

Communism in the Classroom: Long-Run Effects of an Experiment*

Jaakko Meriläinen[†]

Matti Mitrunen[‡]

June 24, 2026

Abstract

This paper documents the long-term economic effects of a psychological experiment in Marxist-Leninist classroom indoctrination conducted in a Finnish municipality between 1973 and 1975. As part of the experiment, two cohorts of fifth-grade students were exposed to a Soviet-influenced history and social studies curriculum aimed at shaping a “functioning [socialist] worldview,” while others followed the national standard curriculum. Using administrative data we find that exposed cohorts, as adults, had significantly lower incomes, reduced labor supply, and were more likely to enter left-leaning and civic-minded occupations. Educational attainment and cognitive or non-cognitive skills were less clearly affected.

Keywords: communism, ideological education, indoctrination, long-run outcomes, persuasion, propaganda, school experiment

JEL: I21, I28, J24, J31, P36

*We thank our discussants Suresh Naidu (NBER SI Political Economy) and Pau Vall-Prat (Workshop in Economic History, Uppsala), Anne Sofie Beck Knudsen, Magnus Carlsson, Ciprian Domnisoru, Anna Dreber Almenberg, Georgy Egorov, Leander Heldring, Andrei Markevich, Jonathan Meer, Laia Navarro-Sola, Nathan Nunn, Tuomas Pekkarinen, Torsten Persson, Miika Päällysaho, James Robinson, Torsten Santavirta, Miri Stryjan, David Strömberg, Matti Sarvimäki, Adam Szeidl, Janne Tukiainen, Roope Uusitalo, Tuomo Virkola, Hans-Joachim Voth, Leonard Wantchekon, Andrea Weber, Ludger Woessmann, David Yanagizawa-Drott, Ekaterina Zhuravskaya, and audiences at various seminars and conferences for their helpful comments. We are particularly grateful to Tuomas Pekkarinen for sharing the comprehensive school reform roll-out information. We also thank Nhi Cao and Maisa Mattila for competent research assistance. Funding from the Yrjö Jahnsson Foundation is gratefully acknowledged. The survey component of our study was approved by the University of Helsinki Ethical Review Board in Humanities and Social and Behavioral Sciences (Statement 42/2024).

[†]Department of Economics, Stockholm School of Economics. Email: jaakko.merilainen@hhs.se.

[‡]Faculty of Social Sciences, University of Helsinki. Email: matti.mitrunen@helsinki.fi.

“Give me four years to teach the children and the seed I have sown will never be uprooted.” —Vladimir Lenin

1 Introduction

Governments routinely embed ideologically biased content into school curricula to shape citizens’ beliefs and behavior. By controlling how history is presented or how economic and political systems are framed, regimes attempt to influence values and actions from childhood. Existing research demonstrates that ideologically charged educational content shapes political preferences (Voigtländer and Voth 2015; Cantoni et al. 2017) as well as national identity and assimilation (Fouka 2020; Carlitz et al. 2024), aligning with broader evidence on the effects of persuasion and propaganda (DellaVigna and Gentzkow 2010; Yanagizawa-Drott 2014; Adena et al. 2015). Yet credible evidence on the long-term economic consequences of classroom indoctrination remains scarce, particularly for content disseminated to children at impressionable ages.

Understanding these effects is of first-order importance as classroom content may shape not only beliefs but also skill acquisition, occupational choice, and labor supply over the entire life cycle. An important related literature documents that childhood interventions can have profound and persistent effects on adult outcomes when followed up decades later (Heckman, Pinto, and Savelyev 2013; Chetty et al. 2011; Gertler et al. 2014; García, Heckman, and Ronda 2023).

This paper documents the long-term economic consequences of exposure to communist classroom content. We study a radical educational experiment in which, between 1973 and 1975, fifth-grade students in the Finnish municipality of Pirkkala were taught a history and social studies curriculum that closely reflected Soviet and Marxist-Leninist views. The rogue school experiment generates variation in exposure to indoctrination across both cohorts and municipalities, which we use to disentangle the impact of curriculum content from contemporaneous changes and to estimate causal effects of classroom indoctrination in a difference-in-differences framework. We find that individuals exposed to the socialist classroom experiment earn substantially lower incomes in adulthood, associated with reduced labor supply and shifts in occupational choice.

