019; WHO, 2018). As such, policy intervention may play an important role for equality of opportunity by ensuring access to nutritious diet for all children, regardless of their social background. Research suggest that healthy nutrition interventions need to occur early in childhood and adolescence in order to prevent or reverse the adverse health effects of obesity and poor eating habits (St-Onge et al., 2003). It is frequently suggested in the public debate that the availability of fast food is a sizable contributor to the increased obesity prevalence (see, e.g., Currie et al., 2010; Han et al., 2020). If this is true, this could have important public health policy implications.

This dissertation consists of three chapters, each of which aims to contribute to the literature on educational interventions, the importance of childhood nutrition on adult health and ability, and how public health interventions affect the economic status of individuals across multiple generations. These questions are explored by utilizing the richness of Norwegian administrative data in combination with external data sources, such as survey data, firm registry and archive data, and modern microeconometric tools:

Chapter 1: Distraction or Teaching Tool: Do Smartphone Bans in Schools Help Students?

How smartphone usage affects learning and well-being among children and teenagers is a concern for schools, parents, and policymakers. However, causal evidence of the effect that new technology, such as smartphones, has on student outcomes remains scarce. This paper studies the effect of banning smartphones from the classroom on students' educational outcomes and incidents of bullying in Norwegian middle schools. Combining detailed administrative data with survey data on middle schools' smartphone policies, together with an event-study design, I show that banning smartphones significantly increases girls' average grades, improves their test scores in mathematics, increases their likelihood of attending an academic high school track, and decreases incidents of bullying. Hence, banning smartphones from school could potentially be a low-cost policy tool to improve educational outcomes and reduce bullying.

Chapter 2: The Effects of Fast Food Restaurants' Proximity During Childhood on BMI and Cognitive Ability

Using spatial and temporal variation in openings of fast food restaurants in Norway between 1980 and 2007, we study the effects of changes in the supply of high caloric nutrition on health and cognitive ability of young adult males. Our results indicate that exposure to these establishments during childhood increases BMI and has negative effects on cognition. Heterogeneity analysis does not reveal meaningful differences in the effects across groups, including for those with adverse prenatal health or high paternal BMI, an exception being that cognition is only affected by exposure at ages 0–12 and is mediated by paternal education.

Chapter 3: Generational Persistence in the Effects of an Early Childhood Health Intervention

We investigate multi-generational impacts of early life health interventions. Using Norwegian administrative data with variation in the timing of infant health care center adoption between 1936–1955, we find strong evidence that the program's long term education and earnings benefits on exposed cohorts extended to their later offspring, but only for offspring who had an exposed mother. A plausible mechanism is that women exposed to the program were more likely to partner with highly educated and high earnings men. We also show that benefits accruing to the second generation are larger for those whose mothers were born in low income municipalities and municipalities that had high infant mortality rates, suggesting that public investments in early childhood health can be important levers towards increasing future generations' equality of opportunity.

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Chapter 1

Distraction or Teaching Tool: Do Smartphone Bans in Schools Help Students?*

Sara Abrahamsson†

Abstract

How smartphone usage affects learning and well-being among children and teenagers is a concern for schools, parents, and policymakers. However, causal evidence of the effect that new technology, such as smartphones, has on student outcomes remains scarce. This paper studies the effect of banning smartphones from the classroom on students' educational outcomes and incidents of bullying in Norwegian middle schools. Combining detailed administrative data with survey data on middle schools' smartphone policies, together with an event-study design, I show that banning smartphones significantly increases girls' average grades, improves their test scores in mathematics, increases their likelihood of attending an academic high school track, and decreases incidents of bullying. Hence, banning smartphones from school could potentially be a low-cost policy tool to improve educational outcomes and reduce bullying.

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1.1 Introduction

The increasing use of technology, particularly the growing smartphone usage, by children and adolescents has led to concerns about the effects on young people's cognitive, physical, and socioemotional development. In the United States, more than 95% of teenagers report owning a smartphone or having access to one (Pew Research Center, 2018) and, on average, teenagers spend more than 2 hours online per day after school (OECD, 2019). Of particular concern is whether screen-based activities are detrimental to children and adolescents' learning and well-being. Currently, there is a policy debate on whether smartphones and tablets should be used as teaching tools in classrooms or whether they distract students. For instance, in France, the government recently banned mobile phones during school hours for students from kindergarten to high schools, arguing that they distract students from learning (Rubin and Peltier, 2018). By contrast, the New York mayor's office removed a 10-year ban on mobile phones in public schools in 2015, stating that the ban increased inequalities among students (Allen, 2015).[3]

While technology has generally been viewed as increasing productivity (Acemoglu and Dell, 2010; Brynjolfsson and Yang, 1996), the impact of smartphone use in schools on student outcomes and well-being is ambiguous (Amez and Baert, 2020; OECD, 2019). Although students might use them in a productive way, smartphones can act as a distraction. Thus, a smartphone ban could potentially either help or hinder student learning. Both the behavioral and psychology literature have found multitasking to be detrimental not only to attention, but also more specifically to learning (see, e.g., Smith et al., 2011; Abouk and Adams, 2013; Rana et al., 2019; Mendoza et al., 2018; Glass and Kang, 2019). Additionally, if phones lower the cost of bullying by making it less salient for teachers and adults, a ban could lower the incidence of bullying and thereby indirectly enhance human capital accumulation. Despite these contrasting theories, there is little causal evidence on the effect that a smartphone ban would have on students' academic outcomes and, particularly, on bullying.

This paper contributes to this debate by studying the effects of banning smartphones from Norwegian middle schools on students' educational outcomes and the incidence of bullying. I leverage quasi-experimental variations in Norwegian middle schools introducing smartphone bans that limited usage among students. I employ a nonparametric event-study design to identify causally the time-varying impact of banning smartphones from the classroom on students' outcomes and bullying. The focus is on four main outcomes: (i) average grades set by teachers, (ii) grade point average (GPA) at the end of middle school, (iii) the probability of attending an academic rather than a vocational high school track, and (iv) incidents of

[3] It was argued that the ban increased financial inequalities and inequity as certain schools, especially those in low socioeconomic areas, had metal detectors at the school entrance and students were required to pay outside vendors to store phones during school hours. In addition, it was argued that lifting the ban assisted parents to stay in contact with their children, especially before and after school.

bullying. Specifically, I evaluate whether a smartphone ban affects average grades set by students' teachers, students' GPA – the diploma received at the end of middle school which is calculated as a weighted average of teachers' assessments combined with externally graded exams and test scores – and their progression into an academic or vocational high school track. In addition, I combine school-level aggregated data on students' perceptions of bullying to evaluate the effect that a smartphone ban has on bullying. All four outcomes are important indicators for later education and success in the labor market. Moreover, I test for important differences in the effect of smartphone bans for test scores on the three core subjects in school: mathematics, Norwegian and English. Additionally, I perform detailed heterogeneity analysis across sub-groups of students.

There are no national guidelines on smartphone use in Norwegian schools. Instead, schools make autonomous decisions on whether to allow or ban smartphones. Over the last 10 years, this has resulted in variations in the timing of smartphone bans being implemented across schools. As there is no centrally collected information on smartphone bans in schools, I used a survey to collect data from Norwegian middle schools on their smartphone policies, and whether and when they had introduced any smartphone regulations. Then, I matched schools' responses from the survey to Norwegian Registry data, which include information on middle-school grades set by students' own teachers and externally corrected exams, GPAs, and individuals' choices of academic or vocational high schools. A bullying measurement is available from the Norwegian Pupil Survey, implemented yearly since 2007 by the Norwegian Directorate for Education and Training.

The validity of my research design rests on the assumption that the timing of a school adopting a smartphone ban is uncorrelated with other determinants of student outcomes. I provide different pieces of evidence that the main identification assumption is likely to hold. First, I show that school, student, and teacher baseline characteristics cannot predict the timing of when a school implements a ban. Second, I show that schools that implemented smartphone bans in different years did not experience changes in baseline characteristics prior to the introduction of the bans. Moreover, the event-study framework demonstrates that both pre- and post-policy, school, teacher, and student characteristics do not change. This suggests that endogenous compositional changes are not driving my results.

The findings show that post-ban, girls who are exposed to a smartphone ban from the start of middle school make gains in average grades set by teachers and externally graded mathematics exams. Post-ban girls gain 0.07 standard deviation in teacher awarded grades and have, on average, 0.25 standard deviations higher mathematics test scores compared to girls not exposed to a ban. Additionally, I find that girls are 4-7 percentage points more likely to attend an academic high school track after experiencing a ban. This effect amounts to an 8–14% point increase in the probability of attending an academic high school track relative to the pre-ban years. These effects are only significant for girls who are exposed to a smartphone ban for at least 2 years or more in middle school.

The effect on teacher awarded grades is largest among students attending middle schools that ban students from bringing their phones to school or alternatively, schools where students must hand their phones in before classes start. Girls at these schools, gain on average 0.12 standard deviations on teacher awarded grades when exposed from the start of their 3-year middle-school education. Additionally, the findings show that post-ban, girls attending middle schools that prohibit and strictly limit smartphone usage during school hours, also have significantly higher middle-school GPA. At these schools, girls exposed to a smartphone ban from the start of their middle-school education gain on average 0.10 of a standard deviation in GPA compared with girls without exposure to the ban. For girls who are partially exposed to a smartphone ban during their middle-school years, or schools that allow their students to use their phones during breaks and only require phones to be on silent mode during classes there is no effect, either on teacher awarded grades or GPA.

The magnitude of teacher awarded grades, GPA, and the probability of attending an academic high school track is larger for girls from low socioeconomic backgrounds. For instance, girls from low socioeconomic families who are exposed from the start of their middle-school education, have on average 0.13 standard deviations higher teacher awarded grades and 0.11 standard deviations higher GPA, and are 6 percentage points more likely to attend an academic high school track compared to unexposed girls. Post-ban, boys from low socioeconomic families, make gains in teacher awarded grades in mathematics and Norwegian. These important differences suggest that unstructured technology is especially distracting for students from low socioeconomic families, whereas students from high socioeconomic families do not experience any negative externalities. However, I find no effect on boys' average grades set by teachers, GPA or on the probability of them attending an academic high school track. The heterogeneity in the patterns between girls and boys could result from the substantially higher phone usage among girls. More than 70% of girls of middle-school age in Norway report that they spend more than 2 hours a day on their phones, whereas only 54% of boys say the same. Additionally, almost 60% of girls report that they spend 2 or more hours on social media, whereas, by comparison, only 32% of boys do the same(Medietilsynet, 2018).

In addition, my results show that banning smartphones overall lowers the incidence of bullying between 0.29–0.40 of a standard deviation among girls when girls are exposed from the start of their middle-school years. For boys, I show that there are important differences in bullying post-ban depending on the exact ban that a school implements, and on the socio-economic status of the schools. Boys attending a lower socio-economic school or a school that bans phones throughout the day experience a decline in bullying 2 years after a ban implementation relative to pre-ban years by -0.35 and -0.83 standard deviations, respectively. In addition, the decline in bullying among both girls and boys is driven by pupils who attend smaller schools. Hence, by banning smartphones, schools not only positively impacted educational outcomes, but also decreased the level of bullying. My main results are robust to a battery of specification checks, including an alternative estimation strategy.

I contribute to the literature in several important ways. Although two existing studies examine the effect of banning mobile phones from the classroom on test scores (Beland and Murphy, 2016; Kessel et al., 2020), my study, to the best of my knowledge, is the first to examine the causal effect of banning smartphones on students' progression into high school education. More generally, a handful of studies investigate phone use and its association with students' higher education outcomes (see, e.g., Amez and Baert, 2020) but the majority of these are descriptive, with the exceptions of Beland and Murphy (2016) and Kessel et al. (2020). Similar to this paper, these two studies investigate how mobile phone bans affect students' test scores in the UK and Sweden, respectively. Beland and Murphy (2016) document that banning mobile phones has a positive effect on test scores, especially for disadvantaged and underachieving pupils. As Beland and Murphy (2016) study student outcomes between 2001–2011, their results largely cover a period when mobile phone ownership was much lower, smartphones barely existed, and phones had little value as a teaching tool. Today, this situation is very different. Additionally, my study provides novel evidence not only on test scores, but also on how banning smartphones affects several dimensions of student outcomes.

Kessel et al. (2020) study the effect of banning mobile phones on test scores in a much more recent period; 1997–2018. They find no effect of banning mobile phones on students' test scores. However, their data are aggregated at the school level, restricting them from examining heterogeneous effects across different individuals. The data I use allow for an in-depth heterogeneity analysis throughout the student's schooling. For this reason, I can shed light on the consequences of unstructured technology in the classroom and the impact it has on the gender and socioeconomic gap in education (Almås et al., 2016; Autor and Wasserman, 2013). In addition, the results from this study are of direct policy relevance as schools and policy makers constantly seek innovative ways of improving student outcomes.

The most novel contribution of this paper lies in providing causal evidence that banning smartphones lowers the incidence of bullying among middle school students. A regional level study, by Beneito and Vicente-Chirivella (2022), showed that after two regions in Spain introduced a ban against mobile phones, bullying decreased in both regions and PISA scores increased in one of the regions. As the authors use regional-level data, in combination with only two treated regions, important heterogeneity differences is not being analyzed. A few previous papers have found a negative relationship between social well-being and later lifetime outcomes, including education and earnings (Currie and Stabile, 2006; Lundborg et al., 2014). More specifically, bullying has been found to have severe physical and emotional long-term consequences for students. The large individual and societal cost has increasingly led teachers, parents, policymakers, and the media to draw attention to bullying and methods to stop it. Despite this, there has been a lack of credible causal evidence how to tackle bullying.

My results suggest that a low-cost intervention such as banning smartphones from schools might be an effective policy tool to reduce bullying. The potential costs of failing to treat bullying in schools are large, as being bullied as a child or teenager influences adult health, education, and earnings (Drydakis, 2014). The long-term societal cost of bullying is estimated at 1.7 million USD per bullied person (Nilson Lundmar et al., 2016).

More generally, this study contributes to the literature on technology in the classroom and the impact on student's achievements. Most previous studies focus on the impact of introducing or having access to technology, such as introducing computers in the classroom and the impact on student achievement. However, the resulting evidence is mixed (Hall et al., 2019; Escueta et al., 2017; Barrow et al., 2009; Banerjee et al., 2007; Angrist and Lavy, 2002). Unlike these studies, I consider a type of technology that is highly accessible to teenagers but, in contrast with the computers in the classroom, is not necessarily considered a teaching tool.

The remainder of the paper is structured as follows. Section 1.2 describes the institutional setting. Section 1.3 describes the data and section 1.4 describes the identification strategy. Section 1.5 presents the empirical findings and section 1.6 discusses several robustness checks of the results. Section 1.7 discusses the results and section 1.8 concludes.

1.2 Institutional Background

Norwegian compulsory education starts at 6 years of age and lasts for 10 years. There are two levels of compulsory education: primary school (grades 1–7) and middle school (grades 8–10). Usually, students commence middle school in the year in which they turn 13 and finish compulsory schooling in the year that they turn 16 years old.

Compulsory education is financed by grants from the central government as well as local income taxes. The syllabuses are centrally determined by the Norwegian Directorate for Education and Training. There is no streaming by ability in compulsory education. In primary school, children are not graded. In middle school, grades are set according to national standardized learning goals and students take standardized national tests in grades 5, 8, and 9. In most counties, the scores from the exit exams in middle school and grades from teachers are crucial for admission into different high schools.[4]

Most students attend public schools. In 2019, only 4% of children attended a private or independent school. Municipalities are responsible for organizing compulsorily schooling in public schools. To receive public funding, schools are not allowed to charge any tuition fee. School assignment in public primary school is based on fixed school catchment areas within municipalities through a distance-from-home-rule.

Despite the clear rules on educational content, the Norwegian Directorate for Education and Training gives school principals the discretion to determine how to allocate funds, what

[4]Assignment to high schools varies across counties. Twelve of the 19 counties in Norway had a free school choice system in 2016. In rural counties, geographic criteria still largely determine student high school choice.

teachers to hire, and what detailed rules are imposed within school grounds. By law, each school must have a stated code of conduct following the Norwegian Education Act, §9 A-10. However, it is up to each school to decide what kind of rules and regulations to include, as long as they are within the framework of the Education Act, the Human Rights Act, and private school laws. Each school's code of conduct should state the rights and obligations of the students and include rules about conduct and the measures that can be used against students who violate the rules. Each municipality is responsible for ensuring that each school has elaborated its code of conduct.

Smartphone bans are one rule that each school can determine. As there are no national guidelines or recommendations over students' phone usage in school, schools are free to decide their own policy. That is, schools are free to regulate students' phone usage within the framework of the school regulations by, for example, prohibiting the use of smartphones during class time. However, schools cannot forbid students from bringing their smartphone to school, as schools cannot regulate the leisure time of the students, i.e., their use on their way to and from school. If students do not comply with the rules, the schools may take measures against the students. In regard, to smartphone usage, for instance, a teacher may seize a student's phone during school hours if they use it in a manner against the school rules. However, schools are not allowed to keep students' phones after the school day ends (Utdanningsdirektoratet, 2020b).

After middle school, students may enroll in high school (grades 11–13). High schools are organized at the county level. All student aged 16 to 23 years in Norway have a statutory right to enrollment at high school. This right is at the county level and does not ensure enrollment in a specific school or program. About 98% of students enroll in high school in Norway in the first year. About 50% of the students enroll in general studies, 45% in vocational programs, and 3% in alternative training plans.[5]

1.3 Data

For this study, I link three primary data sources: a compilation of Norwegian administrative data sets, including the national educational registers, family registers, and tax registries; a nationwide pupil survey; and data on middle schools' smartphone policies. I study a sample of students who completed grade 10 between 2010 and 2018. The combined data sources allow me to explore how smartphone policy affects students' educational outcomes and bullying, using a dynamic event-study design together with a host of robustness checks.

1.3.1 Individual Level Data

The Norwegian Registry data cover the entire population in Norway up to 2018 and are a collection of different administrative registers, including the central population registry, the

[5] Note that only 80% of students initially enrolled in general studies programs graduate and that graduation rates for vocational programs are even lower.

family register, the education register, and the earnings and tax register. From these registers, I obtain detailed background information about children and their parents on demographic variables, including gender, date and place of birth, residency, educational attainment, earnings, and immigration status. The parental identifier enables me to match children to their parents. Earnings are not top-coded and include all pension-qualifying income, that is, labor earnings, taxable sickness and unemployment benefits, and parental leave payments.

Schools report student grades directly to Statistics of Norway, and grades are available for cohorts born between 1986 and 2002. This includes grades set by the teacher and those from externally graded exams. From grade 8, students begin to receive teacher-awarded grades in each subject. In the final year of middle school, students take written and oral exams. Three days before the exams, students are informed which subjects their exams will cover. Their written exam could be in mathematics, Norwegian, or English, and with the exam subject being decided at the school level. Oral exams are quasi-randomly selected at the student level and, in addition to mathematics, English and Norwegian, could cover a second language, social science, religion, or natural science. Both written and oral exams are externally graded, with the grades ranging from 1 (the lowest grade) to 6 (the highest grade).

At the end of grade 10, all students obtain a diploma with a total GPA that represents the weighted total of all teacher-awarded grades combined with the exam grades. The middle-school GPA ranges from 0 to 60, where 60 is the best possible grade. These grades are used when applying for high schools and high school programs in a majority of counties. As such, these are high-stakes tests because the scores have long-run impacts on educational possibilities.

Additionally, the education registry contains national exam test scores for cohorts born between 1997–2002. National exams are nationally organized and externally graded. Students take national exams in mathematics, reading, and English in grades 5 and 8. In grade 9, students take a national exam in mathematics and reading. Information from the national exams forms the basis for undergraduate assessment and quality development at all levels of the school system. I use the test scores from grade 5 to condition on students' achievements before they enter middle school.[6] High school programs are generally divided between academic and vocational tracks. The data allows me to identify in what type of high school program students enroll in the first year.

In my analysis, I use several measurements of student performance as outcome variables. Two of my main outcome variables are average grades set by teachers and middle-school GPA.[7] I separately look at grades that are assigned by a student's own teacher and test scores that are externally graded. Additionally, I use several alternative test score measures to examine heterogeneous effects of the results. In particular, I examine average grades and

[6] In contrast to the test in grade 10, these national exams involve smaller stakes for students.

[7] All grades standardized by cohort, with a mean of zero and standard deviation of one.

externally corrected test scores separately in the core subjects (mathematics, Norwegian, and English) to investigate whether a ban against mobile phones might have heterogeneous effect on different subjects.

While middle-school grades, GPA and test scores focus on short-term impacts, I also study students' progression into high school education. Specifically, I investigate whether a ban against smartphones in schools affects the type of high school track in which students enroll. For this, I construct a measure for whether students attend an academic or vocational program. High school program choice is associated with long-term human capital enhancements in education and labor market outcomes (Hanushek et al., 2017) and thus captures a broader set of skills and aspirations compared with test scores.

Importantly, the registry data allow me to test whether the introduction of a smartphone ban changes the composition of the school intake in terms of student, school, and teacher characteristics. By linking the employer-employee registry with the education registry, I construct teacher and principal characteristics at the school level, including type of education, years of experience, and gender ratio.

1.3.2 Pupil Survey

The Norwegian Education Act, §9 A-9, states that each school is responsible for providing a safe environment for children. Thus, strict measures must be taken against any form of bullying at school, such as physical or mental harassment, regardless of whether it occurs online or in person. The Norwegian Directorate for Education and Training administrates an annual national Pupil Survey in which students are asked about bullying, learning, and social well-being in school. The answers are generally used by the schools, the municipality, and the central government to improve the schools. Participation in the survey is compulsory for all schools. The survey is conducted in grades 7, 10 and 13, the last year of high school (Utdanningsdirektoratet, 2020a). As the Pupil Survey contains unique school identifiers for each school, I can link the survey data to the registry data as well as other school-level data.

The data from the Pupil Survey are aggregated at the school level and are available for years 2007–2019. The exact questions vary across years. However, four areas are consistently covered each year: (i) whether students have experienced bullying, (ii) their level of motivation, (iii) their social well-being, and (v) pupil democracy. The responses are measured on a scale from 1 to 4 for bullying, with a value close to one being desirable as it represents low levels of reported bullying. Motivation, social well-being, and democracy are measured on a scale from 1 to 5; a high value is desirable and expresses better circumstances in terms of these variables.

Bullying is defined as repeated negative actions by one or more person/s, against a student who may have difficulty defending him- or herself. It can be calling another person mean names and teasing them, holding a person off, talking behind their backs, pushing, or hitting.

The measurement of bullying is based on students' answers to several specific questions concerning whether they themselves have been exposed to these kinds of actions, with responses varying from "not at all" (1) to "several times a week" (4). I use this composite variable to measure bullying. Questions regarding motivation refers to students' inner motivation. Students answer questions relating to how interested they are in learning at school, how much they like school work, if they look forward to going to school, if they prioritize school work during classes, and at home and whether they are motivated to work even if they find the subject difficult. Students provide answers on a scale from "completely disagree" (1) to "completely agree" (5). For social well-being, students answers questions concerning whether they thrive at school, with answers ranging from "completely disagree" (1) to "completely agree" (5). For pupil democracy, students answer questions related to whether the school listens to students' suggestions. Similar to motivation and social well-being, this question is answered on a scale from "completely disagree" (1) to "completely agree" (5). These three variables are used to evaluate mechanisms that might explain the results for educational outcomes and bullying. All variables are reported as the mean among students in grade 10 at school and by gender. Answers from grade 10 students are selected because this is the year in which the middle-school GPA is also measured. To assist interpretation, I standardize these variables to have a mean of zero and a standard deviation of one at the yearly level.

1.3.3 School Smartphone Policy Data

The identification strategy that I use relies on comparing the outcomes of cohorts with variations in treatment exposure to smartphone bans at different schools. This requires knowing the exact year in which each school implemented a ban regulating smartphone usage during school hours. As noted above, schools are free to set their own policy regarding smartphones and other electronic devices in the classroom. As there are no centrally collected data on school policies regarding electronic devices, I collected data on mobile phone policies by sending out a short online survey to all middle schools in Norway in 2019. In total 1,250 middle schools received the survey via an email directed to the principal of each school. The survey contained questions about the school's current policy regulating students' phone usage and the year in which any smartphone policy was introduced. The full questionnaire is provided in the Appendix 1.B. Questions regarding the type of policy and how strict it is were also included in the survey. A total of 529 schools had answered the survey by March 2020, for a response rate of 42.3%.

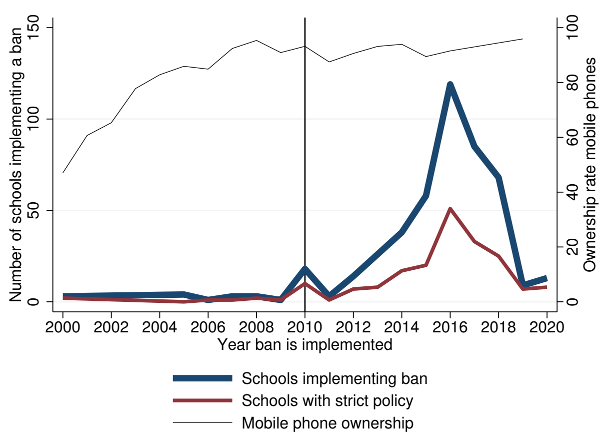
1.3.4 School and Municipality Level Data

I use several supplementary data sources. As my analysis period includes the financial crisis, I control for the unemployment rate in a robustness check. Given that Norway is an oil-producing country, the unemployment level fluctuates with the price of oil. For example, in 2016, unemployment increased to 5% following the decline in the price of oil (Cappelen et al., 2016). I use data on the municipality-level unemployment rate provided by the Norwegian Centre for Research Data to control for the level of unemployment at the municipality level. Additionally, I obtain data on the number of PCs per student in each school for the years 2003–2013 from the Norwegian Directorate for Education and Training. I use this data as a baseline characteristic for technological adaptation at school when performing predictions when a school introduces smartphone bans. The data on municipality-level unemployment and PCs per student are linked to the registry data using municipality and school identification numbers.

1.3.5 Descriptive Statistics

Figure 1.1 plots the number of schools introducing a ban against smartphones in a given year. The figure documents that bans against smartphones in middle schools were unusual before 2010. After 2010, bans were introduced at an increasing rate, with the number of implemented bans peaking in 2016, when 119 middle schools implemented a ban. I define middle schools as having a strict phone policy if they either (i) ask students not to bring their phones to school or (ii) collect phones before classes and store them in a "mobile phone hotel". On average, 42% of the schools that implemented a ban have strict smartphone policies. As shown in Appendix 1.A Figure A1, it is most common for schools to allow students to use their phones under certain conditions, as long as it does not distract the class.

Figure 1.1: Introduction of Smartphone Bans Over Time at Middle Schools



Notes: The number of schools implementing a smartphone ban each year. Strict bans against phones are defined as: (i) students are not allowed to bring phones to school, or (ii) phones must be handed in before the class starts and stored in a “mobile phone hotel". Lenient bans allow phone usage during breaks. The vertical line in 2010 indicates the first year that a cohort in my sample starts middle school. *Source smartphone ban*: Own data. The ownership rate of mobile phones is defined for 12–16-years-olds. *Source ownership rate*: Culture and Media Use Survey, Statistics Norway.

The left-hand y-axis of Figure 1.1 shows that the ownership of smartphones/mobile phones is very common among adolescents in Norway. The average ownership rate is above 95% for adolescents aged 12–16 years. In comparison, computer ownership among adolescents is much lower, with only 70% having their own computer (Medietilsynet, 2020). Survey evidence from Norway indicates that more than 60% of all teenagers in grade 10 spend 2 or more hours on their phone each day. However, the difference by gender is large. More than 70% of girls answered that they spend 2 or more hours on their phone each day, compared with only 54% of boys. The gender difference for social media use is even larger; 60% of girls spend 2 or more hours on social media per day, compared with only 32% of boys (Medietilsynet, 2018).

The high ownership rate of smartphones among Norwegian adolescents is not surprising given that Norway is one of the most advanced countries in the world in terms of consumers adopting digital media and technological implementation. Despite its many remote areas and the mountainous landscape, as early as 2007, 90% of the Norwegian population had access to 3G-coverage. This is documented in Appendix 1.A Figure A2. By 2015, 4G-coverage was fully available for the Norwegian population. Despite not having individual-level data on smartphone usage, these aggregated numbers show that school regulations targeting smartphones affect most adolescents and impact individuals' smartphone usage during school hours.

The geographical coverage of schools implementing a smartphone ban is well spread out across the country. Figure A3 presents Norwegian municipalities with at least one school implementing a smartphone ban. In total the data on smartphone bans covers 288 of the 425 municipalities, or 68%.

My baseline data set contains 151,925 observations. I do not make any sample restrictions except that I include only individuals who attend a middle school with a known smartphone policy and individuals for whom I can observe GPA, grades, and test score data in grade 5. Appendix 1.A Table A1 compares the average characteristics and outcomes of individuals in schools where I was able to obtain information on the smartphone policy to average for individuals attending schools where information is lacking. Comparing standardized test scores, we see that sampled schools have, on average, a somewhat higher GPA and that pupils' parents have slightly higher incomes and education. However, there is no difference in bullying experience, or gender balance between students in responding versus nonresponding schools. While this limits the external validity of my results, these differences pose no threat to my identification strategy that focuses on schools participating in the survey.

1.4 Empirical Strategy

To investigate how students' educational outcomes and bullying are affected by a ban against smartphones, I rely on a difference-in-difference approach. I exploit variation within-school and cross-cohort differences in exposure to smartphone bans induced by the timing of schools' autonomous phone regulation decisions. Although a smartphone ban might have an immediate impact on student outcomes, its effect on students' educational outcomes and experience of bullying could vary over time for two main reasons. First, some cohorts of students are only exposed to a ban for part of their middle-school years. This might generate time-varying treatment effects based on the length of exposure to the policy. Second, the ban itself might have time-varying impacts on local school conditions, norms over phone usage, and resources allocated to students, such as teachers' time and effort. Therefore, the effect of a ban might be different in the first, second, or third year after its introduction. To allow for such time patterns, my main empirical strategy is an event-study model that nonparametrically traces out these time-varying treatment effects.

To estimate unbiased effects, the timing of a school adopting a smartphone ban needs to be uncorrelated with other determinants of student outcomes. I start by presenting evidence from an empirical test to support this key identification assumption. To test for this, I study whether school, student, and teacher characteristics can predict the implementation of a smartphone ban. Table 1.1 presents estimates for η in the following equation:

$$Year_s = \eta X_{s,c0} + \pi_f + \chi_s \tag{1.1}$$

where $Year_s$ is a dummy variable for whether a school s implemented a ban before 2013 or 2015. $X_{s,c0}$ is a vector of pre-ban school-level characteristics for schools, students, and teachers measured in 2007; 2007 is prior to the introduction of smartphone bans for the vast majority of schools and is also the first year when measurements of bullying are available. In particular, columns (i) to (iii) in Table 1.1 show that student characteristics during the time period of interest fail to predict when a smartphone ban is introduced. Importantly, neither students' performance, the share of students later attending an academic high school track, nor bullying can predict when a school implements a smartphone ban. Moreover, the results do not indicate that teacher characteristics, such as gender ratio, education, and experience predict an early implementation of a smartphone ban.

Second, I examine whether the timing of the introduction of smartphone ban was correlated with changes in student, school, and teacher characteristics using Equation 1.1 . This could be the case if, for instance, smartphone bans were implemented earlier in schools that had declining average GPA or increased bullying levels. The results are presented in columns (iv) to (vi) in Table 1.1. There does not appear to be a significant correlation between the timing of the implementation of a smartphone ban and changes in student, school, and teacher characteristics from 2007 to 2010. The only exception is that schools with a larger share of female teachers were more likely to implement a ban before 2015. Controlling for the share of female teachers at schools does not change my main results. Altogether, there seems to be a lack of systematic correlation between when schools implement a ban and both the level of and changes in students' socioeconomic, school, and teacher characteristics, as well as technological adoption at schools.

Table 1.1: The Effect of School, Student and Teacher Characteristics on the Timing of Smartphone Bans

	2007 baseline characteristics		Changes in characteristics baseline between 2007 and 2010	
	Implementing before 2013 (i)	Implementing before 2015 (ii)	Implementing before 2013 (iii)	Implementing before 2015 (iv)
Student characteristics				
Average GPA	0.006	-0.002	0.011	0.023
	(0.014)	(0.018)	(0.011)	(0.015)
Share of students starting at an academic high school track	-0.000	0.062	-0.077	-0.313
	(0.303)	(0.407)	(0.239)	(0.324)
Average income of father\1000	-0.000	-0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Average income of mother\1000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Share of students with fathers with a post-secondary-education	-0.158	-0.517	-0.267	-0.185
	(0.276)	(0.397)	(0.255)	(0.344)
Share of students with mothers with a post-secondary-education	-0.165	0.164	-0.153	-0.028
	(0.310)	(0.389)	(0.274)	(0.357)
Share of students with foreign-born parents	-0.246	-0.317	0.096	0.395
	(0.500)	(0.535)	(0.413)	(0.593)
School and teacher characteristics				
Number of students	-0.000	-0.001	-0.002	-0.001
	(0.001)	(0.001)	(0.001)	(0.002)
Share of foreign-born students	0.696	0.651	-0.101	-0.153
	(0.630)	(0.782)	(0.176)	(0.321)
Share of female students	0.273	0.739	-0.004	0.253
	(0.278)	(0.395)	(0.192)	(0.276)
Average experience of teachers	0.002	-0.001	-0.011	-0.007
	(0.006)	(0.008)	(0.008)	(0.010)
Share of female teachers	0.234	-0.053	0.507	0.233
	(0.175)	(0.231)	(0.237)	(0.317)
Share of employees with a teaching degree	0.079	0.053	0.072	0.128
	(0.220)	(0.295)	(0.282)	(0.415)
Average experience of principal	-0.000	0.000	0.001	0.003
	(0.002)	(0.003)	(0.002)	(0.002)
PCs per student	-0.016	-0.008	0.017	-0.005
	(0.015)	(0.020)	(0.018)	(0.023)
Reported bullying	-0.077	-0.167	0.073	-0.007
	(0.110)	(0.132)	(0.074)	(0.102)
Observations	347	347	347	347
P-value from F-statistics	0.678	0.263	0.403	0.799
R^2	0.089	0.115	0.094	0.101

Notes: Each column represents a separate linear probability model of the likelihood of the implementation of a smartphone ban in a given period in relation to various student, school and teacher characteristics or changes in various student, school and teacher characteristics. Student and school characteristics are measured among students who finished grade 10 in 2007 and among teachers and principals employed during the 2007/2008 school year. The experience of principals and teachers is defined as the number of years employed at any school. The regression controls for county fixed effects. Robust standard errors are shown in parentheses. Significance levels: *** 1% level, ** 5% level, * 10% level.

1.4.1 Event-Study Specification

My empirical strategy exploits the staggered adoption of smartphone bans between schools within a flexible event-study framework in a manner similar to Bailey and Goodman-Bacon (2015). Formally, for individual i, who is in cohort c and attending middle school s:

$$Y_{ics} = \alpha_0 + \lambda_s + \theta_c + \gamma X_{ics} + \sum_{y=-4}^{-2} \psi_y D1(c - T_s^* = y)_{is} + \sum_{y=0}^{5} \psi_y D1(c - T_s^* = y)_{is} + \epsilon_{ics} \quad (1.2)$$

where Y_{ics} is the reduced form outcome of interest (teacher awarded grades, GPA, various test score measures, or the probability of attending an academic high school track). λ_s is a set of school fixed effects that absorb time-invariant differences between schools. This allows for consistent estimates of ψ even in the presence of unobserved differences between schools. The cohort fixed effect, θ, controls for common time-specific shocks within cohorts that might be correlated with the introduction of a smartphone ban or educational outcomes.

X_{ics} is a set of individual and family characteristics, including the individual's gender, parental background characteristics, such as the mother's education and income, mother's age and marital status at birth, and father's education and income, father's age at birth, the individual's birth order, a dummy for whether individual i is 1 year older than his or her peers, and a dummy for whether individual i is 1 year younger than his or her peers. In a robustness check, I additionally control for the yearly unemployment rate at the municipality level, as the period considered includes the years 2007–2008 when the financial crisis emerged, and the decline in the price of oil from 2014. These factors have little effect on my results.

D_s is a binary indicator for treatment that is equal to 1 from year T_s^*, which is when a school implements a ban. The event-year dummy, $1(c - T_s^* = y)$, is equal to the number of years of exposure that a cohort has to a smartphone ban, with c being the cohort and T_s^* being the implementation year of the smartphone ban at school s. For example, a cohort that finishes middle school in 2018 and is attending a middle school that adopted a smartphone ban in 2017 will have an exposure time of 1. On the other hand, a cohort that finishes middle school in 2015 and is attending a middle school that adopts a smartphone ban in 2018 will have an exposure time of –3. As middle school is 3 years, cohorts with an exposure time of 3 are the first cohorts to be fully exposed to a smartphone ban at middle school s.

The ψ estimates measures the intention-to-treat (ITT) effects of the smartphone ban on students' educational outcomes. In the regression, ψ_{-1} is omitted such that all ψ estimates are relative to the year prior to the smartphone ban adoption. Observations more than 4 years before or 5 years after the mobile phone ban is implemented are captured by dummies $1(c - T_s^* = -4)$ and $1(c - T_s^* = 5)$.[8] Standard errors are clustered at the school level.

The ψ coefficient nonparametrically captures pretreatment relative trends (ψ_{-4} to ψ_{-1}) before a smartphone bans was implemented, as well as time-varying treatment effects (ψ_0 to

[8] I choose this event-year window because the sample size is small beyond these values. Note that the binned endpoints are –4 and +5 and that I show estimates from –3 to +4. End point results are not shown in the graphs as they are a combination of several event-years and as such, should not be interpreted as treatment effects (Bailey and Goodman-Bacon, 2015).

ψ_5). ψ_{-4} to ψ_{-1} allow for a direct evaluation of the assumption that cohorts at schools implementing a smartphone ban would have had the same outcomes as other cohorts at schools without a smartphone ban in the absence of the ban. If there are any pretreatment trends before the introduction of a smartphone ban, this would suggest a deviation from the secular trends. In other words, the design allows me to evaluate directly whether the timing of the ban is uncorrelated with other determinants of student outcomes.

Conditional on the control variables, the variations arise from two sources. The first is within-school differences in exposure of different cohorts driven by the schools' decision and implementation of a ban. The second source of variation comes from cross-school differences in the timing of adopting smartphones bans.

Including previous test score results changes the interpretation from the change in test scores in Equation 1.2 to individual i's gain in test scores.

$$Y_{ics} = \alpha_0 + \alpha_1 Y_{ics-1} + \lambda_s + \theta_c + \gamma X_{ics} + \sum_{y=-4}^{-2} \psi_y D1(c - T_s^* = y)_{is} + \sum_{y=0}^{5} \psi_y D1(c - T_s^* = y)_{is} + \epsilon_{ics} \tag{1.3}$$

Y_{ist-1} represents individual i's national exam test score in grade 5 and accounts for ability, family, and school investment up to grade 5. Below, I first show estimates without controlling for previous test scores to evaluate whether there was an increase in test scores. Then, I show estimates controlling for previous test scores to evaluate the gain in test scores after smartphones were banned.

In contrast to the educational outcomes, bullying is measured at the school level. I use the same event-study model as in Equation 1.2 but on the school level to estimate the effect of banning smartphones on incidents of bullying. Formally, I regress the following equation for school s and year t:

$$Y_{st} = \alpha_0 + \lambda_s + \theta_c + \gamma X_{st} + \sum_{y=-4}^{-2} \psi_y D1(t - T_s^* = y)_s + \sum_{y=0}^{5} \psi_y D1(t - T_s^* = y)_s + \epsilon_{st} \tag{1.4}$$

where Y_{st} is a standardized indicator for bullying. For the school-level analysis, I include the average test scores for students in grade 5, together with the average income, education, age, and marital status of mothers and fathers, and the share of one-year older and one-year younger students in X_{ics}. The estimates are weighted by the number of pupils, and standard errors are clustered at the school level.

Another identification problem is the existence of alternative school-cohort-specific policies or events, such as changes in leadership at school, that were implemented concurrently with the smartphone ban and might impact student outcomes. To address this issue, I add a dummy variable controlling for whether an individual is exposed to a leadership change during middle-school years. This allows me to account for the time-varying characteristics of the school.[9] Additionally, as the previous literature has shown, peer effects appear to be an important determinant of students' own achievements (Burke and Sass, 2013). Therefore, I control for peers' previous achievements measured by peers' test scores in grade 5.

Moreover, if the characteristics of new students change post-ban, despite the fact that students are assigned to middle schools based on fixed catchment areas, this might change the school environment and alter student test scores and well-being. Even though the estimated effects could be interpreted as the total policy impact in a partial equilibrium, the aim of this paper is to estimate the effect of a phone ban on students' educational outcomes and bullying. Therefore, I test whether the characteristics of students or teachers change relative to the introduction of a smartphone ban. The results of this exercise are shown in the Appendix 1.A. Conditional on school and cohort fixed effects these figures show that there is little evidence that student intake or teachers' characteristics changed post-ban. The only exception is a decrease in the share of employees with a teaching degree and an increase in the share of students with foreign-born parents. In my main results, I control for these factors. Note that both of these trends would likely reduce the potential positive effects of a smartphone ban, as these figures indicated a decrease in formal pedagogical competence among the staff and an increase in students who, on average, have lower grades in middle school (Statistics Norway, 2020). Importantly, there is no trend in previous achievement or in the intake of number of students.

1.4.2 Alternative Specification

To test for the robustness of my research design, I complement the event-study analysis with an alternative specification to test the joint significance of the event-study estimates in a difference-in-difference specification. I replace the individual event-year indicators of Equation 1.3 with indicators for groups of event-years in three categories. In particular, I estimate the model:

$$\begin{aligned} Y_{ics} = \beta_0 + \beta_1 Y_{ics-1} + \lambda_s + \theta_c + \gamma X_{ics} + \beta_2 Ban_{ics}\mathbf{1}[c - T_s^* \leq -2] \\ + \beta_3 Ban_{ics}\mathbf{1}[0 \leq c - T_s^* \leq 2] + \beta_4 Ban_{ics}\mathbf{1}[c - T_s^* \geq 3] + \epsilon_{ics} \end{aligned} \tag{1.5}$$

Here, β_2 subsumes the impact up to 2 years before the introduction of a smartphone ban, β_3 captures the short-run impact, and β_4 captures the impact for individuals who are exposed to a ban for all 3 years of middle school. The coefficients are ITT effects. Similar to before,

[9] To my knowledge, there were two countrywide policies implemented during the time period considered; the teachers' norm and a homework policy. The teachers' norm was policy implemented in 2018 with the goal of restricting the student-to-teacher ratio to 20 (Utdanningsdirektoratet, 2019b). The homework policy was implemented in 2014 for grades 1–10. In particular, it required that each school provides 8 hours a week for homework assistance, with this time divided between grades 1–10 (Utdanningsdirektoratet, 2019a). As both policies were nationwide, they are absorbed by the cohort fixed effects.

X_{ics} is a set of individual and family characteristics that also includes peer achievement and a dummy for leadership change. Additionally, I run Equation 1.5 with pre-reform trends for the outcome of interest. I estimate a school-specific trend using data up to eight years before a ban was introduced, obtaining a slope coefficient τ for each school. Then, I extrapolate the pre-ban time trends to the post-ban period as follows:

$$\begin{aligned} Y_{ics} = \beta_0 + \beta_1 Y_{ics-1} + \lambda_s + \theta_c + \delta\tau_s + \gamma X_{ics} + \beta_2 Ban_{ics}\mathbf{1}[c - T_s^* \leq -2] \\ +\beta_3 Ban_{ics}\mathbf{1}[0 \leq c - T_s^* \leq 2] + \beta_4 Ban_{ics}\mathbf{1}[c - T_s^* \geq 3] + \epsilon_{ics} \end{aligned} \tag{1.6}$$

I include the slope coefficient τ as a linear pre-ban trend, which should account for omitted trends in outcomes that might be correlated with the introduction of a smartphone ban.

1.5 Results

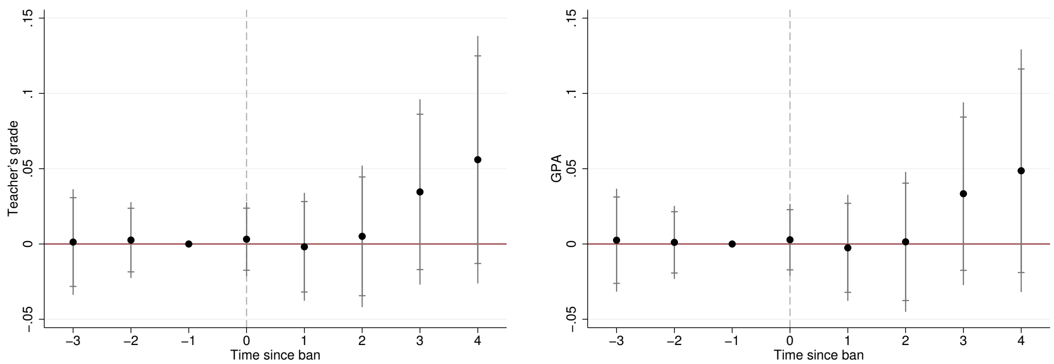
In this section, I first investigate the effects of smartphone bans on educational outcomes. Then, I turn to studying the impacts of smartphone bans on bullying. A virtue of the event study design is that it provides a transparent way of showing pre- and post-treatment trends. To this end, I present graphical depiction of my main results using my main specification.

1.5.1 Educational Outcomes

Appendix 1.A Table A2 shows the estimated coefficients on the impact of a smartphone ban on middle-school average grades set by teachers and GPA. Columns (i) and (v) of Table A2 represent the results for the most basic specification, which accounts only for parental characteristics, school, and cohort fixed effects. I include one additional confounding factor in each column in Table A2. Columns (iv) and (viii) show my preferred estimations. These estimates include school and cohort fixed effects, while also controlling for parental background, individual characteristics, previous achievement, and peers' achievements. They also include and a dummy variable controlling for leadership change. The year before the event (–1) corresponds to the omitted category and is always zero by construction. I find no meaningful statistically significant effects of banning smartphones on students' average middle-school grades set by teachers or middle-school GPA when examining at the full sample. Figure 1.2 shows estimated coefficients in an event study graph.

The analysis based on the full sample indicates that there is no effect on students' average grades set by teachers or GPA. As such, these grades and GPA results are similar to the finding of Kessel et al. (2020). However, the effect on smartphone bans might not be the same for different groups of students. In recent decades, girls have been outperforming boys in school. In addition, it has been shown that girls and boys react differently to resources in the classroom (Fredriksson et al., 2013; Pekkarinen, 2012). Further, phone usage is significantly higher among girls than among boys (Medietilsynet, 2018). Hence, girls could be more intensely affected by the ban and, therefore, the potential effect could be larger for girls.

Figure 1.2: Average Grades Set by Teacher and GPA



(a) Average grades full sample (b) GPA full sample

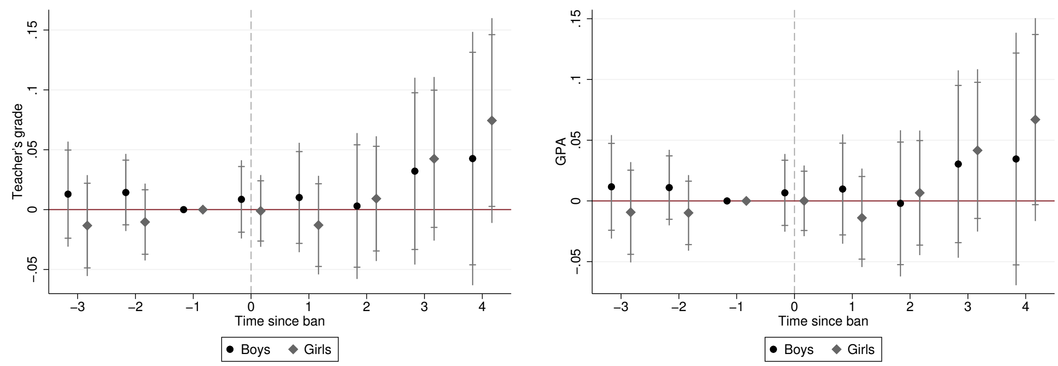
Notes: Each graph is from a separate regression. All specifications include a full set of cohort and school fixed effects. Robust standard errors are clustered at the school level. Additional control variables are a dummy variable for gender, mother's education, mother's age at the birth of the child, mother's marital status at the birth of the child, father's education, father's age at birth, a dummy for having foreign-born parents, a dummy for being 1 year older than classmates, a dummy for being 1 year younger than classmates, and the individual's birth order. The reference year is 1 year prior to the introduction of a smartphone ban. Error bars show 95% and 90% confidence intervals.

Consequently, I present the results separately by gender in Appendix 1.A Table A3. Section A shows the estimates for girls and section B shows the estimates for boys. Examining the effect on average grades set by teachers in Column (i), it is evident that there is no significant effect, for girls or boys. When conditioning on previous achievement, there is a statistically significant gain in teacher awarded grades by 0.08 standard deviations 4 years post-ban for girls. When conditioning on peers' achievement and changes in leadership at school the gain in grades for girls is 0.07 standard deviations. These estimates are significant at the 10% level. There is no effect on girls who are partially exposed to a smartphone ban in middle school. Figure 1.3 illustrates the results on teacher awarded grades and GPA by gender.

In the years prior to the ban, the coefficients between girls' and boys' grades set by teachers are similar, confirming that female and male students share the same trends prior to the smartphone ban. In contrast, after a ban has been introduced, the gain in grades set by teachers diverges between female and male students. However, I cannot reject the null hypothesis that these two coefficients are equal. Looking at the results on GPA in Table A3 Columns (v) to (viii) it is evident that there is no significant effect on GPA, for boys or girls post-ban. Although, not statistically significant there is a positive upward trend in girls' GPA when girls are exposed from the start of their 3-year middle-school education, evident in Figure 1.3.

To examine closer the positive effect on average grades set by teachers, I separately examine the effect on teacher awarded grades by the core subjects mathematics, Norwegian and English. Separating grades by field, post-ban test scores set by teachers in Norwegian

Figure 1.3: Average Grades Set by Teacher and GPA



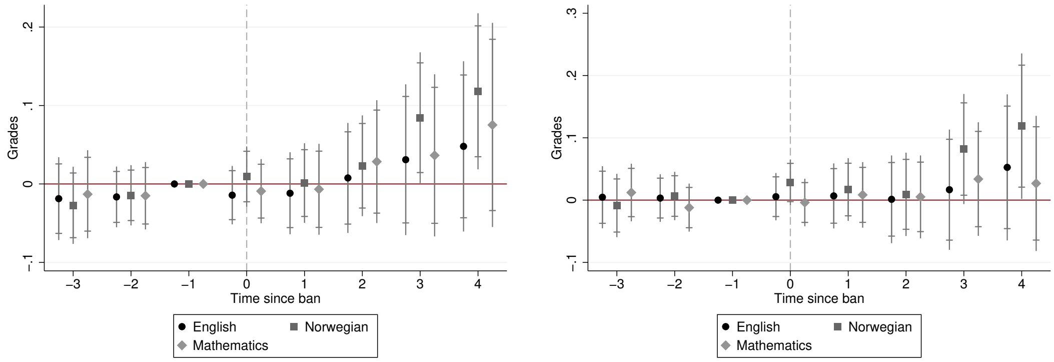
(a) Average grades by gender (b) GPA by gender

Notes: Each graph is from a separate regression. All specifications include a full set of cohort and school fixed effects. Robust standard errors are clustered at the school level. Additional control variables are mother's education, mother's age at the birth of the child, mother's marital status at the birth of the child, father's education, father's age at birth, a dummy for having foreign-born parents, a dummy for being 1 year older than classmates, a dummy for being 1 year younger than classmates, and the individual's birth order. The reference year is 1 year prior to the introduction of a smartphone ban. Error bars show 95% and 90% confidence intervals.

increase for girls between 0.08 (p-value 0.047) and 0.12 standard deviations (p-value 0.020), as documented in Figure 1.4. There is also a positive effect for boys. Similar to girls, boys' test scores in Norwegian increase by 0.08 and 0.12 standard deviations (p-values of 0.069 and 0.046, respectively). These effects are significant when both girls and boys are exposed for 3–4 years post-ban.

The existing literature has found indications that teachers can be biased toward their own students (Terrier, 2020; Carlana, 2019; Lavy, 2008). If students behave better after a smartphone policy is in place, a teacher could potentially award students with a higher grade even if they have made no actual improvement in grades. As the Norwegian Registry data contain not only grades set by the teachers, but also externally graded exams, I test whether there is an improvement in blind test score. Similarly to teacher awarded grades, blind test scores are reported for the subjects mathematics, Norwegian, and English. Figure 1.5 documents these results. Girls have significantly higher test scores in mathematics 4 years post-ban. The gain in mathematics is 0.25 standard deviations (p-value 0.007). The substantial increase in externally graded test scores in mathematics for girls suggests that the ban improved human capital accumulation. Moreover, there is survey evidence indicating that girls feel more anxious about mathematics compared with boys (OECD, 2013). One could speculate that during mathematics classes girls are more likely to turn to non-study-related activities on their phones if they struggle with the task and start feeling anxious. When phone usage is prohibited, they are required to focus on the subject.

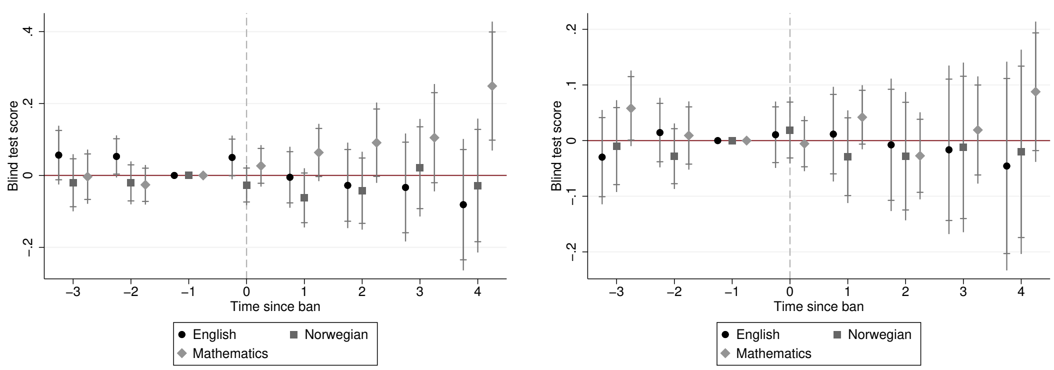
Figure 1.4: Non-Blind Grades Set by Teacher



(a) Grades girls (b) Grades boys

Notes: Each graph is from a separate regression. All specifications include a full set of cohort and school fixed effects. Robust standard errors are clustered at the school level. Additional control variables are mother's education, mother's age at the birth of the child, mother's marital status at the birth of the child, father's education, father's age at birth, a dummy for having foreign-born parents, a dummy for being 1 year older than classmates, a dummy for being 1 year younger than classmates, and the individual's birth order. The reference year is 1 year prior to the introduction of a smartphone ban. Error bars show 95% and 90% confidence intervals.

Figure 1.5: Blind Test Scores from Externally Graded Exams

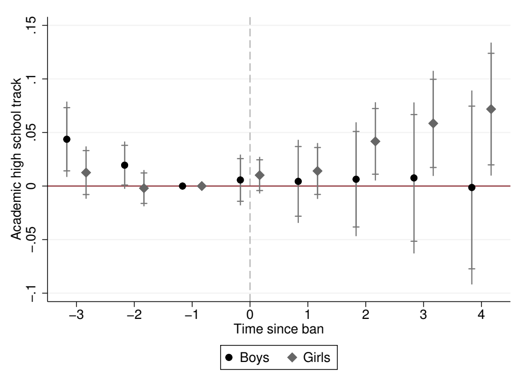


(a) Test scores girls (b) Test scores boys

Notes: Each graph is from a separate regression. All specifications include a full set of cohort and school fixed effects. Robust standard errors are clustered at the school level. Additional control variables are mother's education, mother's age at the birth of the child, mother's marital status at the birth of the child, father's education, father's age at birth, a dummy for having foreign-born parents, a dummy for being 1 year older than classmates, a dummy for being 1 year younger than classmates, and the individual's birth order. The reference year is 1 year prior to the introduction of a smartphone ban. Error bars show 95% and 90% confidence intervals.

Having established that banning smartphones, had a significant impact on girls' average grades set by teachers and mathematics test scores in externally corrected exams, and boys' teacher awarded grades in Norwegian, I go on to analyze the effect of banning smartphones on students' likelihood of progressing into an academic high school track. This enables me to study whether banning smartphones not only improved short-term outcomes such as grades and test scores, but also improved a middle-term outcome such as enrollment into an academic high school track. The last year in the education registry is 2018. As such, these results include individuals who finish middle school during or before 2017. Figure 1.6 presents estimates for the likelihood of progressing into an academic high school track. For girls exposed to a smartphone ban for at least 2 or more years when they are in middle school, the probability of attending an academic high school track increases by 4–7 percentage points. This result shows that banning smartphones increases the probability of them entering a more challenging high school track which thus prepares them for further higher education. These estimates are significant at the 5% level. An alternative way of illustrating this magnitude is to compare it with the pretreatment mean of 49%: the estimated effect suggests there is an 8–14% increase in the number of girls attending an academic high school track compared with the average number of girls who attended an academic high school track relative to pre-ban years. Although the effect on the probability of attending an academic high school track is only significant for girls, these effects are not statistically different from the estimated effect for boys.[10]

Figure 1.6: Likelihood of Attending an Academic High School Track by Gender



(a) P(Academic track=1) by gender

Notes: Each graph is from a separate regression. All specifications include a full set of cohort and school fixed effects. Robust standard errors are clustered at the school level. Additional control variables are mother's education, mother's age at the birth of the child, mother's marital status at the birth of the child, father's education, father's age at birth, a dummy for having foreign-born parents, a dummy for being 1 year older than classmates, a dummy for being 1 year younger than classmates, and the individual's birth order. The reference year is 1 year prior to the introduction of a smartphone ban. Error bars show 95% and 90% confidence intervals.

[10] Note that the positive effect on attending an academic high school track 3 years prior to the introduction of a smartphone ban is driven by boys, for whom there is no effect post-ban.

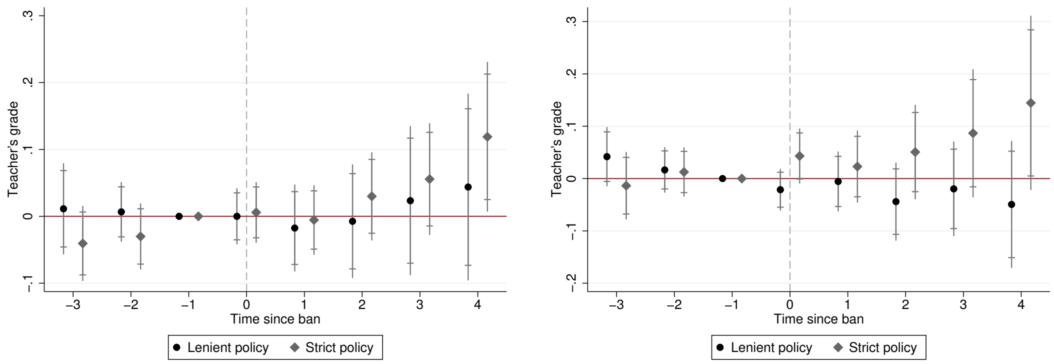
Type of Smartphone Policy

In my survey, approximately 42% of schools responded that phones are prohibited at school or that phones are collected before class and stored in "mobile phone hotels". I define these schools as having a strict smartphone policy. Schools that allow phone use during the breaks are defined as schools with lenient policies. To disentangle the effect between these schools, I separately examine the educational outcomes by type of policy. Figure 1.7 displays the results for average grades set by teachers and Figure 1.8 the results for GPA. In the years prior to the ban, the coefficients between girls at schools with a strict or lenient policy are similar, confirming that girls at these schools share the same trend prior to the smartphone ban. By contrast, after a ban has been introduced, the gain in both average grades set by teachers and GPA diverges between girls attending a school with a strict or a lenient policy toward smartphones. Girls attending a middle school introducing a strict policy against smartphones, experience an increase by 0.12 standard deviations in average grades set by teachers. This estimate is significant 4-years post-ban at the 5% level (p-value 0.037). Additionally, girls attending a middle school with a strict policy have significantly higher GPA 4 years post ban and gain 0.10 of a standard deviation in GPA (p-value 0.088). These results show that both average grades set by teachers and GPA for girls improve after strict smartphone bans in schools are implemented. Boys are not affected. This suggests that the gender gap in education might increase following the introduction of strict smartphone bans in middle school. That fact that there are no significant negative effects for boys implies that there are no negative externalities from smartphone use on boys. However, I cannot reject the null hypothesis that these two coefficients between girls attending schools with strict or lenient policies are different.

I then investigate whether there are any differences in type of ban on teachers' grades and externally graded exams. The results show that girls attending schools with a strict policy are driving the positive effect on grades set by teachers in Norwegian and externally graded exams in mathematics. As shown in Figure 1.9, girls who are fully exposed to a smartphone ban and attend a middle school where students are restrained from accessing their phones during school hours, experience a gain in teacher awarded grades in Norwegian by 0.10 (p-value 0.057) and 0.13 (p-value 0.036) of a standard deviation 3–4 years post-ban respectively. Similar to girls, boys attending a middle school with a strict smartphone policy experience an improvement by 0.18 of a standard deviation in teacher awarded grades in Norwegian.

For externally graded test scores in mathematics, girls already make improvements 1 year post-ban when they attend a school with a strict smartphone policy. One year post-ban girls experience a gain of 0.09 standard deviation (p-value 0.083) and 4 years post-ban girls gain 0.28 of a standard deviation (p-value 0.009) in externally corrected mathematics exams. Figure 1.10 display the results for externally corrected exams in mathematics. There is no effect for boys.

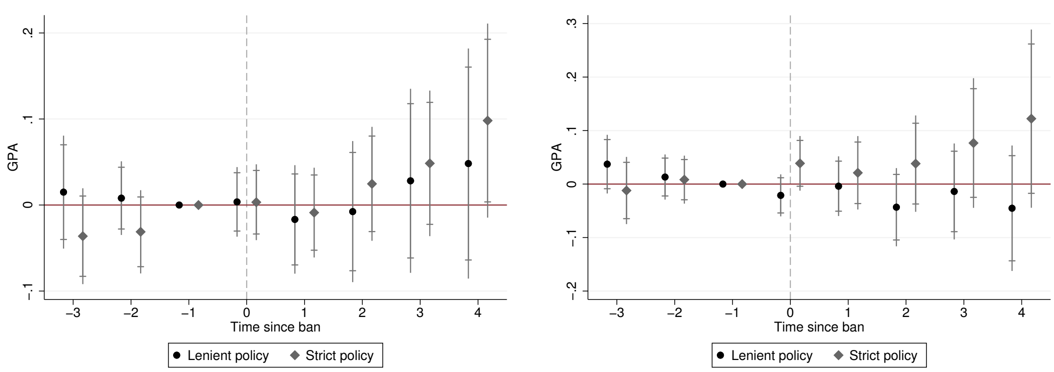
Figure 1.7: Average Grades Set by Teacher by Type of Ban



(a) Average grades girls (b) Average grades boys

Notes: Each graph is from a separate regression. All specifications include a full set of cohort and school fixed effects. Robust standard errors are clustered at the school level. Additional control variables are mother's education, mother's age at the birth of the child, mother's marital status at the birth of the child, father's education, father's age at birth, a dummy for having foreign-born parents, a dummy for being 1 year older than classmates, a dummy for being 1 year younger than classmates, and the individual's birth order. The reference year is 1 year prior to the introduction of a smartphone ban. Error bars show 95% and 90% confidence intervals.

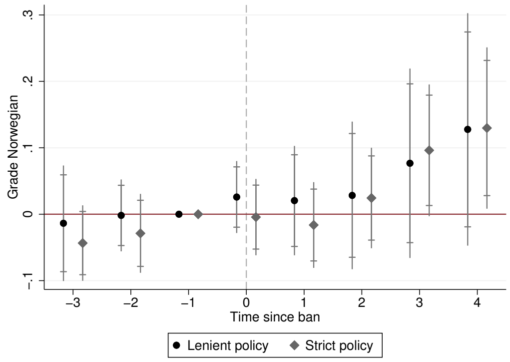
Figure 1.8: GPA by Type of Ban



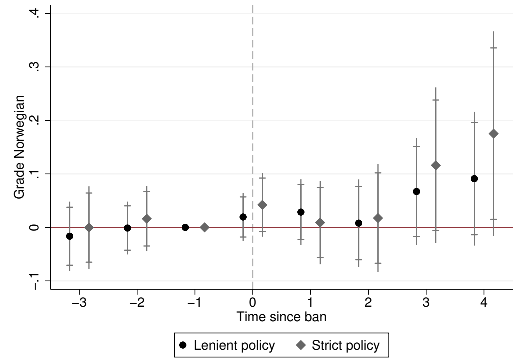
(a) GPA girls (b) GPA boys

Notes: Each graph is from a separate regression. All specifications include a full set of cohort and school fixed effects. Robust standard errors are clustered at the school level. Additional control variables are mother's education, mother's age at the birth of the child, mother's marital status at the birth of the child, father's education, father's age at birth, a dummy for having foreign-born parents, a dummy for being 1 year older than classmates, a dummy for being 1 year younger than classmates, and the individual's birth order. The reference year is 1 year prior to the introduction of a smartphone ban. Error bars show 95% and 90% confidence intervals.

Figure 1.9: Non-Blind Norwegian Grades Set by Teacher by Type of Ban



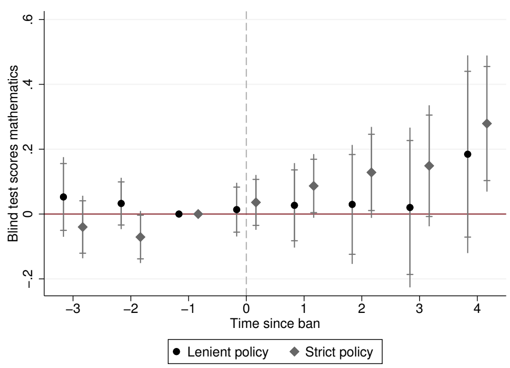
(a) Grades Norwegian girls



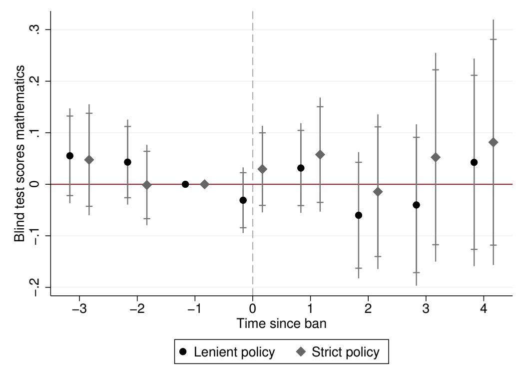
(b) Grades Norwegian boys

Notes: Each graph is from a separate regression. All specifications include a full set of cohort and school fixed effects. Robust standard errors are clustered at the school level. Additional control variables are mother's education, mother's age at the birth of the child, mother's marital status at the birth of the child, father's education, father's age at birth, a dummy for having foreign-born parents, a dummy for being 1 year older than classmates, a dummy for being 1 year younger than classmates, and the individual's birth order. The reference year is 1 year prior to the introduction of a smartphone ban. Error bars show 95% and 90% confidence intervals.

Figure 1.10: Blind Mathematics Test Scores from Externally Graded Exams by Type of Ban



(a) Test scores mathematics girls



(b) Test scores mathematics boys

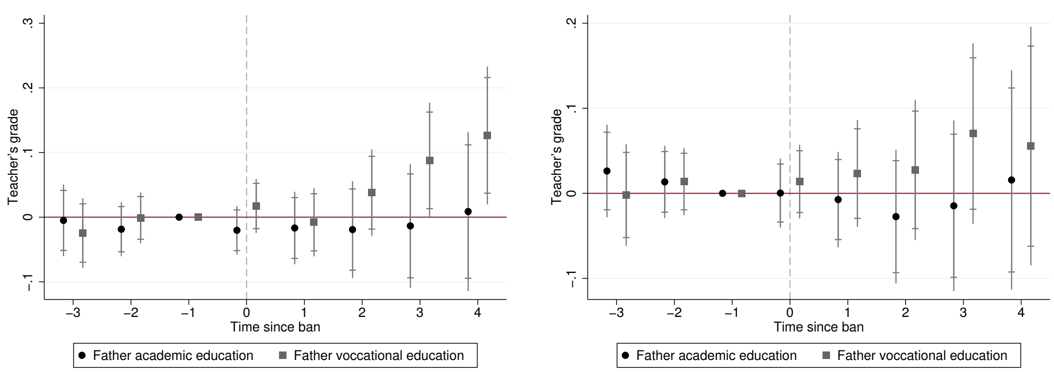
Notes: Each graph is from a separate regression. All specifications include a full set of cohort and school fixed effects. Robust standard errors are clustered at the school level. Additional control variables are mother's education, mother's age at the birth of the child, mother's marital status at the birth of the child, father's education, father's age at birth, a dummy for having foreign-born parents, a dummy for being 1 year older than classmates, a dummy for being 1 year younger than classmates, and the individual's birth order. The reference year is 1 year prior to the introduction of a smartphone ban. Error bars show 95% and 90% confidence intervals.

There are no detectable differences on teachers' grades in mathematics or English, or on externally graded exams in Norwegian and English between schools with strict compared with more lenient policies, either for girls or boys. Additionally, there are no detectable differences on the likelihood of attending an academic high school track between schools with strict compared with more lenient policies. These results are displayed in Appendix 1.A Figure A4 – Figure A8.

Socioeconomic Background

A large literature suggests that family background and school environment characteristics are important traits for student achievement (Björklund and Salvanes, 2011). One important dimension is the parents' socioeconomic status. To disentangle the effect between students of low and high socioeconomic status, I separately examine individuals whose fathers have an academic or vocational high school education.[11] Figure 1.11 shows the results for average grades set by teachers. Girls whose fathers have a vocational education experience an increase in average grades set by teachers by 0.09-0.13 standard deviations (p-values 0.053 and 0.020, respectively) 3–4 years post-ban. Additionally, 3–4 years post-ban, girls gain 0.09-0.11 standard deviations in GPA (p-values of 0.053 and 0.032, respectively) when they have a father with vocational education. Figure 1.12 shows the results for GPA.

Figure 1.11: Average Grades Set by Teacher by Father's Type of High School Education



(a) Average grades girls

(b) Average grades boys

Notes: Each graph is from a separate regression. All specifications include a full set of cohort and school fixed effects. Robust standard errors are clustered at the school level. Additional control variables are mother's education, mother's age at the birth of the child, mother's marital status at the birth of the child, father's education, father's age at birth, a dummy for having foreign-born parents, a dummy for being 1 year older than classmates, a dummy for being 1 year younger than classmates, and the individual's birth order. The reference year is 1 year prior to the introduction of a smartphone ban. Error bars show 95% and 90% confidence intervals.

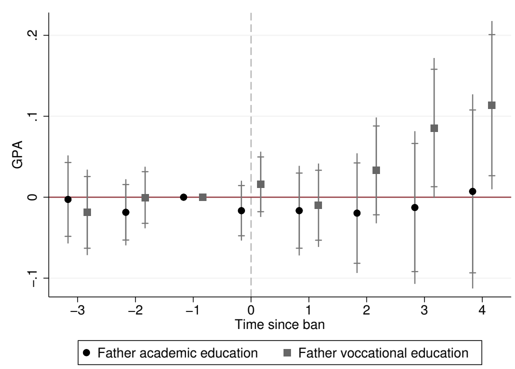
I then look at the results separately in the core subjects mathematics, Norwegian and English. The gain in teacher awarded grades in Norwegian are driven by girls whose fathers have a vocational education as seen in Appendix 1.A Figure A10. Boys from low socioeco-

[11] Fathers with less than high school education are grouped together with fathers with vocational education. Fathers with academic education have attended an academic high school track, or have attained higher education.

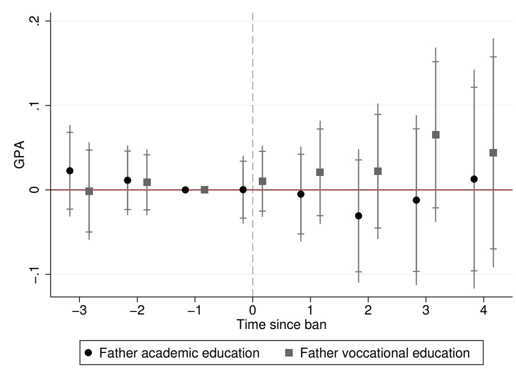
nomic families experience an improvement in teacher awarded grades in both mathematics and Norwegian when fully exposed to a smartphone ban for 3 years. The gain is 0.10 (p-value 0.042) and 0.12 (p-value 0.066) of a standard deviation as shown in Appendix 1.A Figure A10 and Figure A9. The improvement in externally corrected mathematics test scores for girls whose fathers have an academic or vocational education are equal. Results are shown in Appendix 1.A Figure A12.

Figure 1.13 presents results for the likelihood of enrolling in an academic high school track. Girls with vocationally educated fathers are 5 percentage points more likely to attend an academic high school track 3 years post ban and 6 percentage points more likely 4 years post ban (p-values of 0.056 and 0.071, respectively). There is no effect on boys' grades, GPA, or their probability of attending an academic high school track. These important differences suggest that unstructured technology is especially distracting for students from low socioeconomic families, whereas students from high socioeconomic families do not experience any negative externalities.

Figure 1.12: GPA by Father's Type of High School Education



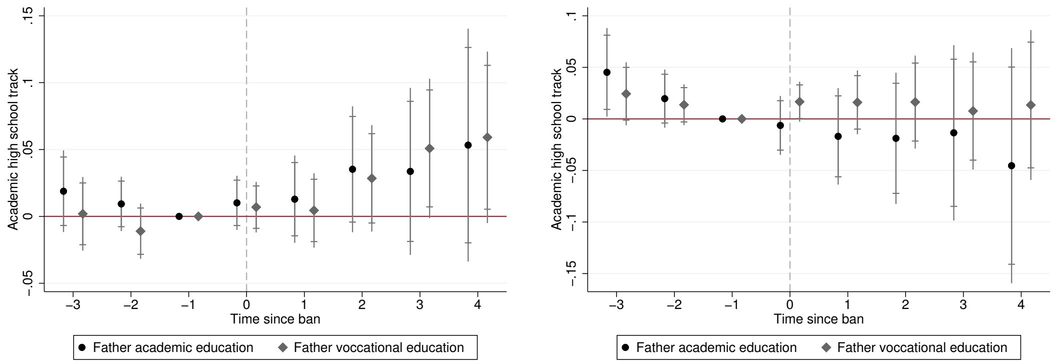
(a) GPA girls



(b) GPA boys

Notes: Each graph is from a separate regression. All specifications include a full set of cohort and school fixed effects. Robust standard errors are clustered at the school level. Additional control variables are mother's education, mother's age at the birth of the child, mother's marital status at the birth of the child, father's education, father's age at birth, a dummy for having foreign-born parents, a dummy for being 1 year older than classmates, a dummy for being 1 year younger than classmates, and the individual's birth order. The reference year is 1 year prior to the introduction of a smartphone ban. Error bars show 95% and 90% confidence intervals.

Figure 1.13: Likelihood of Attending an Academic High School Track by Father's Type of High School Education



(a) P(Academic track=1) girls (b) P(Academic track=1) boys

Notes: Each graph is from a separate regression. All specifications include a full set of cohort and school fixed effects. Robust standard errors are clustered at the school level. Additional control variables are mother's education, mother's age at the birth of the child, mother's marital status at the birth of the child, father's education, father's age at birth, a dummy for having foreign-born parents, a dummy for being 1 year older than classmates, a dummy for being 1 year younger than classmates, and the individual's birth order. The reference year is 1 year prior to the introduction of a smartphone ban. Error bars show 95% and 90% confidence intervals.

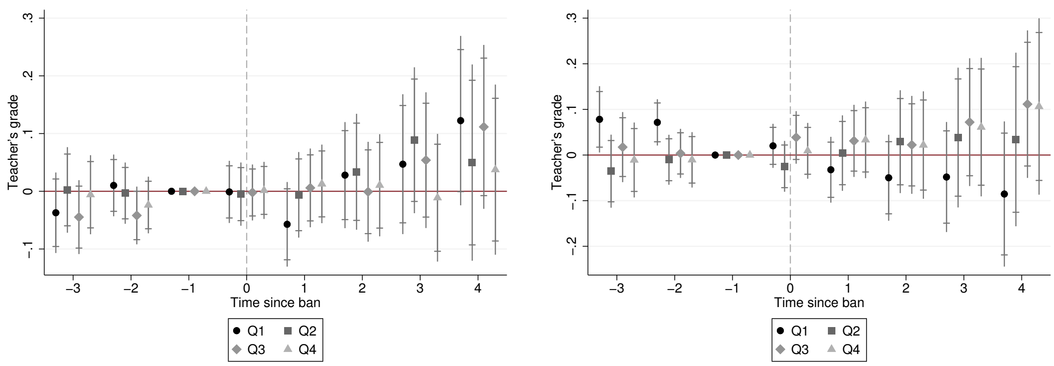
High Versus Low Ability Students

To investigate whether the effects of smartphone bans are more beneficial depending on students' ability, I divide students into four quartiles based on prior school achievement in grade 5 (primary school). Group one consists of students with the lowest ability and group four consists of students belonging to the top of the ability distribution. Figure 1.14 shows the results. In particular, girls in the lower end of the ability distribution, drive the positive increase in teacher awarded grades. Four years post-ban, girls in the first quantile gain on average 0.12 standard deviations on teacher-awarded grades (p-value 0.102).[12] Note, however that the estimates for different ability groups are not statistically different from each other. Figure 1.15 shows the results for GPA, which suggest that there is a positive effect on GPA for low-ability girls 4 years post-ban. However, this coefficient is just below the 10% significance level (p-value 0.125). There is no effect on boys' grades, independent of ability.

The effects along the distribution of teacher-awarded grades and externally corrected exams by the core subjects are shown in Appendix 1.A Figure A16–Figure A20. Girls in the third quantile make gains in teacher-awarded grades in Norwegian by 0.13 (p-value 0.045) and 0.20 (p-value 0.018) standard deviations, and in English by 0.15 (p-value 0.043) and 0.23 (p-value 0.023) standard deviations, 3–4 years post-ban. High-ability boys in the fourth quantile gain 0.22 standard deviation in Norwegian teacher-awarded grades 4 years post-ban. The

[12]This estimate is just below the margin of being significant at the 10% level

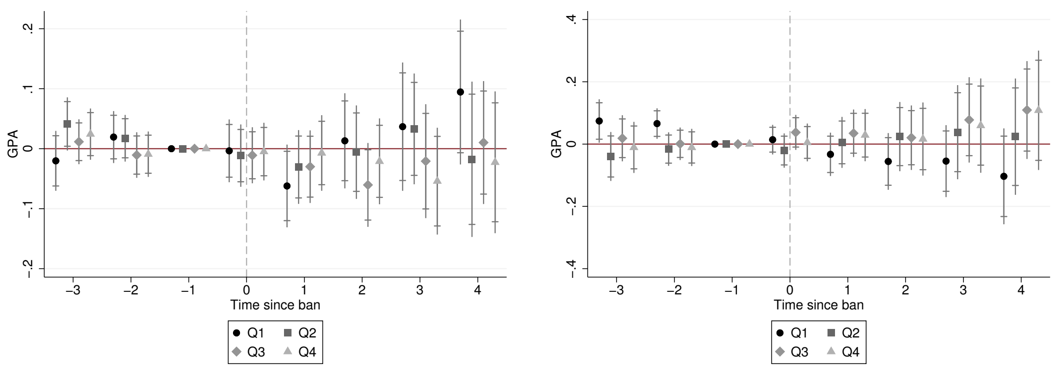
Figure 1.14: Average Grades Set by Teacher by Ability Quartiles



(a) Average grades girls (b) Average grades) boys

Notes: Each graph is from a separate regression. All specifications include a full set of cohort and school fixed effects. Robust standard errors are clustered at the school level. Additional control variables are mother's education, mother's age at the birth of the child, mother's marital status at the birth of the child, father's education, father's age at birth, a dummy for having foreign-born parents, a dummy for being 1 year older than classmates, a dummy for being 1 year younger than classmates, and the individual's birth order. The reference year is 1 year prior to the introduction of a smartphone ban. Error bars show 95% and 90% confidence intervals.

Figure 1.15: GPA by Ability Quartiles



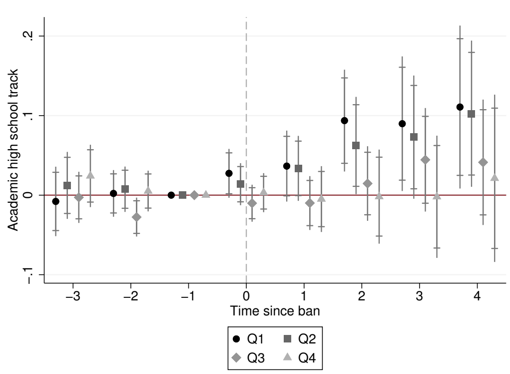
(a) GPA girls (b) GPA boys

Notes: Each graph is from a separate regression. All specifications include a full set of cohort and school fixed effects. Robust standard errors are clustered at the school level. Additional control variables are mother's education, mother's age at the birth of the child, mother's marital status at the birth of the child, father's education, father's age at birth, a dummy for having foreign-born parents, a dummy for being 1 year older than classmates, a dummy for being 1 year younger than classmates, and the individual's birth order. The reference year is 1 year prior to the introduction of a smartphone ban. Error bars show 95% and 90% confidence intervals.

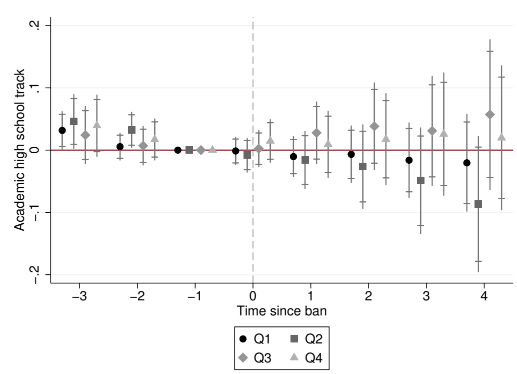
improvement in externally corrected exams in mathematics 1 to 2 years post-ban is driven by high-ability girls from the fourth quartile. However, 4 years post-ban lower-ability girls from the first and second quartile make similar improvement in externally corrected mathematics exams as girls from the highest quartile, by 0.33 (p-value 0.020), 0.24 (p-value 0.063) and 0.29 (p-value 0.050) standard deviations, respectively. Note, however, that the estimates for different ability groups are not statistically different from each other.

I conduct the same exercise by quartiles on the probability of attending an academic high school track. The results are shown in Figure 1.14. Girls in the first and second quartiles, are significantly more likely to attend an academic high school track when they are exposed to a smartphone ban for 2–4 years. In particular, girls in the lowest quartile are 11 percentage points more likely to attend an academic high school track 4 years post-ban (p-value 0.034). Girls in the second quartile are at most 10 percentage points more likely to attend an academic high school track (p-value 0.029). Note, however, that the estimates for different ability groups are not statistically different from each other. Along the ability distribution, there is no significant effect on boys' likelihood of enrolling in an academic high school track.

Figure 1.16: Likelihood of Attending an Academic High School Track by Ability Quartiles



(a) P(Academic track=1) girls



(b) P(Academic track=1) boys

Notes: Each graph is from a separate regression. All specifications include a full set of cohort and school fixed effects. Robust standard errors are clustered at the school level. Additional control variables are mother's education, mother's age at the birth of the child, mother's marital status at the birth of the child, father's education, father's age at birth, a dummy for having foreign-born parents, a dummy for being 1 year older than classmates, a dummy for being 1 year younger than classmates, and the individual's birth order. The reference year is 1 year prior to the introduction of a smartphone ban. Error bars show 95% and 90% confidence intervals.

The heterogeneity analysis suggests that girls attending middle schools with a strict smartphone policy and girls from lower socioeconomic backgrounds experience the largest increase in middle-school grades set by teachers, GPA, and the probability of attending an academic high school track after smartphones have been banned. For boys, there is an improvement in test scores set by teachers in Norwegian and mathematics for boys who attend a middle school with a strict policy against smartphones. Low-ability girls are driving the increase in teacher-awarded grades and experience significant positive impact on enrolling in academic high school track, post-ban. All in all, these results suggest that students from weaker family backgrounds and low ability students are more prone to distraction from new technology, such as smartphones.

Overall, all these effects are only evident when students are exposed for at least 2 years in middle school for any significant impact on educational outcomes to be observed. The lack of an immediate effect could be because it takes time to establish new norms around smartphone use, or it could be that the lower human capital accumulation in the period before the ban cannot be compensated for in the short run.

1.5.2 Bullying

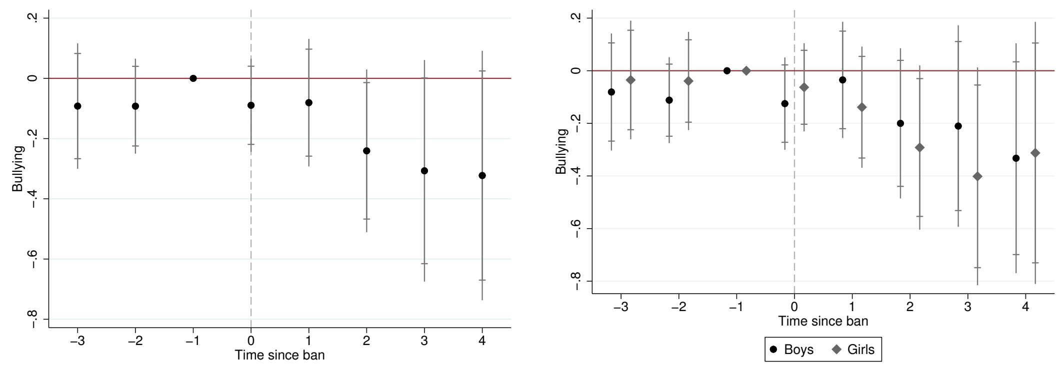
Thus far, I have evaluated the effect of banning smartphones on students' educational outcomes. Another important dimension of outcomes is students' experience of bullying at school. Bullying has been shown in previous research to be predictive of several long-term health, educational, and labor market consequences (Drydakis, 2014). Similar to educational outcomes, I examine at the effect separately for girls and boys, in addition to conducting full sample estimates. The data for bullying is aggregated to the school level, but the cohorts and number of schools are the same as for the estimates at the individual level.

When examining the full sample, the estimates show a decline in the incidents of bullying by 0.24–0.31 standard deviation 2–3 years after a smartphone ban is implemented (p-values of 0.081 and 0.102), as documented by Figure 1.17. Separating the results by gender, shows that girls are driving these results. Girls experience a 0.29 standard deviation decrease in bullying 2 years after a smartphone ban is introduced (p-value 0.067). Girls exposed to a full-time, smartphone ban for 3 years in middle school instead experience a 0.40 standard deviation decline (p-value 0.057) in the incidents of bullying compared to unaffected girls.

For girls, there is no difference dependent on the type of policy implemented. However, boys attending a middle school with a strict smartphone policy experience a decline in the incidents of bullying 2 years after the ban is introduced by 0.35 standard deviations (p-values 0.070), as documented in Appendix 1.A Figure A21. Although the estimates are not significantly different, this suggests that stricter smartphone regulations that include phone bans during breaks, not just during classes, have a larger preventive impact on bullying for boys.

As the bullying data are on the school level, I cannot study differences by socioeconomic status at the individual level. Nevertheless, I divide schools at the mean based on students with fathers educated at academic high schools. The results are shown in Appendix 1.A Figure A22. Boys attending low socioeconomic schools experience a decline in bullying by 0.8 standard deviations 2 years after a ban was introduced (p-value 0.096). There is no significant effect on bullying dependent on the socioeconomic status of the school for girls.

Figure 1.17: Bullying



(a) Bullying full sample (b) Bullying by gender

Notes: Each graph is from a separate regression. All specifications include a full set of cohort and school fixed effects. Robust standard errors are clustered at the school level. Additional control variables are the mean of mothers' education, the mean of mothers' age at the birth of the child, share of students with married parents at birth, the mean of fathers' education, the mean of fathers' age at birth, share of students being 1 year older than classmates, share of students being 1 year younger than classmates, mean of birth order, share of students with foreign-born parents, mean of students' test scores in grade 5, and a dummy controlling for leadership change. The reference year is 1 year prior to the introduction of a smartphone ban. Error bars show 95% and 90% confidence intervals.

1.6 Robustness

In this section, I explore whether three of my most important findings, the improvement in average grades set by teachers, the likelihood of attending an academic high school track, and bullying are robust to heterogeneous treatment effects and the alternative specifications outlined in Section 1.4.2. In addition, I perform several specification checks and examine whether other dimensions of student life are affected. My main findings are robust to most specification checks, such as using an estimation method that is robust to heterogeneous treatment effects, grouping the event-years together, excluding parental background characteristics and excluding observations from the capital of Norway, excluding private schools, and controlling for the level of unemployment in each municipality.

Appendix 1.A Table A4 shows the average grades set by teachers and the results and the probability of attending an academic high school track when grouping event-years together. Columns (i)–(iii) show the baseline specification. There is no significant effect on average teacher awarded grades. However, the probability of attending an academic high school track increases by 5 percentage points for girls who are exposed to a smartphone ban for 3 years or more. Columns (iv)–(vi) include a linear pretreatment trend. The result on the probability of attending an academic high school track is robust to including a linear pre-ban trend. The estimates for the probability of attending an academic high school track are significant at the 1% level.

One concern with event study estimates is whether the results may be biased by heterogeneous effects over time and across individuals (Abraham and Sun, 2018). I implement an estimation method proposed by Callaway and Sant'Anna (2021) that is robust to heterogeneous treatments effects across units, to limit these concerns. This method abstains from using early treated units as control for later treated units, that potentially could lead to biased estimates through negative weighting. The results are shown in Appendix 1.A Figure A23 and is performed at school level for all outcomes. Although not significant, attending an academic high school track an bullying exhibit similar trends as my event study results, while average grades set by teachers shows a no effect.

Appendix 1.A Table A5 shows the results for bullying when grouping the event-years together. Bullying decreases by 0.23 standard deviations overall, and for girls who are exposed to a ban for 3 years or more, by 0.25 standard deviations. This estimate is significant at the 10% level. For girls, this estimate is still significant when including a linear pre-ban trend and the magnitude is similar.

Furthermore, I evaluate whether the results are sensitive to including parental background characteristics. Excluding parental background characteristics has no effect on educational outcomes, as shown in Appendix 1.A Figure A24 and Figure A25.

Additionally, the results are not sensitive to dropping schools situated in the capital, Oslo. This suggests that the results are not driven by the largest city in the sample. Appendix 1.A Figure A26 and Figure A27 show the results when Oslo is dropped for educational outcomes and bullying. However, when dropping Oslo there is a positive impact on girls' middle-school GPA 4 years after a ban was introduced. This point estimate was on the margin of being significant in the main specification (p-value 0.116).

From 2007 to 2009, the unemployment rate in Norway increased from 2.5% to 3.8% following the 2008 financial crisis. From 2014 to 2016, the unemployment rate increased from 3.4% to 5.0% following the decline in the price of oil (Eurostat, 2020). Although I include cohort fixed effects that should control for any unobserved differences between cohorts – such as certain cohorts being differentially affected by the financial crisis – as an additional check, I control for the unemployment rate at the municipality level to account for differences in local exposure to these economic downturns. Doing so has little effect on the results as seen in Appendix 1.A Figure A28 and Figure A25.

Less than 1% of the schools in my sample are private schools. Private schools might be very different from public schools in many dimensions, and parents must apply to these schools for their children to attend. Dropping private schools from the estimates has no impact on my main results, as shown in Appendix 1.A Figure A30 and Figure A31.

I also investigate whether parents or teachers are endogenously sorting in response to the introduction of smartphone bans using changes in the observed composition of those in a given cohort and attending a certain school. Appendix 1.A Figure A32 shows these result. I find little evidence of any systematic change in the composition of students or teachers that would suggest that parents or teachers are systematically sorting between schools due to a smartphone ban.

As a last check, I evaluate the banning of smartphones along three other dimensions: (i) social well-being, (ii) motivation, and (iii) pupil democracy. Results for both girls and boys are displayed in Appendix 1.A Figure A33. Social well-being measures how well students like and enjoy school, and motivation measures their level of inner motivation toward school and schoolwork. There is no change in social well-being or motivation post-ban. Similarly, there is no change in the experience of pupil democracy at school post-ban. Overall, this suggests that smartphone bans do not affect social well-being, motivation, or pupils' decision-making, whereas they do lower bullying incidents and improve educational outcomes. Hence, smartphone bans do not alter multiple dimensions of student life that should be less affected by less access to smartphones at school.

1.7 Discussion

In this section, I compare my results to estimates in the previous literature concerning the magnitude of changes in student outcomes in response to other school-level policies.[13] However, these comparisons must be treated with caution owing to obvious differences in context and research design between studies.

Banning smartphones from school is a policy with relatively small monetary costs, although the enforcement of a ban could be costly if it consumes a great deal of teachers' instruction time. However, the previous literature has evaluated much more monetarily expensive policies, such as introducing computers in classrooms or reducing the number of students in a class. There is a large literature on the reduction of class size (Jepsen, 2015; Krueger, 2002). Fredriksson et al. (2013) study class size effects in Sweden, a country with a similar education system to Norway. They show that reducing class size among primary school students in Sweden increases test scores in middle school by 0.02 standard deviations. Hall et al. (2019) investigate how the educational performance of middle-school pupils who are given a personal laptop or tablet, is affected in Sweden. They find no effect on student achievement after the introduction of laptops. Barrow et al. (2009) study the effect of a randomized control trial involving the introduction of an instructional computer program for algebra. They find that test scores are 0.25 standard deviations higher among students who use computer-aided instructions. I estimate the increase in teacher-awarded grades following a smartphone ban to be around 0.07 standard deviations, and at most 0.13 standard deviation better for girls with low socioeconomic fathers. Moreover, the increase in middle-school GPA following a smartphone ban is at most 0.11 standard deviations for girls with low socioeconomic fathers.

[13]Comparing the estimated effects of bullying with those from the previous literature is difficult as, to my knowledge, no other studies have examined the causal effect on bullying.

These effects are larger compared with those of reducing class size by one student. In addition, my results indicate that 4 years after a ban is introduced, girls gain 0.25 standard deviations in mathematics test scores. This is the same improvement as Barrow et al. (2009) finds in algebra tests after students have used instruction programs for pre-algebra and algebra.

1.8 Conclusion

In this paper, I evaluate the effect of banning smartphones from school on students' outcomes. Specifically, I focus on how banning smartphones impacts students' average grades set by teachers, GPA, test scores, their likelihood of attending an academic high school track, and the incidence of bullying. I combine self-administered survey data on the timing at which smartphone bans were implemented with Norwegian Registry data and a pupil survey on bullying. My identification strategy is based on the staggered adoption of smartphone bans across schools and time. Importantly for the identification strategy, student, teacher, and school characteristics cannot predict when a school implements a smartphone ban.

My results show that banning smartphones leads to a substantial and significant increase in teacher-awarded grades for girls by 0.07 standard deviations. Additionally, post-ban girls' externally graded exams in mathematics improved by 0.25 standard deviations, suggesting that the human capital accumulation of girls is improved post-ban. Girls are also 4–7 percentage points more likely to attend an academic high school track post-ban, suggesting that banning smartphones leads to an improvement in girls' mid-term educational outcomes.

Moreover, my results show that banning smartphones has a significant and positive effect on girls' middle-school GPA when schools introduced a strict smartphone policy. The average gain in middle-school GPA among girls is 0.10 of a standard deviation at schools with a strict smartphone policy. Further, I provide new evidence that bullying decreases by 0.43 of a standard deviation for girls exposed to a ban during all 3 years of middle school. The magnitudes of the estimates for the educational outcomes are larger among girls from low socioeconomic backgrounds, suggesting that this particular group of students are distracted by unstructured technology in the classroom. For boys from low socioeconomic families, there is a positive effect on teacher-awarded grades in mathematics and Norwegian. There are no negative effects on banning smartphones on students from high socioeconomic families. Furthermore, boys attending a middle school with a strict smartphone ban, restricting them from accessing their phone during class hours and breaks, experience a decrease in bullying by 0.35 standard deviations.

These findings are not driven by any compositional change among students or teachers or by different parental socioeconomic characteristics at school. My results are mostly robust to several specification checks. Although this paper shows robust evidence of the impact of smartphone bans on student outcomes, because the policy is quite recent, I cannot yet analyze students' likelihood of completing high school, nor follow their outcomes in terms of higher education or labor market returns. Nevertheless, it is evident that banning smartphones from the classroom is an inexpensive tool with a sizable effect on student outcomes.

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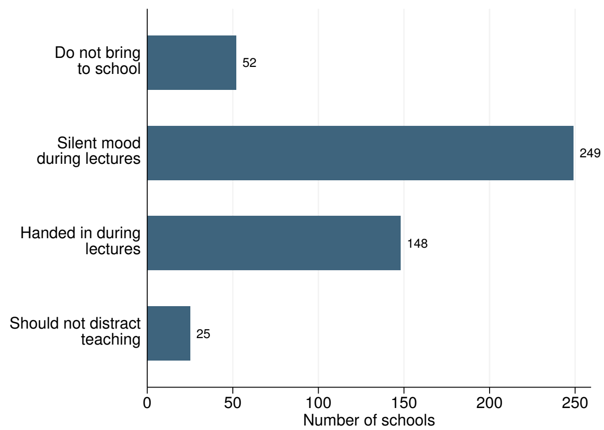
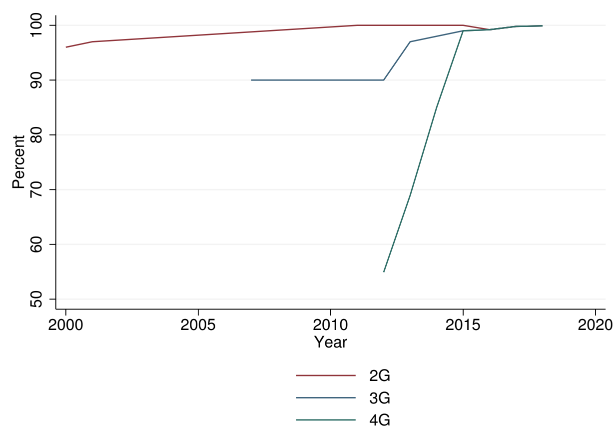
1.A Appendix Table and Figures

Figure A1: Type of Smartphone Ban



Notes: Distribution of type of answer by principal. *Source*: Own data.

Figure A2: Proportion of Population Covered by a Mobile Network



Source: SDG Indicators, United Nations Statistics Division.

Figure A3: Geographical Coverage over Municipalities with at Least one School Implementing a Smartphone Ban

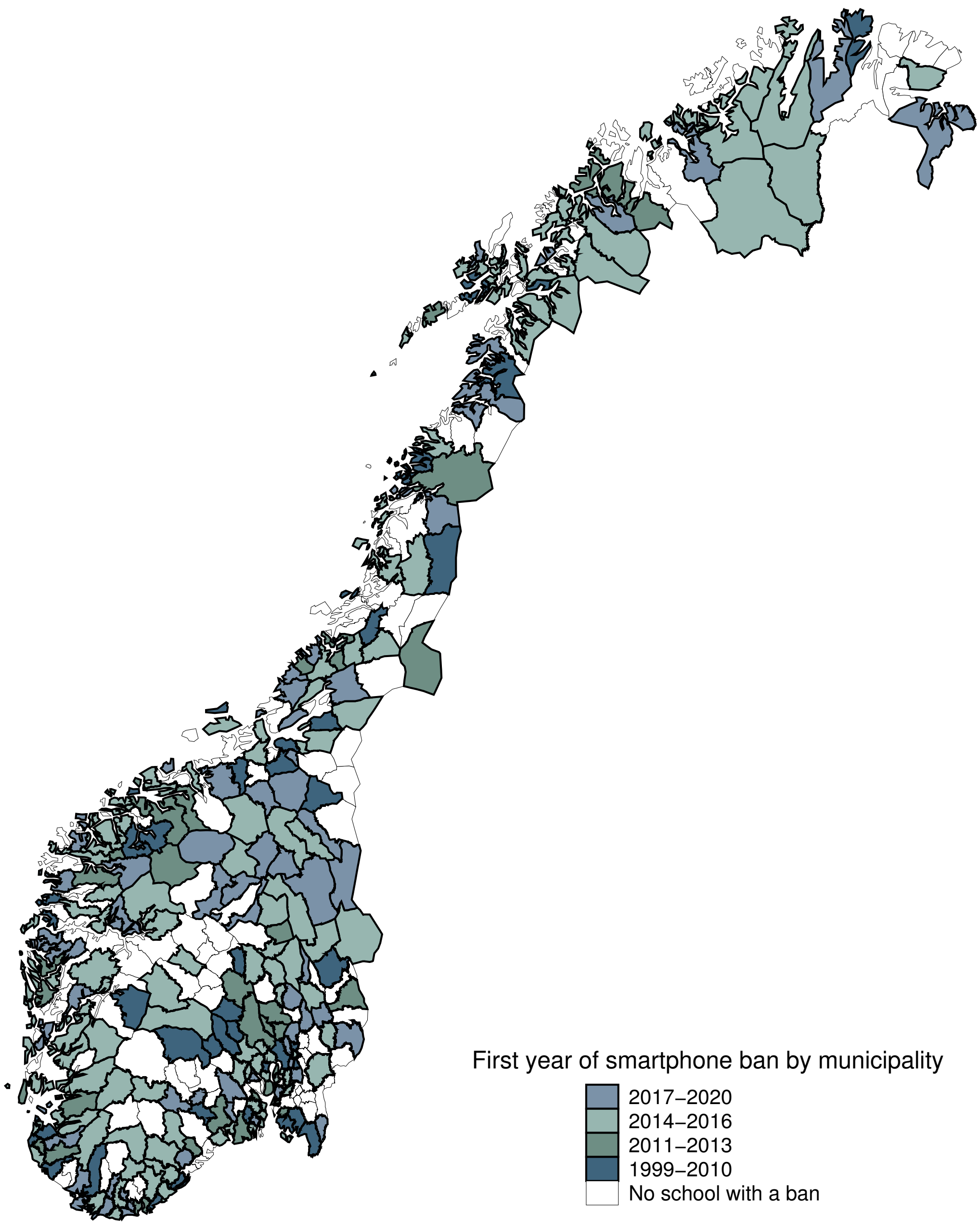


Table A1: Descriptive Statistics for Responding Versus Nonresponding Schools

	All students (i)	Students in non-responding schools (ii)	Students in responding schools (iii)	Difference between column (ii) and (iii) (iv)
	Panel A: Student performance at the individual level			
Teachers average grades	0.01	-0.00	0.02	0.03***
	[1.00]	[1.01]	[0.99]	(7.52)
GPA	0.01	-0.00	0.02	0.03***
	[1.00]	[1.01]	[0.99]	(7.83)
Academic high school track	0.31	0.31	0.31	0.00
	[0.42]	[0.42]	[0.43]	(1.81)
Test score 5^{th} grade	0.01	-0.01	0.03	0.03***
	[1.00]	[1.00]	[0.99]	(9.56)
	Panel B: Bullying at the school level			
Bullying	0.00	0.00	-0.00	-0.01
	[1.00]	[0.99]	[1.01]	(-0.36)
Bullying boys	-0.00	-0.01	0.01	0.02
	[1.00]	[0.98]	[1.01]	(0.81)
Bullying girls	-0.00	0.01	-0.01	-0.02
	[1.00]	[1.00]	[0.99]	(-1.05)
	Panel C: Individual-level characteristics			
Birth year	1999.45	1999.46	1999.45	-0.02**
	[1.66]	[1.65]	[1.67]	(-2.70)
Gender	0.49	0.49	0.49	0.00
	[0.50]	[0.50]	[0.50]	(0.88)
One year older	0.01	0.01	0.01	-0.00***
	[0.09]	[0.10]	[0.08]	(-9.12)
One year younger	0.00	0.00	0.00	-0.00***
	[0.05]	[0.05]	[0.05]	(-3.37)
Birth order	1.88	1.87	1.88	0.01*
	[0.93]	[0.93]	[0.93]	(2.25)
Mothers age	29.05	29.06	29.05	-0.01
	[4.83]	[4.84]	[4.83]	(-0.50)
Fathers age	31.91	31.95	31.86	-0.09***
	[5.72]	[5.74]	[5.70]	(-4.46)
Education mother	13.13	13.09	13.18	0.09***
	[2.46]	[2.46]	[2.46]	(10.05)
Education father	12.75	12.72	12.79	0.07***
	[2.46]	[2.44]	[2.48]	(7.93)
Income mother	388.97	383.25	395.66	12.41***
	[240.51]	[243.01]	[237.37]	(14.78)
Income father	596.77	582.54	613.42	30.88***
	[482.36]	[474.48]	[490.89]	(18.33)
Married	0.92	0.92	0.93	0.01***
	[0.26]	[0.27]	[0.26]	(10.47)
Foreign-born parents	0.07	0.09	0.05	-0.04***
	[0.26]	[0.29]	[0.22]	(-41.46)
Observations	329607	177682	151925	
Number of schools	1736	1232	504	

Notes: Descriptive statistics for key outcome and control variables for all students, students in responding schools, and students in nonresponding schools. Standard deviations are shown in square brackets. Column (iv) shows the difference between students in responding schools versus nonresponding schools over the entire period. T-statistics are shown in parentheses.