The setting we study is unique along three dimensions. First, the Pirkkala socialist school experiment represents a radical curriculum intervention that was both sudden and localized, generating sharp differences in exposure across municipalities and adjacent cohorts as the intervention was first introduced and then withdrawn. Second, unlike historical episodes in which indoctrination coincided with regime change or broader reforms, this intervention

took place in an otherwise stable democratic and non-socialist context, allowing us to isolate the effect of socialist content from contemporaneous shocks. Third, we link individuals to high-quality administrative data up to four decades later, enabling a direct assessment of the long-run labor-market consequences of classroom indoctrination.

Setting Our case study focuses on the long-term consequences of an experiment initiated by a group of psychologists and educational scientists from the University of Tampere. The purpose of this experiment was to analyze the formation of what the researchers called a “functioning worldview” in schools (Ahonen 2008; Leskinen 2016). In practice, this meant a socialist worldview. The fifth-grade teaching material—later dubbed the Pirkkala handout (Penttilä 1974)—was widely condemned by historians and educational experts as “highly erroneous, incomplete, misleading, and completely biased” (Kojo 1975, p. 48), and has since been described as “the most infamous example of Soviet influence on Finnish history schoolbooks” (Ahonen 2008).

Three features characterize the teaching material used in the Pirkkala experiment. First, it framed world history primarily as a Marxist class struggle, with socialism portrayed as the ultimate and inevitable stage of development, achievable only through revolution and collective effort. Second, it idealized the Soviet Union as a country without unemployment or recession, where all citizens were equal thanks to the planned economy. Third, it presented capitalism as unjust and crisis-prone, benefiting only the owning class while the rest suffered. Capitalism was explicitly linked to fascism, described largely as an anti-Soviet movement. Nothing similar was taught in the standard fifth-grade history textbook by Lehtonen and Huttunen (1971), which presented a more balanced view of history and emphasized critical thinking. In contrast, the Pirkkala handout instructed students to “use these lectures to ponder how some became rich and others poor, and how some of the population did all the hard work while others only enjoyed themselves” (Penttilä 1974, p. 2).

The experiment included large amounts of repetition of core Marxist concepts and issues to bolster student learning and aid the development of a “socialist personality” (Leskinen 2016, p. 251). For example, the Pirkkala handout was filled with underlined keywords that the researchers instructed teachers to pay special attention to in teaching (Leskinen 2016, p. 238). Much of this terminology was related to class struggle and even class hatred. Through various exercises in the handout, students also considered drawbacks of capitalism and benefits of socialism, further adding to the bias of the teaching material.

Two cohorts of fifth-grade students in Pirkkala were exposed to the school experiment for several hours per week over the course of a full school year, until it ended abruptly in

1975. Pupils elsewhere followed the standard curriculum, which the children in Pirkkala also resumed after the experiment’s termination.

The set-up of our case study mitigates common empirical challenges in studying the impacts of socialist classroom indoctrination by using variation across both time and place generated by the experiment to isolate the effects from other concurrent changes (cf. [Fuchs-Schündeln and Masella 2016](#) and [Costa-Font, García-Hombrados, and Nicińska 2024](#)). In the 1970s, Finland was a democratic market economy, and the experiment was independent of any larger political shifts. By comparing individuals within the municipality across cohorts, we account for municipality and cohort-specific factors and trends. We show that individuals exposed to the experiment did not differ from other children in their pre-treatment household characteristics obtained from the 1970 census, and that there is no differential income trend across cohorts in Pirkkala.

Overview of the results Using Finnish register data that allow us to track the individuals over time coupled with a generalized difference-in-differences approach, we find that the exposed children had substantially, around 10%, lower incomes in adulthood compared to non-exposed individuals in cohorts before and after within the same municipality.¹ The result is robust to various specifications and methods of measuring adult income. Untreated Pirkkala cohorts attended the same schools and were taught by the same teachers as the treated cohorts at various stages, so the absence of effects among them points to the delivered content rather than school- or teacher-specific factors.

Where does the large income reduction come from? The comprehensive register data let us examine the mechanisms behind it, with secondary outcomes aligning with the specific content of the teaching. A cohort-specific shock to the 5th graders unrelated to the experiment would then have to reproduce this joint pattern while leaving adjacent cohorts and the other experimental grades in the same school unaffected.