Table A2: Effects of Smartphone Ban on Student Average Grades Set by Teachers and GPA, Full Sample

	Average grades				GPA			
	(i)	(ii)	(iii)	(iv)	(v)	(vi)	(vii)	(viii)
-3	0.017	0.000	0.002	0.001	0.019	0.002	0.003	0.003
	(0.017)	(0.019)	(0.018)	(0.018)	(0.016)	(0.019)	(0.017)	(0.017)
-2	0.003	0.004	0.003	0.003	0.001	0.002	0.001	0.001
	(0.013)	(0.014)	(0.013)	(0.013)	(0.012)	(0.013)	(0.012)	(0.012)
0	-0.004	0.006	0.003	0.003	-0.005	0.005	0.003	0.003
	(0.012)	(0.013)	(0.013)	(0.013)	(0.012)	(0.013)	(0.012)	(0.012)
1	-0.015	0.001	-0.002	-0.002	-0.017	-0.000	-0.003	-0.003
	(0.017)	(0.020)	(0.018)	(0.018)	(0.017)	(0.019)	(0.018)	(0.018)
2	-0.002	0.001	0.005	0.005	-0.006	-0.003	0.001	0.001
	(0.022)	(0.026)	(0.024)	(0.024)	(0.021)	(0.026)	(0.024)	(0.024)
3	0.013	0.035	0.035	0.035	0.011	0.034	0.034	0.033
	(0.028)	(0.034)	(0.031)	(0.031)	(0.028)	(0.034)	(0.031)	(0.031)
4	0.041	0.065	0.056	0.056	0.033	0.058	0.048	0.049
	(0.038)	(0.045)	(0.042)	(0.042)	(0.036)	(0.044)	(0.041)	(0.041)
Observations	151925	151925	151925	151925	151925	151925	151925	151925
Pre-ban mean	0.01				0.01			
Test score 5th grade		✓	✓	✓		✓	✓	✓
Peer achievement			✓	✓			✓	✓
Leadership change				✓				✓

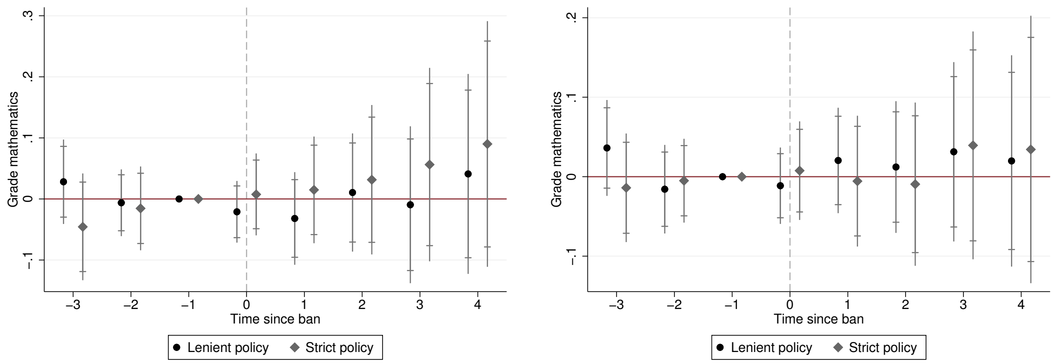
Notes: Columns (i)–(iv) represent regression estimates on student average grades set by students own teachers. Columns (v)–(viii) represent estimates on GPA. All specifications include a full set of cohort and school fixed effects. Robust standard errors are clustered at the school level and shown in parentheses. Additional control variables are a dummy variable for gender, mother's education, mother's age at the birth of the child, mother's marital status at the birth of the child, father's education, father's age at birth, a dummy for having foreign-born parents, a dummy for being 1 year older than classmates, a dummy for being 1 year younger than classmates, and the individual's birth order. The reference year is 1 year prior to the introduction of a smartphone ban. Significance levels: *** 1% level, ** 5% level, * 10% level.

Table A3: Effects of Smartphone Ban on Student Average Grades Set by Teachers and GPA, By Gender

	Average grades				GPA			
	(i)	(ii)	(iii)	(iv)	(v)	(vi)	(vii)	(viii)
	Panel A: Girls							
-3	0.002	-0.014	-0.014	-0.013	0.006	-0.010	-0.010	-0.009
	(0.022)	(0.023)	(0.021)	(0.021)	(0.022)	(0.022)	(0.021)	(0.021)
-2	-0.009	-0.009	-0.010	-0.010	-0.008	-0.009	-0.010	-0.010
	(0.018)	(0.017)	(0.016)	(0.016)	(0.017)	(0.017)	(0.016)	(0.016)
0	-0.013	0.002	-0.001	-0.001	-0.012	0.003	0.001	0.000
	(0.017)	(0.016)	(0.015)	(0.015)	(0.016)	(0.015)	(0.015)	(0.015)
1	-0.040*	-0.010	-0.012	-0.013	-0.042*	-0.011	-0.013	-0.014
	(0.023)	(0.022)	(0.021)	(0.021)	(0.023)	(0.022)	(0.021)	(0.021)
2	-0.013	0.004	0.010	0.009	-0.016	0.002	0.007	0.007
	(0.029)	(0.028)	(0.027)	(0.027)	(0.029)	(0.028)	(0.026)	(0.026)
3	-0.000	0.042	0.043	0.042	-0.003	0.041	0.042	0.042
	(0.037)	(0.037)	(0.035)	(0.035)	(0.036)	(0.037)	(0.034)	(0.034)
4	0.050	0.083*	0.076*	0.074*	0.042	0.076*	0.068	0.067
	(0.047)	(0.047)	(0.044)	(0.044)	(0.046)	(0.046)	(0.043)	(0.043)
Observations	75065	75065	75065	75065	75065	75065	75065	75065
Pre-ban mean	0.25				0.25			
	Panel B: Boys							
-3	0.030	0.012	0.013	0.013	0.030	0.010	0.012	0.012
	(0.022)	(0.024)	(0.022)	(0.022)	(0.021)	(0.023)	(0.022)	(0.022)
-2	0.013	0.015	0.014	0.014	0.010	0.012	0.011	0.011
	(0.017)	(0.017)	(0.016)	(0.016)	(0.017)	(0.016)	(0.016)	(0.016)
0	0.004	0.011	0.008	0.009	0.002	0.009	0.006	0.007
	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)	(0.016)	(0.016)
1	0.009	0.013	0.010	0.010	0.008	0.012	0.010	0.010
	(0.023)	(0.025)	(0.023)	(0.023)	(0.022)	(0.024)	(0.023)	(0.023)
2	0.008	-0.001	0.003	0.003	0.003	-0.006	-0.002	-0.002
	(0.029)	(0.033)	(0.031)	(0.031)	(0.029)	(0.033)	(0.031)	(0.031)
3	0.031	0.033	0.033	0.032	0.029	0.032	0.032	0.030
	(0.038)	(0.042)	(0.040)	(0.040)	(0.037)	(0.042)	(0.039)	(0.039)
4	0.034	0.052	0.043	0.043	0.025	0.043	0.035	0.035
	(0.050)	(0.057)	(0.054)	(0.054)	(0.049)	(0.056)	(0.053)	(0.053)
Observations	76857	76857	76857	76857	76857	76857	76857	76857
Pre-ban mean	-0.23				-0.23			
Test score grade 5		✓	✓	✓		✓	✓	✓
Peer achievement			✓	✓			✓	✓
Leadership change				✓				✓

Notes: Columns (i)–(iv) represent regression estimates on student average grades set by students own teachers. Columns (v)–(viii) represent estimates on GPA. All specifications include a full set of cohort and school fixed effects. Robust standard errors are clustered at the school level and shown in parentheses. Additional control variables are mother's education, mother's age at the birth of the child, mother's marital status at the birth of the child, father's education, father's age at birth, a dummy for having foreign-born parents, a dummy for being 1 year older than classmates, a dummy for being 1 year younger than classmates, and the individual's birth order. The reference year is 1 year prior to the introduction of a smartphone ban. Significance levels: *** 1% level, ** 5% level, * 10% level.

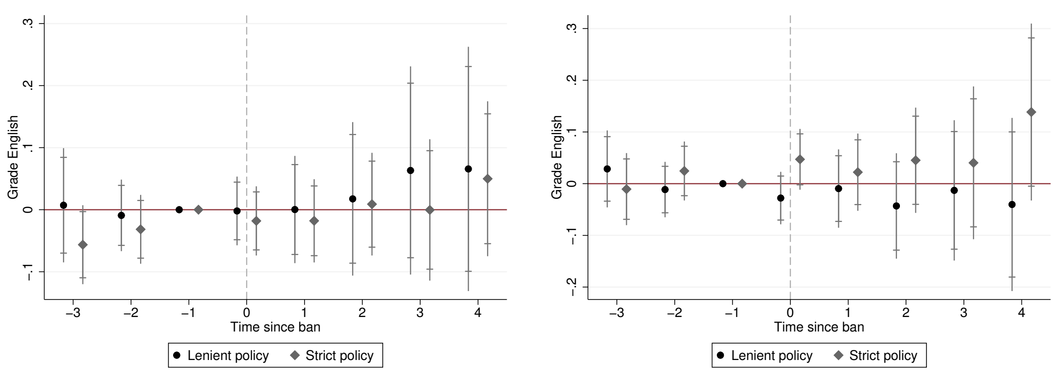
Figure A4: Non-Blind Mathematics Grades Set by Teacher by Type of Ban



(a) Grades mathematics girls (b) Grades mathematics boys

Notes: Each graph is from a separate regression. All specifications include a full set of cohort and school fixed effects. Robust standard errors are clustered at the school level. Additional control variables are mother's education, mother's age at the birth of the child, mother's marital status at the birth of the child, father's education, father's age at birth, a dummy for having foreign-born parents, a dummy for being 1 year older than classmates, a dummy for being 1 year younger than classmates, and the individual's birth order. The reference year is 1 year prior to the introduction of a smartphone ban. Error bars show 95% and 90% confidence intervals.

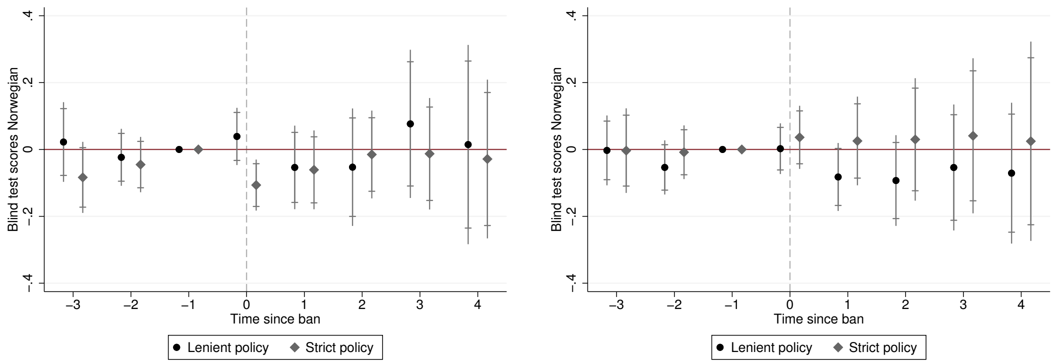
Figure A5: Non-Blind English Grades Set by Teacher by Type of Ban



(a) Grades English girls (b) Grades English boys

Notes: Each graph is from a separate regression. All specifications include a full set of cohort and school fixed effects. Robust standard errors are clustered at the school level. Additional control variables are mother's education, mother's age at the birth of the child, mother's marital status at the birth of the child, father's education, father's age at birth, a dummy for having foreign-born parents, a dummy for being 1 year older than classmates, a dummy for being 1 year younger than classmates, and the individual's birth order. The reference year is 1 year prior to the introduction of a smartphone ban. Error bars show 95% and 90% confidence intervals.

Figure A6: Blind Norwegian Test Scores from Externally Graded Exams by Type of Ban

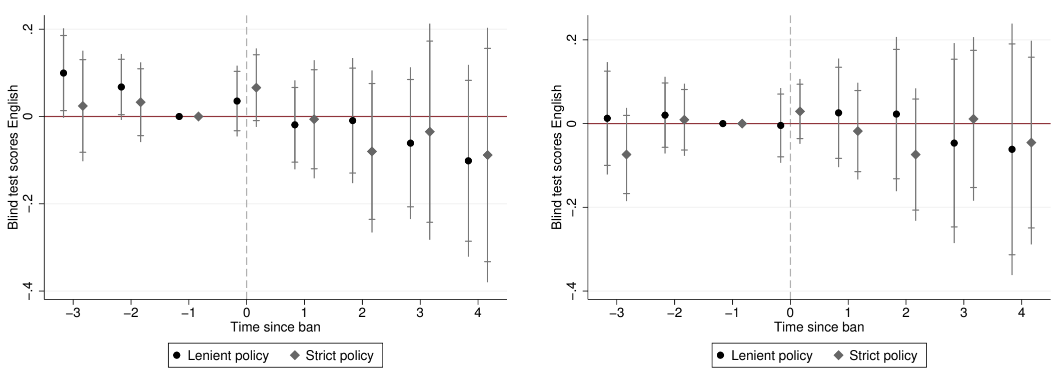


(a) Test scores Norwegian girls

(b) Test scores Norwegian boys

Notes: Each graph is from a separate regression. All specifications include a full set of cohort and school fixed effects. Robust standard errors are clustered at the school level. Additional control variables are mother's education, mother's age at the birth of the child, mother's marital status at the birth of the child, father's education, father's age at birth, a dummy for having foreign-born parents, a dummy for being 1 year older than classmates, a dummy for being 1 year younger than classmates, and the individual's birth order. The reference year is 1 year prior to the introduction of a smartphone ban. Error bars show 95% and 90% confidence intervals.

Figure A7: Blind English Test Scores from Externally Graded Exams by Type of Ban

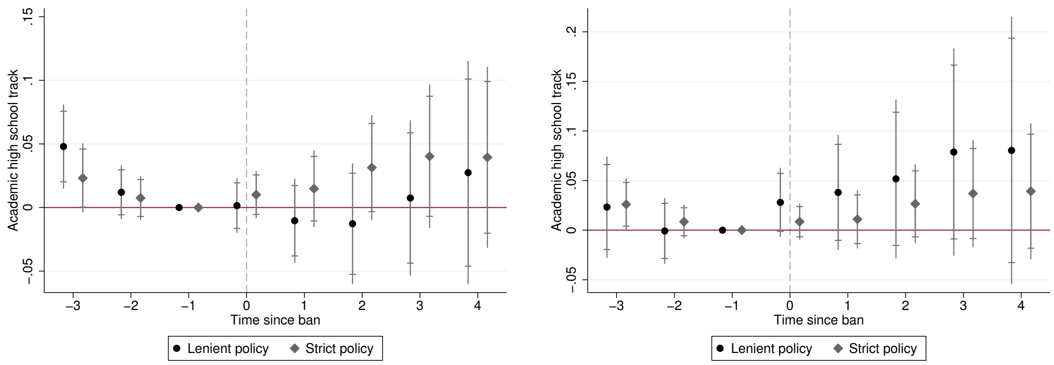


(a) Test scores English girls

(b) Test scores English boys

Notes: Each graph is from a separate regression. All specifications include a full set of cohort and school fixed effects. Robust standard errors are clustered at the school level. Additional control variables are mother's education, mother's age at the birth of the child, mother's marital status at the birth of the child, father's education, father's age at birth, a dummy for having foreign-born parents, a dummy for being 1 year older than classmates, a dummy for being 1 year younger than classmates, and the individual's birth order. The reference year is 1 year prior to the introduction of a smartphone ban. Error bars show 95% and 90% confidence intervals.

Figure A8: Likelihood of Attending an Academic High School Track by Type of Policy

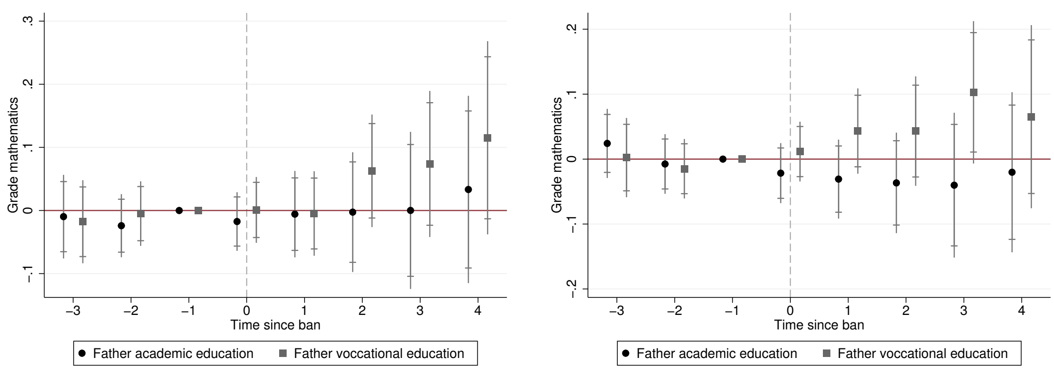


(a) P(Academic track=1 girls

(b) P(Academic track=1 boys

Notes: Each graph is from a separate regression. All specifications include a full set of cohort and school fixed effects. Robust standard errors are clustered at the school level. Additional control variables are mother's education, mother's age at the birth of the child, mother's marital status at the birth of the child, father's education, father's age at birth, a dummy for having foreign-born parents, a dummy for being 1 year older than classmates, a dummy for being 1 year younger than classmates, and the individual's birth order. The reference year is 1 year prior to the introduction of a smartphone ban. Error bars show 95% and 90% confidence intervals.

Figure A9: Non-Blind Mathematics Grades Set by Teacher by Father's Type of High School Education

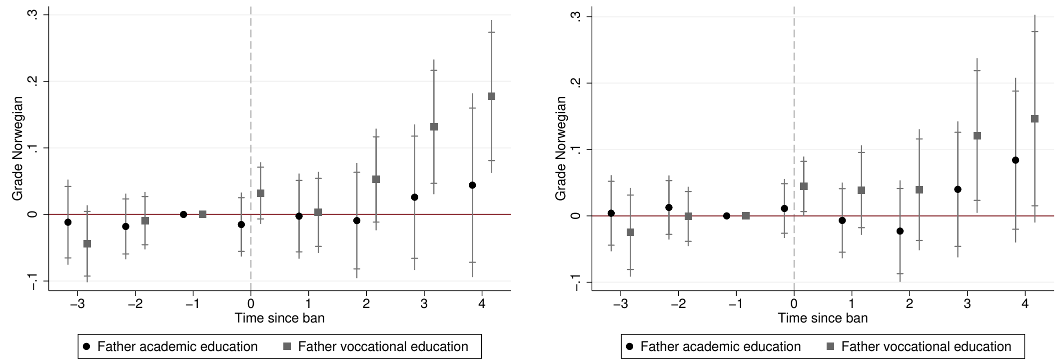


(a) Grades mathematics girls

(b) Grades mathematics boys

Notes: Each graph is from a separate regression. All specifications include a full set of cohort and school fixed effects. Robust standard errors are clustered at the school level. Additional control variables are mother's education, mother's age at the birth of the child, mother's marital status at the birth of the child, father's education, father's age at birth, a dummy for having foreign-born parents, a dummy for being 1 year older than classmates, a dummy for being 1 year younger than classmates, and the individual's birth order. The reference year is 1 year prior to the introduction of a smartphone ban. Error bars show 95% and 90% confidence intervals.

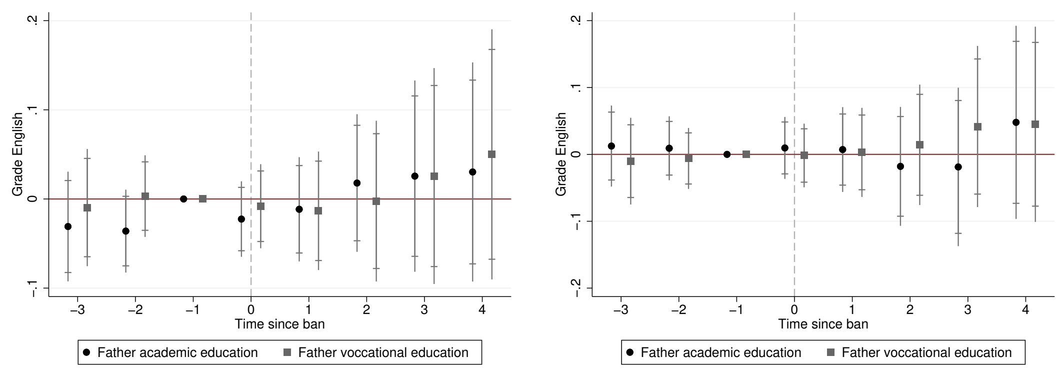
Figure A10: Non-Blind Norwegian Grades Set by Teacher by Father's Type of High School Education



(a) Grades Norwegian girls (b) Grades Norwegian boys

Notes: Each graph is from a separate regression. All specifications include a full set of cohort and school fixed effects. Robust standard errors are clustered at the school level. Additional control variables are mother's education, mother's age at the birth of the child, mother's marital status at the birth of the child, father's education, father's age at birth, a dummy for having foreign-born parents, a dummy for being 1 year older than classmates, a dummy for being 1 year younger than classmates, and the individual's birth order. The reference year is 1 year prior to the introduction of a smartphone ban. Error bars show 95% and 90% confidence intervals.

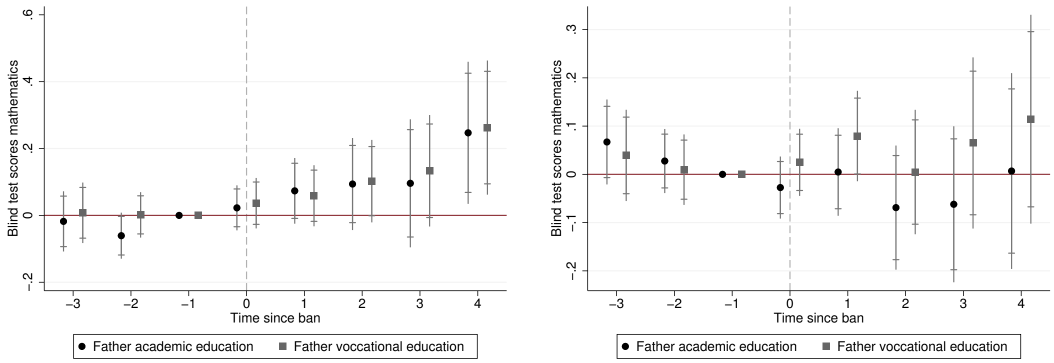
Figure A11: Non-Blind English Grades Set by Teacher by Father's Type of High School Education



(a) Grades English girls (b) Grades English boys

Notes: Each graph is from a separate regression. All specifications include a full set of cohort and school fixed effects. Robust standard errors are clustered at the school level. Additional control variables are mother's education, mother's age at the birth of the child, mother's marital status at the birth of the child, father's education, father's age at birth, a dummy for having foreign-born parents, a dummy for being 1 year older than classmates, a dummy for being 1 year younger than classmates, and the individual's birth order. The reference year is 1 year prior to the introduction of a smartphone ban. Error bars show 95% and 90% confidence intervals.

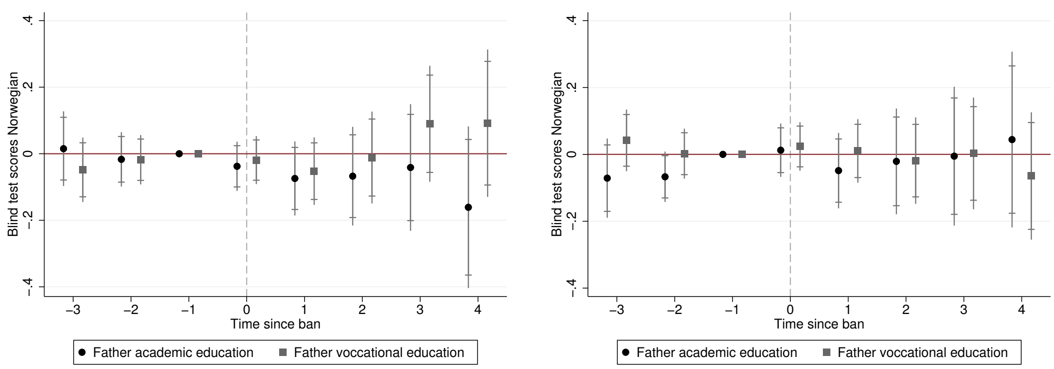
Figure A12: Blind Mathematics Test Scores from Externally Graded Exams by Father's Type of High School Education



(a) Test scores mathematics girls (b) Test scores mathematics boys

Notes: Each graph is from a separate regression. All specifications include a full set of cohort and school fixed effects. Robust standard errors are clustered at the school level. Additional control variables are mother's education, mother's age at the birth of the child, mother's marital status at the birth of the child, father's education, father's age at birth, a dummy for having foreign-born parents, a dummy for being 1 year older than classmates, a dummy for being 1 year younger than classmates, and the individual's birth order. The reference year is 1 year prior to the introduction of a smartphone ban. Error bars show 95% and 90% confidence intervals.

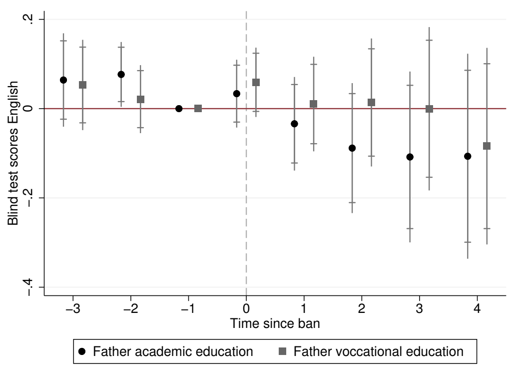
Figure A13: Blind Norwegian Test Scores from Externally Graded Exams by Father's Type of High School Education



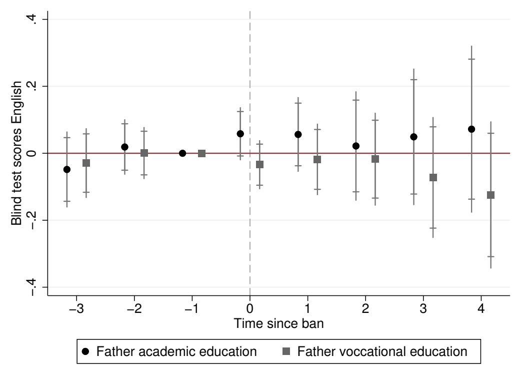
(a) Test scores Norwegian girls (b) Test scores Norwegian boys

Notes: Each graph is from a separate regression. All specifications include a full set of cohort and school fixed effects. Robust standard errors are clustered at the school level. Additional control variables are mother's education, mother's age at the birth of the child, mother's marital status at the birth of the child, father's education, father's age at birth, a dummy for having foreign-born parents, a dummy for being 1 year older than classmates, a dummy for being 1 year younger than classmates, and the individual's birth order. The reference year is 1 year prior to the introduction of a smartphone ban. Error bars show 95% and 90% confidence intervals.

Figure A14: Blind English Test Scores from Externally Graded Exams by Father's Type of High School Education



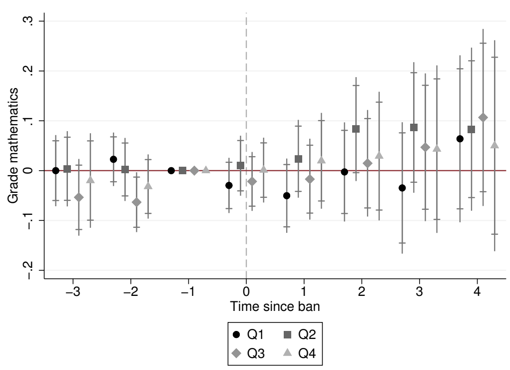
(a) Test scores English girls



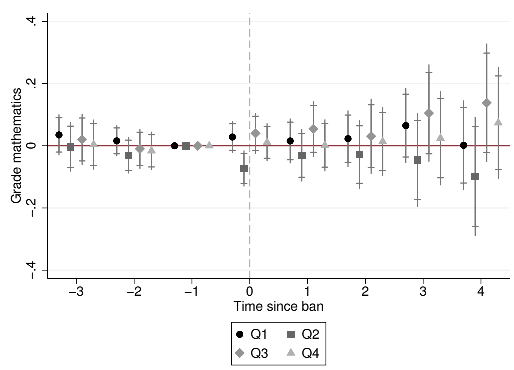
(b) Test scores English boys

Notes: Each graph is from a separate regression. All specifications include a full set of cohort and school fixed effects. Robust standard errors are clustered at the school level. Additional control variables are mother's education, mother's age at the birth of the child, mother's marital status at the birth of the child, father's education, father's age at birth, a dummy for having foreign-born parents, a dummy for being 1 year older than classmates, a dummy for being 1 year younger than classmates, and the individual's birth order. The reference year is 1 year prior to the introduction of a smartphone ban. Error bars show 95% and 90% confidence intervals.

Figure A15: Non-Blind Mathematics Grades Set by Teacher by Ability Quartiles



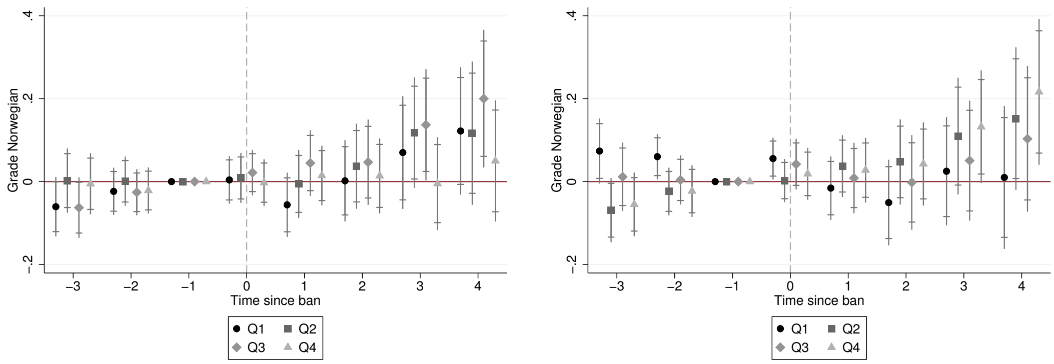
(a) Grades mathematics girls



(b) Grades mathematics boys

Notes: Each graph is from a separate regression. All specifications include a full set of cohort and school fixed effects. Robust standard errors are clustered at the school level. Additional control variables are mother's education, mother's age at the birth of the child, mother's marital status at the birth of the child, father's education, father's age at birth, a dummy for having foreign-born parents, a dummy for being 1 year older than classmates, a dummy for being 1 year younger than classmates, and the individual's birth order. The reference year is 1 year prior to the introduction of a smartphone ban. Error bars show 95% and 90% confidence intervals.

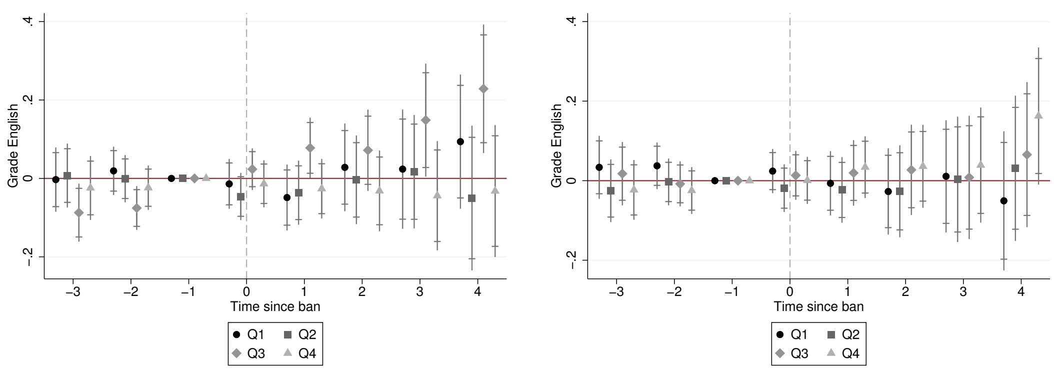
Figure A16: Non-Blind Norwegian Grades Set by Teacher by Ability Quartiles



(a) Grades Norwegian girls (b) Grades Norwegian boys

Notes: Each graph is from a separate regression. All specifications include a full set of cohort and school fixed effects. Robust standard errors are clustered at the school level. Additional control variables are mother's education, mother's age at the birth of the child, mother's marital status at the birth of the child, father's education, father's age at birth, a dummy for having foreign-born parents, a dummy for being 1 year older than classmates, a dummy for being 1 year younger than classmates, and the individual's birth order. The reference year is 1 year prior to the introduction of a smartphone ban. Error bars show 95% and 90% confidence intervals.

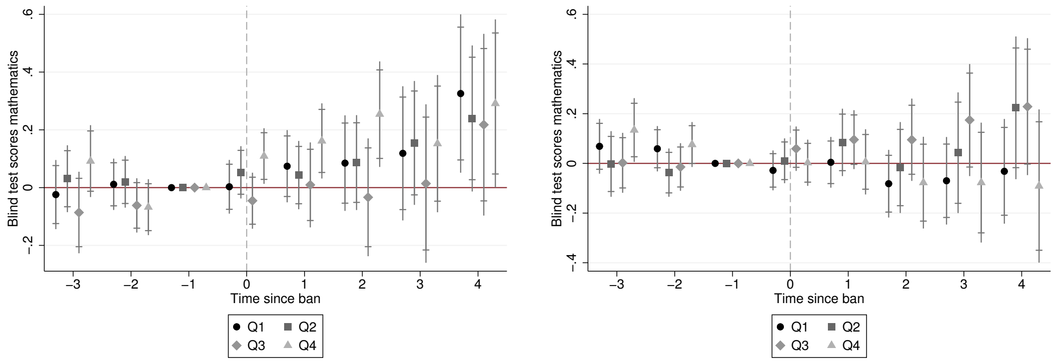
Figure A17: Non-Blind English Grades Set by Teacher by Ability Quartiles



(a) Grades English girls (b) Grades English boys

Notes: Each graph is from a separate regression. All specifications include a full set of cohort and school fixed effects. Robust standard errors are clustered at the school level. Additional control variables are mother's education, mother's age at the birth of the child, mother's marital status at the birth of the child, father's education, father's age at birth, a dummy for having foreign-born parents, a dummy for being 1 year older than classmates, a dummy for being 1 year younger than classmates, and the individual's birth order. The reference year is 1 year prior to the introduction of a smartphone ban. Error bars show 95% and 90% confidence intervals.

Figure A18: Blind Mathematics Test Scores from Externally Graded Exams by Ability Quartiles

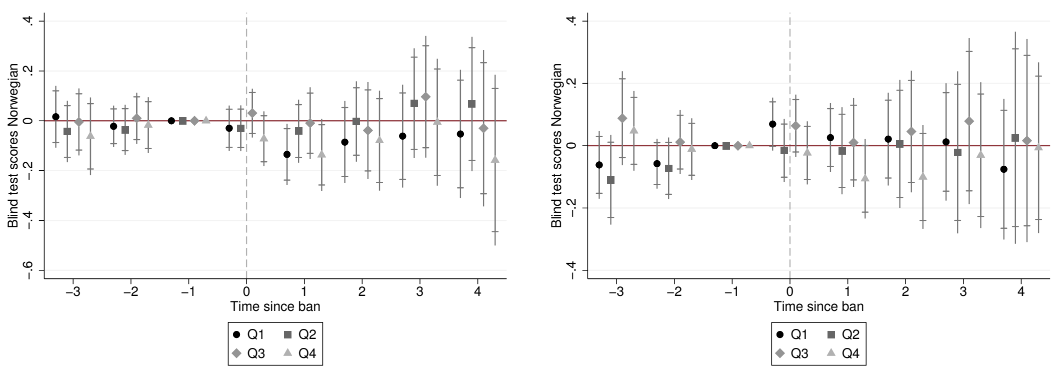


(a) Test scores mathematics girls

(b) Test scores mathematics boys

Notes: Each graph is from a separate regression. All specifications include a full set of cohort and school fixed effects. Robust standard errors are clustered at the school level. Additional control variables are mother's education, mother's age at the birth of the child, mother's marital status at the birth of the child, father's education, father's age at birth, a dummy for having foreign-born parents, a dummy for being 1 year older than classmates, a dummy for being 1 year younger than classmates, and the individual's birth order. The reference year is 1 year prior to the introduction of a smartphone ban. Error bars show 95% and 90% confidence intervals.

Figure A19: Blind Norwegian Test Scores from Externally Graded Exams by Ability Quartiles

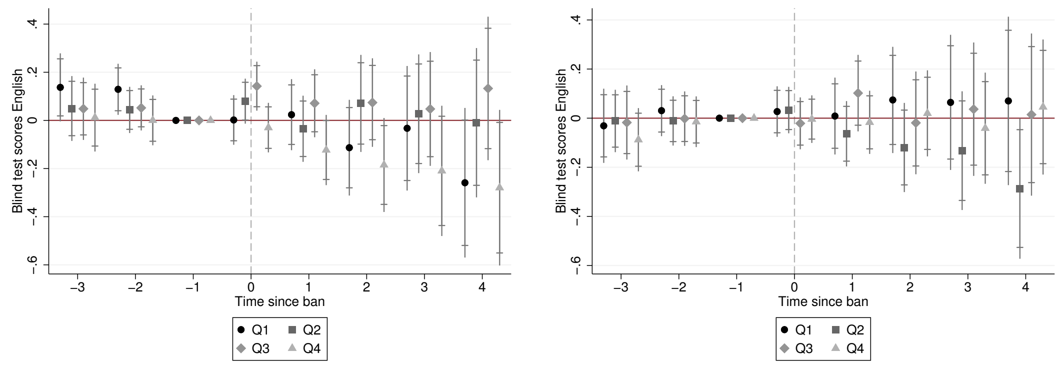


(a) Test scores Norwegian girls

(b) Test scores Norwegian boys

Notes: Each graph is from a separate regression. All specifications include a full set of cohort and school fixed effects. Robust standard errors are clustered at the school level. Additional control variables are mother's education, mother's age at the birth of the child, mother's marital status at the birth of the child, father's education, father's age at birth, a dummy for having foreign-born parents, a dummy for being 1 year older than classmates, a dummy for being 1 year younger than classmates, and the individual's birth order. The reference year is 1 year prior to the introduction of a smartphone ban. Error bars show 95% and 90% confidence intervals.

Figure A20: Blind English Test Scores from Externally Graded Exams by Ability Quartiles

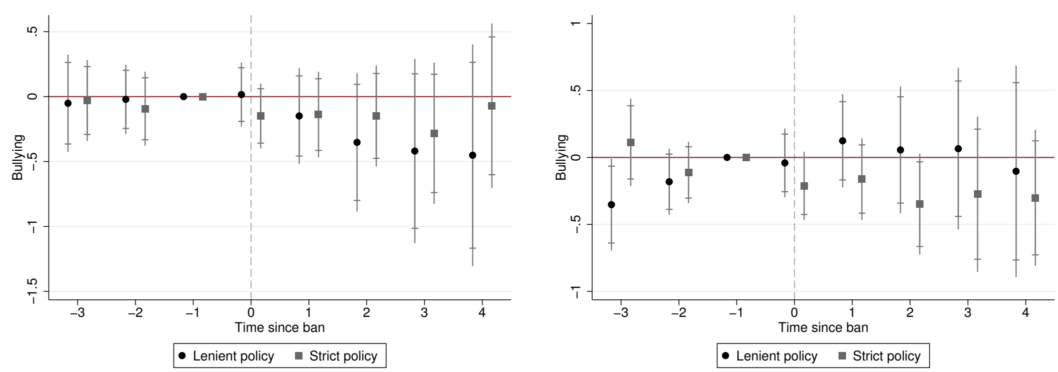


(a) Test scores English girls

(b) Test scores English boys

Notes: Each graph is from a separate regression. All specifications include a full set of cohort and school fixed effects. Robust standard errors are clustered at the school level. Additional control variables are mother's education, mother's age at the birth of the child, mother's marital status at the birth of the child, father's education, father's age at birth, a dummy for having foreign-born parents, a dummy for being 1 year older than classmates, a dummy for being 1 year younger than classmates, and the individual's birth order. The reference year is 1 year prior to the introduction of a smartphone ban. Error bars show 95% and 90% confidence intervals.

Figure A21: Bullying by Type of Ban

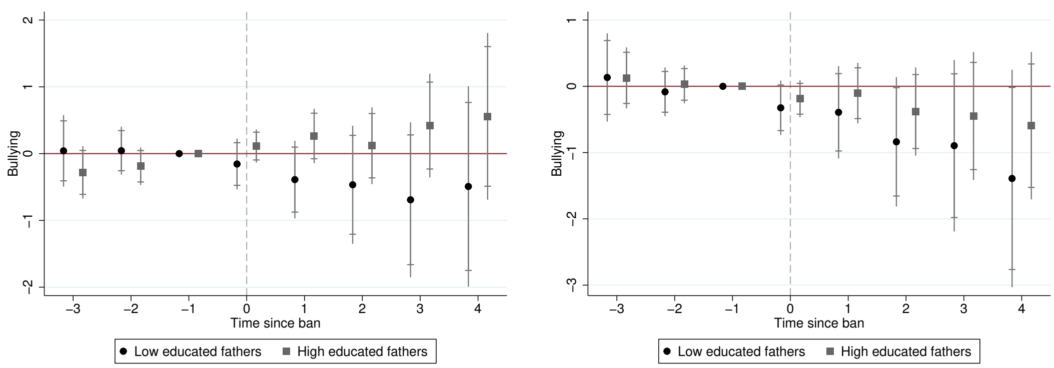


(a) Bullying girls

(b) Bullying boys

Notes: Each graph is from a separate regression. All specifications include a full set of cohort and school fixed effects. Robust standard errors are clustered at the school level. Additional control variables are the mean of mothers' education, the mean of mothers' age at the birth of the child, share of students with married parents at birth, the mean of fathers' education, the mean of fathers' age at birth, share of students being 1 year older than classmates, share of students being 1 year younger than classmates, mean of birth order, share of students with foreign-born parents, mean of students' test scores in grade 5, and a dummy controlling for leadership change. The reference year is 1 year prior to the introduction of a smartphone ban. Error bars show 95% and 90% confidence intervals.

Figure A22: Bullying by Father's Type of High School Education



(a) Bullying girls (b) Bullying boys

Notes: Each graph is from a separate regression. All specifications include a full set of cohort and school fixed effects. Robust standard errors are clustered at the school level. Additional control variables are the mean of mothers' education, the mean of mothers' age at the birth of the child, share of students with married parents at birth, the mean of fathers' education, the mean of fathers' age at birth, share of students being 1 year older than classmates, share of students being 1 year younger than classmates, mean of birth order, share of students with foreign-born parents, mean of students' test scores in grade 5, and a dummy controlling for leadership change. The reference year is 1 year prior to the introduction of a smartphone ban. Error bars show 95% and 90% confidence intervals.

Table A4: Effects of Smartphone Ban on Student Performance Using a Two-Way Difference-in-Difference Specification

	Baseline specification		Linear pre-ban trend	
	Average grades (i)	P(Academic track=1) (ii)	Average grades (iii)	P(Academic track=1) (iv)
	Panel A: Full Sample			
Up to 2 years before ban β_2	0.004	0.015	0.003	0.016
	(0.011)	(0.008)	(0.011)	(0.007)
0–2 years after ban β_3	-0.008	0.016	-0.006	0.014
	(0.011)	(0.009)	(0.011)	(0.009)
3 years or more after ban β_4	0.018	0.039	0.021	0.035
	(0.021)	(0.020)	(0.021)	(0.020)
Observations	151925	127955	151053	127236
	Panel B: Girls			
Up to 2 years before ban β_2	-0.005	0.008	-0.007	0.008
	(0.015)	(0.008)	(0.015)	(0.008)
0–2 years after ban β_3	-0.020	0.014	-0.019	0.013
	(0.014)	(0.008)	(0.014)	(0.008)
3 years or more after ban β_4	0.014	0.052	0.018	0.049
	(0.025)	(0.018)	(0.025)	(0.018)
Observations 75065	63094	74644	62742	
	Panel C: Boys			
Up to 2 years before ban β_2	0.011	0.021	0.012	0.022
	(0.015)	(0.010)	(0.015)	(0.010)
0–2 years after ban β_3	0.005	0.017	0.006	0.015
	(0.015)	(0.012)	(0.015)	(0.012)
3 years or more after ban β_4	0.026	0.029	0.028	0.025
	(0.027)	(0.025)	(0.027)	(0.025)
Observations	76857	64856	76406	64489

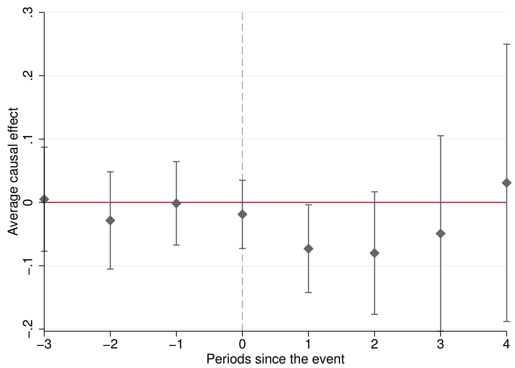
Notes: All specifications include a full set of cohort and school fixed effects. Robust standard errors are clustered at the school level and shown in parentheses. Additional control variables are the individual's test score in grade 5, mother's education, mother's age at the birth of the child, marital status at the birth of the child, father's education, father's age at birth, a dummy for having foreign-born parents, a dummy for being 1 year older than classmates, a dummy for being 1 year younger than classmates and the individual's birth order, peers test score in grade 5, and a dummy controlling for leadership change. Significance levels: *** 1% level, ** 5% level, * 10% level.

Table A5: Effects of Smartphone Ban on Bullying Using a Two-Way Difference-in-Difference Specification

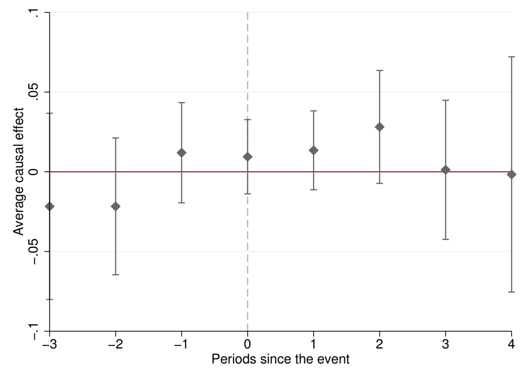
	Baseline specification	Linear pre-ban trend
	Bullying (i)	Bullying (ii)
	Panel A: Full Sample	
Up to 2 years before ban β_2	-0.126*	-0.123*
	(0.073)	(0.073)
0–2 years after ban β_3	-0.090	-0.090
	(0.074)	(0.075)
3 years or more after ban β_4	-0.229*	-0.225
	(0.134)	(0.137)
Observations	2163	2157
	Panel B: Girls	
Up to 2 years before ban β_2	-0.090	-0.087
	(0.080)	(0.081)
0–2 years after ban β_3	-0.094	-0.097
	(0.075)	(0.076)
3 years or more after ban β_4	-0.247*	-0.254*
	(0.141)	(0.143)
Observations	2024	2018
	Panel C: Boys	
Up to 2 years before ban β_2	-0.124	-0.124
	(0.080)	(0.080)
0–2 years after ban β_3	-0.074	-0.072
	(0.085)	(0.086)
3 years or more after ban β_4	-0.159	-0.150
	(0.156)	(0.158)
Observations	2037	2037

Notes: All specifications include a full set of cohort and school fixed effects. Robust standard errors are clustered at the school level. Additional control variables are the mean of mothers' education, the mean of mothers' age at the birth of the child, share of students with married parents at birth, the mean of fathers' education, the mean of fathers' age at birth, share of students being 1 year older than classmates, share of students being 1 year younger than classmates, mean of birth order, share of students with foreign-born parents, mean of students' test scores in grade 5, and a dummy controlling for leadership change. Significance levels: *** 1% level, ** 5% level, * 10% level.

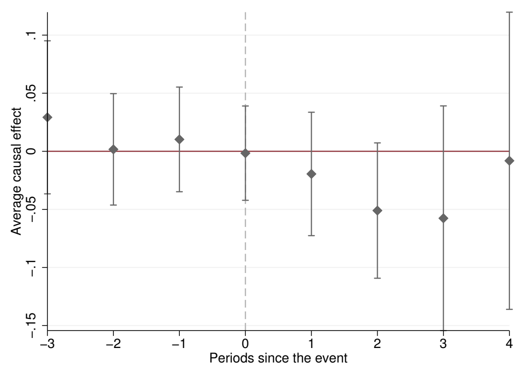
Figure A23: Average Grades Set by Teacher, Likelihood of Attending an Academic High School Track and Bullying for Girls, Robustness to Callaway and Sant'Anna



(a) Average grade



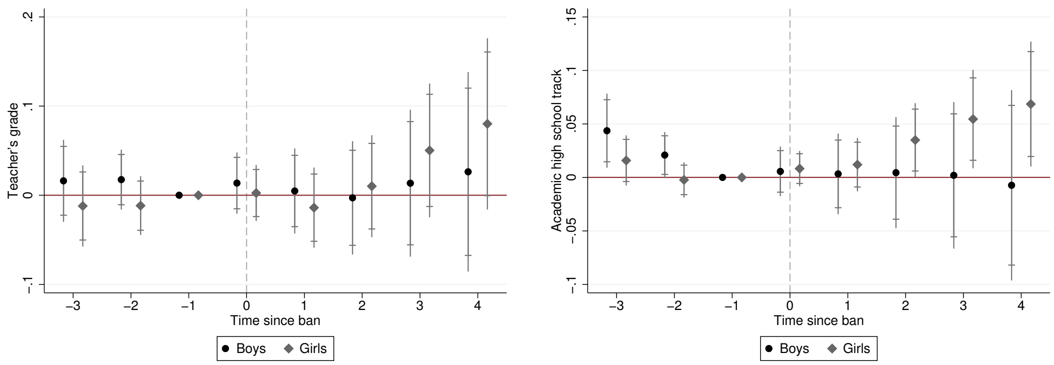
(b) P(Academic track=1)



(c) Bullying girls

Notes: Each graph is from a separate regression. All specifications include a full set of cohort and school fixed effects. Robust standard errors are clustered at the school level. Error bars show 95% confidence intervals.

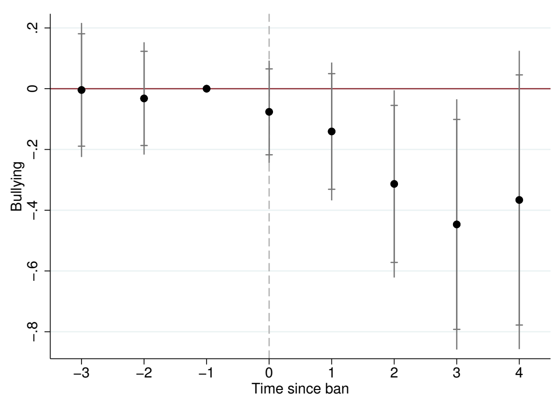
Figure A24: Average Grades Set by Teacher and Likelihood of Attending an Academic High School Track by Gender, Excluding Individual and Parental Control Variables



(a) Average grade by gender (b) P(Academic track=1) by gender

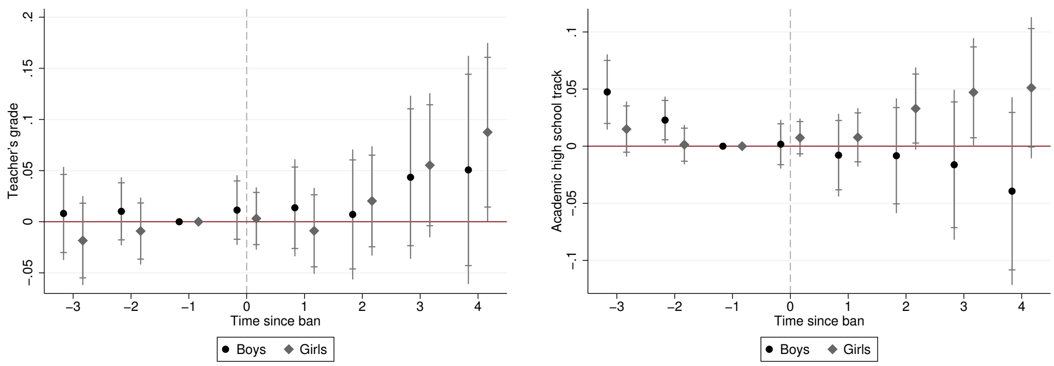
Notes: Each graph is from a separate regression. All specifications include a full set of cohort and school fixed effects. Robust standard errors are clustered at the school level. The reference year is 1 year prior to the introduction of a smartphone ban. Error bars show 95% and 90% confidence intervals.

Figure A25: Bullying Girls, Excluding Control Variables



Notes: The specifications include a full set of cohort and school fixed effects. Robust standard errors are clustered at the school level. The reference year is 1 year prior to the introduction of a smartphone ban. Error bars show 95% and 90% confidence intervals.

Figure A26: Average Grades Set by Teacher and Likelihood of Attending an Academic High School Track by Gender, Excluding The Capital City Oslo

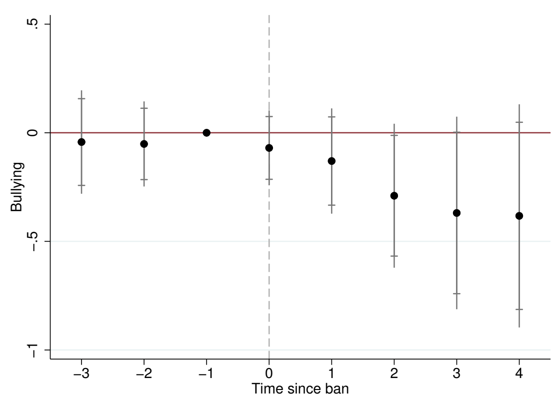


(a) Average grades by gender

(b) P(Academic track=1) by gender

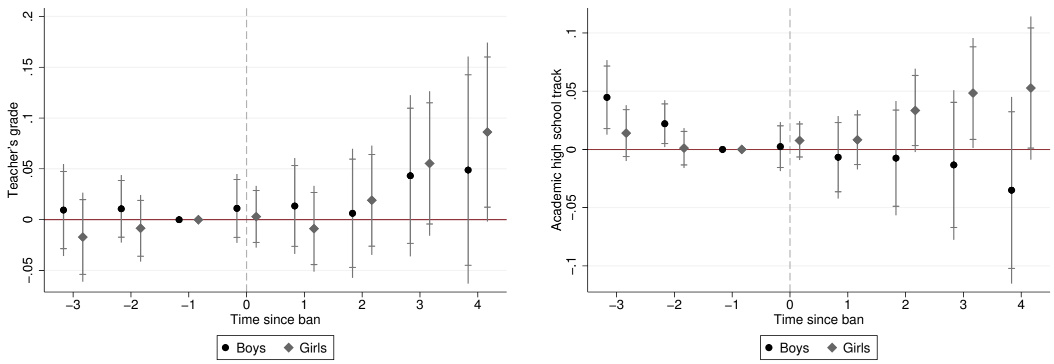
Notes: Each graph is from a separate regression. All specifications include a full set of cohort and school fixed effects. Robust standard errors are clustered at school level. Additional control variables are the individual's test score in grade 5, mother's education, mother's age at the birth of the child, marital status at the birth of the child, father's education, father's age at birth, a dummy for having foreign-born parents, a dummy for being 1 year older than classmates, a dummy for being 1 year younger than classmates and the individual's birth order, peers test score in grade 5, and a dummy controlling for leadership change. The reference year is one year prior to the introduction of a smartphone ban. Error bars show 95% and 90% confidence intervals.

Figure A27: Bullying Girls, Excluding the Capital City Oslo



Notes: Each graph is from a separate regression. All specifications include a full set of cohort and school fixed effects. Robust standard errors are clustered at the school level. Additional control variables are the mean of mothers' education, the mean of mothers' age at the birth of the child, share of students with married parents at birth, the mean of fathers' education, the mean of fathers' age at birth, share of students being 1 year older than classmates, share of students being 1 year younger than classmates, mean of birth order, share of students with foreign-born parents, mean of students' test scores in grade 5, and a dummy controlling for leadership change. The reference year is 1 year prior to the introduction of a smartphone ban. Error bars show 95% and 90% confidence intervals.

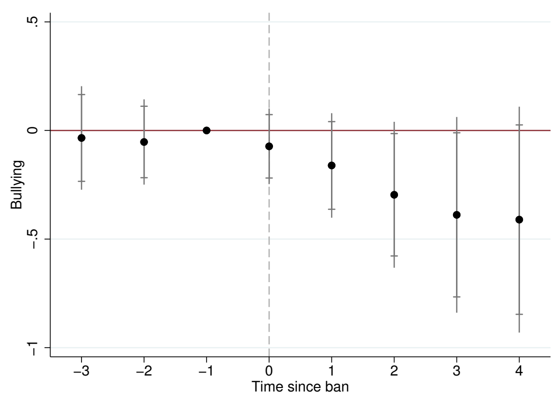
Figure A28: Average Grades Set by Teacher and Likelihood of Attending an Academic High School Track by Gender, Including the Unemployment Level at the Municipality Level



(a) Average grades by gender (b) P(Academic track=1) by gender

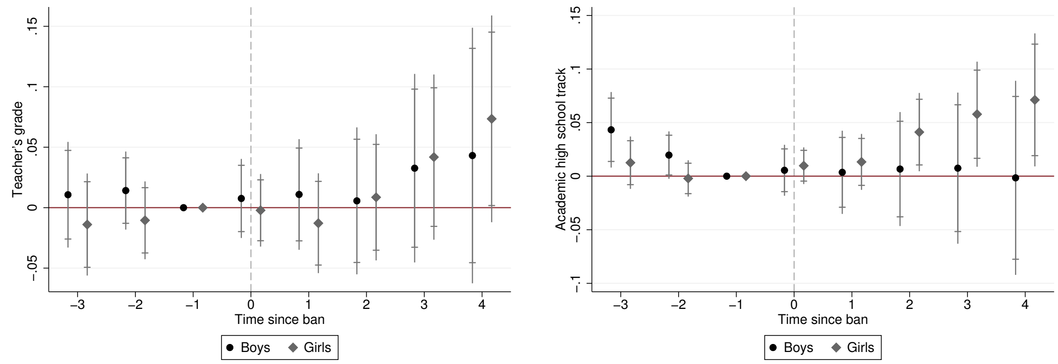
Notes: Each graph is from a separate regression. All specifications include a full set of cohort and school fixed effects. Robust standard errors are clustered at school level. Additional control variables are mother's education, mother's age at birth, marital status at birth, and father's education, father's age at birth, a dummy for having foreign born parents, a dummy for being 1 year older than classmates, a dummy for being 1 year younger than classmates, the individual's birth order and the level of unemployment at municipality level for the year the individual starts middle school. The reference year is 1 year prior to the introduction of a smartphone ban. Error bars show 95% and 90% confidence intervals.

Figure A29: Bullying Girls, Including the Unemployment Level at the Municipality Level



Notes: Each graph is from a separate regression. All specifications include a full set of cohort and school fixed effects. Robust standard errors are clustered at the school level. Additional control variables are the mean of mothers' education, the mean of mothers' age at the birth of the child, share of students with married parents at birth, the mean of fathers' education, the mean of fathers' age at birth, share of students being 1 year older than classmates, share of students being 1 year younger than classmates, mean of birth order, share of students with foreign-born parents, mean of students' test scores in grade 5, a dummy controlling for leadership change and the level of unemployment at municipality level. The reference year is 1 year prior to the introduction of a smartphone ban. Error bars show 95% and 90% confidence intervals.

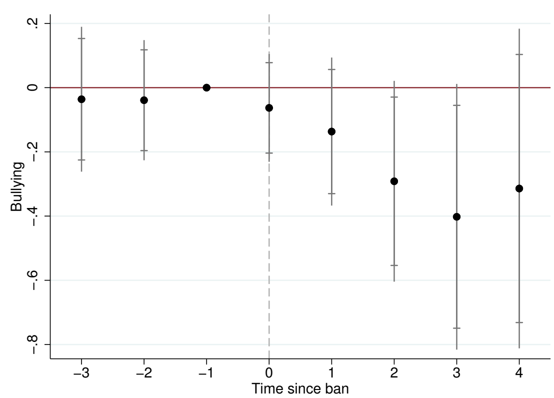
Figure A30: Average Grades Set by Teacher and Likelihood of Attending an Academic High School Track by Gender, Excluding Private Schools



(a) Average by gender (b) P(Academic track=1) by gender

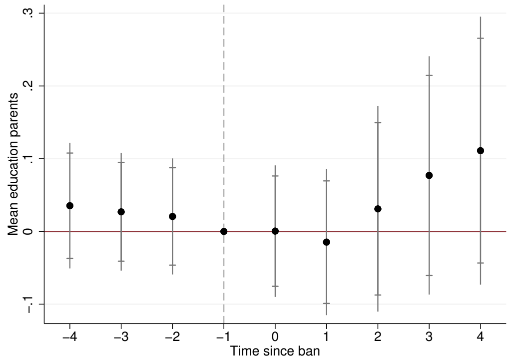
Notes: Each graph is from a separate regression. All specifications include a full set of cohort and school fixed effects. Robust standard errors are clustered at school level. Additional control variables are the individual's test score in grade 5, mother's education, mother's age at the birth of the child, marital status at the birth of the child, father's education, father's age at birth, a dummy for having foreign-born parents, a dummy for being 1 year older than classmates, a dummy for being 1 year younger than classmates and the individual's birth order, peers test score in grade 5, and a dummy controlling for leadership change. The reference year is one year prior to the introduction of a smartphone ban. The outcome mathematics represent test scores from externally corrected exams. Error bars show 95% and 90% confidence intervals.

Figure A31: Bullying Girls, Excluding Private Schools

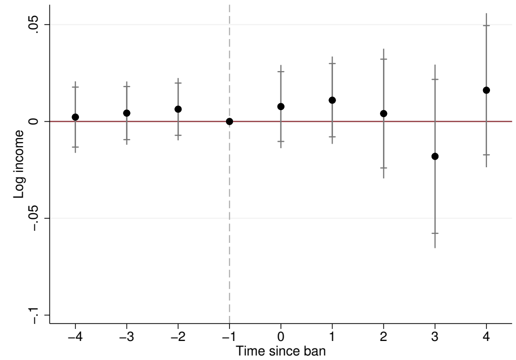


Notes: Each graph is from a separate regression. All specifications include a full set of cohort and school fixed effects. Robust standard errors are clustered at the school level. Additional control variables are the mean of mothers' education, the mean of mothers' age at the birth of the child, share of students with married parents at birth, the mean of fathers' education, the mean of fathers' age at birth, share of students being 1 year older than classmates, share of students being 1 year younger than classmates, mean of birth order, share of students with foreign-born parents, mean of students' test scores in grade 5, and a dummy controlling for leadership change. The reference year is 1 year prior to the introduction of a smartphone ban. Error bars show 95% and 90% confidence intervals.

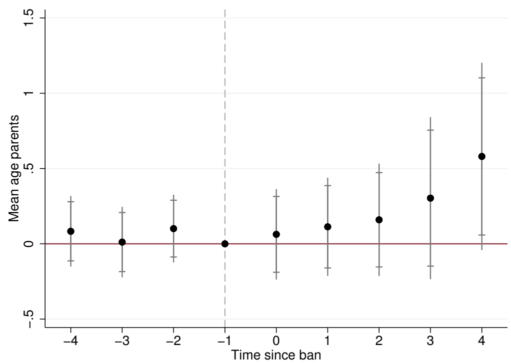
Figure A32: Event-Study Figures for Compositional Changes at the School Level



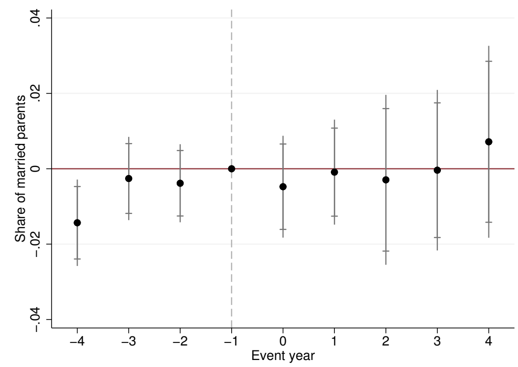
(a) Education of parents



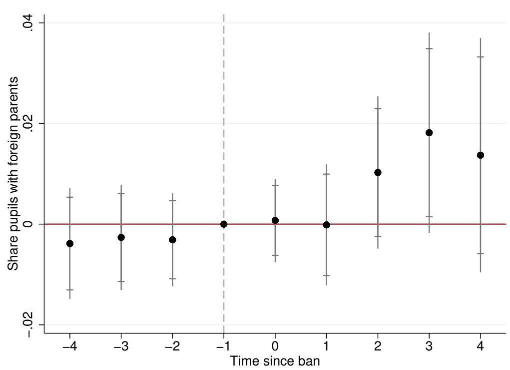
(b) Income parents



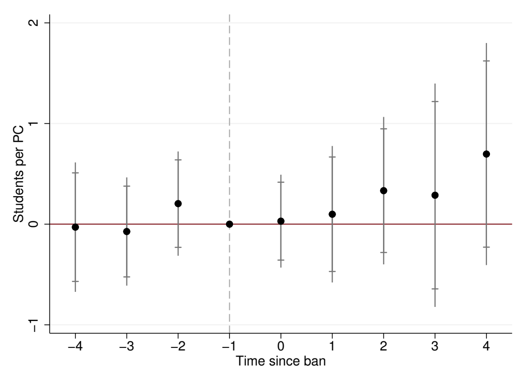
(c) Age of parents



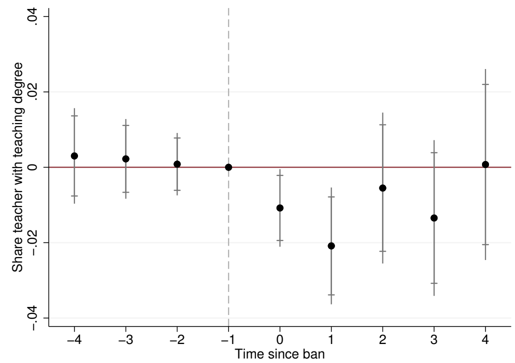
(d) Share of students with married parents



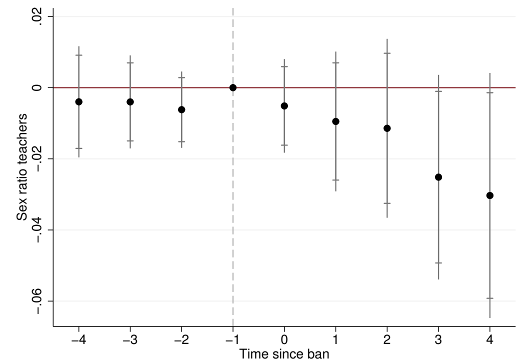
(e) Share of students with foreign parents



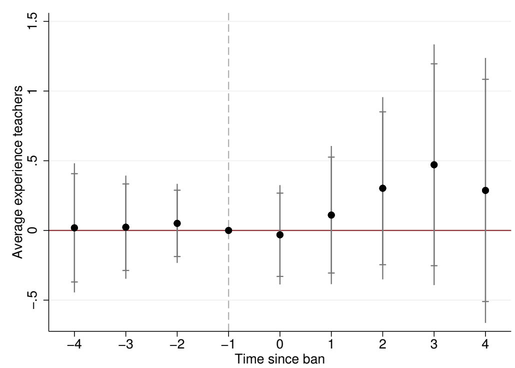
(f) Students per PC



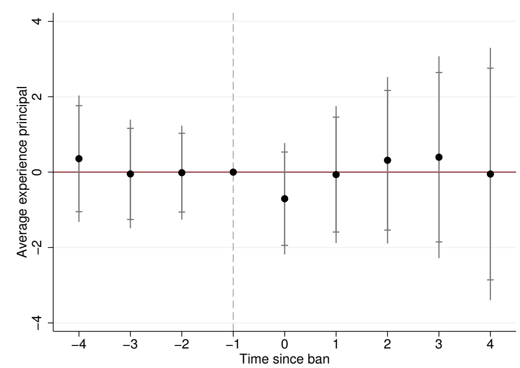
(g) Share of teachers with teaching degrees



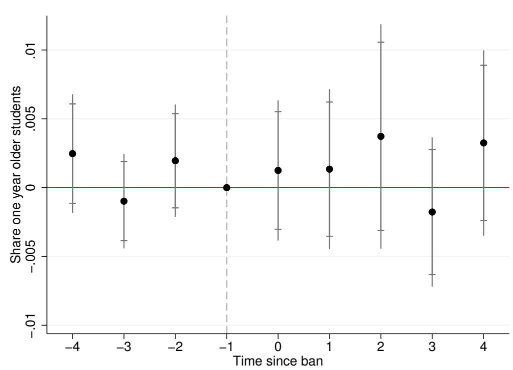
(h) Sex ratio teachers



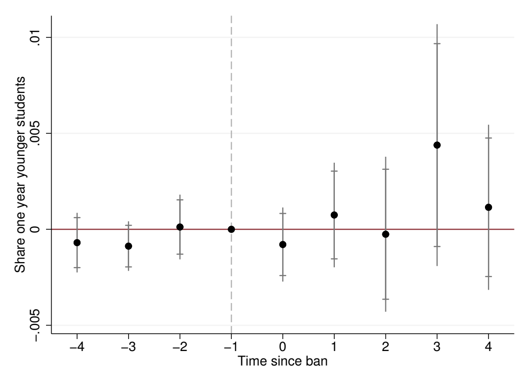
(i) Average experience of teachers



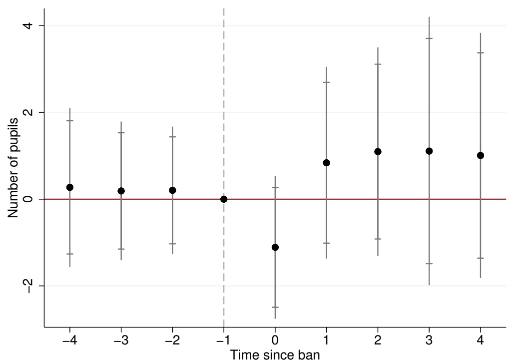
(j) Average experience of principal



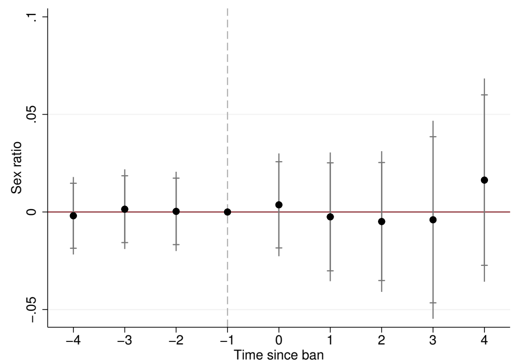
(k) Share of students 1 year older by cohort



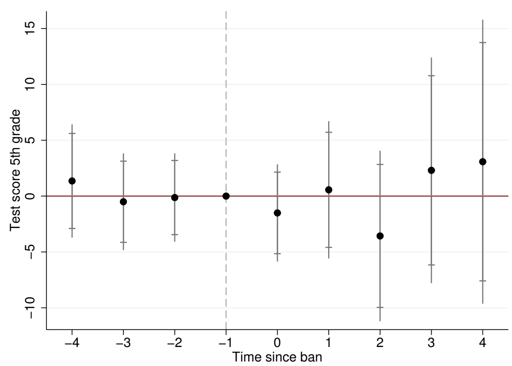
(l) Share of students 1 year younger by cohort



(m) Number of pupils



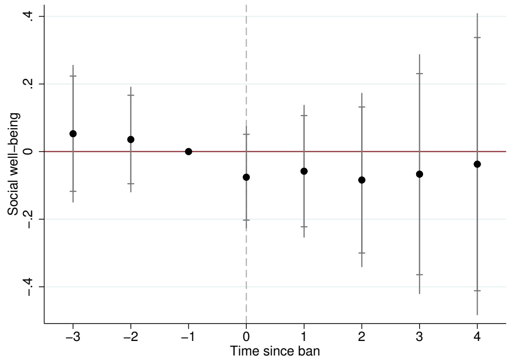
(n) Sex ratio of pupils



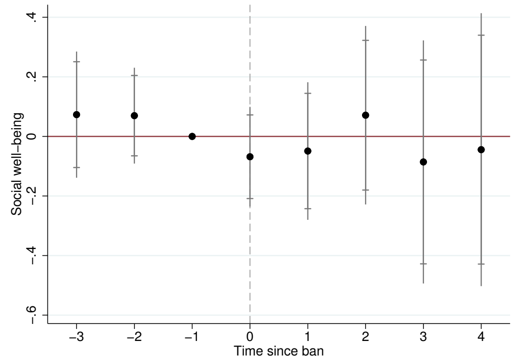
(o) Previous achievement

Notes: Estimated impact on various student, teacher, and socioeconomic characteristics of parents to students, conditional on school and year fixed effects. The reference year is 1 year prior to the introduction of a smartphone ban. Error bars show 95% and 90% confidence intervals.

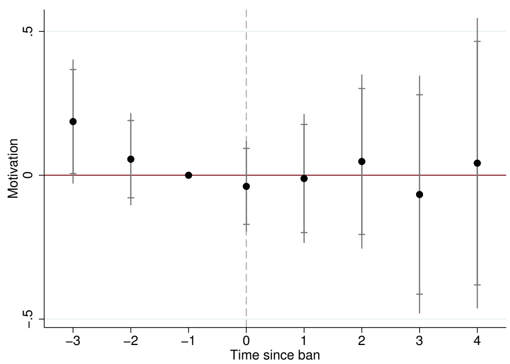
Figure A33: Social Well-Being, Motivation, and Pupil Democracy



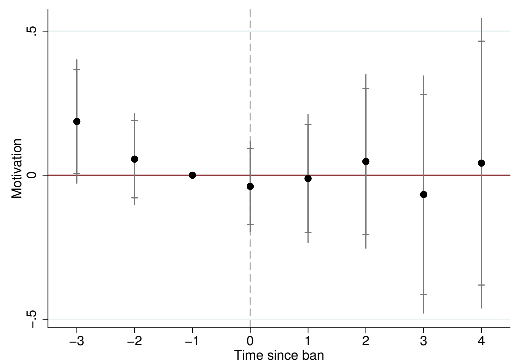
(a) Social well-being girls



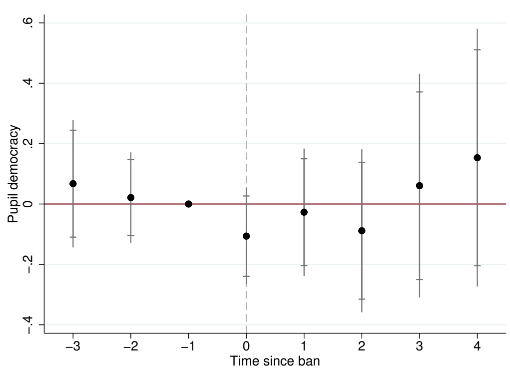
(b) Social well-being boys



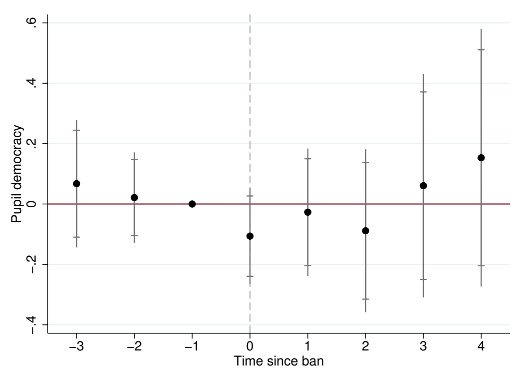
(c) Motivation girls



(d) Motivation boys



(e) Pupil democracy girls



(f) Pupil democracy boys

Notes: Each graph is from a separate regression. All specifications include a full set of cohort and school fixed effects. Robust standard errors are clustered at the school level. Additional control variables are the mean of mothers' education, the mean of mothers' age at the birth of the child, share of students with married parents at birth, the mean of fathers' education, the mean of fathers' age at birth, share of students being 1 year older than classmates, share of students being 1 year younger than classmates, mean of birth order, share of students with foreign-born parents, mean of students' test scores in grade 5, and a dummy controlling for leadership change. The reference year is 1 year prior to the introduction of a smartphone ban. Error bars show 95% and 90% confidence intervals.

1.B Appendix Survey

Survey Questions

The survey was sent out by email. It was originally in Norwegian, although questions and answer categories are documented in English.

Survey

1. Which school are you answering on behalf of?
2. Which alternative best describes your school's mobile phone policy?
   (a) Mobile phones are not allowed on school premises
   (b) Mobile phones are allowed, but should always be turned off or kept in "mobile phone hotels"
   (c) Mobile phones are allowed, but should always be on silent mode and turned off during class
   (d) Mobile phones are allowed, but should always be on silent mode
   (e) Mobile phones are allowed, but should not disturb during class
   (f) No mobile phone policy
   (g) Other
3. If "other", what mobile phone policy do you have?
4. Which year was your present mobile phone policy introduced?
5. Did you have another mobile phone policy before your present policy?
6. If yes, which alternative best describes your previous mobile phone policy?
   (a) Mobile phones are not allowed on school premises
   (b) Mobile phones are allowed, but should always be turned off or kept in "mobile phone hotels"
   (c) Mobile phones are allowed, but should always be on silent mode and turned off during class
   (d) Mobile phones are allowed, but should always be on silent mode
   (e) Mobile phones are allowed, but should not be disturbing during class
   (f) No mobile phone policy
   (g) Other
7. Do you have any other questions or comments?

Chapter 2

The Effects of Fast Food Restaurants' Proximity During Childhood on BMI and Cognitive Ability*

Sara Abrahamsson† Aline Bütikofer‡ Krzysztof Karbownik§

Abstract

Using spatial and temporal variation in openings of fast food restaurants in Norway between 1980 and 2007, we study the effects of changes in the supply of high caloric nutrition on health and cognitive ability of young adult males. Our results indicate that exposure to these establishments during childhood increases BMI and has negative effects on cognition. Heterogeneity analysis does not reveal meaningful differences in the effects across groups, including for those with adverse prenatal health or high paternal BMI, an exception being that cognition is only affected by exposure at ages 0–12 and is mediated by paternal education.

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2.1 Introduction

Obesity is one of the leading causes of preventable morbidity and mortality in Western countries, with the childhood and adolescent period being of particular concern. Excess weight during these critical developmental years is associated with higher incidence of asthma, diabetes, adult cardiovascular problems, increased risk of cancer, as well as adverse social and economic outcomes in early adulthood (WHO, 2016). It affects both poor and rich countries alike, with the United States having comparable obesity prevalence to Libya (about 30–35 percent) and Norway to Haiti (about 23 percent) (World Obesity Federation, 2022). Thus, the rise in obesity rates over the past five decades is a global phenomenon, and although many actions are taken to stop it, the forecasts suggest an urgent need to counter the obesogenic environment by addressing vital elements in children's lives such as diet and physical activity (OECD, 2017).

Physiologically, the cause of obesity is excessive food energy intake, while its social and economic foundations have long been disputed, and researchers struggled to provide population-level causal estimates for the hypothesized channels (Keith et al., 2006). Above and beyond genetics and biological factors, two major contributors to the raise in obesity that have been proposed are: (1) food supply and marketing practices that increase the consumption of highly processed meals; and (2) declining physical activity (Wareham et al., 2005; Chandon and Wansink, 2012). The former factor in particular has gained attention in both the media and policy circles in recent years. For example, the Royal College of Pediatrics and Childhood Health in the UK has proposed banning fast food restaurants from opening in close proximity to schools, and multiple jurisdictions worldwide have imposed additional taxes on sodas intended to reduce the consumption of drinks with added sugar (WHO, 2015; Marsh, 2018). Likewise, there are currently proposals in the UK to ban promotions and discounts on unhealthy food items (Forrest, 2020) and to limit the advertisement of these products (Siddique, 2020). During the COVID-19 pandemic, many US states banned fast food drive-throughs in order to limit the accessibility of unhealthy eating options (Helmer, 2020). Empirical evidence on such restrictions supports the notion that banning fast food outlets decreases their spatial density and thus the supply of unhealthy food options (Brown et al., 2022). In Canada, ban on the sale of junk food in schools also led to a reduction in the students' BMI (Leonard, 2017). Furthermore, less extreme policy measures such as calorie posting (Bollinger et al., 2011; Restrepo, 2017; Aranda et al., 2021), common sense consumption acts (Carpenter and Tello-Trillo, 2015), advertisement bans (Dubois et al., 2018), and soda taxes (Dubois et al., 2020; Seiler et al., 2021) may also prove effective by either directly changing consumption patterns or indirectly by encouraging increases in healthy behaviors.

The above-mentioned policy interventions are often motivated by economic arguments. Given that the average fast food meal consists of more than twice the energy density of a recommended healthy meal (Prentice and Jebb, 2003), and excessive consumption of sugar-sweetened beverages leads to gains in weight (Malik et al., 2013), it seems plausible that changing the cost of access or availability of such products could lead to health benefits. At the same time, proximity to fast food restaurants facilitates easier access to high caloric nutrition by lowering both monetary and non-monetary costs. Moore et al. (2009) document that an increased density of fast food outlets in a neighborhood leads to higher consumption of such products and substitution away from healthy diet. However, the negative health effects of this broader availability might not unravel if healthier options are available and affordable (Niebylski et al., 2015), if consumption patterns differ by demographic group (Dunn et al., 2012), or if positive changes in physical activity occur in parallel (Courtemanche et al., 2021). Ultimately, it is an empirical question of whether increased access to fast food outlets leads to worse health and non-health outcomes. At the same time, it is clear that the consumption of such food items is increasing given the expansion and profits seen in this sector of the economy (NACS, 2018).

In this paper, we ask the following questions to understand the effects of fast food restaurants' expansion on children's well-being: Does an increased supply of fast food outlets lead to worse health outcomes as measured by BMI? Do these negative effects extend beyond health capital and into human capital and cognition? And finally, how homogeneous are the effects across a variety of individual characteristics including propensity for being overweight and prenatal health?

We answer these questions by leveraging data on all fast food restaurants that opened in Norway between 1980 and 2007 paired with a universe of conscription and education data for all Norwegian males born between 1980 and 1989. Norway is a country where more than 50 percent of all adult males are currently overweight (SSB, 2017), and this rate has increased more than seven-fold, from about 7 percent in the early 1980s when the first Western fast food restaurants opened (FHI, 2017). Our main empirical approach exploits quasi-random variation in changes in access to fast food using a two-way fixed effects methodology. Thus, we compare the outcomes of individuals residing in narrow geographical locations in Norway with and without a restaurant opening, and before versus after its establishment. This is an intent-to-treat effect that estimates the consequences of facilitating access to rather than consumption of fast food.

We find that growing up in a neighborhood that has a fast food restaurant increases BMI and the likelihood of being overweight in young adult males. These effects appear economically meaningful given that a mean exposure confers a BMI increase of 1.5%, or about 35% of the growth in average BMI between the first and the last cohort observed in our data. The overweight rate increases at about 1.6% per year of exposure to a fast food establishment, which, given the average exposure, amounts to over a third of the growth in overweight rates across the cohorts included in this study. These health effects appear homogeneous across the groups and we do not find meaningful heterogeneity by paternal BMI, household socioeconomic status, or the child's neonatal health. However, at least when it comes to the probability of being overweight, the effects are larger when children are exposed at age 13–19 compared to age 0–12.

Parallel to the adverse health effects, we also find declines in cognition of about 0.56% of a standard deviation (SD) per year of exposure. These estimates, although quantitatively smaller, suggest a decline in cognitive ability of 4% of a SD for the average number of years of exposure. We further find evidence that exposure to fast food restaurants lowers the likelihood of pursuing an academic track in high school. In contrast to the health outcomes, however, we find that cognition is solely affected by early life exposure to fast food at age 0–12. Furthermore, the negative cognitive effects are reduced by approximately half if the father has an academic high school degree.

Our findings on BMI and cognitive ability are robust. Point estimates and statistical significance are not materially affected by the choice of econometric specification, estimation sample, definition of treatment distance, or transformations of the dependent variable. We also demonstrate that our results are unlikely to be driven by differential pre-trends or selection by considering an event-study and a randomization inference approach that randomizes the set of locations with fast food restaurants, holding their actual number fixed. These tests mitigate concerns that selection or spurious trends are driving our results.

This paper makes three contributions to the existing literature. First, almost all previous research focuses on health consequences of access to fast food, while we investigate the effects on both young adults' BMI and cognition for the same population. Second, population-level administrative data allow us to conduct extensive heterogeneity analyses, including the potential interactions between access to fast food restaurants and individual measures of fetal health and propensity for obesity. Much previous research has relied on smaller scale administrative or survey data, which prevents any such detailed analysis. At the same time, understanding the heterogeneity is of particular relevance to policy given the potential for targeted vs. universal interventions (Dubois et al., 2020; Griffith, 2022) and the observed intergenerational associations in obesity (Classen, 2010; Classen and Thompson, 2016). Finally, to the best of our knowledge, this is the first set of causal estimates on access to fast food restaurants in the context of the Nordic countries where, despite their relatively healthy populations, high per capita income, and universal free healthcare, the obesity rate is growing rapidly.[5]

Our main contribution is to the literature on the effects of supply of fast food on health outcomes. Research in economics, epidemiology, medicine, and public health have studied this relationship before but the results have been inconclusive, appear to be context specific, and most of this literature focuses on the US (see e.g., Rosenheck (2008), Papoutsi et al. (2013), Williams et al. (2014), Cawley (2015), and (Jia et al., 2019) for recent reviews). For example, Davis and Carpenter (2009), Currie et al. (2010), and Sánchez et al. (2012) demonstrate that teenagers attending a school nearby a fast food restaurant have elevated BMI and

[5]Our findings are also some of the first from a country outside of the US where most of the research has been conducted to date.

other measures of excess weight. On the other hand, Howard et al. (2011), Asirvatham et al. (2019), and Langellier (2012) report no such relationship. Other papers examine exposure at the place of residence with the estimates likewise ranging from increases in BMI (Elbel et al., 2020; Qian et al., 2017) to no effects (Lee, 2012; Dolton and Tafesse, 2022). The literature studying exposure in adulthood is also inconclusive, with Anderson and Matsa (2011) finding no link between fast food restaurants and obesity while Giuntella (2018) reporting a positive association and excess weight gain in pregnant mothers.[6]

The majority of studies to date have focused on the US where for certain demographic groups, fast food and soda are the most easily accessible and cheapest sources of food. Indeed, prior work has shown that poverty is associated with both a higher consumption of fast food meals and increased obesity (Drewnowski and Specter, 2004). This could be driven either by supply side factors via food deserts and lack of access to higher quality nutrition (Wrigley et al., 2003) or demand side factors with different demographic groups having different preferences regarding fast food Allcott et al. (2019). Despite very different institutional and cultural environments, some studies from other countries such as China (Kong and Zhou, 2021), Mexico (Giuntella et al., 2020), and Sweden (Hamano et al., 2017) have likewise found positive associations between fast or Western food and elevated weight. The last study is of particular interest as it is the only analysis addressing a Scandinavian country, and while in some models the authors find statistically significant associations, this result is not robust to all modeling choices.[7] On the other hand, Dolton and Tafesse (2022) is a notable counterexample as they do not find any meaningful effects in their study carried out in England. Given this limited international evidence, there is a growing interest in studies from countries where healthier food alternatives are easily available and accessible for most people, and where the fast food business is still relatively new, albeit growing at a high rate.

The aforementioned demographic differences in consumption and obesity rates further call for a detailed heterogeneity analysis. Rich administrative data allow us to not only study heterogeneity by parental employment, education, or place of residence, but also to investigate BMI in the context of prenatal and intergenerational health. For example, Ravelli

[6]There is also some evidence on the effects of convenience stores and supermarkets on obesity. Howard et al. (2011), Zeng et al. (2019a), and Rummo et al. (2020) document that access to convenience/corner stores is associated with increases in BMI of school age children in California, Arkansas, and New York City, respectively. Furthermore, Courtemanche and Carden (2011) document that the proliferation of Walmart Supercenters in the US can explain up to 10.5% of the rise in obesity since the late 1980s. On the other hand, Zeng et al. (2019b) find no relationship between supermarket openings or closures and weight. Our primary interest in this paper is access to fast food restaurants but given the prior research in all empirical specifications, we control for proximity to convenience stores and supermarkets. This also addresses alternative supply channels of sugar-sweetened beverages.

[7]We are not aware of any other studies from Scandinavia that relate fast food supply to health, although, Svastisalee et al. (2012) and Gebremariam et al. (2012) study the relationship between (fast) food outlets and children's diets in Denmark and Norway, respectively. We believe it is particularly relevant to study the Scandinavian population as it can be thought of as a lower bound for the effects we might expect in other developed countries where health capital and healthcare access are at lower levels.

et al. (1976); Te Velde et al. (2003); Fall (2011) all document that nutritional deficiencies during the prenatal period – often manifesting through lower birth weight (LBW) – lead to obesity problems later in life. We therefore also examine the interaction between birth weight and access to fast food. In an intergenerational context, it could be the case that propensity for obesity is genetically (Comuzzie and Allison, 1998; Rankinen et al., 2006) or socially driven. For example, Stoklosa et al. (2018) document that "impatient time preferences" and the present bias of parents are associated with both their own and their children's increased obesity. Datar et al. (2022) further show that in the US, exposure to counties with higher obesity rates increases the likelihood of obesity among less patient, but not among the more patient, adolescents. Although we cannot measure the time preferences of either children or parents, we observe complete information about their BMI at age 19 for both generations. We can thus document intergenerational elasticity in obesity (Classen, 2010; Classen and Thompson, 2016) and investigate whether access to fast food moderates this association.

We consider our results to have three main policy implications. First, despite relatively high levels of human and health capital as well as a more accessible and equitable healthcare system in Scandinavian societies compared with the rest of the developed world, we provide evidence that even in such a setting, the supply of fast food could lead to increased BMI. This is concerning given that many studies on weight reduction find small or no effects (see e.g., Franz et al. (2007) and Dombrowski et al. (2014) for meta-analyses) and that the increasing penetration of unhealthy food providers is increasing (e.g., in Norway between 1980 and 2007, we observed a five-fold increase in the number of fast food restaurants). Because of these factors some predictions suggest that within a decade more than 30% of Norwegian adult male population could be obese which increases the need for effective public health interventions reducing the obesogenic environment (Lobstein et al., 2022). Furthermore, obesity and higher BMI have been linked to increased healthcare costs (Allison et al., 1999), lower educational achievement (Black et al., 2015), and worse labor market outcomes (Lundborg et al., 2014), thus imposing a direct burden on a country's healthcare system and workforce affecting the whole society as well as potentially increasing inequality. Second, we document that fast food may not only affect health but also cognition, thereby increasing the stakes of a potential lack of counter-measures or regulation of such establishments. This extends the literature on negative consequences to sugar-rich diet (Gracner and Gertler, 2019) to fast food consumption. Finally, except for the age at exposure differences, the homogeneity of our treatment effects suggests that any interventions or campaigns should target a broad population rather than specific groups e.g., those with a history of obesity in their families (Griffith, 2022).

2.2 Data

We use Norwegian administrative data on individuals and firms, which allows us to link information on the opening of fast food restaurants with individual-level data on health and cognitive outcomes. The data allow us to track individuals over time and space and facilitate a host of heterogeneity analyses and robustness checks.

2.2.1 Firm Data

The data on firms come from the Norwegian Register of Business Enterprises and include the exact address, opening year, and if applicable the year of closing of all enterprises in Norway. We include the opening and closing-down of businesses between 1980 and 2007, and select establishments based on industry codes.[8] A fast food restaurant is defined as a businesses specializing in serving prepared processed food using counter service at any time of the day (code 56.102). This includes traditional Norwegian fast food providers (e.g., sausage stands), Western-style fast food restaurants (e.g., McDonald's), as well as independent kebab, hamburger, and pizza stands, most of which were established after 2000. The first Western-style fast food restaurants opened in the early 1980s in Oslo and marked the arrival of a new food concept. Since then, their number has expanded dramatically and today there are more than 300 of them.[9] A separate code (56.101) is used for full-service restaurants that offer seating options. Since some fast food restaurants might be classified under this code, we also use it to define treatment. However, to reduce the likelihood of including non-fast food establishments, we only extract the opening and closing-down dates of restaurants linked by name to fast food chains.[10]

Figure 2.1:A presents the evolution of the fast food market, as defined by the above-listed industry codes, in Norway between 1980 and 2007. In total, our data set includes the openings of 1,074 and closing-downs of 173 fast food establishments; and each decade we consider is characterized by an increase in new suppliers.[11] For example, in the decade prior to the first births included in our sample (1970–1979), there were 34 new fast food restaurants opened, while in the last decade we consider (2001–2010), there were 492 new fast food restaurants opened. Importantly for our identification, the location of fast food restaurants is not uniform across the country and over time. Figure A1 presents Norwegian municipalities with at least one operational fast food supplier in different decades. Since as noted above in most cases a

[8] The system of industry codes is tied to the European Industry Classification System (NACE) and groups industries into five-digit numerical codes. The first four digits are the same across all European countries while the fifth is specific to Norwegian legislation and distinguishes firms according to their most important activity.

[9] The biggest fast food chains in Norway include Burger King, McDonald's, Big Bite, Pizza Hut, Peppes Pizza, Dolly Dimple's, and Subway. In 2000, the market share of McDonald's when it comes to fast food restaurants was 33 percent and they operated 52 establishments.

[10] Other small-scale fast food restaurants could be coded with the industry code for traditional restaurants (56.101) or with the industry code for pubs (56.301). We identify these potentially single-site fast food suppliers by extracting all operations including the words "pizza", "hamburger", and "kebab" from the business registry and then manually checking that these establishments were indeed fast food restaurants. Our results are substantively unchanged regardless of whether we include these single operating firms or not, as documented in Table 2.4.

[11] Since in the vast majority of cases a restaurant that closes-down in a specific postcode is almost immediately replaced by another similar outlet throughout the paper we only refer to "restaurant openings" as a shorthand. Our results are unchanged if we exclude all closings from coding of the treatment variable.

restaurant that closes down is replaced by another fast food establishment, we do not observe a situation in any of the municipalities where there is a restaurant in an earlier decade but not in a later one.

Given prior research (see, e.g., Zeng et al., 2019b,a), it is important to differentiate fast food restaurants from other processed food providers such as supermarkets, grocery stores, convenience stores, and gas stations. Thus, we geocode their locations as well as openings and closing-downs, and use these additional variables as controls in our preferred specification. Convenience stores are defined based on industry code 41.112, gas stations based on industry code 47.300, and grocery stores and supermarkets are based on industry code 41.111. The number of convenience stores in Norway increased from 82 in 1980 to 328 in 2010. Equivalent numbers for grocery stores were 1,040 and 1,591, respectively.

2.2.2 Individual Level Data

We draw on information from multiple interconnected databases containing individual-level records. The central population register contains annual data on the place of residence (including postcode) as well as the municipality and postcode of birth, which allows us to assign exposure to fast food restaurants from birth to age 18. Since the major increase in fast food supply takes place after 1980, we limit the sample to individuals born in 1980–1989. We exclude individuals born outside of Norway or those who migrate out of the country since we are interested in cumulative exposure from birth to the individual's health assessment at age 18–19. We do not make any further sample restrictions when it comes to individual health or demographic characteristics.[12]

We assign individuals to fast food restaurant exposure according to their geographic proximity with the idea that closer distance increases accessibility and consumption through lower monetary and non-monetary costs (Moore et al., 2009). We measure the distance as the crow flies between the centroid of the individual's postcode of birth and the exact coordinates of the restaurant. We define treatment at the postcode of birth – rather than contemporaneous postcode of residence – to avoid any issues related to endogenous migration possibly correlated with the changing landscape of fast food supply.[13] Since the average radius of a postcode in Norway is around 400 meters, we use a distance of less than 500 meters (0.5 kilometer (km)) as

[12] Prior research assigned fast food exposure either at home or school location level. In Norway, a large majority of children attend local primary and middle schools that are assigned based on strict zoning regulations tied to a home address. For this reason, there is little difference between exposure at place of residence and at place of schooling prior to high school. Furthermore, even at the high school level, children in most municipalities attend local school. Nevertheless, there is a degree of school choice or grade-based assignments is larger cities. We therefore use the education registry to code exposure to fast food restaurants at high school and use this variable as an additional control in select regressions.

[13] We are not concerned that people migrate to be closer to fast food restaurants but rather that people in Norway tend to move into cities and areas with increased economic activity, and this is exactly where the fast food restaurants are more likely to be located. Thus, we prefer using the postcode of birth as a more conservative measure. In Section 2.4.2, we also present robustness checks where we limit the sample to non-movers.

our most conservative exposure measure. However, we also consider exposures at distances of less than 1,000 meters (1 km) and 2,000 meters (2 km) in the main set of results. We construct our treatment variable of interest, for a given geographical proximity cutoff, as the number of years of exposure to a fast food restaurant, taking values from 0 for never exposed individuals to 19 for those always exposed. Since we do not observe actual consumption, our estimates should be interpreted as reduced-form intent-to-treat effects.

Most of our outcome variables come from Norwegian military records, which include weight, height, and cognitive ability assessments for the universe of males. Since military service is only mandatory for males, we necessarily exclude females from the analysis.[14] Before conscripts start their service, their medical and cognitive suitability is screened at around the age of 18. In our estimation sample, 73 percent of males are assessed when they turn 18 years, 25 percent at the age of 19 years, 1 percent at the age of 20 years, and the remaining 2 percent at other ages. Since this examination is compulsory for all men, there is no selection on fitness or ability in the data, a concern that would arise when using data from countries where military service is voluntary, e.g., the US.

Our empirical sample includes males born between 1980 and 1989 for whom the outcomes were measured between 1998 and 2007. In the main analysis, we focus on two health outcomes: log BMI and the probability of being overweight, i.e., having a BMI of 25 or more. We multiply these variables by 100 in order to avoid rounding issues when displaying coefficients. In addition, we analyze the effect on cognitive ability, which is measured as a mean score from three IQ tests: arithmetic, word similarities, and figures. We standardize this score by cohort to have a mean of zero and standard deviation of one hundred. To complement the data on cognitive ability, we also investigate an earlier educational outcome – the probability of being enrolled in an academic track in high school. This information comes from a separate data source – the education registry – and is a good proxy for future earnings. Yearly lifetime earnings for men born between 1967 and 1989 are on average 366,088 NOK for those who graduated from an academic track and 331,074 for those who graduated from a vocational track, implying at least a 10% premium for the academic track.[15] In our data, about 98% of students enroll in the first year of high school: 50% enroll in an academic track, 45% enroll in a vocational track, 3% in alternative training plans, and 2% drop out after compulsory education. Among those who continue into high school, 97% enroll the year they turn 16 while 3% enroll at other ages.[16] When analyzing this outcome, we code exposure to fast food restaurants

[14]Women are allowed to enroll in the Norwegian military, and for women who do, we have the same information as for men. However, with only a 2 percent participation rate among women during our sample period, it is clearly a selected group.

[15]Note that only about 80% of students who initially enroll in an academic track graduate, while graduation rates for vocational programs are even lower.

[16]Students start high school on-time the year they turn 16, and usually finish within 3 years, but have the right to apply to high school until the year they turn 24. The education registry also has information on school grades, however, this data is only available for cohorts born after 1986 (or for about 40% of our

between ages 0 and 16 (rather than 0 and 19 for the outcomes based on military registry).

Family identifiers in the data allow us to link children with their parents, and provide information on family structure and socioeconomic background, including labor force participation, earnings, as well as the education of both mothers and fathers.[17] The military records of fathers, another reason for focusing on children born after 1980, further allow for a novel analysis where we use the father's BMI as an intergenerational proxy for offspring predispositions, either genetic or environmental, to be overweight. For example, the likelihood that we observe an overweight son is 43% when the father is also overweight compared with just 21% when the father is not. Likewise, intergenerational elasticity in BMI is 0.4, meaning that a 10% increase in the father's BMI increases the son's BMI by about 4% (see Appendix Table A1). This association, although not causal, is almost invariant to including a rich set of controls for both generations, suggesting that parental BMI might meaningfully mediate the effects of access to fast food restaurants.[18] Mean probabilities of being overweight for fathers and sons, presented in this table, further illustrate the policy relevance of the obesity epidemic with the younger generation having three times higher likelihood of being overweight compared to their parent's generation.

2.2.3 Descriptive Statistics

We first present descriptive evidence on the expansion of fast food restaurants and parallel changes in BMI as well as cognitive ability. We limit the data to individuals born between 1980 and 1989 in postcodes that at some point had a fast food restaurant within a 30 km radius of their centroid – our primary empirical sample. The left-hand axis of Figure 2.1:B documents the increase in BMI (dashed line) and the right-hand axis the decline in cognitive ability (dotted line) of Norwegian males. The mean BMI increased from 22.5 for the 1980 birth cohort to 23.4 for the 1989 birth cohort, equivalent to 4 percent. These modest differences in mean BMI over time mask more substantial changes in the shape of the upper-tail of the BMI distribution. For example, over the same period of time, the likelihood of being overweight (BMI $>$ 25) increased from 14 to 18%, or by about 30%, while the likelihood of being obese (BMI $>$ 30) increased from 4 to 7%, or by about 75%. These changes in the tail of the distribution are depicted in Appendix Figure A2, which confirms an outward shift in the

sample). For this reason we choose to study academic track enrollment rather than grades.

[17]In principle, the data structure allows us to use sibling fixed effects as an alternative identification strategy. We do not focus on this type of analysis, however, because families with two male siblings born between 1980 and 1989 represent only a small sub-sample (14% of the sample) that is highly positively selected since wealthier families are more likely to have two or more children. Furthermore, there is limited within-family variation in the treatment variable remaining after including mother-fixed effects, subjecting this strategy to selection-into-identification issues (Miller et al., 2022).

[18]Classen (2010) finds intergenerational BMI elasticities in the US of about 0.35, and while investigating multiple countries, both Classen and Thompson (2016) and Dolton and Xiao (2017) find BMI elasticities of about 0.2. One reason why our elasticity is higher could be that we rely on complete administrative data rather than survey information, which tend to suffer from selection and measurement error biasing the associations towards zero.

upper-tail of BMI among the more recent birth cohorts. Notably, despite these meaningful increases over time, the mean BMI of those born at the end of our sample in 1989, at 23.4, is still somewhat lower than the numbers reported in previous research based on US data (Davis and Carpenter, 2009; Anderson and Matsa, 2011).

Cognitive ability exhibits a similar qualitative pattern with average raw scores declining from 5.14 for the 1980 birth cohort to 4.86 for the 1989 birth cohort, or by more than 5%. Declining cognitive ability in recent decades is not only a Norwegian phenomenon and has been documented in multiple other countries (Dutton et al., 2016), but the exact reasons for this drop are unclear. At the same time, prior research suggest a link between obesity and intelligence (Yu et al., 2010; Belsky et al., 2013). To the extent that increased access to fast food restaurants increases BMI, it could also affect the cognitive ability of these individuals and ultimately play a role in the observed aggregate trends. When discussing the main results, we descriptively explore to what extent the drop in cognitive ability is mediated by the increase in BMI.

Comparing panels A and B of Figure 2.1 suggests a positive (negative) relationship between fast food restaurants and BMI (cognitive ability) In 1980 there were 156 fast food restaurants, in 1989 there were 229, while in 2007 there were 769. Thus, the last exposure year we consider represents approximately a five-fold and 3.5-fold increase in supply compared to 1980 and 1989 levels, respectively. At the same time, BMI of exposed cohorts increased 4% while cognitive ability declined by 5%. The main goal of our paper is to investigate to what extent these time series relationships reflect causality.

To further contextualize our setting, Table 2.1 provides individual-level summary statistics of the various outcome and control variables. Column (i) provides information on the full population of Norwegian males while column (ii) limits the sample to males who at any time have been exposed to a fast food restaurant within 30 km of their place of birth. We consider the sample in column (ii) as our baseline population of interest, which we then divide into treatment and control groups depending on the proximity to a fast food outlet. Almost 90% of Norwegian males born between 1980 and 1989 have at some point (between ages 0 and 19) been exposed to at least one fast food restaurant within 30 km of their place of birth, and this high exposure rate stems from the fact that the majority of the Norwegian population lives in urban agglomerations, which is exactly where fast food restaurants tend to locate. Despite this, there are no meaningful demographic differences (panel C of Table 2.1) between the samples in columns (i) and (ii), which should increase the external validity of our estimates.

The subsequent columns in Table 2.1 divide the estimation sample into two subgroups: males who at some point had a fast food restaurant within 2 km of their place of birth (column (ii)) and those who never had access at such a close proximity (column (iv)). The former group have somewhat better educated and richer parents, on average, which makes sense given that more affluent people in Norway tend to cluster closer to the urban core – exactly where the fast food restaurants tend to locate. At the same time, we do not observe any striking differences in birth order or parental age at birth. Somewhat surprisingly, we find that people residing closer to fast food restaurants appear to be healthier and have higher cognitive scores compared to those residing further away, which contradicts the time-series evidence presented in Figure 2.1. On the other hand, this might simply reflect sorting along the socioeconomic dimensions documented in panel C, with the children of more affluent parents having more favorable outcomes. These somewhat contradictory descriptive patterns are therefore likely to reflect the endogeneity of both family and firm choices with respect to geographical location, and motivate the need for our quasi-experimental design.

2.3 Empirical Approach

We are interested in estimating the effect of access to fast food restaurants on the health and ability of young Norwegian males. To overcome the potential endogeneity issues discussed above, we utilize a two-way fixed effects identification strategy.[19] As such, we exploit the quasi-random variation in the openings of fast food restaurants across different narrow geographical locations in Norway and over time. We estimate the following reduced-form equation:

$$Y_{ipt} = \alpha + \gamma \text{Years of exposure}_{pt} + \beta X_{ipt} + \lambda_p + \theta_t + \varepsilon_{ipt} \tag{2.1}$$

where Y_{ipt} are the outcomes of interest for individual i born in postcode p in year t; Years of exposure$_{pt}$ measures the number of years an individual would have been exposed to a fast food restaurant based on their postcode of birth; X_{ipt} is a set of individual and family controls (mother's education, mother's age and marital status at birth, father's education, father's age at child's birth, the individual's birth order) as well as postcode-specific characteristics (number of years of access to a supermarket or a grocery store and number of years of access to a convenience store); λ is a set of postcode fixed effects to control for time invariant location characteristics; and θ is a set of birth cohort fixed effects to control for common time specific shocks.[20] In our baseline empirical sample we include all individuals in a radius of 30 km from a fast food restaurant based on the centroid of their postcode of birth.[21] We cluster the standard errors at the municipality of birth.[22]

[19]Several previous papers have employed an instrumental variable strategy using the distance to the nearest highway (from the place of residence or the school) as an instrument for access to fast food. In the context of Norway, where the highway network is very limited and only a small percentage of fast food restaurants are located close to major highways, this estimation strategy cannot be used.

[20]Postcode-specific characteristics included in X_{ipt} are measured at the same proximity as Years of exposure$_{pt}$. Thus, if we define treatment at a 1 km radius, we code it in the same way for both fast food restaurants as well as other food suppliers included in X_{ipt}. In the heterogeneity analysis presented in Section 2.4.3 we exclude X_{ipt}.

[21]We test both the sensitivity of our choice of proximity to a fast food restaurant (treatment group definition) in Figure A4 and choice of radius for inclusion in empirical sample in Figure A5.

[22]Although larger than a postcode, municipalities are the smallest governmental units in Norway and they are largely responsible for the planning of local business developments. We cluster standard errors at

The coefficient of interest, γ, is the per year effect of proximity to fast food restaurant on the outcome variables of interest. It is identified by variation in the time and location of restaurants openings under two assumptions. First, that these events are not perfectly correlated with other unobserved determinants of health and cognition. To ensure this, we have studied a variety of reforms and laws implemented over the time period in question that may have affected our outcomes, but we did not find that these are correlated with fast food restaurant openings.[23] Second, we assume that the treated locations would have had the same health and cognitive outcomes as nearby control locations in the absence of the treatment. In Section 2.4.1, we provide suggestive evidence that this parallel trends assumption is likely to hold.

Under these two assumptions, γ can be interpreted as a causal intention-to-treat (ITT) effect of proximity to a fast food restaurant in the postcode of birth. First, the treatment is defined for all individuals born in a specific year and location, but not all of these individuals would have regularly dined at these restaurants. In our data, we cannot observe individual-level consumption, which would have allowed us to compute the treatment-on-the-treated effects of fast food. Therefore, our reduced-form estimates should be viewed through the lens of easier access to fast food rather than its direct consumption. Nonetheless, we believe that there is a strong first-stage relationship between the presence of restaurants and the demand for fast food since otherwise these suppliers would go out of business. In a stark contrast, the profits of fast food restaurants in Norway have been increasing over time (Moe, 2019; Foss, 2011). Furthermore, prior literature supports this assumption (Moore et al., 2009; Svastisalee et al., 2012). Second, to avoid endogenous sorting on factors correlated with the treatment, we assign the treatment at the time and place of birth rather than using a contemporaneous place of residence, which could potentially be endogenous.

We also estimate an event study model to verify the parallel trends assumption:

$$Y_{ipt} = \alpha + \sum_{s=-7, s\neq -1}^{20} \gamma_s 1(y - F_p^* = s) + \lambda_p + \theta_t + \varepsilon_{ipt} \tag{2.2}$$

these larger units to allow for potential correlation across postcodes within a municipality. There are 430 municipalities in our empirical sample. Our conclusions are unchanged if we cluster at the postcode level.

[23] These reforms include extensions of the maternity leave from 18 to 24 weeks in three stages for individuals born after May 1, 1987, June 1, 1988, and April 1 1989; a school choice reform in Oslo in 1997 – affecting cohorts born after 1981 in Oslo; and childcare subsidies for low income households. The maternity leave reforms had no effect on children's school outcomes (Dahl et al., 2016) or mothers' health outcomes (Bütikofer et al., 2021) and there is no difference in proximity to fast food restaurants among families that were treated or not by these changes. The school choice reform had no effects on student outcomes but lowered some house prices (Machin and Salvanes, 2016). In one of the robustness checks we directly control for exposure to this reform and our results remain unchanged. Families of children born in the later half of our sample had access to childcare subsidies if their household income was below a specific value; an intervention that was shown to increase student performance (Black et al., 2014). Since fast food restaurants are more likely to open in more affluent postcodes (Table 2.1) thus, if anything, this reform could bias our estimates towards zero. Given the similarly of estimate with and without control variables, however, we are not concerned that this is a major confounder.

where Y_{ipt} are the outcomes of interest for individual i born in postcode p in year t. y is the year when an individual undergoes the military service screening and thus when our health and cognitive ability outcomes are measured. In this equation s indicates the time periods relative to exposure, while the regressors of interest are dummy variables defined by $1(y - F_p^* = s)$. These dummy variables take on a value of one for each event time s. Our reference period, $s = -1$, is an opening of a fast food restaurant in close proximity to the postcode of birth one year after an individual underwent the military screening. The event plots omit $s = -7$ and $s = 20$, which are binned endpoints.[24] Event times $-6 \leq s \leq -2$ denote pre-trends relative to $s = -1$ while event times $0 \leq s \leq 19$ are treatment effects where greater values imply both longer exposure and at an earlier age. Thus, one caveat with the interpretation of our results is that we cannot differentiate between time of exposure and length of exposure as is common with studies relying on cohort variation. Spatially, akin to preferred specification based on Equation 2.1, we define treated individuals as those whose centroid of postcode of birth is within 2 km of a fast food restaurant while individuals at a distance of 2–30 km are considered a control group.[25]

Before presenting the main results we first investigate to what extent the location of fast food restaurants is correlated with observable characteristics. We already know based on the discussion of Table 2.1 in Section **??** that there is a degree of spatial sorting and that more affluent individuals are more likely to live in a close proximity to fast food suppliers, likely because they can afford housing in the urban core. In Table 2.2 we formalize this conjecture through a regression analysis. In particular we correlated pre-determined post-code characteristics (columns (ii) and (iii)) or changes in these pre-determined characteristics (column (iv) and (v)) with indicators for post-code centroid being within 2 km of a fast food restaurant opening before 1980 (columns (ii) and (iv)) or before 1985 (columns (iii) and (v)). First and foremost, critical for our identification strategy, fast food restaurants do not appear to locate in either places where local population has higher BMI or in places where BMI has been increasing prior to the opening. In other words, the treatment does not appear to be correlated

[24]Our results are invariant to not binning the end points and reporting estimates for $-6 \leq s \leq 19$. When we use academic track as an outcome the highest value of s we consider is 16 since this is the age at which children decide on their high school tracks. We do not include control variables in the even study for ease of interpretation and to be in line with recent recommendations in the literature. The results are almost identical if we include the vector of controls use in Equation 2.1.

[25]Our treatment of interest is the number of years exposed to a fast food restaurant at close proximity which is a discrete variable taking values between 0 and 19. It represents cumulative exposure to fast food from birth until outcomes measurement which is similar to e.g., Hollingsworth et al. (2022) who study effects of cumulative exposure to lead by specific grade on test scores. Given the structure of our treatment variable, we are unable to implement any of the modern difference-in-differences designs proposed by Borusyak and Jaravel (2017); De Chaisemartin and d'Haultfoeuille (2020); Goodman-Bacon (2021); Callaway and SantAnna (2021); Sun and Abraham (2021); Athey and Imbens (2022), as these papers require binary treatment variables which take value of one in the first year of treatment and all subsequent years. Since our outcomes are measured at a single point in time (at ages 18/19 or at age 16) while our exposure/treatment is multiyear we cannot easily convert our setting to a single binary treatment variable. When we use a binary treatment variable – defined as any exposure year between ages 0 to 19 – the estimates become less precise and we lose statistical significance in select specifications.

with lagged values of one of our outcomes of interest hinting that parallel trends assumption is likely to hold (as we confirm in Section 2.4.1). Second, this analysis confirms aforementioned anecdotal evidence that fast food retailers in Norway tend to concentrate in cities and the urban core – we find positive and statistically significant associations with population, age, and education. Third, the imbalances to the extent they exist, appear quantitatively small. Even the larger coefficients from column (iii), imply sorting of between 0.001% (statistically insignificant for BMI) to 2.6% (statistically insignificant for rural indicator); while the statistically significant coefficients are in the rage of 0.02% to 1.1%. Overall, we conclude that net of the known factors determining location of food suppliers (or retailers more broadly) there is limited evidence for sorting that could invalidate our quasi-experimental two-way fixed effects design. Since the balance is not perfect, however, in Sections 2.4.1 and 2.4.2 we further probe the sensitivity of our results.

2.4 Results

2.4.1 Main Results

Table 2.3 presents estimates of γ from Equation 2.1 for a 0.5 km (columns (i) and (iv)), 1 km (columns (ii) and (v)) and 2 km (columns (iii) and (vi)) radii around the closest fast food restaurant (approximately 0.31, 0.62, and 1.24 miles, respectively). The treatment variable – the number of years exposed to a fast food restaurant based on post-code and time of birth – varies from 0 (for never exposed) to 19 (for always exposed). The coefficients therefore represent the effect of an additional year of exposure to a fast food restaurant on log BMI and the probability of being overweight (panel A) as well as ability and probability of being in an academic track in high school (panel B).[26]

Focusing on the health outcomes first, we find that a close proximity to fast food restaurants increases both BMI and the probability of being overweight at ages 18–19. The point estimates increase somewhat when we expand the radius but are generally statistically indistinguishable from each other and all are statistically significant at conventional levels. The point estimate of 0.198 implies that an additional year of exposure increases BMI by approximately 0.2%. Given that the average exposure in the data is slightly over 7 years, this coefficient translates to an effect size of 1.5%.[27] We find larger effects for the upper-tail outcome, the probability of being overweight, at 1.6% per year relative to the pre-treatment sample mean; or over 11% increase for the average number of years individuals are exposed in our sample. These effect sizes are meaningful given that the average BMI and overweight rate in our last cohort considered (1989) were 4 and 30% higher compared with the 1980 co-

[26] As noted above our treatment variable varies from 0 (for never exposed) to 16 (for always exposed) when we consider high school academic track as an outcome.

[27] We include never-treated individuals in this calculation. Conditional on ever being exposed to a fast food restaurant at 2 km before age 19, the average number of years of exposure is 13.5 which would yield an effect size of about 2.7%.

hort, respectively. Thus, using this back-of-the-envelope calculation, it appears that we can attribute a non-trivial fraction of the increase in weight observed across cohorts in Norway to exposure to fast food restaurants.

Our estimates confirm prior findings from the US on the negative health consequences of exposure to fast food restaurants at either place of residence (Qian et al., 2017) or school (Currie et al., 2010), while extending them to Norway – a country with not only substantially lower obesity rates but also lower fast food access and socioeconomic inequalities. Qian et al. (2017) finds that an additional fast-food restaurant within half a mile of a child's residence increases their BMI z-score by 7.9% of a standard deviation (SD). This is very similar to our estimates which, reflected in standard deviations, as presented in Table A2, would imply a 1.3% of a SD effect per year, or 9.4% of a SD for the average exposure time in our sample. Most other US studies focus on exposure at school rather than at the place of residence. For example, Currie et al. (2010) find that a fast food restaurant within 0.1 mile of middle school increases the likelihood of a child being obese by at least 5.2%. In our setting, we chose to investigate the likelihood of being overweight rather than obese because our population is healthier, and even the former rate of 22% is smaller than the average probability of being obese in the California data at almost 33%. When we estimate the effects on obesity rate, at 2 km radius, our effect size is 2.9% higher probability per year of exposure (Table A2). Likewise, Davis and Carpenter (2009) find that Californian students whose schools are within 0.5 miles of a fast food restaurant have increased probabilities of being overweight and obese by 6 and 7%, respectively. In Arkansas, Alviola IV et al. (2014) find an increase in obesity rates of 5.8% per additional restaurant within a 1 mile radius of a school.[28] Overall, we view our results as largely consistent with some prior work from the US, one that found adverse health effects of exposure to fast food restaurants, despite a very different institutional, economic, and cultural setting.

Prior research suggests that proper nutrition could affect education and test scores (see, e.g., **?**Bütikofer et al., 2018; Gracner and Gertler, 2019), while epidemiological studies attempted to directly link obesity and intelligence (see, e.g., Yu et al., 2010; Belsky et al., 2013). Thus, going beyond health outcomes, we also investigate whether access to fast food restaurants affects cognitive ability. The first three columns in panel B of Table 2.3 suggest reductions in cognitive ability of up to 0.56 percent of a standard deviation per one additional year of exposure to a fast food restaurant, or 0.041 SD for the average number of years of exposure. Strikingly, these estimates are very close to the effects of contracting with healthy meal vendors in California where children's test scores increased by 0.03 to 0.04 SD (**?**).[29]

[28]Since our postcodes are quite small there is rarely more than one fast food restaurant per postcode. In fact, only 6% the post codes in our sample have more than one fast food vendor. Thus, in Norway, we cannot analyze intensive margin treatment akin to Alviola IV et al. (2014).

[29]Considering unstandardized scores as an outcome (Table A2) we find a point estimate of -0.010 which based on pre-treament mean yields an effect size of 0.2% per year or 1.45% for an average exposure. Since the decline in cognitive ability between 1980 and 1989 birth cohorts is 0.34 points or 6.6%, our estimate could

We likewise find negative effects on the probability of academic track enrollment. The point estimate for our preferred radius of 2 km implies a reduction in academic track enrollment of 0.5% per each year of exposure, or 3.0% for the average exposure. We view these cognitive and schooling effects as quantitatively meaningful and as important from a policy perspective since they show that an increased supply of fast food could also lead to lower cognition beyond its negative health effects.[30]

The validity and interpretation of the aforementioned effects critically depends on both parallel trends and no correlated shocks assumptions. In Figure 2.2 we therefore provide event study estimates based on Equation 2.2 where we define treated group using the 2 km cutoff. The top row of this figure focuses on health outcomes (panels A and B) while the bottom row presents event studies for cognitive ability (panel C) and high school academic track attendance (panel D). Irrespective of the outcome, we do not find any evidence of pre-trends. Out of the 20 estimated pre-treatment coefficients none are statistically significant at conventional level and for no outcome we can detect statistically significant pre-trend. At the same time, for the health outcomes we find clearly increasing treatment effects in the post-periods implying both higher BMI and increased probability of being overweight. Both effects grow in the length (and age) of exposure. The cognitive effects are somewhat more muted, albeit we still estimate statistically significant post-opening treatment effects that are consistent with our results presented in Table 2.3. Importantly, from the perspective of our heterogeneity analysis, results in panels C and D suggest that cognitive and schooling effects are concentrated among those who were exposed for a longer period of time and at younger ages.

2.4.2 Robustness Checks

The results presented in Table 2.3 imply that boys growing up in close proximity to fast food restaurants have a higher BMI and lower cognitive ability in young adulthood. In Figure 2.2 we have shown that these effects are not driven by differential pre-trends: our main testable identifying assumption. In this section, we present a multitude of additional robustness checks. In particular, we ensure that our results are unaffected by the choice of estimation sample, definition of treatment, econometric specification, or transformation of dependent variable. We further present results from randomization inference test.

First, Table 2.4 presents a variety of alternative specifications for our preferred exposure radius of 2 km, with each outcome in a separate panel. Column (i) replicates our main results from Table 2.3 to ease the comparisons. Column (ii) drops all control variables and estimates

account for about 22% of this decline.

[30] Controlling for log BMI in Table A3 does not affect the effects on ability or probability of attending an academic high school track despite a negative and statistically significant association between log BMI and either of these outcomes. Coefficients on log BMI are -0.020 (p-value < 0.001) and -0.392 (p-value < 0.001) when we consider academic track probability and cognitive ability scores as outcomes, respectively. This means that the effects on cognitive outcomes are largely orthogonal (and plausibly additive) to any effects that could have operated through access to fast food restaurants increasing BMI.

the model with only postcode and cohort fixed effects. To the extent that our variation is quasi-random, the inclusion of additional controls should not substantially affect the results. Column (iii) excludes Oslo, the capital, which is a major population and commerce center. In fact, almost 10% of the population resided in the city during the 1980s, while 18% of fast food restaurant openings in our sample are observed in Oslo. Column (iv) addresses a concern that regional trends simultaneously driving the supply of fast food restaurants, unhealthy behaviors, and economic activity could also be driving the observed increases in BMI. We do this by controlling for linear municipality-specific time trends. Column (v) addresses the issue related to the measurement of BMI and cognitive ability at age 18 for 73% of the sample and age 19 for 25% of the sample, and that both outcomes might change with age, by controlling for age at measurement.[31] Column (vi) directly addresses the concern of reforms that might be co-timed with the expansion in the supply of fast food restaurants discussed in Section 2.3. At the at individual level, we therefore control for the two aforementioned reforms: (a) maternity leave and (b) increased school choice within the municipality of Oslo.[32] In column (vii), we control for exposure at high school level, which for those students who are able to choose which school they attend, could be different from exposure at their place of residence. Finally, in column (viii), we exclude single site restaurants which we have coded based on searching from phrases such as “pizza", “hamburger" or “kebab" among establishments with industry codes 56.101 and 56.301. Since this was done manually, our concern here is that those outlets could thus have more measurement error. Irrespective of the exact permutation, the coefficients remain largely unchanged both in terms of magnitude and statistical significance, thus supporting the robustness of our preferred specification.

Second, we conduct a randomization inference test where we randomly allocate fast food restaurant openings from our sample at postcode and cohort level. We repeat this exercise 1,000 times for each outcome and plot the resulting coefficient distribution together with our preferred estimates at a 2 km radius from Table 2.3. This exercise addresses both the possibility of spurious correlations and provides empirically driven alternative p-values for exact sharp nulls (Young, 2019). Figure A3 presents these results for our four outcomes of interest. Here, the vertical black line denotes the preferred coefficients from Table 2.3, the gray shaded area depicts the 95% confidence intervals around these coefficients, and the orange areas present the distributions from the estimates when we randomly assign exposure to fast food restaurants. In all cases, the placebo distributions are bell-shaped and centered around zero as expected if there was indeed no sorting or spurious correlations. Furthermore,

[31] When considering academic track as an outcome we control for age at which students enter high school since 3% do not enter immediately following compulsory education at age 16.

[32] We do not have information about the low-income childcare subsidies at individual level, but address this by controlling for parental income. Comparing columns (i) and (ii), with and without additional controls (including parental income), suggests that these do not materially affect our results; if anything the results with controls are on the conservative side.

the black vertical lines are always outside of the simulated distributions implying empirical p-values of less than 0.001. Taking a more conservative view, for BMI, the probability of being overweight, and cognitive ability, these distributions do not even overlap with 95% confidence intervals of our preferred estimates.

Third, we experimented with distance definitions of both the treatment group and the inclusion in the empirical sample. Our preferred estimates define treatment at 2 km proximity to a fast food restaurant, and we include all individuals between 2 and 30 km from the restaurant as a control group. In Table 2.3, we have already documented that results are not sensitive to smaller radii. In Figure A4, we further document that they are stable when we increase the radius up to about 5 to 8 km, but at larger distances, the effect fades out and becomes statistical insignificant. This is consistent with the declining likelihood of (frequently) using a restaurant that is father away from one's place of residence. Thus, we posit that distance and consumption are inversely related, which would give us a pattern of results depicted in Figure A4. Another concern is that we include either too few or too many individuals in our empirical sample, which is then divided into treatment and control groups. In Figure A5, we present the results where we vary the inclusion cutoff from 5 to 100 km while keeping the definition of the treatment group at 2 km radius from a fast food outlet.[33] To the extent that individuals beyond a 30 km radius should never be affected by any openings we consider when defining treatment, our results should theoretically not change, and this is precisely what the estimates in Figure A5 imply.

Fourth, there could be a concern that our results are downward biased due to spillovers. It is clear that access to fast food restaurants does not change discretely at a 2 km radius and thus, individuals included in the control group – e.g., between a 2 and 3 km – are likely also treated to a certain degree. To address this issue we re-estimated our main results while defining the treatment group as in Table 2.3 at 0–2 km, and the control group as those between 5 and 30 km, and dropping individuals in the "donut" between the treatment and control group cutoffs. Panel B of Table A4 presents these results, which are substantively unchanged, ensuring that our main results are not downward biased due to meaningful spatial spillovers. This is also consistent with evidence from Figure A4 where we observe relatively constant effects up to the 5 km definition of treatment.

Fifth, we verify that our results are not driven by how we define the outcome variables measuring health and cognition. Appendix Table A2 presents results for alternative dependent variables: BMI (column (i)), BMI z-score (column (ii)), probability of being obese (column (iii)), weight in kilograms (column (iv)), height in centimeters (column (v)), and raw cognitive ability score (column (vi)). These are in contrast to using log BMI, probability of being overweight, and standardized cognitive ability score in Table 2.3. Irrespective of the

[33]This means that at 5 km the treatment group is 0–2 km and control group is 2–5 km, while at 100 km, the treatment group is 0–2 km and control group is 2–100 km.

outcome, we always find that access to fast food restaurants positively affect proxies for increased weight. Column (v) further shows that the BMI effects are not moderated by parallel increases in height. Likewise, we find negative effects on unstandardized measure of cognitive ability. Thus, we conclude that our results are not sensitive to how we proxy for lower health and cognitive ability.

Sixth, we assign treatment based on postcode of birth, which we view as the most exogenous location from the perspective of potentially treated children. This renders our estimates as an intend-to-treat effect, but removes any potential endogeneity due to migration. As noted before, we are not concerned with households sorting based on access to fast food but rather with sorting on characteristics correlated with access to fast food such as urbanicity. To ensure that our results are not driven by this choice, in panel C of Table A4 we focus on non-movers only. In particular, we restrict the sample to individuals with the same postcode between (1) birth and age 6 (first panel), (2) birth and age 12 (second panel), as well as (3) birth and age 18 (third panel), which is approximately when we measure the outcomes in the military registers. Regardless of the exact age cutoff, our point estimates are largely similar. In few specifications we lose statistical significance, however, we note that this is mostly due to inflated standard errors and much smaller sample sizes rather than point estimates converging to zero. We conclude that our conservative approach of assigning exposure at the postcode of birth is not driving the results.

Our final robustness check verifies that the results are not driven by always treated individuals. This could be a concern if these people are different from the overall population as well as since their parents chose to live in a location that already had a fast food outlet nearby prior to the child's birth (rendering even our assignment of treatment based on postcode of birth potentially endogenous). Panel D of Table A4 presents the results excluding always treated individuals from the sample. If anything, these coefficients are larger than our preferred point estimates. This makes sense if always exposed individuals are less sensitive to marginal changes in fast food supply. We conclude that including always treated individuals in our sample is not a major empirical concern for the results.

Overall, we consider our preferred estimates to be remarkably robust across a multitude of estimation and sample permutations. Thus, we think about coefficients in Table 2.3 as reliably representing the causal effects of proximity to a fast food restaurant on young adult men's health and cognitive ability.

2.4.3 Heterogeneity

The effects of access to fast food on obesity can vary across a wide range of demographic characteristics including gender, race, and socioeconomic background (see, e.g., Currie et al., 2010). In this section, we therefore analyze whether the effects documented in Table 2.3 differ by socioeconomic background (proxied by father's education and employment status of parents), urbanicity, health endowments at birth, and father's BMI during adolescence.[34] In each case, we execute the heterogeneity analysis through a model with interactions, meaning that we expand Equation 2.1 to include number of years of exposure within 2 km of a fast food restaurant, a variable of interest in the heterogeneity analysis (either indicator or continuous), and an interaction between those two variables. The interaction term documents whether our treatment effect is different across the heterogeneity dimension in question. We do not include any auxiliary control variables beyond birth postcode and birth year fixed effects in this analysis.

Allcott et al. (2019) suggest that there are differences in demand for healthy food between poor and rich households. We therefore study whether children from a low socioeconomic backgrounds are more affected by the proximity to fast food restaurants than children with richer parents.[35] Panels A and B of Table 2.5 stratify the sample by paternal education and parental employment.[36] We do not find any sizable, statistically significant or consistently signed differences in our treatment effects across these groups when we consider health outcomes. This is despite the fact that overweight rates are 34% higher in families where the father have no high school or a vocational high school education, compared with families in which the father have at least an academic high school education. On the other hand, for both of the cognitive outcomes there is clear mediation of the fast food restaurants effect by paternal education. In households where fathers do not have an academic high school degree sons have 0.67% of a SD lower cognitive ability and 0.36 percentage points lower probability of choosing an academic track in high school per year of exposure to a fast food restaurant. These penalties are reduced to only 0.34% of a SD and 0.19 percentage points in families where father has an academic high school degree; or by about 50%.

Since, fast food restaurants in Norway tend to locate in cities and urban areas, we also investigate differential effects by being born in the top 10 biggest cities in Norway (panel C). These children could have even easier access to fast food due to either higher density of suppliers or more efficient public transport. We do not find any statistically significant differences here either. Interestingly, individuals living in big cities have lower BMI and higher cognitive outcomes which makes sense given that much of the country's white collar economic activity is concentrated in these areas.

Another question is whether exposure to fast food restaurants matters differentially for younger vs. older children. On the one hand, at younger ages, parents have arguably more control over what their children are eating. Thus, we view early age exposure as primarily

[34] Because our outcomes are limited to men, we cannot study differences by gender. Furthermore, Norway has insufficient racial diversity to investigate this dimension.

[35] Unlike in the US, fast food in the Norwegian context might be thought of as more of a luxury good because it is often more expensive than fairly accessible unprocessed food. Therefore, it is also plausible that the effects could be more pronounced in higher-SES families that can more easily afford such consumption.

[36] Another channel through which parental employment could affect obesity is traditional gender norms where a stay-at-home mother might do the cooking for the family, thereby limiting the reliance on food consumption outside of the household.

driven by parental choices while exposure during teenagehood as driven by both child and parental choices; and perhaps to a larger degree by the former than the latter. On the other hand, to the extent that parents allow fast food in their children's diet, this could be more consequential for a rapidly developing human body and brain in early childhood. Table A5 presents these results.[37] For health outcomes, the estimates are not statistically different across the two age groups but they do suggest somewhat larger effects for exposure in teenagehood when the youth can make more independent nutritional choices. For the cognitive and schooling measures, the effects are concentrated only in the early years.[38] This is consistent with evidence on critical periods and brain development (Heckman, 2007) as well as with the notion that proper nutrition might matter especially for young children (?Bütikofer et al., 2018). These results are also broadly consistent with event studies presented in Figure 2.2.

Returning to Table 2.5, we now move to two heterogeneity analyses that have been impossible to study in the extant literature due to data limitations. Thanks to population-level registry data, we can ask if the effects of access to fast food restaurants are mediated by prenatal health endowments as well as intergenerational propensity for elevated BMI. First, since in utero health and nutrition (often proxied by birth weight) have long-term consequences (Black et al., 2007; Figlio et al., 2014), individuals with low levels of prenatal health might be particularly vulnerable to changes in later life nutrition. Using birth weight as a marker of neonatal health, we analyze whether pre-determined health endowments are compensated or reinforced by subsequent negative nutritional shocks. Panel D of Table 2.5 shows that children who were born with a higher birth weight have a higher BMI and improved cognitive ability, however, we do not find any statistically significant or sizeable interaction between birth weight and access to fast food restaurants.[39]

Second, we ask whether the consequences of easier access to fast food are different for males whose fathers had a high BMI (panel E). For a subset of our sample (62 percent), we know the BMI of the fathers at the age of 18/19 from the military records (the same data source we use to measure our outcomes for young adults), and can therefore use this information about predispositions for being overweight.[40] Although the father's BMI at age 18/19

[37]In this case, we do not use an interaction terms but rather split the exposure variable from Equation 2.1 into two exposure variables: number of years of exposure at ages 0–12 and number of years of exposure at ages 13–19. We acknowledge the limitation that children exposed at younger ages are also likely exposed at older ages and thus we cannot differentiate age at exposure from length of exposure.

[38]Again, controlling for log BMI in Table A3 does not alter the results on ability. Our take away from this exercise is that the decline in cognitive ability cannot be explained by the orthogonal increase in BMI.

[39]We have also verified that this result is not driven by the way we measure prenatal health. Using an indicator for low birth weight likewise does not yield any sizeable or statistically significant interaction terms. The fact that higher birth weight children have higher BMI is in contrast with some prior research suggesting that lower prenatal health leads to increased BMI (Gluckman et al., 2007).

[40]To keep the sample constant we opted to run the heterogeneity analysis on the full sample and we additionally control for an indicator of missing father's BMI as well as its interaction with number of years exposed to fast food restaurant (not shown). Results are similar when we restrict the sample to those with available father's BMI albeit the standard errors increase. Furthermore, our preferred estimates for the effects of

years is a very good predictor of their sons' BMI (positive), as well as cognition and schooling (negative), we do not find any statistically significant interactions between access to fast food restaurants and paternal BMI. If anything, some of the coefficients have signs opposite from what we expected. However, quantitatively these difference are small both compared to estimates on father's BMI and on the years of exposure.

2.5 Conclusions

Our findings suggest that in a country like Norway where healthier food options are available and affordable, the negative causal effects of access to high caloric nutrition provided by fast food restaurants are still present and could have contributed to the increase in obesity rates among adolescents in recent decades. We further document that increased access to this type of food, which likely also leads to increased consumption, could have negative effects on cognitive ability. Back-of-the-envelope calculations suggest that increased penetration of fast food suppliers could be responsible for as much as 35% of the increase in average BMI and almost 22% of the decrease in average cognitive ability for the cohorts in our sample. These are much larger shares than what was documented in the US with respect to Walmart Supercenters (Courtemanche and Carden, 2011) or fast food exposure in middle school (Currie et al., 2010), which may not be that surprising given lower BMI levels, limited other obesogenic factors and easier access to healthy substitutes in Norway. Unlike in some prior research, in our setting, we do not find meaningful heterogeneity in these effects when considering health outcomes. For cognitive outcomes we find differences by age at exposure and by father's education. Our results are generally robust to alternative specifications and support the identifying assumptions.

Nevertheless, given that our point estimates can explain much less than a half of the increases in BMI and declines in cognitive ability observed in Figure 2.1, it is worth asking what other factors could contribute to these worrying trends. One potential contributor to obesity, and related chronic diseases, has been the significant shift to unhealthy diets at home, in particular to calories from sugar, refined carbohydrates, and fat (see, e.g., Cutler et al., 2003). Yet another factor could be increased sedentary lifestyle. Focusing on adults, Griffith et al. (2016) suggest that the increase in obesity is less likely due to an increase in calories consumed, and rather that it is caused by the decline in the strenuousness of work and daily life. Moreover, Aguiar et al. (2021) demonstrate using time-use data that younger men (21–30 years) in the US have shifted their leisure activities to video gaming and other recreational computer activities since 2004, and conclude that innovations in these leisure activities explain an important share of the decline in labor market activities of younger men relative to older men. More than 70 percent of Norwegian adolescents spend more than three hours each day in front of a TV, computer, or smartphone screen outside of school (Bakken,

number of years exposed to fast food restaurant for this sub-sample are similar to those reported in Table 2.3.

2022). Hence, a shift in leisure activities among children from more active forms of recreation to video gaming/social media might also contribute to the increase in BMI.

Notwithstanding these additional channels, which are plausibly responsible for the remaining share of the increase in BMI, we view our results as an important addition to the inconclusive literature on the effects of fast food restaurants penetration on health, and particularly the health of young adults. We also complement this literature by documenting adverse effects on cognition. Furthermore, we note that fast food regulation might be more policy-actionable and effective than attempts at altering consumption behaviors at home or exercise habits (Leonard, 2017; Griffith, 2022; Xiang et al., 2022). Indeed, some governments are currently looking into imposing stricter regulations on both advertisements and the location of fast food outlets.

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2.6 Figures and Tables

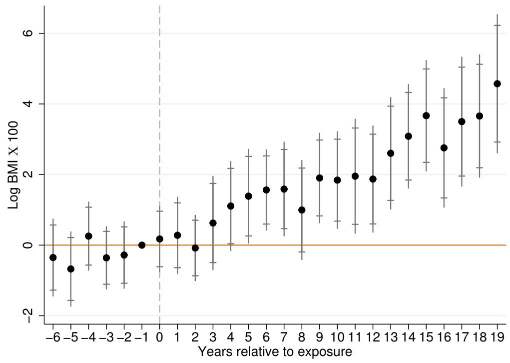
Figure 2.1: Trends in Number of Fast Food Restaurants, BMI, and Cognitive Ability

(a) Fast food restaurants

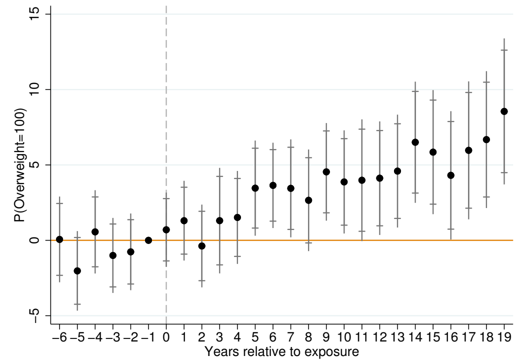
(b) BMI and cognitive ability

Notes: Panel A of this figure documents the increase in the number of fast food restaurants between 1980–2007. Panel B of this figure documents trends in Body Mass Index (BMI) and cognitive ability (IQ) scores of Norwegian male recruits by birth cohort (1980 to 1989) for those born in a postcode that at some point had a fast food restaurant within a 30 km radius.

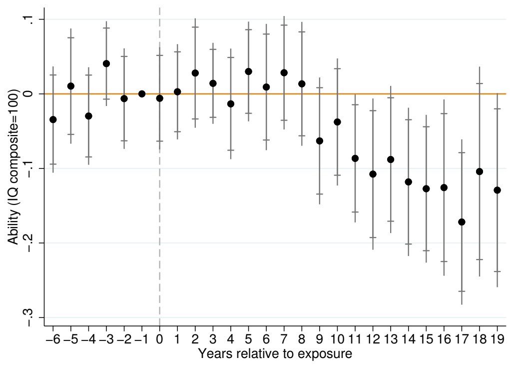
Figure 2.2: Event Study Estimates of Proximity to Fast Food Restaurants on Health, Ability and Education Outcomes



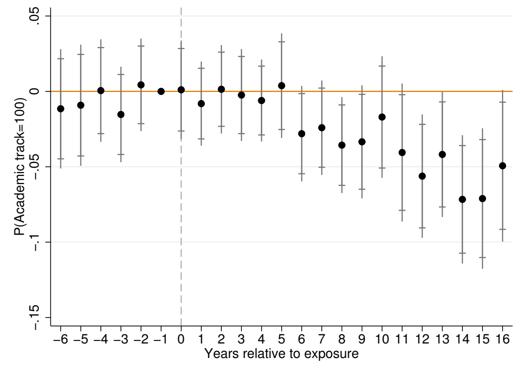
(a) Log BMI×100



(b) P(Overweight=100)



(c) Ability(IQ composites)×100



(d) P(Academic track=100)

Notes: Each figure is from a separate regression on the pre-treatment and post-treatment even-time dummies based on Equation 2.2. The treatment group is defined at 0-2 km distance while the control group is defined as 2-30 km distance. The sample includes individuals born 1980–1989 that at some point had access to at least one fast food restaurant within a 30-km radius from their place of residence at birth. All specifications include a full set of birth postcode and birth year fixed effects and no additional control variables. Standard errors are clustered at the municipality of birth. Significance levels: *** 1% level, ** 5% level, * 10% level.

Table 2.1: Descriptive Statistics

		All males within 30km radius		
			Residence	
	Full sample (i)	Estimating sample (ii)	Ever (iii)	Never (iv)
	Panel A: Outcome variables			
Log BMI×100	312.23	312.03	311.77	312.34
	[15.67]	[15.61]	[15.53]	[15.70]
BMI	22.99	22.94	22.88	23.02
	[3.90]	[3.87]	[3.84]	[3.91]
P(Overweight=100)	22.09	21.67	21.14	22.30
	[41.48]	[41.20]	[40.83]	[41.63]
Standardized ability×100	0.00	0.67	4.83	-4.19
	[100.00]	[99.96]	[100.47]	[99.14]
Raw ability score	5.09	5.09	5.15	5.01
	[1.72]	[1.72]	[1.73]	[1.71]
P(Academic track=100)	51.74	52.12	54.91	48.87
	[49.97]	[49.96]	[49.76]	[49.99]
	Panel B: Treatment variable			
Years exposed to fast food restaurants	6.41	7.26	13.48	0.00
	[7.98]	[8.12]	[6.21]	[0.00]
Years exposed to fast food restaurants before starting high school	5.04	5.71	10.60	0.00
	[6.74]	[6.91]	[6.06]	[0.00]
	Panel C: Individual level characteristics			
Birth order	1.84	1.82	1.76	1.90
	[0.94]	[0.93]	[0.89]	[0.96]
Mother's age at birth	26.66	26.68	26.75	26.60
	[5.00]	[4.96]	[4.89]	[5.05]
Father's age at birth	29.32	29.50	29.49	29.50
	[6.13]	[5.54]	[5.48]	[5.61]
Parents married at birth	0.78	0.79	0.77	0.80
	[0.42]	[0.41]	[0.42]	[0.40]
Years of education mother	12.43	12.45	12.62	12.25
	[2.42]	[2.43]	[2.48]	[2.34]
Years of education father	12.81	12.85	12.62	12.59
	[2.70]	[2.71]	[2.48]	[2.58]
Mother's income/1000	112.33	112.76	118.61	105.95
	[74.06]	[74.87]	[79.00]	[69.13]
Father's income/1000	243.52	247.63	258.97	234.41
	[169.72]	[175.25]	[206.12]	[129.13]
Observations	177790	156699	84421	72278

Notes: The sample is based on all males from birth cohorts 1980 to 1989 with valid military assessment outcomes. Panel A presents the means of outcomes, panel B the means of treatment variables and panel C the means of individual-level characteristics. Column (i) present values for all males while columns (ii)–(iv) values for all males with a birth postcode centroid within a 30 km radius of a fast food restaurant opened between 1980 and 2007. Column (iii) displays the means for individuals exposed to a fast food restaurant within 2 km of their place of residence at birth. Column (iv) displays the means for individuals never exposed to a fast food restaurant within 2 km of their place of residence at birth.

Table 2.2: Postcode-Level Correlates of Fast Food Restaurants Openings

		1977 postcode characteristics		Changes in postcodes characteristics 1977–1979	
	Mean in 1977	Opening before 1980	Opening before 1985	Opening before 1980	Opening before 1985
	(i)	(ii)	(iii)	(iv)	(v)
Log BMI × 100	311.21	0.000	0.001	0.001	0.003
		(0.001)	(0.002)	(0.003)	(0.004)
Age	30.86	0.003*	0.006***	0.039**	0.043**
		(0.002)	(0.002)	(0.016)	(0.018)
Population	1.48	0.012***	0.017***	0.329***	0.485***
		(0.002)	(0.003)	(0.064)	(0.072)
Education	10.33	0.042***	0.062***	-0.071	0.028
		(0.013)	(0.014)	(0.066)	(0.084)
Log income	10.53	-0.056	-0.100*	0.070	0.011
		(0.047)	(0.051)	(0.071)	(0.077)
Rural	0.19	-0.005	-0.005	0.006	0.002
		(0.006)	(0.009)	(0.005)	(0.006)
R^2		0.064	0.109	0.053	0.081
Observations		1898	1898	1898	1898

Notes: Each column represents a separate linear probability regression model of the likelihood of a fast food restaurant opening before 1980 (columns (ii) and (iv)) or before 1985 (columns (iii) and (v)) within 2 km in relation to postcode characteristics measured in 1977 (columns (ii) and (iii)) or changes in these characteristics between 1977 and 1979 (columns (iv) and (v)). Population is given in thousands, log income is the log of average income in thousands of NOK, education is years of education, rural is a dummy for whether the postcode is situated in a municipality counted as rural or not. Significance levels: *** 1% level, ** 5% level, * 10% level.

Table 2.3: Effect of Proximity to Fast Food Restaurants on Health, Ability and Education Outcomes

	≤0.5km (i)	≤1km (ii)	≤2km (iii)	≤0.5km (iv)	≤1km (v)	≤2km (vi)
	Panel A: Health outcomes					
	Log BMI×100			P(Overweight=100)		
Years exposed	0.128**	0.181***	0.198***	0.246**	0.280***	0.350***
	(0.053)	(0.046)	(0.041)	(0.124)	(0.103)	(0.100)
Observations	156699	156699	156699	156699	156699	156699
Pre-treatment mean	312.14	312.21	312.34	21.91	22.05	22.31
	Panel B: Ability and education outcomes					
	Ability (IQ composites)×100			P(Academic track=100)		
Years exposed	-0.393	-0.528**	-0.558**	-0.325**	-0.359***	-0.256**
	(0.282)	(0.243)	(0.223)	(0.148)	(0.136)	(0.127)
Observations	142434	142434	142434	156561	156561	156561
Pre-treatment mean	-1.59	-2.71	-4.19	50.86	50.25	49.24

Notes: Each point estimate is from a separate regression of an outcome variable on number of years of exposure to a fast food restaurant at a given distance from the individual's place of residence at birth. Outcomes are: log BMI (columns (i) to (iii) of panel A), probability of being overweight (columns (iv) to (vi) of panel A), standardized cognitive ability scores (columns (i) to (iii) of panel B), and probability of enrolling in an academic track in high school (columns (iv) to (vi) of panel B). All outcome variables are multiplied by 100. We consider three distances when defining the treatment group: ≤ 0.5 km (columns (i) and (iv)), ≤ 1 km (columns (ii) and (v)), and ≤ 2 km (columns (iii) and (vi)). The sample includes individuals born 1980–1989 that at some point had access to at least one fast food restaurant within a 30 km radius of their place of residence at birth. All specifications include a full set of birth postcode fixed effects and birth year fixed effects. Each regression controls separately for years of exposure to grocery stores and convenience stores within the indicated distance. Additional control variables include: mother's education, mother's age at birth, parents' marital status at birth, father's education, father's age at birth, and the individual's birth order. Standard errors are clustered at the municipality of birth. Significance levels: *** 1% level, ** 5% level, * 10% level.

Table 2.4: Effect of Proximity to Fast Food Restaurants on Health, Ability and Education Outcomes: Sensitivity Analysis

	Main estimates (i)	No controls (ii)	Excluding Oslo (iii)	Municipality specific time trend (iv)	Controlling for age at measurement (v)	Other reforms (vi)	Controlling for exposure at high school (vii)	Dropping single site restaurants (viii)
Log BMI×100	0.198***	0.202***	0.181***	0.203***	0.198***	0.187***	0.197***	0.163***
	(0.041)	(0.039)	(0.042)	(0.041)	(0.041)	(0.040)	(0.041)	(0.045)
Observations	156699	156699	148933	156699	156699	156699	156699	134183
P(Overweight=100)	0.350***	0.396***	0.322***	0.358***	0.348***	0.327***	0.347***	0.321***
	(0.100)	(0.095)	(0.107)	(0.101)	(0.100)	(0.100)	(0.100)	(0.115)
Observations	156699	156699	148933	156699	156699	156699	156699	134183
Ability (IQ composites)×100	-0.558**	-0.824***	-0.594**	-0.545**	-0.560**	-0.548**	-0.534**	-0.748***
	(0.223)	(0.248)	(0.234)	(0.221)	(0.223)	(0.222)	(0.226)	(0.275)
Observations	142434	142434	135310	142434	142434	142434	142434	142434
P(Academic track=100)	-0.256**	-0.434***	-0.227*	-0.257**	-0.244*	-0.221*		-0.405***
	(0.127)	(0.127)	(0.132)	(0.128)	(0.126)	(0.124)		(0.144)
Observations	156561	156561	148815	156561	156561	156561		134056

Notes: Each point estimate is from a separate regression of an outcome variable on number of years of exposure to a fast food restaurant at 2 km distance from the individual's place of residence at birth. Each outcome is in a separate row: log BMI (row 1), probability of being overweight (row 2), standardized cognitive ability scores (row 3), and probability of enrolling in an academic track in high school (row 4). All outcome variables are multiplied by 100. Column (i) replicates preferred specification from Table 2.3. Column (ii) drops all control variables except for birth postcode and birth year fixed effects. Column (iii) drops Oslo municipality. Column (iv) includes municipality-specific linear trends. Column (v) controls for age at measurement of the outcome. Column (vi) controls for maternity leave and Oslo school choice reforms at individual level. Column (vii) controls for exposure to fast food restaurants at attended high school address (this is not available for high school track since this choice is made prior to attendance). Column (viii) drops all the single-site restaurants from industry codes 56.101 and 56.301. Standard errors are clustered at the municipality of birth. Significance levels: *** 1% level, ** 5% level, * 10% level.

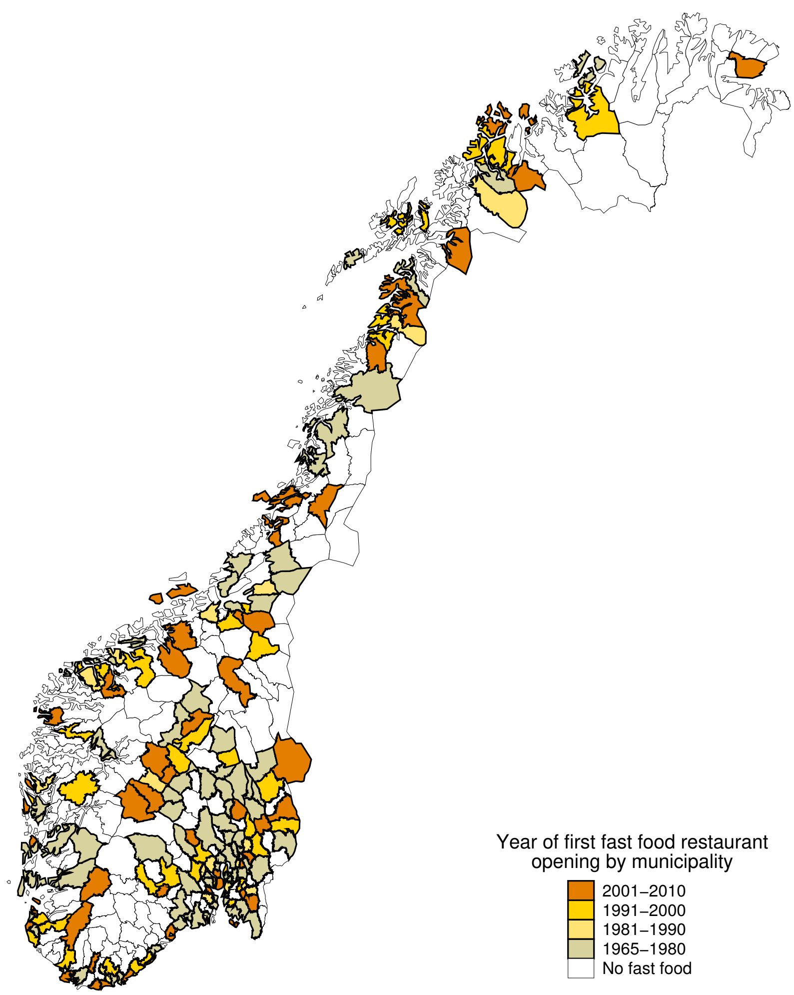
Table 2.5: Effect of Proximity to Fast Food Restaurants on Health, Ability and Education Outcomes: Heterogeneity

	Log BMI×100 (i)	P(Overweight=100) (ii)	Ability (IQ composites×100) (iii)	P(Academic track=100) (iv)
	Panel A: Father's education			
Years exposed	0.197***	0.331***	-0.667***	-0.361***
	(0.041)	(0.101)	(0.245)	(0.133)
Years exposed × father	-0.005	0.010	0.325***	0.167***
academic high school	(0.012)	(0.028)	(0.087)	(0.048)
Father academic high school	-1.965***	-5.318***	52.527***	29.201***
	(0.180)	(0.456)	(1.183)	(0.684)
Observations	156699	156699	142434	156561
	Panel B: Full time working parents			
Years exposed	0.199***	0.348***	-0.719***	-0.406***
	(0.042)	(0.103)	(0.243)	(0.129)
Years exposed ×	0.018	0.065	-0.021	-0.014
full time working parents	(0.016)	(0.040)	(0.090)	(0.048)
Full time working parents	-0.150	-1.113	27.154***	15.698***
	(0.271)	(0.683)	(1.727)	(0.884)
Observations	156699	156699	142434	156561
	Panel C: 10 biggest cities			
Years exposed	0.169***	0.291***	-0.482*	-0.210
	(0.040)	(0.100)	(0.256)	(0.137)
Years exposed × city	0.032	0.032	-0.270	-0.097
	(0.019)	(0.052)	(0.204)	(0.104)
City	-1.634*	-4.729**	19.047***	15.241***
	(0.910)	(2.285)	(3.228)	(3.662)
Observations	156699	156699	142434	156561
	Panel D: Birth weight			
Years exposed	0.210***	0.385***	-1.116***	-0.533***
	(0.050)	(0.137)	(0.333)	(0.205)
Years exposed ×	-0.000	-0.000	0.000	0.000
birth weight	(0.000)	(0.000)	(0.000)	(0.000)
Birth weight	0.002***	0.003***	0.014***	0.005***
	(0.000)	(0.001)	(0.001)	(0.001)
Observations	156699	156699	142434	156561
	Panel E: Father's log BMI			
Years exposed	0.464**	0.855*	-0.715	-0.690
	(0.212)	(0.502)	(1.200)	(0.688)
Years exposed ×	-0.093	-0.180	-0.024	0.078
father's log BMI	(0.067)	(0.161)	(0.385)	(0.216)
Father's log BMI	40.564***	76.345***	-22.948***	-12.141***
	(0.728)	(1.789)	(4.913)	(1.914)
Observations	156699	156699	142434	156561

Notes: Each column in each panel is from a separate regression of an outcome variable on number of years of exposure to a fast food restaurant at 2 km distance from the individual's place of residence at birth, heterogeneity dimension variable considered, and interaction between these two variables. The heterogeneity of interest variables include: father's academic education indicator (panel A), indicator for both parents working (panel B), indicator for birth in the 1 biggest Norwegian cities (panel C), birth weight in grams (panel D), and father's log BMI (panel D). The specifications are otherwise akin to those in Table 2.3 but exclude all the control variables except for birth postcode and birth year fixed effects. Standard errors are clustered at the municipality of birth. Significance levels: *** 1% level, ** 5% level, * 10% level.

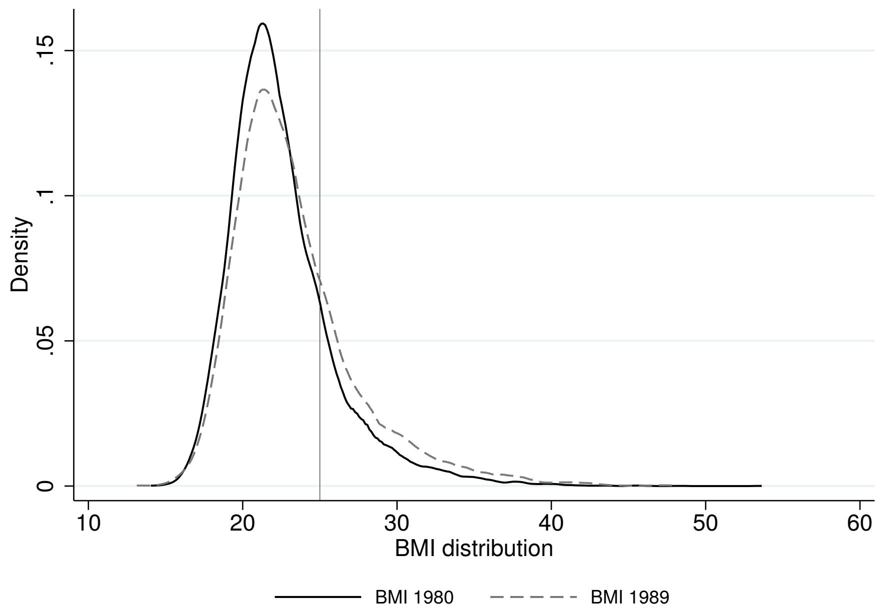
2.A Appendix Tables and Figures

Figure A1: Municipalities with Fast Food Restaurants Openings over Time



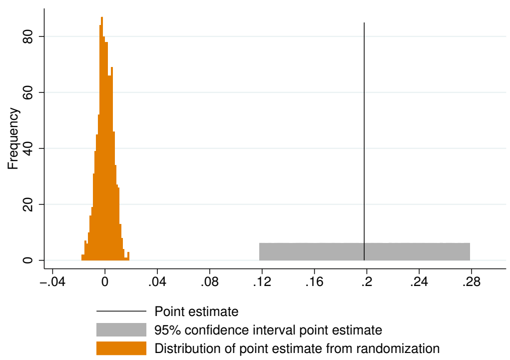
Notes: The map displays Norway's 428 municipalities. The different colors indicate when the first fast food restaurant opened in these municipalities. There are no fast food restaurants in the white municipalities in the period of interest.

Figure A2: Distribution of BMI by Birth Cohort

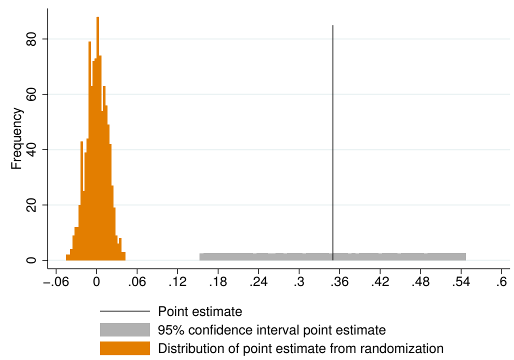


Notes: The figure plots the distribution of BMI for men born in 1980 and 1989 in a postcode that at some point had a fast food restaurant within a 30 km radius. The vertical line marks the threshold for overweight (BMI>25).

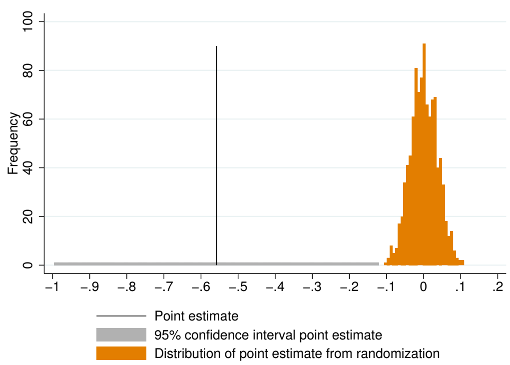
Figure A3: Effect of Proximity to Fast Food Restaurants on Health, Ability and Education Outcomes: Randomization Inference Analysis



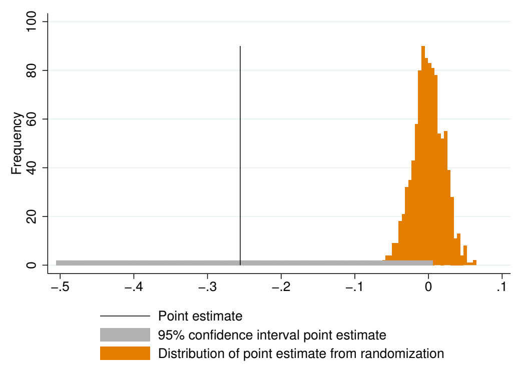
(a) Log BMI×100



(b) P(Overweight=100)



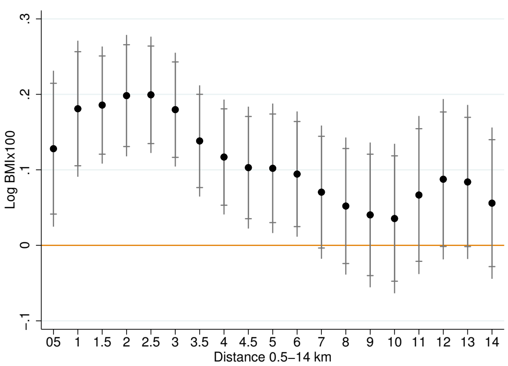
(c) Ability(IQ composites)×100



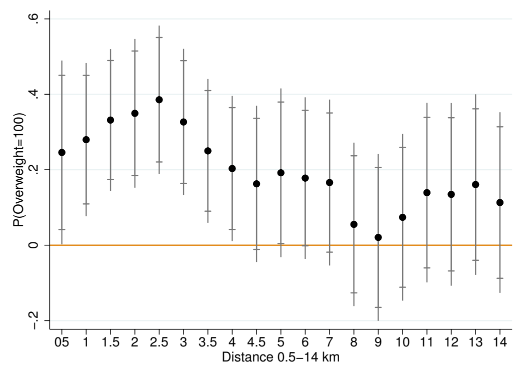
(d) P(Academic track=100)

Notes: The figure shows the distributions (orange histograms) of 1,000 coefficients from a randomization test for log BMI (panel A), probability of being overweight (panel B), cognitive ability (panel C), and probability of enrolling in an academic high school track (panel D). All outcome variables are multiplied by 100. In order to generate the randomization inference distributions of estimates, we randomly allocate postcodes of restaurant openings holding the number of fast food outlet openings in each year identical to the one observed in true data; we then compute the number of years exposed given these randomly allocated exposures and re-estimate our preferred specification from Table 2.3. The vertical black line depicts our preferred estimates from Table 2.3 while the gray shaded areas indicate 95% confidence intervals around these estimates.

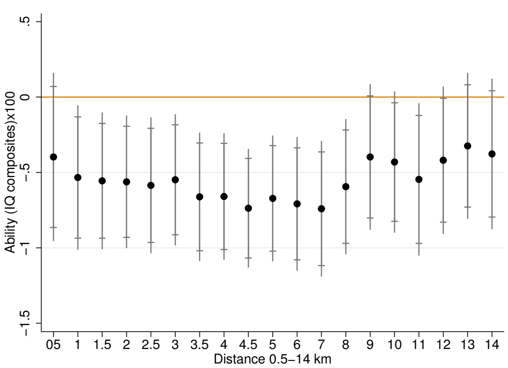
Figure A4: Effect of Proximity to Fast Food Restaurants on Health, Ability and Education Outcomes: Varying Definition of Treatment Group Radius Cutoff



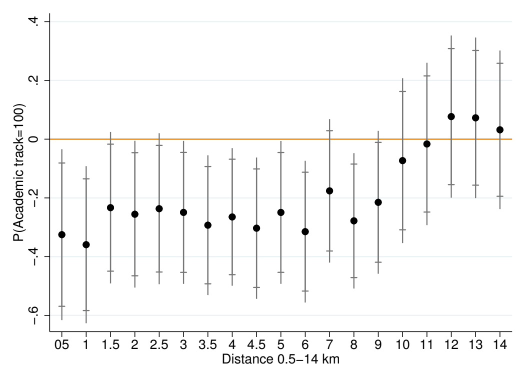
(a) Log BMI×100



(b) P(Overweight=100)



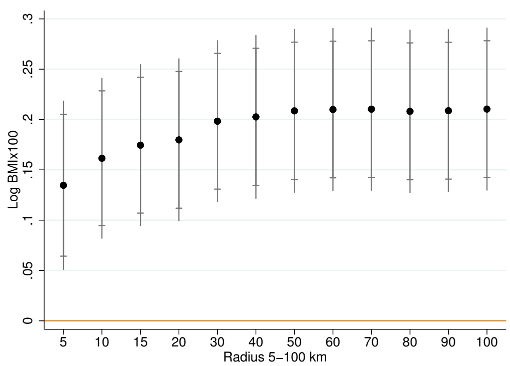
(c) Ability(IQ composites)×100



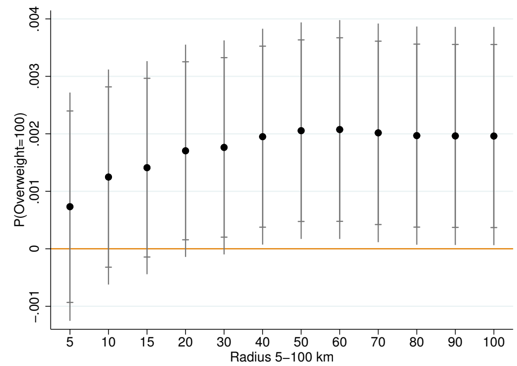
(d) P(Academic track=100)

Notes: Each panel of this figure shows robustness checks where we vary the radius defining the treatment group. Outcomes are: log BMI (panel A), probability of being overweight (panel B), cognitive ability (panel C), and probability of enrolling in an academic high school track (panel D). All outcome variables are multiplied by 100. Each point estimate (circles) and 95% (spikes) and 90% (whiskers) confidence intervals comes from a separate regression. The sample includes individuals born 1980–1989 that at some point had access to at least one fast food restaurant within a 30-km radius from their place of residence at birth. Definition of treatment group radius varies every 0.5 km from 0.5 km to 5 km and every 1 km from 5 to 14 km. In each case the control group is the complement distance up to 30 km. Each regression controls separately for years of exposure to grocery stores and convenience stores within the indicated distance. Additional control variables are the mother's education, mother's age at birth, parents' marital status at birth, father's education, father's age at birth, and the individual's birth order. Horizontal orange line denotes zero. Standard errors are clustered at the municipality of birth.

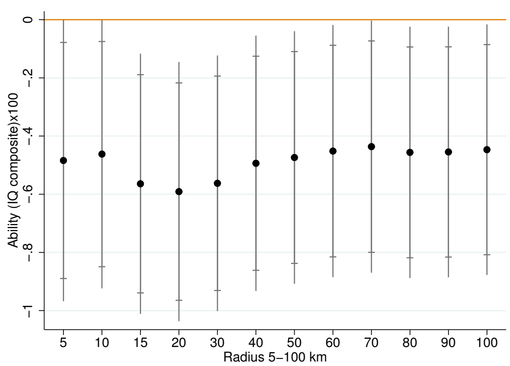
Figure A5: Effect of Proximity to Fast Food Restaurants at 2 km on Health, Ability and Education Outcomes: Varying Radius for Inclusion in Empirical Sample



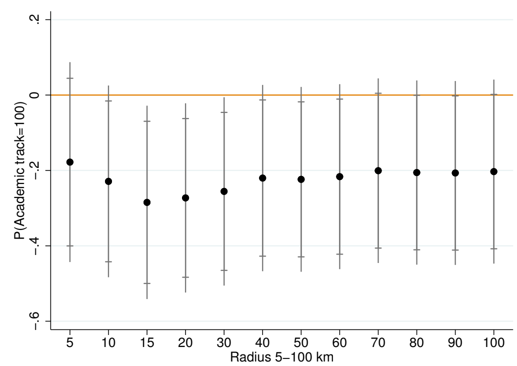
(a) Log BMI×100



(b) P(Overweight=100)



(c) Ability(IQ composites)×100



(d) P(Academic track=100)

Notes: Each panel of this figure shows robustness checks where we vary the radius defining inclusion in the analysis sample. Each point estimate is from a separate regression of an outcome variable on number of years of exposure to a fast food restaurant at 2 km distance from the individual's place of residence at birth. Circles denote point estimates, spikes denote 95% while whiskers denote 90% confidence intervals. Outcomes are: log BMI (panel A), probability of being overweight (panel B), cognitive ability (panel C), and probability of enrolling in an academic high school track (panel D). The samples includes individuals born 1980–1989 that at some point had access to at least one fast food restaurant within a 5–100km radius from their place of residence at birth. All specifications include a full set of birth postcode fixed effects and birth year fixed effects. Each regression controls separately for years of exposure to grocery stores and convenience stores within the indicated distance. Additional control variables are the mother's education, mother's age at birth, parents' marital status at birth, father's education, father's age at birth,and the individual's birth order. Standard errors are clustered at the municipality of birth.

Table A1: Intergenerational Transmission of BMI and Likelihood of Being Overweight

	Log BMI×100 (i)	P(Overweight=100) (ii)
	Panel A: Without control variables	
Log BMI father×100	0.399*** (0.007)	
P(Father overweight=100)		0.217*** (0.007)
Mean father	306.92	7.45
Mean son	312.35	22.34
Observations	121847	121847
	Panel B: With control variables	
Log BMI father×100	0.396*** (0.007)	
Father overweight		0.217*** (0.007)
Observations	121847	121847
	Panel C: With control variables and 30km radius	
Log BMI father×100	0.400*** (0.006)	
P(Father overweight=100)		0.211*** (0.007)
Mean father	306.92	7.51
Mean son	312.03	21.67
Observations	97166	97166

Notes: Each point estimate comes from a separate regressions of paternal BMI on son's BMI (column (i)) or paternal likelihood of being overweight on son's likelihood of being overweight (column (ii)). Panel A presents univariable associations, panel B adds father, mothers, and child's control variables, and panel C limits the sample to households where son's birth postcodes was within 30 km of the opening of fast food restaurant between 1980 and 2007. All regressions include 1980–1989 birth cohorts for sons and 1950-1972 birth cohorts for fathers. Means of father and son BMI as well as probability of being overweight are defined as the means in empirical sample. Standard errors are clustered at son's municipality of birth. Significance levels: *** 1% level, ** 5% level, * 10% level.

Table A2: Effect of Proximity to Fast Food Restaurants on Health and Ability: Alternative Measures of Outcomes

	BMI (i)	BMI z-score (ii)	P(Obesity=100) (iii)	Weight (iv)	Height (v)	Ability (vi)
Years exposed	0.049***	0.013***	0.173***	0.143***	-0.019	-0.010**
	(0.010)	(0.003)	(0.052)	(0.037)	(0.021)	(0.004)
Observations	156699	156699	156699	156699	156699	142434
Pre-treatment mean	23.02	0.01	6.04	74.64	179.97	5.00

Notes: Each point estimate comes from separate regression. Samples and econometric specifications based on column (iii) from panel A of Table 2.3 for columns (i) to (v) and based on column (iii) of panel B of Table 2.3 for column (vi). Dependent variables are BMI in levels (column (i)), standardized mean 0 and SD 1 BMI (column (ii)), probability of being obese multiplied by 100 (column (iii)), weight in kilograms (column (iv)), height in centimeters (column (v)), and unstandardized ability scores from military records (column (vii)). Obesity is defined as BMI > 30. Standard errors are clustered at the municipality of birth. Significance levels: *** 1% level, ** 5% level, * 10% level.

Table A3: Effect of Proximity to Fast Food Restaurants on Health, Ability and Education Outcomes Controlling for Log BMI

	Ability (IQ composites)×100	P(Academic track=100)
	(i)	(ii)
	Panel A: Main estimates	
Years exposed	-0.548**	-0.236*
	(0.223)	(0.127)
Observations	142434	156561
	Panel B: Age heterogeneity	
Years exposed 0-12	-0.958***	-0.282**
	(0.282)	(0.138)
Years exposed 13-19	0.096	
	(0.377)	
Observations	142434	156561

Notes: Point estimates in each panel and each column come from separate regressions. Panel A replicates results from columns (iii) and (vi) of panel B of Table 2.3 while controlling for child's log BMI. Panel B replicates results from panel B of Table A5 while controlling for child's log BMI. Standard errors are clustered at the municipality of birth. Significance levels: *** 1% level, ** 5% level, * 10% level.

Table A4: Effect of Proximity to Fast Food Restaurants on Health, Ability and Education Outcomes: Additional Sensitivity Analyses

	Log BMI×100 (i)	P(Overweight=100) (ii)	Ability (IQ composites) ×100 (iii)	P(Academic track=100) (iv)
	Panel A: Main estimates			
Years exposed	0.198*** (0.041)	0.350*** (0.100)	-0.562** (0.224)	-0.256** (0.127)
Observations	156699	156699	142434	156561
	Panel B: Control group residing in 2–5 km radius excluded			
Years exposed	0.205*** (0.041)	0.351*** (0.101)	-0.561** (0.224)	-0.255** (0.128)
	Panel C: Non-movers different age cutoffs			
Age 6	0.238*** (0.050)	0.443*** (0.116)	-0.440 (0.267)	-0.403** (0.162)
Observations	100476	100476	91409	100407
Age 12	0.307*** (0.062)	0.526*** (0.151)	-0.593* (0.305)	-0.262 (0.173)
Observations	75679	75679	68746	75628
Age 18	0.262*** (0.080)	0.319 (0.197)	-0.795* (0.441)	
Observation	49202	49202	44843	
	Panel D: Always treated excluded			
Years exposed	0.218*** (0.052)	0.366*** (0.123)	-0.791*** (0.270)	-0.412*** (0.131)
Observations	120231	120231	109523	120140

Notes: Each point estimate is from a separate regression of an outcome variable on number of years of exposure to a fast food restaurant at 2 km distance from the individual's place of residence at birth. Each outcome is in a separate column: log BMI (column (i)), probability of being overweight (column (ii)), standardized cognitive ability scores (column (iii)), and probability of enrolling in an academic track in high school (column (iv)). All outcome variables are multiplied by 100. Panel A replicates the results from columns (iii) and (vi) of panel A and columns (iii) and (vi) of panel B of Table 2.3. Panel B drops a bandwidth between 2 and 5 km from the control group creating a donut regression with treatment group defined as 0–2km and control group defined as 5-30 km. Panel C limits the sample to children whose postcode at birth and postcode of residence residence up to age 6 (row 1), up to age 12 (row 2), and up to age 18 (row 3) are identical. Panel D drops individuals who were always exposed to fast food restaurant based on their postcode of birth. Standard errors are clustered at the municipality of birth. Significance levels: *** 1% level, ** 5% level, * 10% level.

Table A5: Effect of Proximity to Fast Food Restaurants on Health, Ability and Education Outcomes: Heterogeneity by Age

	(i)	(ii)
	Panel A: Health outcomes	
	Log BMI×100	P(Overweight=100)
Years exposed age 0-12	0.178***	0.261*
	(0.050)	(0.137)
Years exposed age 13-19	0.225***	0.531***
	(0.074)	(0.165)
Observations	156699	156699
P-value from F-statistics	0.625	0.256
	Panel B: Ability and education outcomes	
	Ability (IQ composites)×100	P(Academic track=100)
Years exposed age 0-12	-0.965***	-0.300**
	(0.282)	(0.138)
Years exposed age 13-19	0.079	
	(0.376)	
Observations	142434	156561
P-value from F-statistics	0.026	

Notes: Point estimates in each panel and each column come from separate regressions. Each regression in (except for choosing academic track) divides the treatment into number of years exposure to fast food restaurant at ages 0–12 and at ages 13–19 separately. Since we measure academic track selection at age 16 we limit this analysis (column (ii) of panel B) to number of years of exposure to fast food restaurant at ages 0–12. Except for this change the econometric specification is the same as in columns (iii) and (vi) of panels and B of Table 2.3. Standard errors are clustered at the municipality of birth. Significance levels: *** 1% level, ** 5% level, * 10% level.

Chapter 3

Generational Persistence in the Effects of an Early Childhood Health Intervention*

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Abstract

We investigate multi-generational impacts of early life health interventions. Using Norwegian administrative data with variation in the timing of infant health care center adoption between 1936–1955, we find strong evidence that the program's long term education and earnings benefits on exposed cohorts extended to their later offspring, but only for offspring who had an exposed mother. A plausible mechanism is that women exposed to the program were more likely to partner with highly educated and high earnings men. We also show that benefits accruing to the second generation are larger for those whose mothers were born in low income municipalities and municipalities that had high infant mortality rates, suggesting that public investments in early childhood health can be important levers towards increasing future generations' equality of opportunity.

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3.1 Introduction

Economic status is highly persistent across generations (see, e.g., Clark, 2014; Lindahl et al., 2015; Solon, 2018). Relative to more advantaged children, those born to low-educated, low-earnings parents are at substantially higher risk of growing up to be low-educated and have low earnings themselves. Recent research suggests that at least part of this association is causal – that is, that external or policy induced changes in parents' economic circumstances also produce changes in their offsprings' economic success (Bastian and Michelmore, 2018; Aizer et al., 2016; Hoynes et al., 2016; Akee et al., 2010; Oreopoulos et al., 2008, 2006). This suggests that public investments that successfully alter one generations human capital or labor market outcomes may be important levers for breaking the cycle of poverty. As yet, however, we know very little about the extent to which government interventions impact multi-generational linkages, or the processes by which such changes might occur.

We push the state of knowledge forward by documenting generationally persistent impacts of a widespread early childhood health investment. A rapidly expanding literature documents that programs that improve children's nutrition or access to health care have long-lasting effects on their later health, human capital and economic outcomes (Bailey et al., 2020; Brown et al., 2020; Bütikofer et al., 2019; Miller and Wherry, 2019; Bitler et al., 2018; Cohodes et al., 2016; Hoynes et al., 2016; Fitzsimons and Vera-Hernández, 2013; Bharadwaj et al., 2013; Bhalotra and Venkataramani, 2012; Chay et al., 2009; Almond et al., 2007), making such programs strong candidates for intergenerational spillovers. Moreover, results from a handful of emerging studies seem to be consistent with this prior: harnessing state-year variation in the timing of the 1980's Medicaid expansions, which increased low-income pregnant women's access to health related services, East et al. (2022) find that both exposed infants and their later offspring had better birth outcomes. Similarly, Almond and Chay (2006) document that in the wake of the Civil Rights Act, Black women who had gained access to integrated hospitals during their first year of life were at lower risk of giving birth to infants with low birthweights. Baker et al. (2022) also find suggestive evidence of second generation birthweight improvements in their analyses of a randomized trial that extended the duration of a Danish universal home visiting program to age three. Since birthweight has been positively linked to future earnings (Royer, 2009; Black et al., 2007) it is likely that the benefits of these early interventions also extended to the next generations economic outcomes – outcomes that are critical to breaking the cycle of poverty. In a similar vein, Barr and Gibbs (2017) and Rossin-Slater and Wust (2020) find that children of mothers who were exposed to targeted preschool programs that provided a combination of education and health services have better human capital outcomes in adulthood than children whose mothers were not exposed.

The limited work in this area to date likely results from the dual challenges of identifying exogenous variation in widespread early life policies, and linking that variation to data with relevant information on multiple generations.[6] We overcome these challenges by analyzing multi-generational effects of a universal infant health program using national registry data linking families across multiple generations, and capitalizing on variation in program exposure that resulted from Norway's staggered adoption of mother-child health care centers. The centers, which were established during the 1930s and have been described in detail in Bütikofer et al. (2019), provided free medical exams during the first year of life, as well as information about nutrition, disease, and other aspects of early child development. Bütikofer et al. (2019) document that children who gained access to the centers had substantially better health and economic outcomes in adulthood, making this an ideal natural experiment with which to explore the presence of, and processes that generate, multi-generational spillovers.[7]

Our large-scale population-level data allow us to measure long-run outcomes for two generations following initial program exposure, and evaluate mechanisms underlying generational persistence, including the relative roles of both mothers and fathers. Beyond these contributions, the Norwegian setting provides a unique opportunity to consider the value of a universal program. This is important, as recent policy discussions have shifted away from an emphasis on targeted interventions towards a more universalistic approach (Savchuck, 2012; Yemtsov, 2016; Desai, 2016). Proponents argue that relative to more targeted policies, universal programs promote social inclusion, and may be easier for administrators to implement, while targeted policies reinforce social disparities and reduce individuals' autonomy. In addition, because universal programs do not disappear as income increases, they are likely to generate relatively smaller labor supply disincentives (Kearney, 2021).

Our empirical strategy builds upon the validated methodology described in Bütikofer et al.

[6] In light of these challenges, most of the evidence on generationally persistent effects of early life environments is based on negative in utero shocks such as disease and malnutrition (see, e.g., Almond et al., 2010; Painter et al., 2005; Richter and Robling, 2015) or exposure to radioactivity (Black et al., 2019). All of these studies find evidence that these types of extreme shocks to the in utero environment produce effects that persist to later generations. One study by Bütikofer and Salvanes (2020) considers the impacts of a health intervention that occur in adolescence, and finds that a Norwegian tuberculosis intervention reduced generational persistence in educational attainment. Another related study by Colmer and Voorheis (2020) finds improved educational outcomes among the grandchildren of cohorts who benefited from reductions in pollution exposure following the 1970 Clean Air Act Amendments.

[7] Studies of similar programs in Sweden and Denmark have also found evidence of positive long-run effects (Bhalotra et al., 2017; Rossin-Slater and Wust, 2020; Wüst et al., 2018; Hjort et al., 2017)

(2019). We estimate a difference-in-differences model that relies on variation in program availability across birth cohorts and locations. To limit concerns about program endogeneity, we include cohort and municipality fixed effects, and a rich set of individual and municipality level controls that vary over time. We also estimate event study models, and we rule out heterogeneous treatment effects (Sun and Abraham, 2020) by implementing a new estimation approach proposed by Callaway and SantAnna (2020).

We find substantive impacts of infant health care on the second generation's long term economic outcomes, but only among second generation offspring who had treated mothers. Relative to children whose mothers did not have access to well-child visits, those whose mothers were exposed were 7 percent more likely to complete 16 years of education and had earnings that were 1.8 percent higher. Estimates for offspring whose fathers had early life access to the program are substantially smaller, and are rarely statistically different from zero. This is consistent with previous findings in Akee et al. (2010), Black et al. (2005a) and Duflo (2003) and suggests that targeting resources towards mothers may be important to improving children's later life success.

Our results are not driven by the first generation's selection into motherhood. Treated mothers and fathers did have fewer children, which may have lead to better second generation outcomes by increasing parents' time investments, but we believe that this is unlikely to be a critical mechanism because the decline in family size is observed among both treated mothers and treated fathers, while our main effects are only observed among the offspring of treated mothers. Instead, we show that the program increased the likelihood that first generation mothers partnered with highly educated and high earnings men. This program-induced change in mating patterns increased the resources available to the children of treated mothers relative to those whose mothers were not treated. To the extent that higher earnings men also had higher abilities, the improvement in second generation outcomes might also reflect a passing on of these abilities through genetic or learned processes. Importantly, we observe much smaller changes in mating patterns among treated fathers.

Although prior studies suggest that the second generation's gains might also result from improved health, we do not observe improvements in the second generation's health at birth or in their adult height (a common measure of nutritional inputs). This may reflect the fact that, by the 1960s equal health care access was well established in all municipalities, and was partly compensating for differences in mothers' health and human capital.

Our estimates also hint that public investments in early childhood health may be useful levers towards increasing future generations' equality of opportunity. Although data constraints prevent us from directly examining the program's impact on standard measures of intergenerational mobility, we show that the increase in earnings that was experienced by the second generation was larger among those whose first generation mothers were born in low income municipalities and in municipalities that had high infant mortality rates prior to the program's introduction.

This paper provides new evidence on the intergenerational reach of early childhood investments – evidence that is critical for calculating accurate returns to social policies. We proceed as follows. Section 2 discusses the history of Norwegian mother-child health centers and how the improved health and economic outcomes experienced by those who gained access to these centers may have lead to intergenerational spillovers. Section 3 describes our historical and administrative data sources, and Section 4 discusses our research design and identifying assumptions. We present our results and sensitivity analyses in Section 5 and provide conclusions in Section 6.

3.2 Background and Potential Channels for Intergenerational Transmission

3.2.1 Infant Health Care Centers

In the late 19th and early 20th centuries infant mortality was high in both Europe and the United States: in 1930, nearly 45 out of 1000 Norwegian infants did not survive their first year of life (Backer, 1963). This sparked public demands for government investments targeting infant health, and lead to local initiatives by a philanthropic institution called the Norwegian Women's Public Health Association (NKS), which established mother child health centers across the country . The centers were run according to national state-of-the-art medical guidelines, and all services were free of charge. While the centers were mainly targeted at poor families, they were open to everyone, and quickly became widely popular among mothers of all socioeconomic backgrounds. By 1939, 80 percent of infants in Oslo were receiving check-ups and the centers were an important part of the city's universal health care services (Schiøtz, 2003).

On average, a child would visit an infant health care center three to four times during their first year of life. The centers provided two main services: infant check-ups by doctors and nurses, and advice for mothers on adequate infant nutrition, infant hygiene measures, and adequate infant clothing. Centers' medical equipment was limited to what was needed for standard check-ups; ill infants were therefore referred to doctors or hospitals. As breastfeeding rates in Norway were relatively low and declining in the first half of the 20th century

(Liestøl et al., 1988), staff at the centers promoted breastfeeding, and mothers were taught to make adequate milk formulas. The centers' costs were mainly comprised of doctors' and nurses' salaries and travelling expenses, and expenses related to printing information materials for mothers. The costs were financed by funds from the state lottery, financial support from philanthropic contributions, local governments, counties and the state.

By 1960, most infants had access to free infant check-ups at NKS run health care centers. Starting in the 1960s, centers began to switch from private to public ownership, and since 1972, all Norwegian municipalities are obliged by law to provide public health care centers for infants, regulated by the Health Directorate's official guidelines. Thus, municipality-run centers are still an important and integral part of universal health care provision for infants, small children, and mothers. Today, the centers provide 14 free check-ups between the ages of 0 to 5, along with vaccinations and basic health education. They are staffed by pediatricians, nurses, midwives, physiotherapists, and psychologists.

While access to infant health care varied substantially across geographic areas during the initial rollout period, this was not the case by the time the treated cohorts gave birth to their children. Hence, our estimates will capture an indirect effect of "first generation" health care access on the second generation.

3.2.2 Potential Channels for Intergenerational Transmission

Bütikofer et al. (2019) study the "first generation" consequences resulting from the introduction of infant health care centers. Their results suggest that exposed cohorts' access to regular early life checkups increased their education, earnings and adult height. They also find that the centers decreased infant mortality from diarrhea, likely due to the nutrition and hygiene advice that mothers received.

Assuming that parents' education and household income have a positive effect on children (Currie and Moretti, 2003; Black et al., 2005a), the program might lead to intergenerational spillovers because of its positive effect on the first generation's socioeconomic outcomes. Such effects could also be magnified if better educated parents chose different partners (Dahl and Lochner, 2012). Importantly, these influences would have the potential to affect the second generation prenatally, in early childhood, and when making educational and labor market choices.

Center effects on the first generation's health could also be an important pathway. Halliday et al. (2021), document that there is substantial intergenerational persistence in health outcomes, particularly among families with lower levels of education. Moreover, improvements in the second generation's early life health would be expected to improve their labor market outcomes (see, e.g., Almond et al., 2018).

A further channel through which access to infant health care might effect the next generation's outcomes is through changes in treated cohorts' fertility patterns (Becker and Lewis, 1973; Becker and Tomes, 1976). Policies that increase women's education have been shown to affect their likelihood of having a teen birth (see, e.g., Black et al., 2008; Geruso and Royer, 2018), and reduce their fertility along both the intensive and extensive margin (Aaronson et al., 2014). Although there is little evidence for a child-quality/child-quantity trade-off (Angrist et al., 2010; Black et al., 2005a), changes in fertility timing or the types of individuals who have children might affect outcomes observed in the next generation (Aizer et al., 2020).

3.3 Data

We link historical data on the centers' rollout with individual-level administrative data. In particular, we use a compilation of different Norwegian administrative registers, including the central population register, the education register, the birth and the cause of death registers, and the tax and earnings register. These linked administrative data cover the Norwegian population up to 2018, and provide information about individuals' place of birth and residence, educational attainment, labor market status, birth outcomes, and earnings, as well as a set of demographic variables. In addition, a multi-generational register matches Norwegian children to their parents and grandparents.[8] As a result, we can link earnings, education, and birth outcome data over several generations. The historical data on the health care centers are collected from public and private archives. In what follows, we briefly describe the historical data, summarize the sample definitions and the registry data, and describe the variables and summary statistics for our sample.

3.3.1 Historical Data

The main data sources we use to document the rollout of mother and child health care centers are two surveys that the Norwegian Women's Public Health Association (NKS) sent to all health care centers in 1939 and 1955. The surveys collected data on each center's exact address and date of establishment. In addition, we collected data from centers' yearly reports, including the number of children served. Comparing the size of a birth cohort in a municipal-

[8]We have information on schooling for about 65% of the first generation's parents. Because this information is only available for individuals who survived until the late 1950s, however, analyses of the sample of first (second) generation individuals whose parents (grandparents) education is available would be contaminated by selection.

ity with the number of children examined at a health care center in each year, we find that the uptake rate was about 40 percent in the year of the center opening and about 60 percent two to three years after the opening. Figure 3.1 shows the rollout of the mother and health care centers across Norwegian municipalities between 1935–1955. Bütikofer et al. (2019) provide a more detailed description of the historical data.

3.3.2 Administrative Data

The central population register contains individuals' municipality of birth. We assign access to health care centers during the primary years of the centers' rollout (1936–1955) based on the municipality and year in which the directly exposed "first generation" was born. The main sample consists of the children born to these directly exposed individuals, whom we call the "second generation." This second generation sample includes individuals born between 1961 and 1988 and consists of 220,815 distinct mother-child pairs and 214,445 distinct father-child pairs. Focusing on these second generation cohorts simultaneously ensures that no individuals in the second generation were directly impacted by the rollout (health centers continued to be introduced in a few municipalities through the late 1950s) while including all births that occurred to the first generation between the ages of 25 and 33.[9] We focus on generational spillovers onto common measures of socioeconomic success including measures of educational attainment, earnings and IQ.

Educational attainment is taken from the educational registry database, and is measured in 2018. We consider total years of education and attainment along the education distribution by including indicator variables for completion of primary education (9 years),[10] high school (13 years) and university (16 years),[11] We also examine whether the individual was enrolled in an academic (vs. vocational) track during high school. The data are based on school reports sent directly from educational institutions to Statistics Norway, thereby minimizing any measurement error due to misreporting.

Annual earnings data are obtained from the tax registry and include labor earnings, taxable sickness benefits, unemployment benefits, and parental leave payments. They are not

[9]This sample restriction means that our analyses do not include second generation individuals whose parents were born at the beginning of the rollout period (late 1930s and early 1940s) and who gave birth at young ages. The resulting imbalance in parental age at birth across cohorts does not drive our results, as estimates are very similar when we restrict the analyses to second generation individuals whose mothers were older than 25 when they gave birth.

[10]We only consider this outcome for the first generation since completing 9 years of education was compulsory for the second generation. 99.9% of individuals in the second generation completed primary education.

[11]For the first generation, we use 15 years of education to denote a university degree, since the most common university degree at that time would have been completed in 15 years.

top-coded. For both generations, we use average earnings from 1967 to 2017, measured in 2015 Norwegian Kroner (NOK).

IQ scores are included in Norwegian military records, which are available for cohorts born from 1950 to 2010. In Norway, military service is compulsory for every male, and their medical and psychological suitability is assessed before they enter the service. For the great majority of men, this occurs between their eighteenth and twentieth birthday. Because women are not required to enlist, we do not have information on IQ for a representative population of females. The IQ measure that is reported is the individual's average score from three IQ tests that cover arithmetic, word similarities, and figures. We standardize this measure so that it has a mean of zero and standard deviation of one.

After documenting the program's impact on the second generation's education and earnings, we consider potential underlying mechanisms, including changes in their health at birth and in early adulthood, and changes in their parents' fertility patterns, partnering and migration decisions. We examine standard measures of infant health that are available from the Medical Birth Registry, which contains records for all births since 1967.[12] The records include information on date of birth, and variables related to infant health at birth, including the child's birthweight, whether the child was below the low birthweight threshold (under 2500 grams) and whether the child was born prematurely (before 37 weeks). Information on deaths occurring after the first year of life are obtained from the Cause of Death Registry. We obtain information on (male) adult height (measured in centimeters) from the Norwegian military records described above.

To investigate the program's impact on fertility patterns we consider the first generation's likelihood of ever having a child, number of births, age at first birth, and probability of giving birth as a teenager.[13] We obtain these outcomes from the central population registry. All of them have the potential to influence our second generation results either directly, or indirectly through selection into the sample.

We examine the program's impact on family structure by analyzing whether the father is missing from the birth certificate.[14] We also investigate whether program exposure influenced the type of partner chosen using information on partner's years of education, likelihood of enrolling in an academic high school track, and average earnings, all of which are provided in

[12]Restricting the second generation sample to cohorts born after 1967 does not substantively change our main results.

[13]We use WHO's definition of teenage pregnancy: pregnancy during adolescents year as defined by a pregnancy occurring under the age of 20.

[14]Information on birth certificates is is provided from 1967 onward by the Medical Birth Registry of Norway.

the registries described above. We identify an individual's partner from information on the first born child's mother and father that is provided on their birth certificate.

Finally, we conduct several exercises that are based on municipality level variables. First, we consider whether the program influenced the first generation's location decisions. Specifically, we consider whether, prior to the child's birth, an exposed parent was more likely to move to one of the seven largest urban areas in Norway,[15] and whether they were more likely to move out of a low income municipality. Information on individuals' municipality of residence is available annually from the central population register. Average income for each municipality is taken from the 1930 population census. We rank each municipality by its average income and use the ranking to determine whether the municipality's average income was above or below the median. A low income municipality is a municipality with an average income below the median.[16]

We also use municipality level measures to gauge whether generational persistence in the program's impact differed depending on the community's baseline levels of need. In addition to considering effect heterogeneity across high and low income municipalities, we also examine differences across municipalities with high vs. low infant mortality rates. We calculate infant mortality using data on infant births and deaths in 1920, which is available from the archive at the Norwegian Centre for Research Data.[17] Similar to our definition of high and low income municipalities, we define a municipality as having a high or low infant mortality rate based on whether it is above or below the median across all municipalities.[18]

3.4 Empirical Strategy

Our identification strategy is based on variation in exposure to the program resulting from the program's staggered rollout across municipalities. We replicate Bütikofer et al. (2019)'s estimates for the first generation using the following reduced form model:

$$y_{imt} = \alpha + \gamma D_{mt} + \beta X_{imt} + \lambda_m + \theta_t + \rho_m t + \varepsilon_{imt} \tag{3.1}$$

where y_{imt} are the outcomes of interest for individual i born in municipality m at time t. D_{mt}

[15]These urban areas are: Oslo, Bergen, Trondheim, Stavanger, Drammen, Fredrikstad, and Skien.

[16]There are four municipalities lacking information on average income in 1930: Ski, Ringeriket, Arendal and Hammerfest.

[17]Infant mortality is calculated as the number of deaths in the first year of life divided by the total number of live births, multiplied by 1000.

[18]There are seven municipalities lacking information on infants death rates in 1920: Ringeriket, Hole, Arendal, Sømna, Flakstad, Lavangen, and Hammerfest.

is an indicator variable equal to one if an individual is born in the year before, or after, the center opening in their municipality of birth, and zero otherwise.[19] X_{imt} controls for the individual's gender. We control for common time effects by including cohort fixed effects θ, and for non time-varying differences across municipalities by including municipality fixed effects λ. We also include municipality-specific time trends, $\rho_m t$, to ensure that our estimates are not contaminated by differential linear pre-trends. These local trends are estimated using the two-step procedure implemented in Goodman-Bacon (2021). That is, we estimate a linear trend using data only from years prior to the program adoption, and then extrapolate the estimated trend through all the years of data and subtract the predicted outcome from the observed outcome.

To understand whether the benefits associated with the first generation's access to infant health centers spills over to their children, we replace first generation outcomes with outcomes of the offspring to women and men who were born in municipality m at time t. We estimate the impacts of having an exposed mother or exposed father in separate regressions. In these second generation regressions, X_{imt} also includes an indicator for the offspring's gender, and fixed effects denoting the offspring's cohort. The variable of interest is γ, which shows the effect of a parent's access to infant health care on various second generation outcomes, including education, earnings, birth weight, adult height, and cognitive ability.

For comparability of the estimates across generations, the regressions are weighted such that each first generation parent gets equal weight. Standard errors are clustered at the municipality level to account for correlated errors within a municipality.

3.4.1 Event Study

To test for evidence of pre-trends we also estimate event study models:

$$Y_{imt} = \alpha + \sum_{k=-5,\neq -2}^{8} \gamma_t 1(t - T_m^* = k) + \beta X_{imt} + \lambda_m + \theta_t + \rho_m t + \varepsilon_{imt} \tag{3.2}$$

where y_{imt} is the reduced form outcome for individual i born in municipality m at time t. The key regressors are the series of dummy variables defined by the equation $1(t - T_m^* = k)$ that

[19] Note that we classify an individual as treated if she was born in the year before a center opened because infants were eligible for the services until the age of one year. Hence, many infants born in the year before a center opening had access to infant health care during their first year of life. As shown in panel E in Table B5 in Appendix 3.B, the estimates are qualitatively similar when treatment is defined in the year of or after a center opening as in Bütikofer et al. (2019).

take on a value of one for each event time year.[20] Since individuals born in the year before a center opens have access to a center most of their first year of life, we omit the year $k-2$, so that all γ estimates are relative to two years prior to a center opening. The γ coefficients nonparametrically trace out dynamic pre-treatment relative trends as well as dynamic time-varying treatment effects. Assuming that treatment effects are homogenous across treatment groups, Equation 3.1 and Equation 3.2 will produce unbiased estimates. We consider the importance of this assumption using the method proposed by Callaway and SantAnna (2020), which uses never-treated municipalities as controls, and obtain similar results for most of our estimates.

3.5 Results

3.5.1 First Generation Estimates

Table 3.1 provides our replication of Bütikofer et al. (2019) and documents that our first generation sample yields very similar results. In this and all future tables, estimated effects for binary outcomes are multiplied by 100.[21] The first panel shows that access to well-child visits in the first year of life increased the treated generation's education by 0.2 years, or 1.8 percent of the pre-treatment mean. These effects are observed throughout the education distribution, with a distinct shift towards high school completion and post high school education: access to infant health care centers increased the probability of completing primary education by 4 percent of the pre-reform mean, the probability of completing high school by 13 percent, and the probability of obtaining an advanced education by 5.6 percent. Consistent with this pattern, we also observe large increases in the likelihood of choosing an academic track in high school instead of a vocational track. The earnings estimates are also in line with Bütikofer et al. (2019), and indicate that access to the program increased the first generation's earnings by 2.4 percent. Take-up of the program was about 60 percent during the first few years of a center opening, so the estimated effects on the first generation's education and earnings translate into treatment on the treated estimates of 3 percent, and 4 percent.

Panel B and C in Table 3.1 replicate existing first generation results by gender, and show

[20] We bin event time observations that are more than four years before and more than eigth years after the event. Our full event windows spans from -19 to $+41$. Event years with less than 0.1% are only used as controls. For pre-event years this accounts for observations beyond -6 and for post-event year this applies to observations beyond $+19$.

[21] As a reminder, we do not expect our estimates to be exactly the same as Bütikofer et al. (2019) because we limit our first generation sample to individuals born from 1936 to 1955, while Bütikofer et al. (2019) include birth cohorts up to and including 1960. Another differences is that we define an individual as treated if they were born the year before, during, or in any year after program adoption, whereas Bütikofer et al. (2019) define an individual as treated if they were born in the year of, or after the program was adopted.

that the impacts are larger for men than women. First generation men who had access to the program experienced a 2.5 percent increase in their years of schooling, whereas the increase for women was 1 percent. Exposed men were also more likely to get an advanced degree and had higher earnings compared to their unexposed counterparts, while the estimates for women are small and insignificant.[22] This is unsurprising given that labor market opportunities differed substantially between men and women born in the 1930s–1950s, but it may be useful to keep in mind when we consider the program's impacts on the next generation.[23]

3.5.2 Second Generation Estimates

Table 3.2 shows estimates of the effects of the first generation's access to infant health care on the next generation's adult outcomes. The top of panel A shows estimates for those whose mothers had access to well-child visits during their first year of life, and the bottom half provides estimates for those who had an exposed father. It is immediately clear that the impacts of program spilled over to the next generation's human capital and earnings, but only among second generation offspring who had treated mothers. Estimates for offspring whose fathers had early life access to the program are substantially smaller, and are not statistically significant. Data constraints have restricted most multi-generational studies to examination of maternal linkages, leaving the role of paternal transmission largely unexplored, but our finding is consistent with Akee et al. (2010); Duflo (2003) and Black et al. (2005b) who find that children benefit more from increases in parents' human capital and financial resources when the targeted parent is the mother.

Relative to children whose mothers did not have access to well-child visits, those whose mothers were treated grew up to have 0.09 additional years of education, an increase of 0.7 percent relative to the pre-treatment mean. Notably, these gains were more concentrated towards the top of the distribution: the probability of completing an advanced education increased by 7.4 percent of the pre-reform mean, and the probability of choosing an academic high school track increased by 4.5 percent. Since educational attainment was increasing in Norway during this period,[24] it is unsurprising that, relative to first generation, effects for the second generation are shifted further to the top of the distribution. As median education levels increased across generations, mothers whose own educational attainment was positively affected by the program and who made commensurate human capital investments in their

[22]Table B4 in Appendix 3.B shows that the estimates are statistically different from each other.

[23]We do not replicate Bütikofer et al. (2019)'s investigation of mechanisms because the health survey data that were analyzed in their prior work cannot be linked to our intergenerational sample.

[24]See Table 3.1 and Table 3.2

children, may have encouraged their children to signal their higher skill levels by obtaining a level of education that was higher than the second generation's median. We also see that treated mothers' offspring have earnings that are 1.8 percent higher than those whose mothers did not have access to the program.

Our estimates provide strong evidence that the rollout of infant health care centers had long-lasting effects that persisted beyond the treated generation. The magnitude of the spillover effect is large. Using the estimated first generation effects for women in the first column of Table 3.1 together with the second generation estimates in Table 3.2 we calculate an intergenerational transmission coefficient for educational attainment of about 0.75, which is similar to previous estimates of the intergenerational transmission of cognitive ability (see, e.g., Black et al., 2019).

The next two panels in Table 3.2, present results separately for men and women. Across the two genders, the magnitudes of the estimates are quite similar and not statistically different from each other,[25] so moving forward our analyses will continue to group second generation men and women together. In panel B the estimated effect of treated fathers on their son's likelihood of completing high school becomes statistically significant, but is still substantially smaller than the estimated effect of having a treated mother. A nice feature of our data is that we are also able to analyze impacts on male IQ, which are shown in the last column of the table. As with education and earnings, we see that the impacts of first generation access to well-child visits persist to the next generation's IQ, but only when the exposed parent is the mother. Maternal access to the program in early childhood leads to a statistically significant increase in her child's IQ score of 0.05 standard deviations. To put this estimate into perspective, Black et al. (2011) find that for 1962–1988 cohorts each additional year of age is associated with about a 0.1 standard deviation increase in IQ. As another comparison, (Black et al., 2007) find that a 10 percent increase in birthweight is associated with a 0.03 standard deviation increase in IQ.[26]

3.5.3 Sensitivity Analyses

Table A5 shows that our second generation estimates are robust to a large number of specification checks.[27]. First, we estimate a version of Equation 3.1 that excludes second generation individuals whose parents were born during World War II. This is important, as Norway was occupied by the Nazis from April 1940-May 1945, and food was scarce during this time.

[25]See Table A4 in Appendix 3.A for the interaction effect by gender.

[26]This estimate is based on cohorts born in Norway between 1967 and 1987.

[27]We show results for a complete set of specification checks for the first generation in Table B5 in Appendix 3.B

Second generation individuals whose parents were born during WWII might, therefore, differ from those whose parents were born during other periods because of differences in their parents' nutritional endowments. Excluding these individuals does not affect the estimates.[28]

Next, we estimate a version of the baseline model that controls for changes in Norwegian compulsory schooling laws that increased the educational attainment of cohorts born during the second half of the rollout period (1946–1961) Black et al. (2005a).[29] This check is important because some parents in our sample were simultaneously exposed to the health care centers and to the new compulsory schooling requirements. We control for this potential contaminant by including an indicator equal to 1 when a first generation cohort and municipality of birth is affected by the school reform. Including this control does not substantively change the estimates.

Panel C shows what happens when we re-estimate Equation 3.1 but define a first generation parent as treated if they were born in the same year (or after) a center opened, rather than defining a parent as treated if they were born in the year before the center opened. Again, the estimates are very similar to those in Table 3.2, although, as expected, they fall slightly when we move the first treatment year forward.[30,31]

In panels D and E we show that estimated effects are unlikely to be driven by underlying municipality specific trends. Estimates are similar to our baseline results when we include a cubic municipality-specific trend, and when we eliminate municipality-specific trends altogether. Similarly, in Figure A4, which provides event study estimates for second generation children of exposed mothers, none of the outcomes exhibit pre-trends. Positive jumps in outcomes around the time of program adoption are consistent with the magnitude of the difference-in-differences estimates in Table 3.2.[32] Event study estimates for children of treated

[28]Big cities and points of strategic interest, such as Narvik, were most affected by the occupation. Geographic variation in the impacts of the war could also potentially bias our estimates.

[29]In 1959, the Norwegian Parliament increased the number of years of compulsory schooling from seven to nine years. The reform was gradually implemented across the country between 1960 and 1972.

[30]As another robustness check, Bütikofer et al. (2019) also estimate maternal fixed effects models. To invoke the same strategy in this study we would need to estimate grandmother fixed effect models and compare outcomes among second generation cousins for whom one first generation mother was treated and one first generation mother was not. The number of first generation sisters who had different treatment status and who both become mothers is substantially smaller than the total number of first generation siblings who could be analyzed in Bütikofer et al. (2019), however, so we do not have the power to estimate grandmother fixed effect models with any precision.

[31]We perform the same robustness check for first generation in Table B5. Across all specifications results are similar to those in Table 3.1. Results are available in Appendix 3.B.

[32]A challenge with the event study analysis is that several of the larger municipalities in our sample adopted the program during the early part of our sample period. This limits the number of pre-adoption

fathers are shown in Figure A5 in Appendix 3.A.

A remaining concern with our research design is that the estimates are contaminated by the presence of heterogeneous treatment effects (Sun and Abraham, 2020). We address this concern by implementing a new estimation approach proposed by Callaway and SantAnna (2020) that is robust to heterogeneous treatment effects across treatment timing groups. Intuitively, this approach avoids using earlier treated municipalities as controls for later treated municipalities (which can lead to biased estimates). The results for treated mothers are shown in Figure A8 and are broadly similar to the main estimates.[33]

Next, following Bütikofer et al. (2019), we consider the possibility that our results are driven by the program's impact on the first generation's mortality. In our case, a program induced increase in the first generation's probability of survival would be expected to influence second generation estimates through two mechanisms: the "selection effect" will bias our estimates downward if the additional first generation survivors come from the lower end of the underlying health distribution and pass their health (and associated outcomes) onto their later offspring. In contrast, the "scarring effect" – the direct effect of the disease environment proxied by the lower infant mortality rate – would be expected to generate improvements in both first and second generation outcomes Bozzoli et al. (2009). Hatton (2011) suggests that in early 20th century Europe the scarring effect was more important than the selection effect, nevertheless, we consider the potential importance of selection into the sample by dropping 1% of the treatment group at different percentiles of the education and earnings distributions.[34] Table A6 shows that across all sub-samples, the estimates are comparable to our main results.[35]

Finally, since we are testing multiple hypotheses against a single treatment variable, Ta-

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years of data that they are able to contribute to the event study, and may raise concerns about whether the absence of observable pre-trends results from the unbalanced nature of the municipality data, with some municipalities contributing more years of pre-adoption information than others. To address this concern, we have also estimated event study models adding cohorts born in 1934 and 1935, which allows us to include more pre-adoption event years for those municipalities that adopted early. The results of this exercise are shown in Figure A6 and Figure A7 and are very similar to those in Figure A4 and Figure A5. Results are available in Appendix 3.A. The extended sample is not preferred, however, because we have less complete data coverage for cohorts born prior to 1936.

[33]The results for treated fathers using Callaway and SantAnna (2020) estimation approach are shown in Figure A9 in Appendix 3.A. Figure B6 in Appendix 3.B implements Callaway and SantAnna (2020) estimation approach for the first generation and shows that the results are robust to heterogeneous treatment effects.

[34]Bütikofer et al. (2019) find that the Norwegian mother-child centers increased exposed infants' probability of surviving into their second year by about 1–4%.

[35]Table B6 in Appendix 3.B shows the results for the first generation when dropping 1% of the treatment group at different percentiles. Our results are not altered by this exercise.

ble A7 shows results adjusted for multiple hypotheses using the *rwolf* STATA package (Clarke et al., 2020).[36] On average, the re-sampled Romano-Wolf adjusted p-values are slightly larger than those associated with Table 3.2, but our second generation estimates remain statistically significant at conventional significance levels.

3.5.4 Mechanisms

What are the mechanisms generating these intergenerational spillovers, and why do they operate largely through treated mothers? As discussed in Section 3.2, exposure could be transmitted across generations through biological and/or socioeconomic channels. Our finding that treated mothers matter more than treated fathers is somewhat puzzling in light of the fact that the economic returns to center exposure were larger for first generation men than women. One possibility is that mothers have a higher tendency to direct resources towards their children,[37] but in order to explore this possibility more directly we need information on household expenditures that we do not have. The dominance of maternal effects could also reflect program induced improvements in the second generation's in utero environment, selective changes in the first generation's fertility, or changes in other maternal behaviors that result from the program's impacts on the first generation's health and human capital. We consider these mechanisms below.

Second Generation Health Outcomes

Previous studies document that the first generation's access to health centers reduced their likelihood of experiencing metabolic syndrome and cardiovascular disease in adulthood (Bütikofer et al., 2019; Hjort et al., 2017; Bhalotra et al., 2017).[38] The presence of these conditions during pregnancy puts offspring at risk for poor birth outcomes (Catalano and Ehrenberg, 2006), which negatively impact individuals' later life education and success in the labor market (see, e.g., Almond et al., 2018; Black et al., 2007; Figlio et al., 2014). Therefore, it is natural to investigate whether the program's effects on second generation economic outcomes resulted in part from program induced improvements in their early life health.

Table 3.3 provides results from examining the program's impact on measures of the second generation's health that are readily available from birth and military records. Specifically, we

[36] The Romano-Wolf method is implemented with 1000 replications of the main specification, with re-sampling clustered at the municipality level.

[37] (Armand et al., 2020; Rubalcava et al., 2009; Bobonis, 2009; Ward-Batts, 2008; Attanasio and Lechene, 2002; Lundberg et al., 1997; Thomas, 1993)

[38] Hjort et al. (2017) and Bhalotra et al. (2017) examine first generation effects of centers similar to the Norwegian health care centers in Denmark and Sweden.

use Equation 3.1 to investigate the program's effect on the second generation's birthweight, likelihood of being born below the low birthweight threshold (2500 grams), and likelihood of being born prematurely.[39] We also examine height, which is recorded for males when they register with the military. Height is affected by nutrition in the first years of life (Deaton, 2007; Rivera et al., 1995) and height increased among first generation cohorts who were exposed to the health centers (Bütikofer et al., 2019). Moreover, height is known to be correlated across generations, and may be passed from mother to child through non-genetic mechanisms (Venkataramani, 2011). We find no evidence that these common measures of early and young adult health persisted to the second generation, however. Most of the estimates in Table 3.3 do not approach conventional levels of statistical significance. The one exception is the estimate in column (ii), which indicates a large reduction in the incidence of low birthweight among children with treated fathers, but this estimate does not hold up to further sensitivity analyses.[40] The lack of persistent health effects may reflect the continued evolution of health care in Norway: by the 1960s, well-child visits following national guidelines had been established in all municipalities, and the vast majority of births occurred in hospitals. The second generation's equal health care access may therefore partly compensate for differences between treated and untreated mothers' health and human capital.

Selection

Next, we consider changes in sample composition. It is possible that instead of reflecting "real" improvements in the second generation's economic outcomes, the estimates in Table 3.2 result instead from changes in the types of individuals we observe becoming parents. In particular, previous research suggests that program induced improvements in first generation women's human capital and earnings may have lead to delays in child bearing and reductions in their desired number of children (Black et al., 2008).

We consider these possibilities in panel A of Table 3.4. Focusing on the first two columns, we find no evidence that the program affected first generation women's probability of giving birth or having a teenage birth, but we do see a statistically significant decline in completed fertility that is equivalent to about 2 percent of the baseline mean. Since second generation regressions are weighted such that each first generation parent gets equal weight, we should not be concerned that selection is driving the second generation estimates in Table 3.2, but it is possible that the program's effect on the first generation's completed fertility allowed

[39] Data on infants health are only available for cohorts born after 1967, but Table A8 confirms that restricting our economic analyses to these cohorts does not qualitatively affect the estimates.

[40] See event-study estimates Figure C5 for treated fathers and Figure C4 for treated mothers in Appendix 3.A

some parents to invest more time and resources in their (slightly fewer) children. We also see that, counter to expectation, age at first birth is one tenth of a year younger among first generation women who had access to well-child visits during their first year of life (0.5 percent of the baseline mean). Existing research on the relationship between maternal age and children's outcomes (Duncan et al., 2018, 2017), suggests that this phenomenon should bias the estimates in 3.2 downward, however, and is unlikely to explain the second generation's improved economic outcomes.

Looking at the last column, we note that the estimated effects on completed fertility and age at first birth are similar for treated men and treated women. Therefore, program induced changes in these outcomes are unlikely to explain why treated mothers have a bigger influence on children's economic outcomes. Interestingly, the program had a statistically significant positive effect on men's probability of ever having a child, but no effect on women. This is consistent with previous work by (Schaller, 2016; Aksoy, 2016) documenting that improvements in men's labor market opportunities increase men's fertility but improvements in women's labor market opportunities reduce women's fertility.

Assortative Mating

We next consider whether the program affected the first generation's partnering decisions. Here, there are at least two types of changes that could be important. First, the program may have changed the first generation's probability of marrying or co-habitating with the child's other parent. A long literature documents that children growing up in single parent families fair worse than those growing up in two parent families, so this is an important consideration. To investigate, we examine whether treatment changed the probability that the father's information is missing from the second generation child's birth certificate, which is a strong indicator for the child's likelihood of being born into a single parent household. The estimate is shown in the first row of panel B in Table 3.4 and does not approach statistical significance.[41]

A second possibility is that the program induced improvements in the first generation's human capital lead to changes in marital sorting. This might happen if the additional education received by treated women changed their probability of meeting certain types of partners,

[41]Marriage is not a good indicator for whether the child is born into a single vs. two parent family because during the time period under consideration formal marriage rates in Norway were declining rapidly in favor of co-habitation. Moreover, this trend was more pronounced for highly educated women. We might therefore expect that treated mothers would be less likely to marry their child's father. Indeed, we do observe that the share of married mothers declines with treatment, but this does not inform the question of whether treatment altered the next generation's likelihood of growing up in a two parent family. Therefore we do not include the results in Table 3.4.

or altered their preferences. Previous work suggests that marital sorting can have important implications for the intergenerational transmission of socioeconomic status (Holmlund, 2022; Butler et al., 2008; Mare and Maralani, 2006). Under this scenario, the program's impact on the second generation would reflect both the direct effects of mothers' higher skill levels and indirect effects resulting from changes in the composition of parents within the household.

Consistent with this premise, the next row in panel B shows that exposed women partnered with men who were better educated and had life-time earnings that were significantly higher than the partners of women who were not exposed. Specifically, we find that the partners of exposed mothers had 0.25 more years of education, and were 10 percent more likely to have been enrolled in an academic track than the partners of mothers who were not exposed. We also see that their partner's earnings were about 3 percent higher.[42] Therefore, treated mothers' positive impacts on their offspring's outcomes reflect the combined effects of program induced increases in their own human capital as well as ensuing improvements in the characteristics of their partners. In particular, children with treated mothers ended up with fathers who had higher levels of education and earnings than children of untreated mothers. Whether the second generation effects are due to additional financial resources or "better" paternal genes cannot be determined.

In contrast, while our estimates also indicate that exposed fathers partnered with more educated women, the magnitude of the estimates is much smaller, and treatment does not affect their propensity to chose mates with high earnings. The differences in the partnering profiles of exposed mothers and fathers might explain why the program's spillover effects are concentrated on second generation offspring whose mothers were exposed.[43]

To gauge the importance of the program induced change in maternal partner choice, we next consider what happens to our baseline estimates when we add controls for parental education to Equation 3.1. Table 3.5 shows how the estimated effect of having a treated mother is affected. Panel A replicates the main estimates from Table 3.2. Panel B shows that the main estimates change little when we restrict our sample to second generation offspring for

[42]The estimates are similar to estimates for the full sample of exposed women, including those who did not have children.

[43]One might ask whether the estimated improvement in treated mothers' partner characteristics arise because treated mothers are more likely to marry treated fathers, as would be expected if partners tend to be close in age and grow up in close geographic proximity. Table A9 shows that more than 30 percent of our second generation sample has only one treated parent. In addition, Table A10 shows the results from regressions similar to Equation 3.1 that include both parents in the regression simultaneously. These regressions show clearly that the impact of treated mothers on the second generation's outcomes dominates that of treated fathers, and that the estimates in Table 3.2 are not driven by treated mothers tendency to marry treated fathers.

whom we have information on both mothers and fathers. In panel C we see that controlling for maternal education reduces the second generation estimates by 25-50%, suggesting that a substantial part of the program's generational persistence is due to the center's effect on treated mothers' education. Simultaneously controlling for the mother's education and that of her partner, further reduces the estimates although the estimated effect on the second generation's earnings remains substantive and statistically different from zero at the 90 percent confidence level. Our take away from these analyses is that the program's effect on the second generation's education outcomes can be largely explained by its effects on first generation mothers education and marital responses, but that these responses are not enough to explain the program's effect on the second generation's earnings.

Migration

Finally, we consider the potential role of program induced migration. As existing literature documents substantial positive sorting of individuals into urban areas (see, e.g., Combes et al., 2012; Roca and Puga, 2017), the program's effect on the first generation's human capital may have also lead to changes in where parents chose to live. Indeed, panel C of Table 3.4 shows that both men and women who had access to infant health care were substantially more likely to move to one of Norway's urban areas before having their first child. Although not statistically different, the estimates for women are somewhat bigger than those for men (13 percent vs. 8 percent).

During this period, earnings and education in Norwegian cities were higher than in more rural areas (Bennett et al., 2021). The next row of panel C shows that treated men and women were also more likely to move out of low income areas, with the estimates for women being statistically larger than those for men. Much has been written about cities' human capital externalities (see, e.g., Moretti, 2004), which include potential spillovers onto children. Moreover, there is emerging consensus that the place in which a child grows up has a substantial causal effect on the child's prospects for upward mobility (Chetty et al., 2014, 2016; Chetty and Hendren, 2018; Chyn, 2018; Deutscher, 2018; Laliberté, 2021; Nakamura et al., 2016). The estimates in Table 3.4 would therefore seem to suggest that changes in the first generation's location could be an important mechanism. However, when we restrict our main analyses to non-movers in Table 3.6, the resulting estimates are similar in magnitude to those in Table 3.2. This suggests that migration to more economically beneficial areas is not driving our results for the second generation.

3.5.5 Heterogeneity

The final set of results focuses on heterogeneity. As described in Section 3.2, infant health centers were available to all mothers free of charge, but reaching poor families was an explicit goal of the program. We would like to be able to directly investigate the program's effect on upward mobility across multiple generations – specifically whether second generation gains were larger among those whose parents were born into families with low socioeconomic status – but we only have relevant information (e.g. education) for the selected subset of generation zero that survived until 1967 (roughly 65 percent of the first generation's parents). In lieu of conducting standard mobility analyses, we split the sample according to whether the first generation was born into a municipality with an average income that was above- or below-the municipality level median. We call municipalities with income levels below the median "high poverty" areas and municipalities with above median income "low poverty" areas. We also conduct parallel analyses where we split the sample by whether the first generation's municipality of birth was above or below the median infant mortality rate. Results are provided in Table 3.7 and suggest that the program had bigger effects on both the likelihood of obtaining an advanced degree and on earnings among second generation individuals whose parents were born into more disadvantaged areas.

3.6 Conclusion

Existing literature documents that early childhood health interventions have long lasting effects on individuals' economic outcomes, making such policies strong candidates for breaking the cycle of poverty. To date, however, we know little about the extent to which the economic impacts of policy driven health investments persist to later generations, and even less about underlying mechanisms. Understanding these effects can shed light on why economic status is linked across generations, and provide insights on the extent to which government interventions might be able to compensate for inequalities that are perpetuated by historical differences in early life environments. Evidence of spillovers would also suggest that standard benefit-cost calculations understate the value of early life investments.

Despite these questions' importance, there are few contexts that allow for causal examination. We overcome this challenge by combining historical information on the timing of infant health care center openings across Norwegian municipalities with administrative data on over 200,000 linked parent-child pairs. We show that the economic returns to early life investments are strongly persistent across generations, but that the magnitude of the return varies substantially with the treated parent's gender: children whose mothers were exposed to during their first year of life grew up to have 0.05 standard deviation higher IQ scores, 0.09 more years of schooling, and 1.8 percent higher earnings, while estimates for children whose fathers were exposed are substantially smaller, and rarely statistically significant. This finding adds to a small but growing body of evidence that children benefit more from increases in parents' resources when the targeted parent is the mother.

Our results do not appear to be driven by changes in the first generation's fertility behavior, selection into childbearing, or intergenerational transmission of common early life health measures. Instead, we find evidence that exposed first generation women partnered with men who were better educated and had higher earnings. Treated mothers' positive impacts on their children's later economic success therefore reflect the combined effects of program induced increases in their own human capital as well as subsequent improvements in the characteristics of their partners. Whether these "partnering effects" result from improved financial resources or paternal genes cannot be determined, but would be a fascinating (and challenging!) avenue for future research.

Lastly, we find that that second generation effects were larger among offspring whose parents were born into more disadvantaged areas. This is a hopeful finding to the extent that persistent group differences in economic outcomes reflect persistent differences in groups' baseline health that result from historical disparities in early life environments. While low-income children continue to face significant health and educational disadvantages, universal health care can offset some of these initial shortcomings and reduce economic inequalities that persist across generations.

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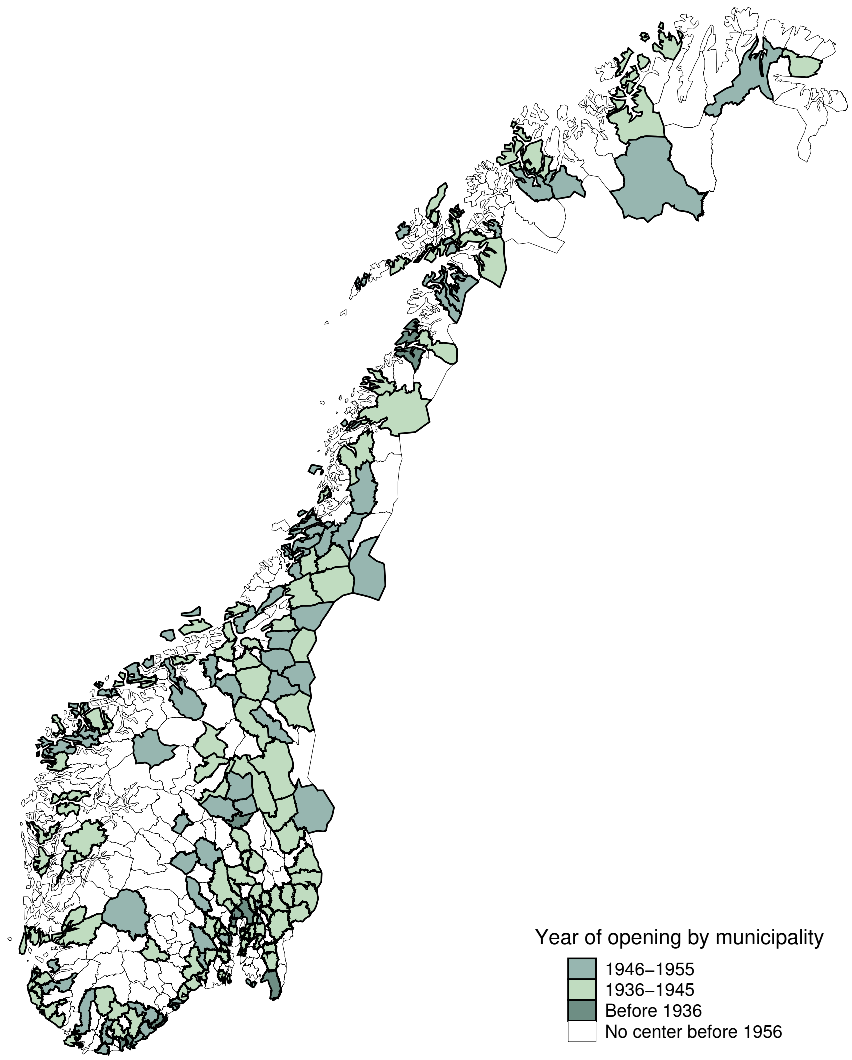
3.7 Tables and Figures

Figure 3.1: Rollout of Mother and Child Health Care Centers



Notes: The map displays Norway's 428 municipalities. The different colors indicate when the first NKS mother and child health care center was opened in these municipalities. There were no NKS mother and child healthcare centers opened in the white municipalities in the period of interest.

Table 3.1: First Generation Education and Labor Market Outcomes

	Education registry					Earnings registry
	Total years of education	Primary education	High school	Academic high school track	Advanced education	Average earnings 1967-2017
	(i)	(ii)	(iii)	(iv)	(v)	(vi)
	Panel A: Full sample					
Center exposure	0.200***	3.312***	3.292***	3.200***	0.587*	4903.776***
	(0.039)	(0.814)	(0.538)	(0.861)	(0.313)	(1705.854)
Observations	348511	348511	348511	348511	348511	349741
Pre-treatment mean	11.06	80.39	25.10	31.77	10.39	207394.3
	Panel B: Men					
Center exposure	0.292***	4.012***	5.014***	3.537***	1.168***	8024.609***
	(0.048)	(0.904)	(0.743)	(0.739)	(0.445)	(2449.497)
Observations	176753	176753	176753	176753	176753	177888
Pre-treatment mean	11.49	80.54	33.25	31.31	12.71	287669.1
	Panel C: Women					
Center exposure	0.107**	2.585***	1.516***	2.896**	-0.013	1371.948
	(0.042)	(0.821)	(0.579)	(1.133)	(0.343)	(1369.436)
Observations	171758	171758	171758	171758	171758	171853
Pre-treatment mean	10.69	80.91	17.84	33.23	8.40	128045.1

Notes: Each column is from a separate regression on access to a mother and child health care center. Robust standard errors adjusted for clustering at the level of the municipality of birth are shown in parentheses. Birth cohorts are born between 1936–1955. Health care centers opened between 1936 to 1955. Earnings presented are average discounted earnings from 1967 to 2017. All specifications include a full set of cohort and municipality fixed effects, municipality specific pre-treatment trend and a dummy variable controlling for the gender of the individual. Significance levels: *** 1% level, ** 5% level, * 10% level.

Table 3.2: Second Generation Education and Labor Market Outcomes

	Education registry				Earnings registry	Military registry
	Total years of education (i)	High school (ii)	Academic high school track (iii)	Advanced education (iv)	Average earnings 1967–2017 (v)	Ability (vi)
	Panel A: Full sample					
Treated mother	0.091***	1.950***	2.393***	0.788***	5617.071***	0.050***
	(0.024)	(0.493)	(0.575)	(0.288)	(1884.192)	(0.015)
Observations	348882	348882	348882	348882	349549	162840
Pre-treatment mean	12.29	50.90	53.03	10.63	307361.8	-0.02
Treated father	0.030	0.484	0.446	0.156	2247.241	0.013
	(0.022)	(0.427)	(0.577)	(0.276)	(1460.618)	(0.014)
Observations	331897	331897	331897	331897	332462	155142
Pre-treatment mean	12.33	52.33	55.13	10.81	307418.7	-0.02
	Panel B: Men					
Treated mother	0.085***	2.077***	2.448***	0.545	7533.792***	0.050***
	(0.030)	(0.696)	(0.702)	(0.335)	(2506.465)	(0.015)
Observations	178594	178594	178594	178594	179038	162840
Pre-treatment mean	12.24	51.58	46.84	8.65	362969.9	-0.02
Treated father	0.054**	1.144**	0.542	0.232	3483.628	0.013
	(0.026)	(0.534)	(0.683)	(0.315)	(2239.377)	(0.014)
Observations	170153	170153	170153	170153	170530	155142
Pre-treatment mean	12.27	52.91	48.58	8.92	361427.7	-0.02
	Panel C: Women					
Treated mother	0.098***	1.808***	2.362***	1.068**	3731.986**	N/A
	(0.028)	(0.550)	(0.685)	(0.417)	(1890.981)	
Observations	170288	170288	170288	170288	170511	
Pre-treatment mean	12.35	50.18	59.48	12.68	249438.9	
Treated father	0.006	-0.160	0.461	0.103	1481.786	N/A
	(0.031)	(0.684)	(0.731)	(0.400)	(1371.795)	
Observations	161744	161744	161744	161744	161932	
Pre-treatment mean	12.39	51.72	62.09	12.82	249872	

Notes: Each column is from a separate regression on access to a mother and child health care center for treated generation. Robust standard errors adjusted for clustering at the level of the municipality of birth for treated generation are shown in parentheses. All regressions are weighted by the number of siblings. Birth cohorts for second generation are born between 1961–1988. Health care centers opened between 1936 to 1955. Earnings presented are average discounted earnings from 1967 to 2017. All specifications include a full set of cohort and municipality fixed effects, municipality specific pre-treatment trend, this generations year-of-birth fixed effects and a dummy variable controlling for the gender of the individual. Significance levels: *** 1% level, ** 5% level, * 10% level.

Table 3.3: Second Generation Health at Birth Outcome

	Birth registry			Military registry
	Birthweight (i)	Low birthweight (ii)	Premature (iii)	Height (cm) (iv)
Treated mother	6.177	-0.185	-0.500	0.020
	(9.493)	(0.257)	(0.355)	(0.084)
Observations	280798	280798	269276	172624
Pre-treatment mean	3511.30	4.27	9.11	179.51
Treated father	4.517	-0.454**	-0.167	-0.147
	(6.513)	(0.184)	(0.266)	(0.095)
Observations	288490	288490	275668	164669
Pre-treatment mean	3513.48	4.12	8.68	179.68

Notes: Each column is from a separate regression on access to a mother and child health care center for treated generation. Robust standard errors adjusted for clustering at the level of the municipality of birth for treated generation are shown in parentheses. All regressions are weighted by the number of siblings. Birth cohorts for are born between 1967–1988. Health care centers opened between 1936 to 1955. All specifications include a full set of cohort and municipality fixed effects, municipality specific pre-treatment trends, this generations year-of-birth fixed effects and a dummy variable controlling for the gender of the individual. Column (i)–(iii) includes cohorts born 1967–1988 and column (iv) include cohorts born 1961–1988. Significance levels: *** 1% level, ** 5% level, * 10% level.

Table 3.4: Mechanisms First Generation

	Female		Male	
	Pre-reform mean	Original controls	Pre-reform mean	Original controls
	(i)	(ii)	(iii)	(iv)
	Panel A: Fertility outcomes			
Ever having a child	91.26	0.003	86.05	1.048***
		(0.312)		(0.352)
Observations		173341		178326
Completed fertility	2.74	-0.061***	2.66	-0.045***
		(0.015)		(0.014)
Observations		157357		153683
Age at first child	23.44	-0.109**	26.61	-0.195***
		(0.049)		(0.061)
Observations		157357		153683
Teenage pregnancy	14.26	0.360	2.29	0.090
		(0.422)		(0.228)
Observations		157357		153683
Missing father	5.63	-0.148		N/A
		(0.318)		
Observations		131608		
	Panel B: Partner characteristics			
Total years of education	11.30	0.254***	11.00	0.075**
		(0.050)		(0.035)
Observations		154009		152589
Academic high school track	28.92	2.796***	38.03	1.599*
		(0.743)		(0.850)
Observations		154009		152589
Average earnings 1967-2017	279908.26	7829.537***	133693.23	2505.397*
		(2806.450)		(1351.463)
Observations		155154		152749
	Panel C: Location characteristics			
Moved to urban area	19.81	2.621*	19.37	1.502**
		(1.357)		(0.742)
Observations		173169		178228
Moved out of low income area	25.66	8.075***	29.18	6.028***
		(1.618)		(1.290)
Observations		173169		178228

Notes: Each column is from a separate regression on access to a mother and child health care center. Robust standard errors adjusted for clustering at the level of the municipality of birth are shown in parentheses. Birth cohorts are born between 1936–1955. Health care centers opened between 1936 to 1955. Earnings presented are average discounted earnings from 1967 to 2017. All outcomes in panel A and panel B, except "Ever having a child", are based on a sample of individuals having a child. All specifications include a full set of cohort and municipality fixed effects, municipality specific pre-treatment trend and a dummy variable controlling for the gender of the individual. Significance levels: *** 1% level, ** 5% level, * 10% level.

Table 3.5: Second Generation Education and Labor Market Outcomes: Controlling for First Generation Mother's and her Partner's Education and Income

	Education registry				Earnings registry	Military registry
	Total years of education (i)	High school (ii)	Academic high school track (iii)	Advanced education (iv)	Average earnings 1967–2017 (v)	Ability (vi)
	Panel A: Main estimates					
Treated mother	0.091***	1.950***	2.393***	0.788***	5617.071***	0.050***
	(0.024)	(0.493)	(0.575)	(0.288)	(1884.192)	(0.015)
Observations	348882	348882	348882	348882	349549	162840
	Panel B: First generation sample with non-missing partner information					
Treated mother	0.087***	1.875***	2.357***	0.730**	5605.785***	0.046***
	(0.024)	(0.497)	(0.588)	(0.298)	(1872.818)	(0.016)
Observations	341600	341600	341600	341600	342124	159720
	Panel C: Controlling for first generation mother's education					
Treated mother	0.056**	1.466***	1.222**	0.366	4323.382**	0.015
	(0.022)	(0.476)	(0.537)	(0.279)	(1835.057)	(0.013)
Observations	341600	341600	341600	341600	342124	159720
	Panel D: Controlling for partner's education of first generation's mothers					
Treated mother	0.045*	1.315***	0.767	0.230	3806.929**	0.010
	(0.024)	(0.489)	(0.590)	(0.292)	(1763.807)	(0.014)
Observations	341600	341600	341600	341600	342124	159720
	Panel E: Controlling for first generation mother's education, and her partner's education					
Treated mother	0.036	1.195**	0.439	0.122	3434.356*	-0.001
	(0.023)	(0.478)	(0.563)	(0.283)	(1773.072)	(0.013)
Observations	341600	341600	341600	341600	342124	159720
	Panel F: Controlling for first generation mother's average earnings					
Treated mother	0.076***	1.724***	1.943***	0.610**	4924.074***	0.036**
	(0.023)	(0.487)	(0.538)	(0.289)	(1815.051)	(0.014)
Observations	341600	341600	341600	341600	342124	159720
	Panel G: Controlling for partner's average earnings of first generation's mothers					
Treated mother	0.070***	1.621***	1.743***	0.545*	4089.128**	0.034**
	(0.022)	(0.470)	(0.541)	(0.288)	(1815.223)	(0.015)
Observations	341600	341600	341600	341600	342124	159720
	Panel H: Controlling for first generation mother's average earnings, and her partner's average earnings					
Treated mother	0.063***	1.524***	1.472***	0.467	3685.502**	0.028*
	(0.022)	(0.466)	(0.514)	(0.284)	(1787.888)	(0.014)
Observations	341600	341600	341600	341600	342124	159720
	Panel I: Controlling for first generation mother's education and average earnings, and her partner's education and average earnings					
Treated mother	0.034	1.173**	0.410	0.114	3275.688*	-0.001
	(0.022)	(0.465)	(0.537)	(0.281)	(1768.052)	(0.013)
Observations	341600	341600	341600	341600	342124	159720

Notes: Each column is from a separate regression on access to a mother and child health care center for treated generation. Robust standard errors adjusted for clustering at the level of the municipality of birth for treated generation are shown in parentheses. All regressions are weighted by the number of siblings. Birth cohorts for second generation are born between 1961–1988. Health care centers opened between 1936 to 1955. Earnings presented are average discounted earnings from 1967 to 2017. All specifications include a full set of cohort and municipality fixed effects, municipality specific pre-treatment trend, this generations year-of-birth fixed effects and a dummy variable controlling for the gender of the individual. Significance levels: *** 1% level, ** 5% level, * 10% level.

Table 3.6: Second Generation Education and Labor Market Outcomes: Non-movers

	Education registry				Earnings registry	Military registry
	Total years of education (i)	High school (ii)	Academic high school track (iii)	Advanced education (iv)	Average earnings 1967–2017 (v)	Ability (vi)
Treated mother	0.090***	2.020***	1.854***	0.791**	5967.730***	0.040**
	(0.026)	(0.523)	(0.652)	(0.312)	(2135.032)	(0.017)
Observations	285297	285297	285297	285297	285893	133087
Pre-treatment mean	12.25	50.16	50.91	10.07	305427.1	-0.06
Treated father	0.039*	0.686	0.435	0.247	2262.516	0.012
	(0.024)	(0.464)	(0.637)	(0.304)	(1585.317)	(0.015)
Observations	278385	278385	278385	278385	278879	130125
Pre-treatment mean	12.29	51.70	52.92	10.21	305778.7	-0.06

Notes: Each column is from a separate regression on access to a mother and child health care center for treated generation. Robust standard errors adjusted for clustering at the level of the municipality of birth for treated generation are shown in parentheses. All regressions are weighted by the number of siblings. Birth cohorts for second generation are born between 1961–1988. Health care centers opened between 1936 to 1955. Earnings presented are average discounted earnings from 1967 to 2017. All specifications include a full set of cohort and municipality fixed effects, municipality specific pre-treatment trend, this generations year-of-birth fixed effects and a dummy variable controlling for the gender of the individual. Significance levels: *** 1% level, ** 5% level, * 10% level.

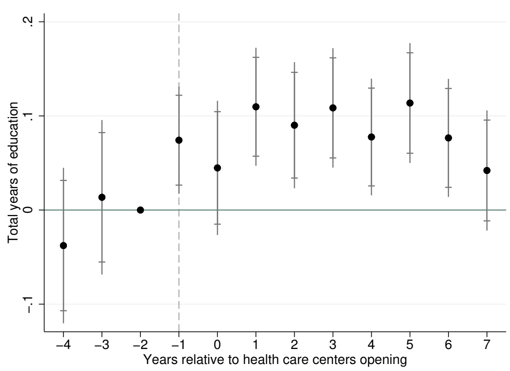
Table 3.7: Second Generation Education and Labor Market Outcomes by Pre-Reform Municipality Poverty and Infant Mortality Levels

	Education registry				Earnings registry	Military registry
	Total years of education (i)	High school (ii)	Academic high school track (iii)	Advanced education (iv)	Average earnings 1967-2017 (v)	Ability (vi)
	Panel A: Low poverty areas					
Treated mother	0.071*	1.862**	1.513**	0.625	1791.784	0.034
	(0.037)	(0.766)	(0.717)	(0.433)	(2657.159)	(0.021)
Observations	233057	233057	233057	233057	233523	108596
Pre-treatment mean	12.30	50.93	52.10	10.53	314854.6	0.02
	Panel B: High poverty areas					
Treated mother	0.073**	1.169*	2.848***	0.652*	6490.563**	0.055**
	(0.030)	(0.663)	(0.878)	(0.375)	(2486.566)	(0.023)
Observations	110378	110378	110378	110378	110555	51684
Pre-treatment mean	12.29	50.89	53.48	10.67	303866.9	-0.03
	Panel C: Low infant mortality areas					
Treated mother	0.091***	1.957***	2.489***	0.671	3084.903	0.041*
	(0.033)	(0.671)	(0.807)	(0.482)	(2913.341)	(0.023)
Observations	138181	138181	138181	138181	138389	64570
Pre-treatment mean	12.36	52.10	54.09	11.15	310509.6	-0.00
	Panel D: High infant mortality areas					
Treated mother	0.101***	1.979**	2.538***	1.139***	7487.927***	0.069***
	(0.035)	(0.783)	(0.883)	(0.368)	(2392.847)	(0.019)
Observations	203809	203809	203809	203809	204248	95063
Pre-treatment mean	12.36	52.92	55.80	11.24	308863.9	-0.01

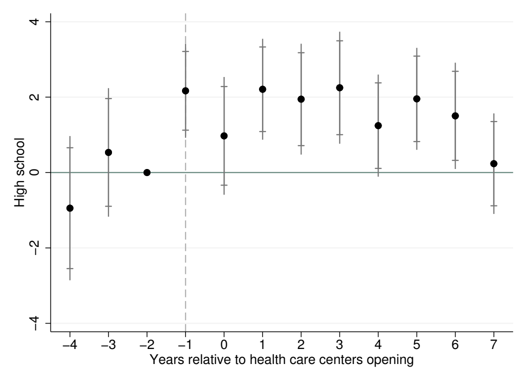
Notes: Each column is from a separate regression on access to a mother and child health care center for treated generation. Robust standard errors adjusted for clustering at the level of the municipality of birth for treated generation are shown in parentheses. All regressions are weighted by the number of siblings. Birth cohorts for second generation are born between 1961–1988. Health care centers opened between 1936 to 1955. Earnings presented are average discounted earnings from 1967 to 2017. All specifications include a full set of cohort and municipality fixed effects, municipality specific pre-treatment trend, this generations year-of-birth fixed effects and a dummy variable controlling for the gender of the individual. Significance levels: *** 1% level, ** 5% level, * 10% level.

3.A Appendix Sensitivity Analyses: Main Second Generation Estimates

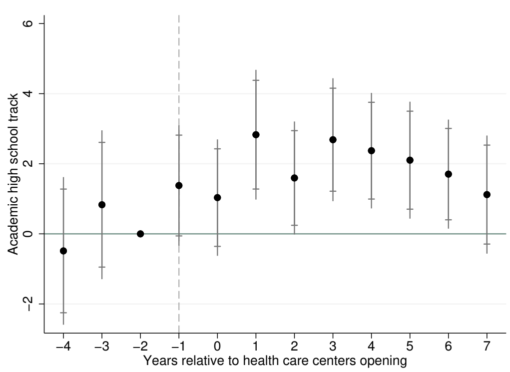
Figure A4: Event Study Graphs for Second Generations' Education and Labor Market Outcomes: Treated Mothers



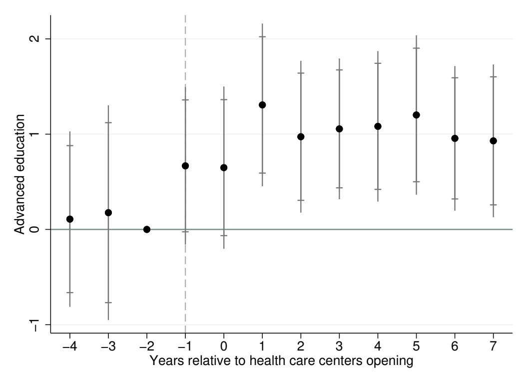
(a) Total years of education



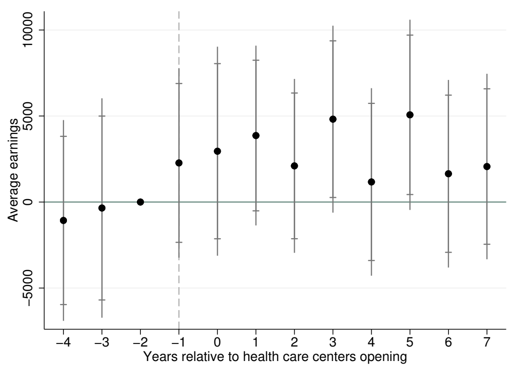
(b) High school



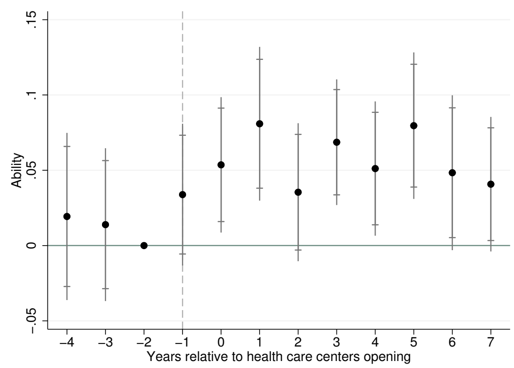
(c) Academic high school track



(d) Advanced education



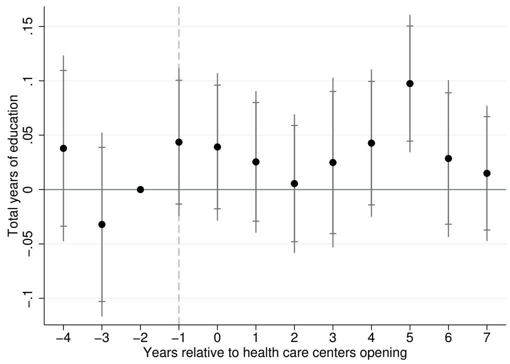
(e) Average earnings 1967–2017



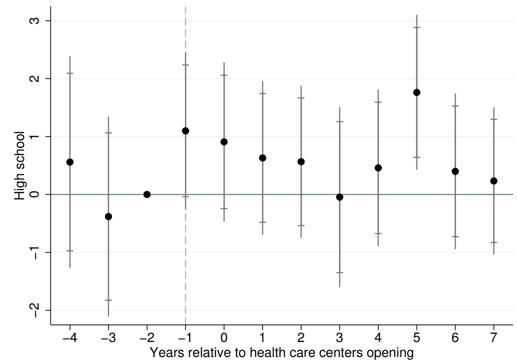
(f) Ability

Notes: Each figure is from a separate regression on access to a mother and child health care center for treated generation. Robust standard errors adjusted for clustering at the level of the municipality of birth for treated generation are shown in parentheses. All regressions are weighted by the number of siblings. Birth cohorts for second generation are born between 1961–1988. Health care centers opened between 1936 to 1955. Earnings presented are average discounted earnings from 1967 to 2017. All specifications include a full set of cohort and municipality fixed effects, municipality specific pre-treatment trend, this generations year-of-birth fixed effects and a dummy variable controlling for the gender of the individual. Significance levels: *** 1% level, ** 5% level, * 10% level.

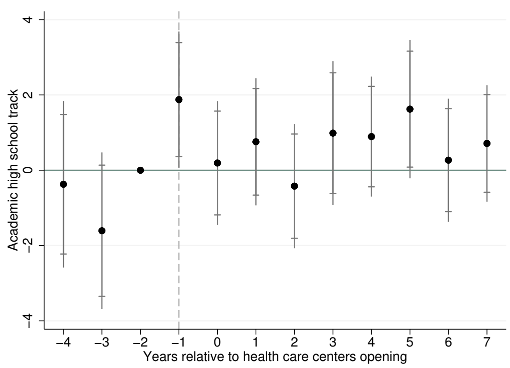
Figure A5: Event Study Graphs for Second Generations' Education and Labor Market Outcomes: Treated Fathers



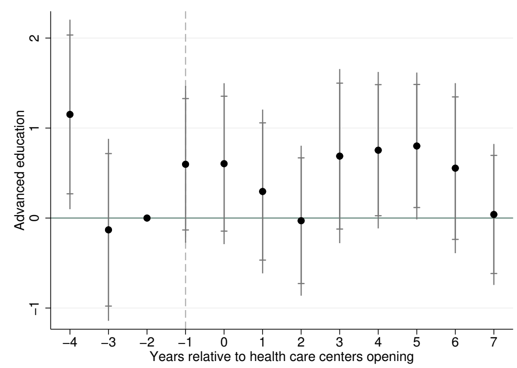
(a) Total years of education



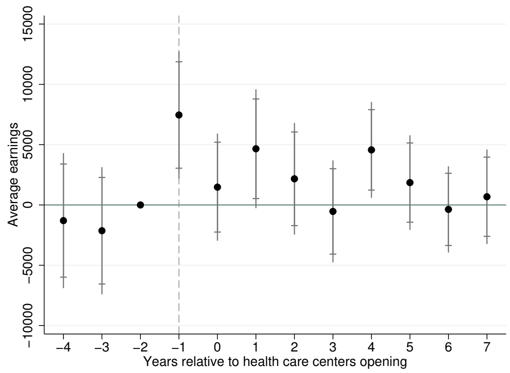
(b) High school



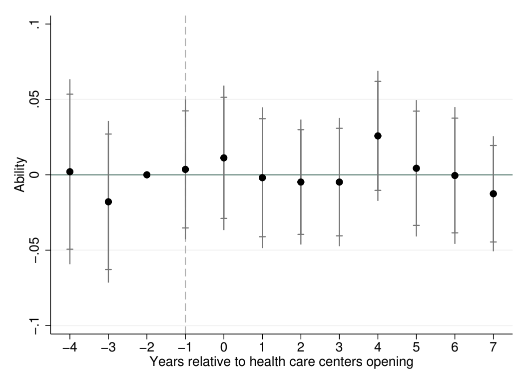
(c) Academic high school track



(d) Advanced education



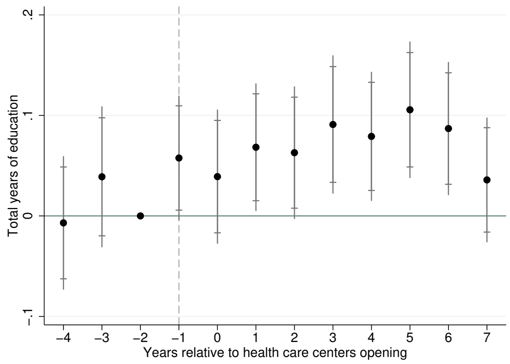
(e) Average earnings 1967–2017



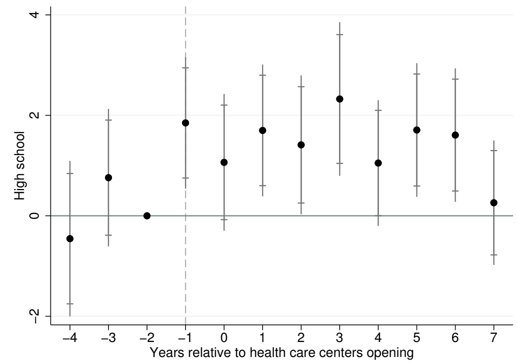
(f) Ability

Notes: Each figure is from a separate regression on access to a mother and child health care center for treated generation. Robust standard errors adjusted for clustering at the level of the municipality of birth for treated generation are shown in parentheses. All regressions are weighted by the number of siblings. Birth cohorts for second generation are born between 1961–1988. Health care centers opened between 1936 to 1955. Earnings presented are average discounted earnings from 1967 to 2017. All specifications include a full set of cohort and municipality fixed effects, municipality specific pre-treatment trend, this generations year-of-birth fixed effects and a dummy variable controlling for the gender of the individual. Significance levels: *** 1% level, ** 5% level, * 10% level.

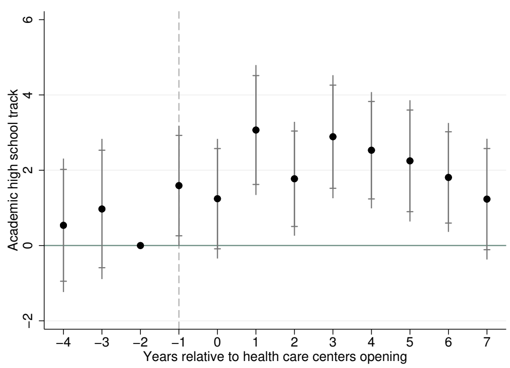
Figure A6: Event Study Graphs for Second Generations' Education and Labor Market Outcomes for Treated Mothers: Extended First Generation Born 1934–1955



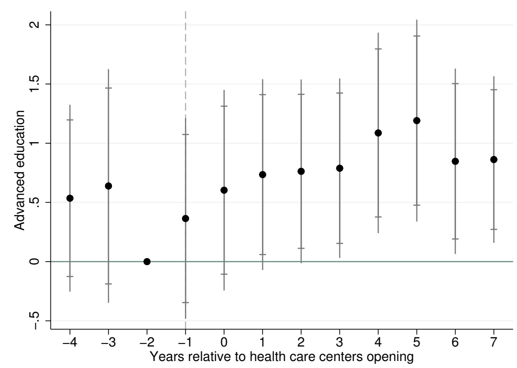
(a) Total years of education



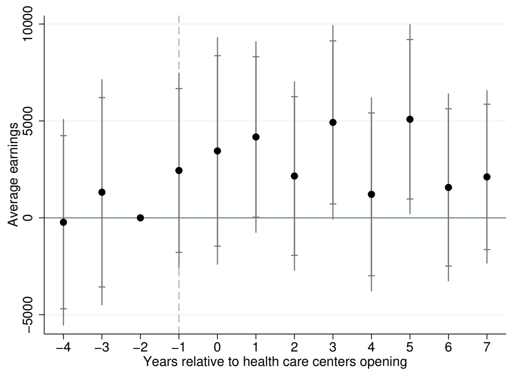
(b) High school



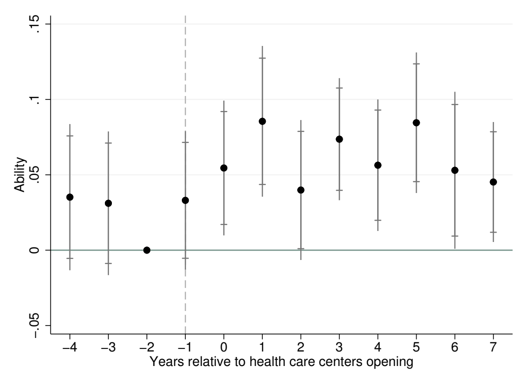
(c) Academic high school track



(d) Advanced education



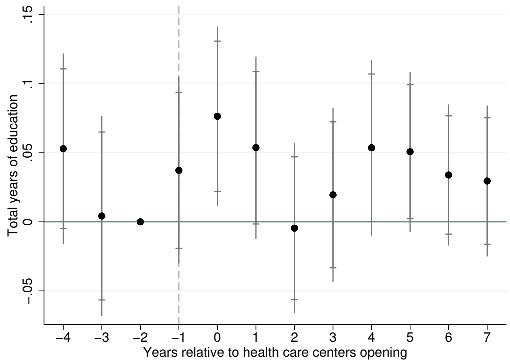
(e) Average earnings 1967–2017



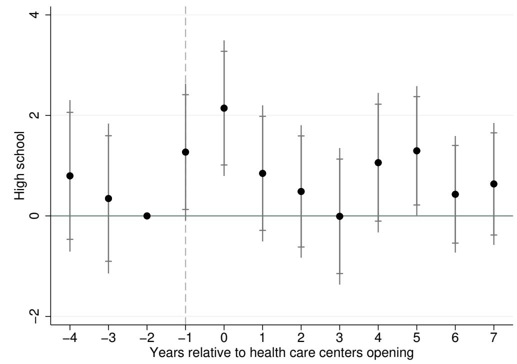
(f) Ability

Notes: Each figure is from a separate regression on access to a mother and child health care center for treated generation. Robust standard errors adjusted for clustering at the level of the municipality of birth for treated generation are shown in parentheses. All regressions are weighted by the number of siblings. Birth cohorts for second generation are born between 1961–1988. Health care centers opened between 1936 to 1955. Earnings presented are average discounted earnings from 1967 to 2017. All specifications include a full set of cohort and municipality fixed effects, municipality specific pre-treatment trend, this generations year-of-birth fixed effects and a dummy variable controlling for the gender of the individual. Significance levels: *** 1% level, ** 5% level, * 10% level.

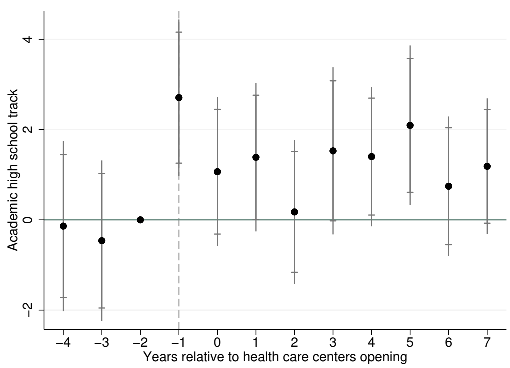
Figure A7: Event Study Graphs for Second Generations' Education and Labor Market Outcomes for Treated Fathers: Extended First Generation Born 1934–1955



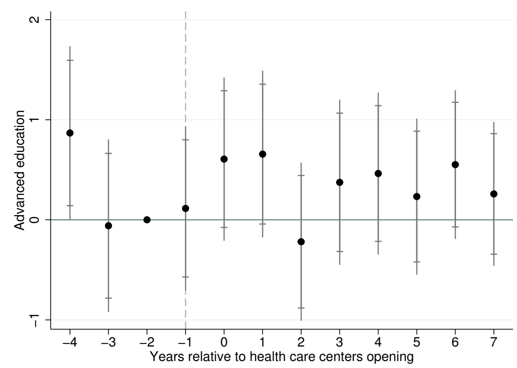
(a) Total years of education



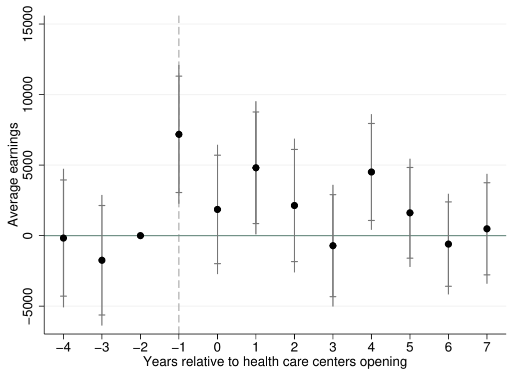
(b) High school



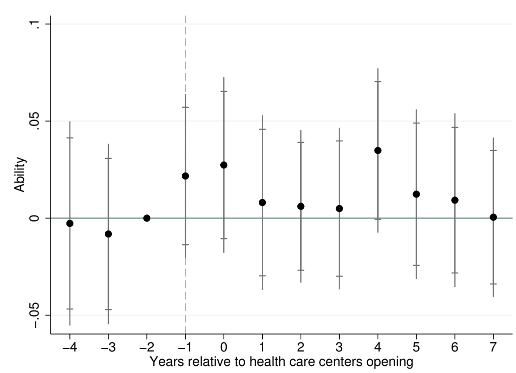
(c) Academic high school track



(d) Advanced education



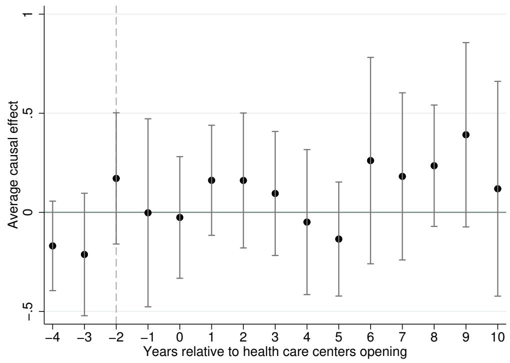
(e) Average earnings 1967–2017



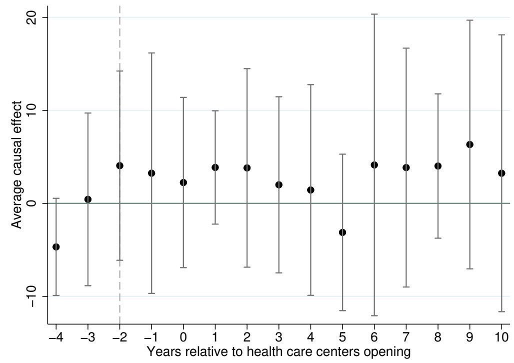
(f) Ability

Notes: Each figure is from a separate regression on access to a mother and child health care center for treated generation. Robust standard errors adjusted for clustering at the level of the municipality of birth for treated generation are shown in parentheses. All regressions are weighted by the number of siblings. Birth cohorts for second generation are born between 1961–1988. Health care centers opened between 1936 to 1955. Earnings presented are average discounted earnings from 1967 to 2017. All specifications include a full set of cohort and municipality fixed effects, municipality specific pre-treatment trend, this generations year-of-birth fixed effects and a dummy variable controlling for the gender of the individual. Significance levels: *** 1% level, ** 5% level, * 10% level.

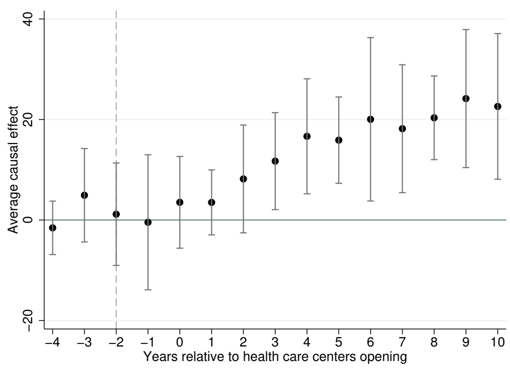
Figure A8: Event Study Graphs for Second Generations' Education and Labor Market Outcomes with Treated Mothers: Robustness to Callaway and Sant'Anna



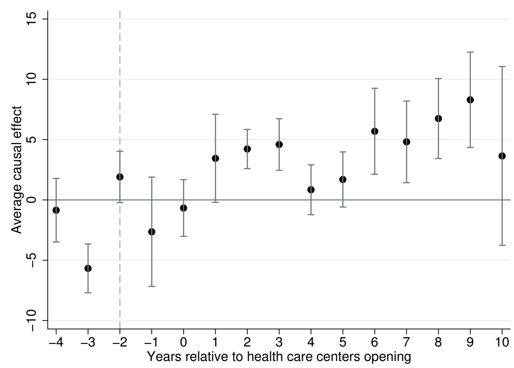
(a) Total years of education



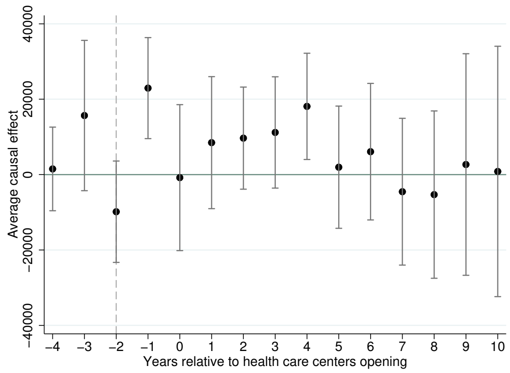
(b) High school



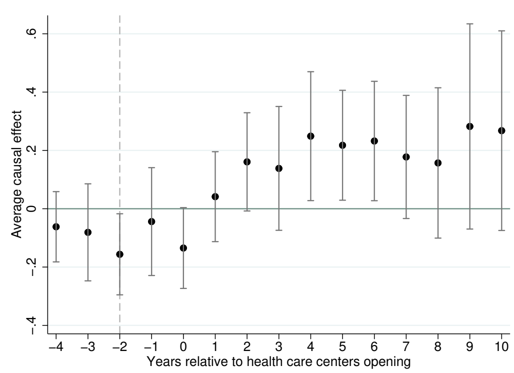
(c) Academic high school track



(d) Advanced education



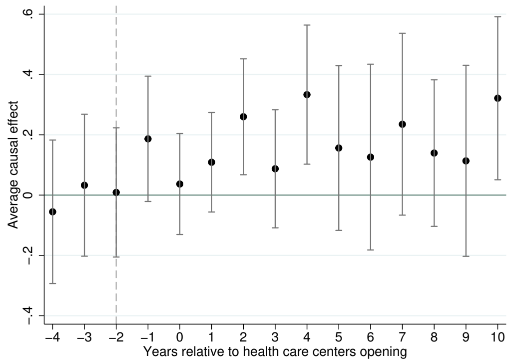
(e) Average earnings 1967–2017



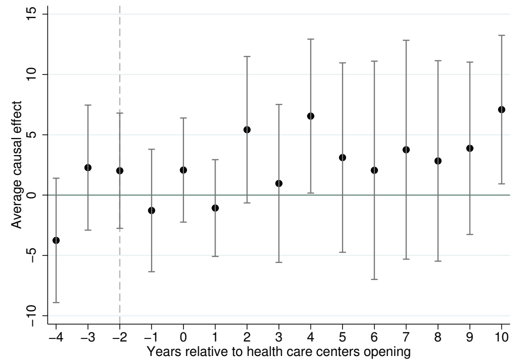
(f) Ability

Notes: Each figure is from a separate regression on access to a mother and child health care center for treated generation. Birth cohorts for second generation are born between 1961–1988. Health care centers opened between 1936 to 1955. Earnings presented are average discounted earnings from 1967 to 2017. All specifications include a full set of cohort and municipality fixed effects, this generations year-of-birth fixed effects and a dummy variable controlling for the gender of the individual. Municipalities never receiving a health centers used as control.

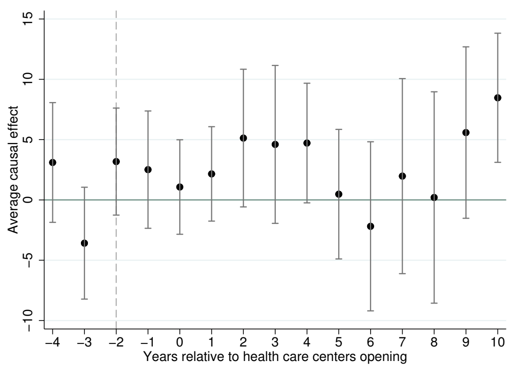
Figure A9: Event Study Graphs for Second Generations' Education and Labor Market Outcomes with Treated Fathers: Robustness to Callaway and Sant'Anna



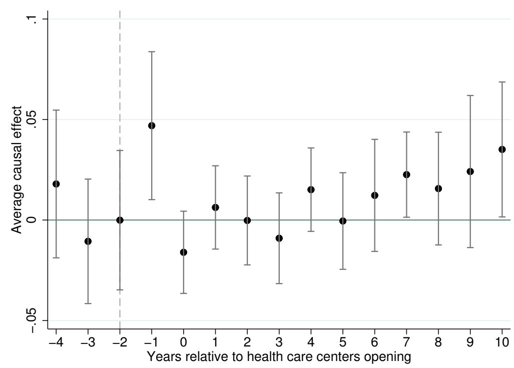
(a) Total years of education



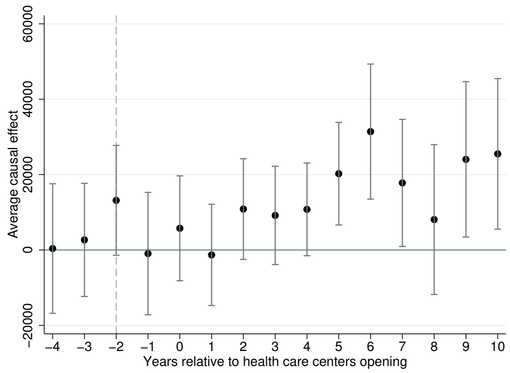
(b) High school



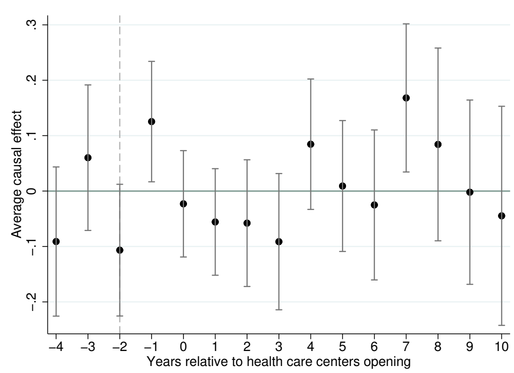
(c) Academic high school track



(d) Advanced education



(e) Average earnings 1967–2017



(f) Ability

Notes: Each figure is from a separate regression on access to a mother and child health care center for treated generation. Birth cohorts for second generation are born between 1961–1988. Health care centers opened between 1936 to 1955. Earnings presented are average discounted earnings from 1967 to 2017. All specifications include a full set of cohort and municipality fixed effects, this generations year-of-birth fixed effects and a dummy variable controlling for the gender of the individual. Municipalities never receiving a health centers used as control.

Table A4: Second Generation Education and Labor Market Outcomes by Gender

	Education registry				Earnings registry
	Total years of education (i)	High school (ii)	Academic high school track (iii)	Advanced education (iv)	Average earnings 1967-2017 (v)
Treated mother	0.085***	2.077***	2.448***	0.545	7533.792**
	(0.030)	(0.697)	(0.703)	(0.335)	(2507.166)
Treated mother × female	0.013	-0.269	-0.086	0.523	-3801.806
	(0.034)	(0.786)	(0.784)	(0.489)	(2381.718)
Female	0.060	-2.424	5.022**	3.202***	-135477.582***
	(0.068)	(2.064)	(1.968)	(0.889)	(6571.033)
Observations	348882	348882	348882	348882	349549
Treated father	0.006	-0.160	0.461	0.103	1481.786
	(0.031)	(0.685)	(0.731)	(0.401)	(1372.189)
Treated father × female	0.048	1.304	0.081	0.130	2001.842
	(0.036)	(0.875)	(0.806)	(0.462)	(2339.714)
Female	-0.034	1.670	-3.947*	-2.440*	134412.395***
	(0.097)	(2.781)	(2.071)	(1.372)	(6768.306)
Observations	331897	331897	331897	331897	332462

Notes: Each column is from a separate regression on access to a mother and child health care center for treated generation. Robust standard errors adjusted for clustering at the level of the municipality of birth for treated generation are shown in parentheses. All regressions are weighted by the number of siblings. Birth cohorts for second generation are born between 1961–1988. Health care centers opened between 1936 to 1955. Earnings presented are average discounted earnings from 1967 to 2017. All specifications include a full set of cohort and municipality fixed effects, municipality specific pre-treatment trend, this generations year-of-birth fixed effects and a dummy variable controlling for the gender of the individual. Significance levels: *** 1% level, ** 5% level, * 10% level.

Table A5: Robustness Second Generation Education and Labor Market Outcomes

	Education registry				Earnings registry	Military registry
	Total years of education (i)	High school (ii)	Academic high school track (iii)	Advanced education (iv)	Average earnings 1967–2017 (v)	Ability (vi)
	Panel A: Excluding cohorts born during WWII					
Treated mother	0.081***	1.480***	2.016***	0.964***	5581.321**	0.063***
	(0.029)	(0.555)	(0.712)	(0.358)	(2378.723)	(0.020)
Observations	269046	269046	269046	269046	269482	125073
Treated father	0.044	0.781	1.197*	0.356	1540.232	0.045***
	(0.027)	(0.545)	(0.675)	(0.360)	(1830.733)	(0.016)
Observations	253694	253694	253694	253694	254071	118127
	Panel B: Controlling for school reform					
Treated mother	0.093***	1.951***	2.397***	0.807***	5512.581***	0.050***
	(0.023)	(0.492)	(0.583)	(0.287)	(1864.495)	(0.015)
Observations	348882	348882	348882	348882	349549	162840
Treated father	0.031	0.519	0.503	0.176	2149.689	0.015
	(0.021)	(0.423)	(0.576)	(0.265)	(1499.413)	(0.014)
Observations	331897	331897	331897	331897	332462	155142
	Panel C: Original treatment					
Treated mother	0.067***	1.086**	1.950***	0.720**	4327.066**	0.049***
	(0.024)	(0.508)	(0.561)	(0.288)	(1955.583)	(0.015)
Observations	348882	348882	348882	348882	349549	162840
Treated father	0.017	0.180	0.505	0.091	1039.217	0.010
	(0.024)	(0.466)	(0.579)	(0.295)	(1522.534)	(0.014)
Observations	331897	331897	331897	331897	332462	155142
	Panel D: No-trend					
Treated mother	0.092***	1.968***	2.412***	0.787***	5674.216***	0.051***
	(0.024)	(0.494)	(0.574)	(0.289)	(1885.843)	(0.015)
Observations	348882	348882	348882	348882	349549	162840
Treated father	0.026	0.442	0.362	0.154	2254.868	0.013
	(0.022)	(0.420)	(0.579)	(0.278)	(1463.326)	(0.014)
Observations	331897	331897	331897	331897	332462	155142
	Panel E: Cubic trend					
Treated mother	0.093***	1.975***	2.360***	0.811***	5529.709***	0.051***
	(0.024)	(0.498)	(0.570)	(0.289)	(1885.844)	(0.015)
Observations	345599	345599	345599	345599	346266	161389
Treated father	0.025	0.434	0.308	0.162	2229.737	0.012
	(0.022)	(0.422)	(0.582)	(0.280)	(1456.685)	(0.014)
Observations	328758	328758	328758	328758	329316	153747

Notes: Each column is from a separate regression on access to a mother and child health care center for treated generation. Robust standard errors adjusted for clustering at the level of the municipality of birth for treated generation are shown in parentheses. All regressions are weighted by the number of siblings. Birth cohorts for second generation are born between 1961–1988. Health care centers opened between 1936 to 1955. Earnings presented are average discounted earnings from 1967 to 2017. All specifications include a full set of cohort and municipality fixed effects, municipality specific pre-treatment trend, this generations year-of-birth fixed effects and a dummy variable controlling for the gender of the individual. Significance levels: *** 1% level, ** 5% level, * 10% level.

Table A6: Second Generation Estimate Sensitivity to Dropping Different Percentiles of the Treatment Group

	Baseline (i)	Dropping 60^{th} percentile (ii)	Dropping 70^{th} percentile (iii)	Dropping 80^{th} percentile (iv)	Dropping 90^{th} percentile (v)	Dropping 100^{th} percentile (vi)
	Panel A: Total years of education					
Treated mother	0.091***	0.094***	0.089***	0.092***	0.090***	0.092***
	(0.024)	(0.024)	(0.024)	(0.024)	(0.024)	(0.024)
Observations	348882	345376	345376	345402	345373	345377
Treated father		0.027	0.031	0.028	0.030	0.029
		(0.022)	(0.022)	(0.022)	(0.022)	(0.022)
Observations	348882	328596	328574	328572	328568	328580
	Panel B: Average earnings 1967-2017					
Treated mother	5617.071***	5640.683***	5575.420***	5660.605***	5473.899***	5136.236***
	(1884.192)	(1874.507)	(1922.015)	(1881.559)	(1894.270)	(1882.379)
Observations	349549	346029	346088	346063	346081	346080
Treated father	2247.241	2308.314	2278.567	2850.678*	1995.603	1739.893
	(1460.618)	(1452.015)	(1450.656)	(1510.321)	(1498.210)	(1449.841)
Observations	332462	329142	329115	329165	329183	329170

Notes: Each column is from a separate regression on access to a mother and child health care center for treated generation. Robust standard errors adjusted for clustering at the level of the municipality of birth for treated generation are shown in parentheses. All regressions are weighted by the number of siblings. Birth cohorts for second generation are born between 1961–1988. Health care centers opened between 1936 to 1955. Earnings presented are average discounted earnings from 1967 to 2017. All specifications include a full set of cohort and municipality fixed effects, municipality specific pre-treatment trend, this generations year-of-birth fixed effects and a dummy variable controlling for the gender of the individual. In column (ii) we drop individuals in the 60^{th} percentile of the predicted education or income distribution. In columns (ii)-(vi) this procedure is repeated for the 70^{th}, 80^{th}, 90^{th} and 100^{th} percentiles. Significance levels: *** 1% level, ** 5% level, * 10% level.

Table A7: Multiple Hypothesis Testing

	Model p-value (i)	Romano-Wolf p-value (ii)
	Panel A: First generation	
Total years of education	0.0000	0.0020
Primary education	0.0001	0.0020
High school	0.0000	0.0020
Academic high school track	0.0003	0.0020
Advanced education	0.0621	0.0020
Average earnings 1967-2017	0.0045	0.0020
	Panel B: Second generation treated mother	
Total years of education	0.0002	0.0020
High school	0.0001	0.0020
Academic high school track	0.0000	0.0020
Advanced education	0.0068	0.0020
Average earnings 1967-2017	0.0033	0.0020
Ability	0.0014	0.0020

Notes: Each row is from a separate regression on access to a mother and child health care center for treated generation. Model P-value and P-value from Romano-Wolf multiple hypothesis correction is shown.

Table A8: Second Generation Education and Labor Market Outcomes for Individuals Born After 1967

	Education registry				Earnings registry	Military registry
	Total years of education (i)	High school (ii)	Academic high school track (iii)	Advanced education (iv)	Average earnings 1967–2017 (v)	Ability (vi)
Treated mother	0.103***	1.848***	2.701***	0.812**	6271.88***	0.062***
	(0.024)	(0.512)	(0.608)	(0.344)	(1764.895)	(0.018)
Observations	283255	283255	283255	283255	283635	131751
Pre-treatment mean	12.44	54.35	57.70	11.77	306689.9	-0.02
Treated father	0.020	0.245	0.554	-0.050	1346.044	0.013
	(0.023)	(0.459)	(0.637)	(0.318)	(1586.233)	(0.016)
Observations	290280	290280	290280	290280	290663	135171
Pre-treatment mean	12.44	54.98	58.40	11.67	306300.5	-0.01

Notes: Each column is from a separate regression on access to a mother and child health care center for treated generation. Robust standard errors adjusted for clustering at the level of the municipality of birth for treated generation are shown in parentheses. All regressions are weighted by the number of siblings. Birth cohorts for second generation are born between 1961–1988. Health care centers opened between 1936 to 1955. Earnings presented are average discounted earnings from 1967 to 2017. All specifications include a full set of cohort and municipality fixed effects, municipality specific pre-treatment trend, this generations year-of-birth fixed effects and a dummy variable controlling for the gender of the individual. Significance levels: *** 1% level, ** 5% level, * 10% level.

Table A9: Second Generation Treatment Status of Parents

Treatment status mother	Treatment status father		
	0	1	Total
0	104,028	74,866	178,894
1	78,009	243,862	321,871
Total	182,037	318,728	500,765

Table A10: Second Generation Education and Labor Market Outcomes: Controlling for Both Parents Treatment Status

	Education registry				Earnings registry	Military registry
	Total years of education (i)	High school (ii)	Academic high school track (iii)	Advanced education (iv)	Average earnings 1967–2017 (v)	Ability (vi)
Treated mother	0.100***	1.894***	2.761***	0.824***	6594.866***	0.050***
	(0.030)	(0.674)	(0.704)	(0.305)	(2190.698)	(0.018)
Treated father	0.024	0.528	0.170	-0.102	1888.775	0.001
	(0.026)	(0.493)	(0.633)	(0.315)	(1815.420)	(0.014)
Observations	294868	294868	294868	294868	295233	138724

Notes: Each column is from a separate regression on access to a mother and child health care center for treated generation. Robust standard errors adjusted for clustering at the level of the municipality of birth for treated generation are shown in parentheses. All regressions are weighted by the number of siblings. Birth cohorts for second generation are born between 1961–1988. Health care centers opened between 1936 to 1955. Earnings presented are average discounted earnings from 1967 to 2017. All specifications include a full set of cohort and municipality fixed effects, municipality specific pre-treatment trend, this generations year-of-birth fixed effects and a dummy variable controlling for the gender of the individual. Significance levels: *** 1% level, ** 5% level, * 10% level.

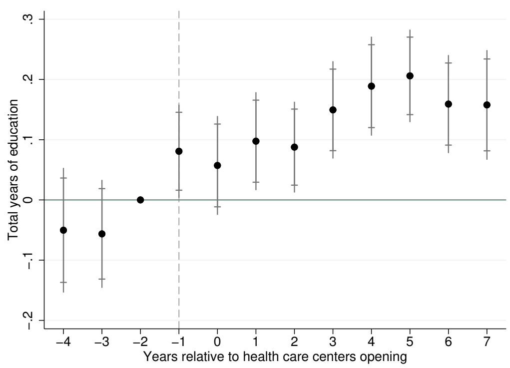
Table A11: Second Generation Education and Labor Market Outcomes by Pre-Reform Municipality Poverty and Infant Mortality Levels: Non-movers

	Education registry				Earnings registry	Military registry
	Total years of education (i)	High school (ii)	Academic high school track (iii)	Advanced education (iv)	Average earnings 1967-2017 (v)	Ability (vi)
	Panel A: Low poverty areas					
Treated mother	0.059	1.654**	0.568	0.505	2958.025	0.026
	(0.039)	(0.783)	(0.884)	(0.493)	(2882.872)	(0.023)
Observations	194083	194083	194083	194083	194506	90497
Pre-treatment mean	12.25	50.27	50.20	9.98	312326.5	-0.03
	Panel B: High poverty areas					
Treated mother	0.088**	1.585**	2.790***	0.828**	5957.789**	0.042
	(0.034)	(0.732)	(0.940)	(0.400)	(2729.348)	(0.027)
Observations	86732	86732	86732	86732	86885	40490
Pre-treatment mean	12.24	50.11	51.26	10.11	302208.9	-0.07
	Panel C: Low infant mortality areas					
Treated mother	0.096***	2.129***	2.308**	0.746	1593.275	0.034
	(0.035)	(0.703)	(0.881)	(0.469)	(3242.012)	(0.026)
Observations	106801	106801	106801	106801	106978	49854
Pre-treatment mean	12.31	51.41	51.96	10.59	309048	-0.04
	Panel D: High infant mortality areas					
Treated mother	0.094**	1.827**	1.997**	1.163***	9161.036***	0.062***
	(0.038)	(0.840)	(0.903)	(0.419)	(2548.442)	(0.020)
Observations	172726	172726	172726	172726	173129	80541
Pre-treatment mean	12.17	48.94	49.63	9.44	301755.6	-0.08

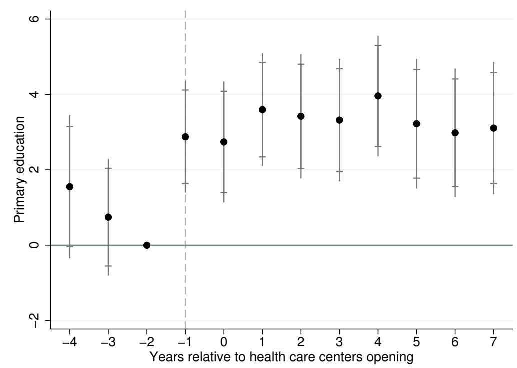
Notes: Each column is from a separate regression on access to a mother and child health care center for treated generation. Robust standard errors adjusted for clustering at the level of the municipality of birth for treated generation are shown in parentheses. All regressions are weighted by the number of siblings. Birth cohorts for second generation are born between 1961–1988. Health care centers opened between 1936 to 1955. Earnings presented are average discounted earnings from 1967 to 2017. All specifications include a full set of cohort and municipality fixed effects, municipality specific pre-treatment trend, this generations year-of-birth fixed effects and a dummy variable controlling for the gender of the individual. Significance levels: *** 1% level, ** 5% level, * 10% level.

3.B Appendix Sensitivity Analyses: Main First Generation Estimates

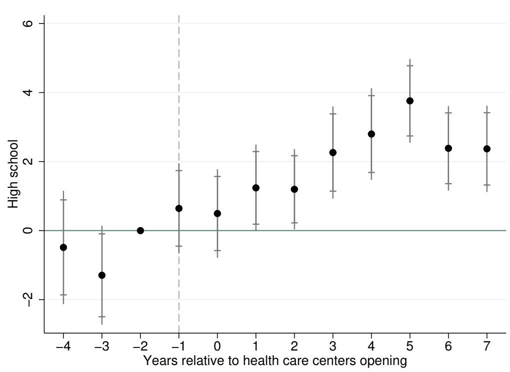
Figure B4: Event Study Graphs for First Generations' Education and Labor Market Outcomes



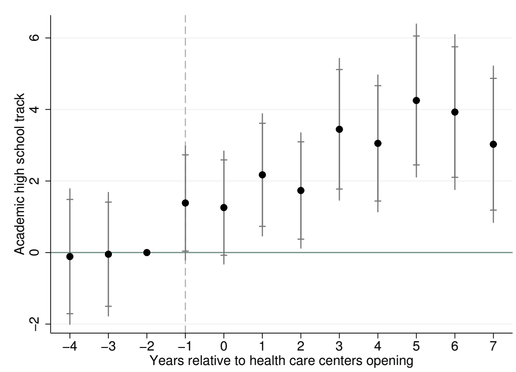
(a) Total years of education



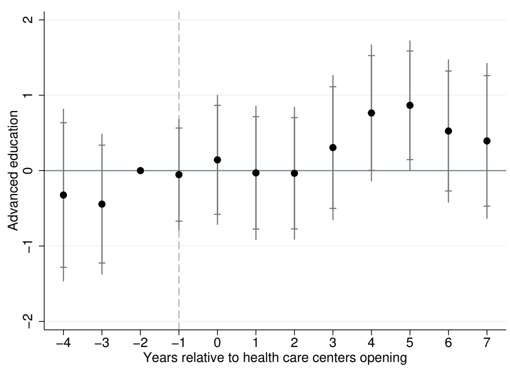
(b) Primary education



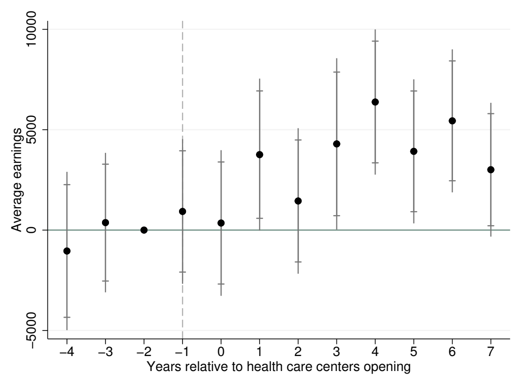
(c) High school



(d) Academic high school track



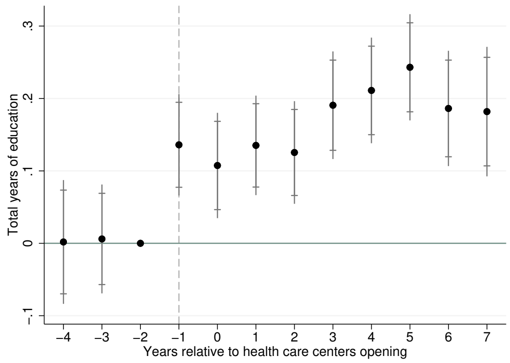
(e) Advanced education



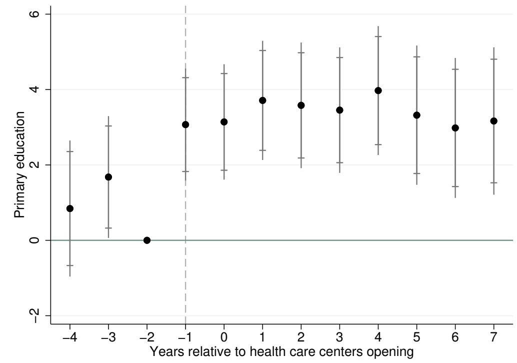
(f) Average earnings 1967–2017

Notes: Each figure is from a separate regression on access to a mother and child health care center. Robust standard errors adjusted for clustering at the level of the municipality of birth are shown in parentheses. Birth cohorts are born between 1936–1955. Health care centers opened between 1936 to 1955. Earnings presented are average discounted earnings from 1967 to 2017. All specifications include a full set of cohort and municipality fixed effects, municipality specific pre-treatment trend and a dummy variable controlling for the gender of the individual. Significance levels: *** 1% level, ** 5% level, * 10% level.

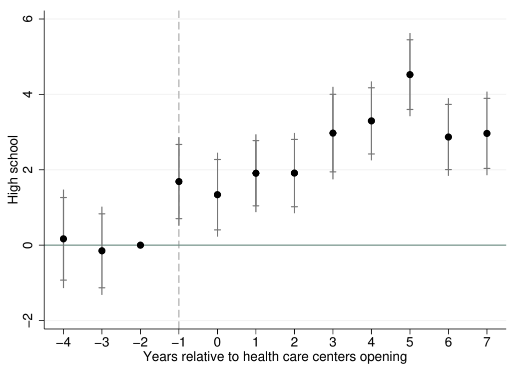
Figure B5: Event Study Graphs for First Generation Born 1934–1955: Education and Labor Market Outcomes



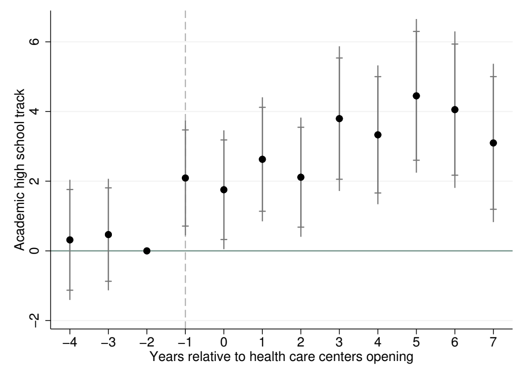
(a) Total years of education



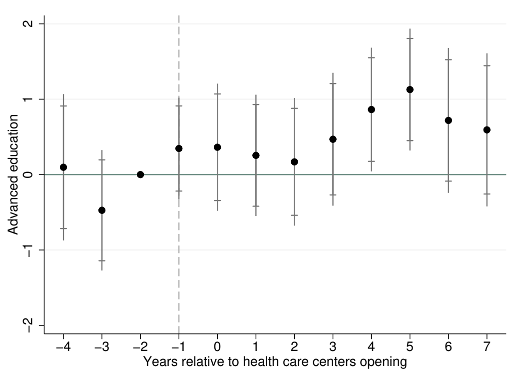
(b) Primary education



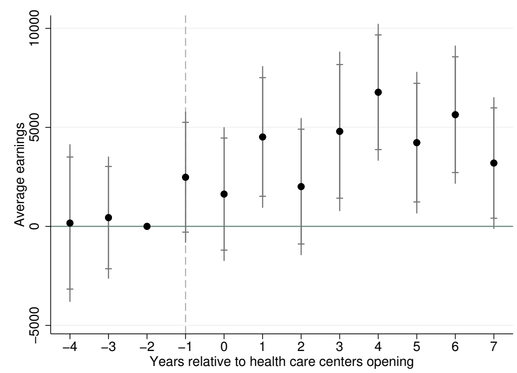
(c) High school



(d) Academic high school track



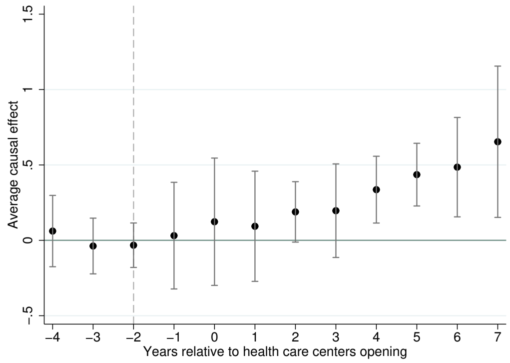
(e) Advanced education



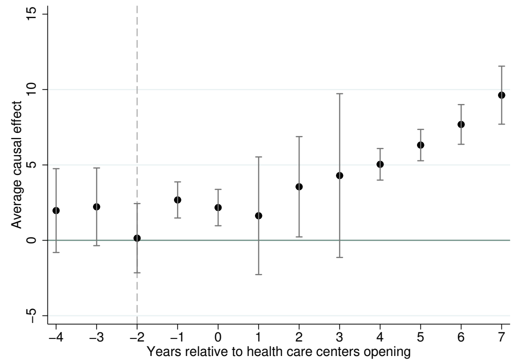
(f) Average earnings 1967–2017

Notes: Each figure is from a separate regression on access to a mother and child health care center. Robust standard errors adjusted for clustering at the level of the municipality of birth are shown in parentheses. Birth cohorts are born between 1934–1955. Health care centers opened between 1936 to 1955. Earnings presented are average discounted earnings from 1967 to 2017. All specifications include a full set of cohort and municipality fixed effects, municipality specific pre-treatment trend and a dummy variable controlling for the gender of the individual. Significance levels: *** 1% level, ** 5% level, * 10% level.

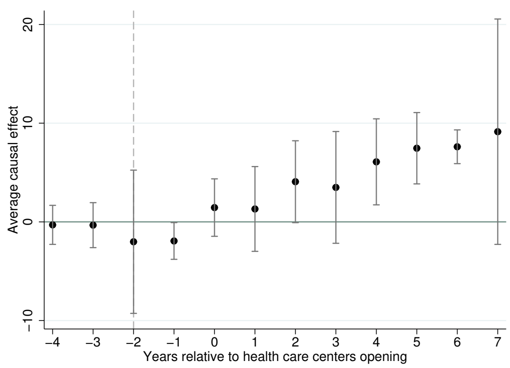
Figure B6: Event Study Graphs for First Generations' Education and Labor Market Outcomes: Robustness to Callaway and Sant'Anna



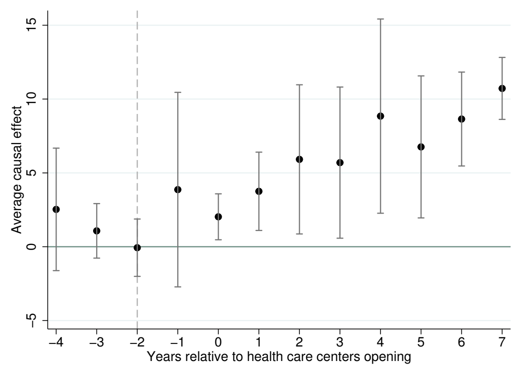
(a) Total years of education



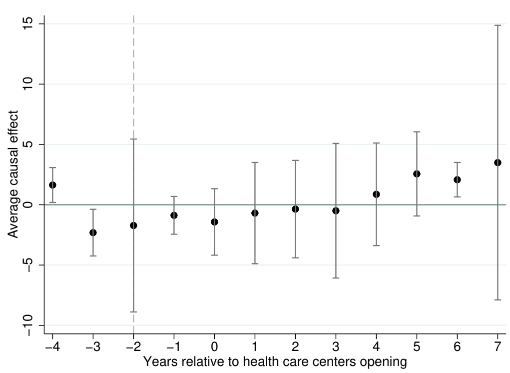
(b) Primary education



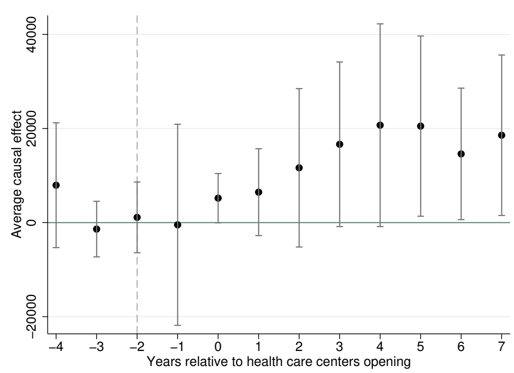
(c) High school



(d) Academic high school track



(e) Advanced education



(f) Average earnings 1967–2017

Notes: Each figure is from a separate regression on access to a mother and child health care center. Birth cohorts are born between 1934–1955. Health care centers opened between 1936 to 1955. Earnings presented are average discounted earnings from 1967 to 2017. All specifications include a full set of cohort and municipality fixed effects, and a dummy variable controlling for the gender of the individual. Municipalities never receiving a health centers used as control.

Table B4: First Generation Education and Labor Market Outcomes by Gender

	Education registry					Earnings registry
	Total years of education (i)	Primary education (ii)	High school (iii)	Academic high school track (iv)	Advanced education (v)	Average earnings 1967-2017 (vi)
Center exposure	0.292***	4.012***	5.014***	3.537***	1.168***	8024.609***
	(0.048)	(0.904)	(0.743)	(0.740)	(0.445)	(2450.176)
Center exposure × female	-0.185***	-1.427**	-3.499***	-0.641	-1.181**	-6652.661***
	(0.046)	(0.576)	(0.788)	(0.826)	(0.497)	(2264.622)
Female	-0.460***	3.193	-14.096***	6.696***	-0.630	-167398.776***
	(0.118)	(2.394)	(0.989)	(0.991)	(1.937)	(7940.523)
Observations	348511	348511	348511	348511	348511	349741

Notes: Each column is from a separate regression on access to a mother and child health care center. Robust standard errors adjusted for clustering at the level of the municipality of birth are shown in parentheses. Birth cohorts are born between 1936–1955. Health care centers opened between 1936 to 1955. Earnings presented are average discounted earnings from 1967 to 2017. All specifications include a full set of cohort and municipality fixed effects, municipality specific pre-treatment trend and a dummy variable controlling for the gender of the individual. Significance levels: *** 1% level, ** 5% level, * 10% level.

Table B5: Robustness First Generation Education and Labor Market Outcomes

	Education registry					Earnings registry
	Total years of education (i)	Primary education (ii)	High school (iii)	Academic high school track (iv)	Advanced education (v)	Average earnings 1967–2017 (vi)
	Panel A: Excluding cohorts born during WWII					
Center exposure	0.240***	4.281***	3.750***	3.826***	0.834**	4643.649**
	(0.043)	(0.966)	(0.568)	(0.961)	(0.375)	(2046.747)
Observations	275841	275841	275841	275841	275841	276840
	Panel B: Controlling for school reform					
Center exposure	0.209***	3.489***	3.238***	2.456***	0.592*	4861.069***
	(0.039)	(0.835)	(0.534)	(0.741)	(0.309)	(1685.179)
Observations	348511	348511	348511	348511	348511	349741
	Panel C: Original treatment					
Center exposure	0.163***	2.562***	2.936***	2.831***	0.559*	4429.062***
	(0.037)	(0.732)	(0.540)	(0.827)	(0.293)	(1561.989)
Observations	348511	348511	348511	348511	348511	349741
	Panel D: No trend					
Center exposure	0.198***	3.370***	3.286***	3.232***	0.642**	4909.200***
	(0.039)	(0.800)	(0.540)	(0.848)	(0.308)	(1715.372)
Observations	348511	348511	348511	348511	348511	349741
	Panel E: Cubic trend					
Center exposure	0.195***	3.355***	3.256***	3.200***	0.605**	4766.363***
	(0.039)	(0.804)	(0.540)	(0.856)	(0.306)	(1724.063)
Observations	345061	345061	345061	345061	345061	346287

Notes: Each column is from a separate regression on access to a mother and child health care center. Robust standard errors adjusted for clustering at the level of the municipality of birth are shown in parentheses. Birth cohorts are born between 1936–1955. Health care centers opened between 1936 to 1955. Earnings presented are average discounted earnings from 1967 to 2017. All specifications include a full set of cohort and municipality fixed effects, municipality specific pre-treatment trend and a dummy variable controlling for the gender of the individual. Significance levels: *** 1% level, ** 5% level, * 10% level.

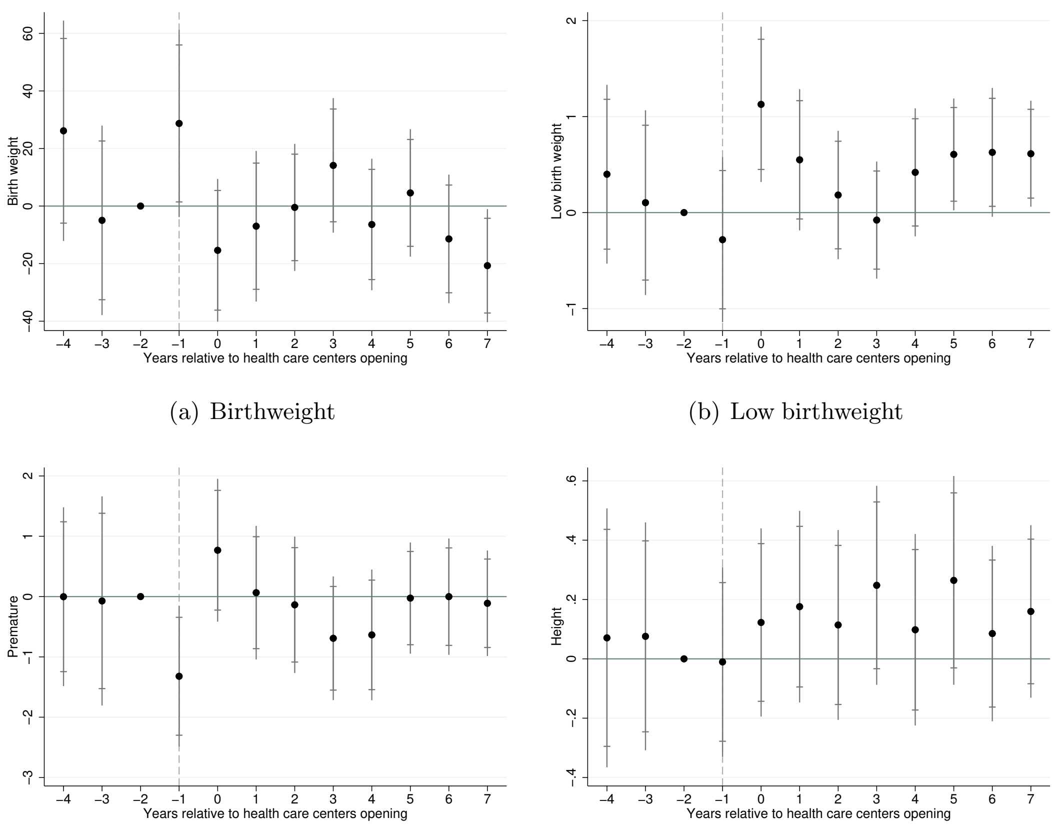
Table B6: First Generation Estimate Sensitivity to Dropping Different Percentiles of the Treatment Group

	Baseline (i)	Dropping 60^{th} percentile (ii)	Dropping 70^{th} percentile (iii)	Dropping 80^{th} percentile (iv)	Dropping 90^{th} percentile (v)	Dropping 100^{th} percentile (vi)
	Panel A: Total years of education					
Center exposure	0.200***	0.195***	0.201***	0.200***	0.202***	0.200***
	(0.039)	(0.039)	(0.039)	(0.039)	(0.039)	(0.039)
Observations	348511	345201	345297	345123	344900	345099
	Panel B: Average earnings 1967-2017					
Center exposure	4903.776***	5141.941***	5098.535***	4809.130***	4857.109***	4847.990***
	(1705.854)	(1695.511)	(1636.988)	(1688.047)	(1684.098)	(1711.716)
Observations	349741	346202	346182	346300	346366	346250

Notes: Each column is from a separate regression on access to a mother and child health care center. Robust standard errors adjusted for clustering at the level of the municipality of birth are shown in parentheses. Birth cohorts are born between 1936–1955. Health care centers opened between 1936 to 1955. Earnings presented are average discounted earnings from 1967 to 2017. All specifications include a full set of cohort and municipality fixed effects, municipality specific pre-treatment trend and a dummy variable controlling for the gender of the individual. In column (ii) we drop individuals in the 60^{th} percentile of the predicted education or income distribution. In columns (ii)-(vi) this procedure is repeated for the 70^{th}, 80^{th}, 90^{th} and 100^{th} percentiles. Significance levels: *** 1% level, ** 5% level, * 10% level.

3.C Appendix Sensitivity Analyses: Health Outcomes

Figure C4: Event Study Graphs for Second Generations' Health at Birth: Treated Mothers



(a) Birthweight

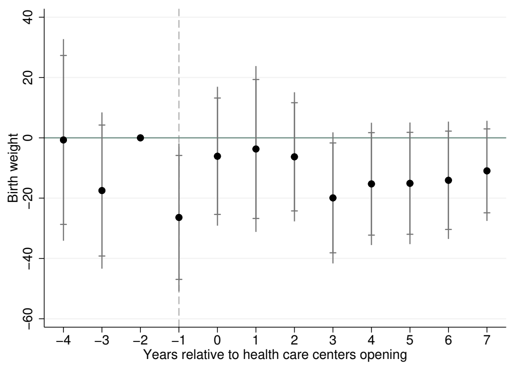
(b) Low birthweight

(c) Premature

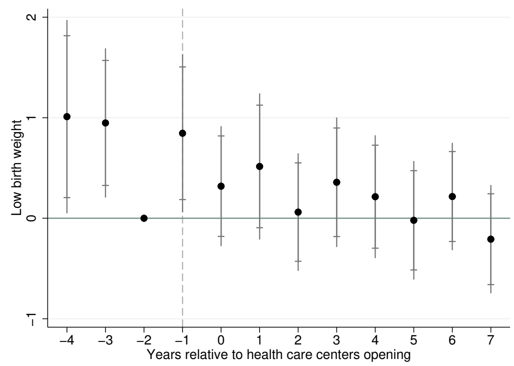
(d) Height

Notes: Each figure is from a separate regression on access to a mother and child health care center for treated generation. Robust standard errors adjusted for clustering at the level of the municipality of birth for treated generation are shown in parentheses. All regressions are weighted by the number of siblings. Birth cohorts for second generation are born between 1961–1988. Health care centers opened between 1936 to 1955. All specifications include a full set of cohort and municipality fixed effects, municipality specific pre-treatment trend, this generations year-of-birth fixed effects and a dummy variable controlling for the gender of the individual. Figure (i)–(iii) includes cohorts born 1967–1988 and figure (iv) include cohorts born 1961–1988. Significance levels: *** 1% level, ** 5% level, * 10% level.

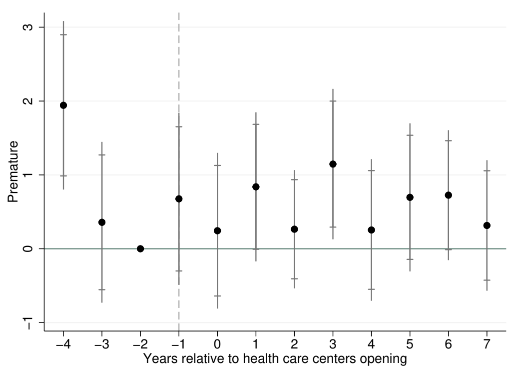
Figure C5: Event Study Graphs for Second Generations' Health at Birth: Treated Fathers



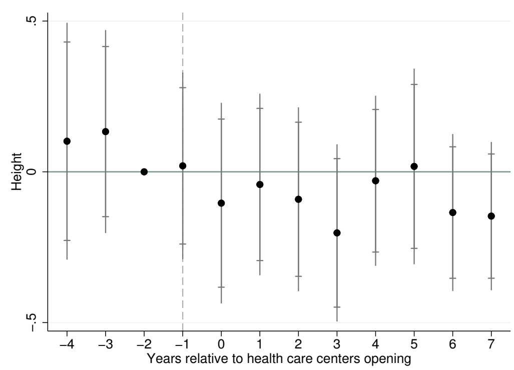
(a) Birthweight



(b) Low birthweight



(c) Premature



(d) Height

Notes: Each figure is from a separate regression on access to a mother and child health care center for treated generation. Robust standard errors adjusted for clustering at the level of the municipality of birth for treated generation are shown in parentheses. All regressions are weighted by the number of siblings. Birth cohorts for second generation are born between 1961–1988. Health care centers opened between 1936 to 1955. All specifications include a full set of cohort and municipality fixed effects, municipality specific pre-treatment trend, this generations year-of-birth fixed effects and a dummy variable controlling for the gender of the individual. Figure (i)–(iii) includes cohorts born 1967–1988 and figure (iv) include cohorts born 1961–1988. Significance levels: *** 1% level, ** 5% level, * 10% level.