We find negative effects on labor supply, which are substantial enough to explain much of the income reduction. This is consistent with the socialist school experiment shifting preferences and values away from market work in a capitalist economy. Evidence from former socialist economies shows that longer exposure to communist regimes is associated with a weaker work ethic ([Linz and Chu 2013](#)) and a reduced sense that outcomes are driven by individual effort ([Alesina and Fuchs-Schündeln 2007](#); [Bondar and Fuchs-Schündeln 2023](#)). Most directly, [Fuchs-Schündeln and Masella \(2016\)](#) and [Costa-Font, García-Hombrados, and Nicińska \(2024\)](#) document consistent labor market

¹This evidence is comparable to estimates of the labor-market effects of socialist education in the prior literature ([Fuchs-Schündeln and Masella 2016](#); [Costa-Font, García-Hombrados, and Nicińska 2024](#)); our contribution is to isolate ideological content from the broader institutional features of socialism.

consequences of socialist education itself. Theoretically, the channel maps onto models in which preferences and values shape both occupational choice and labor supply (Doepke and Zilibotti 2008; Bowles 1998). To assess whether this interpretation is consistent with broader attitudinal patterns, we show in Finnish World Values Survey data that left-leaning respondents report systematically weaker pro-work attitudes.

We also find that childhood exposure to the experiment matters for occupational sorting, a margin known to explain a large share of income variation (Acemoglu and Autor 2011). Exposure to the socialist school experiment is associated with a greater likelihood of selecting into civic-minded occupations, such as teaching and nursing, and a lower likelihood of entering managerial positions. To demonstrate that the occupational choices are in line with the content of the education offered in Pirkkala, we use survey data that capture how the valuation of occupations varies by ideology. The exposed children were more likely to choose occupations more appreciated by left-wing respondents and, conversely, were less likely to choose occupations more appreciated by right-wing respondents.

There is less clear evidence that the socialist school experiment substantially affected later educational attainment. The experiment decreased the years of schooling marginally but did not have an effect on the propensity to graduate from high school or university. Employing enlistment data from the Finnish Defense Forces (see, e.g., Jokela et al. 2017), we also document that the exposure did not affect the affected individuals' later cognitive ability or (economically valuable) personality traits such as achievement striving, measured around the age of 18, to a great extent. We thus see that it is less likely that our findings would reflect differences in effort alone. We observe a substantial reduction in military service, which in Finland has historically been associated more with conservative ideology.

Our overall findings are consistent with the experiment shaping individuals' beliefs and preferences rather than constraining their opportunities. To further examine this mechanism, we conducted a follow-up survey. Although the response rate was low and produces imprecise estimates, the patterns are broadly consistent with this interpretation and suggest that the Pirkkala school experiment had lasting effects on exposed students.

Our research design exploits a sharply concentrated source of experimental variation (a single treated municipality, two adjacent cohorts, and 221 exposed students) to identify the long-run effects of the ideological classroom content. Treatment groups of this order are common both in experimental research published in leading outlets (Bursztyn, González, and Yanagizawa-Drott 2020; Corno, La Ferrara, and Burns 2022; Ghosh et al. 2025) and in long-term follow-ups of childhood interventions that have shaped our understanding of

human capital formation (Heckman, Pinto, and Savelyev 2013; Chetty et al. 2011; Gertler et al. 2014; García, Heckman, and Ronda 2023).²

Few-treated-cluster settings, like ours, are also a recurring feature of quasi-experimental work (Cantoni et al. 2017; Fouka 2020); our setting has the additional feature that the treatment turns on and then off. We use inference methods tailored to settings with few treated clusters. Throughout the paper, we report wild cluster bootstrap and randomization inference p -values alongside conventional standard errors. We further implement the Conley and Taber (2011) procedure designed for difference-in-differences designs with a single treated unit. Our estimates remain statistically significant at traditional levels under each of these approaches. The randomization inference exercise re-assigns the Pirkkala cohorts' treatment status to each control municipality-cohort and asks how often a placebo treatment generates an effect as adverse as the one we estimate. The resulting p -value is 0.01: a placebo treatment produces an income decline at least as large as our estimate in roughly one percent of municipality-cohort assignments. This suggests that shocks of comparable magnitude are rare in the data, reducing the concern that our result is driven by an idiosyncratic shock specific to Pirkkala's exposed cohorts.

The long-term income results are robust across alternative estimation strategies. We recover consistent estimates from a difference-in-discontinuities design exploiting the sharp birth-date cutoffs around treatment eligibility and from a data-driven counterfactual using synthetic control and synthetic difference-in-differences methods. We further obtain highly similar estimates from an individual-level matching strategy with balanced sample size.

Importantly, there are no known shocks to the treated cohorts in Pirkkala that could plausibly generate the pattern we observe, turning on and then off for the specific cohorts. We investigate several leading alternative explanations. First, the exposed cohorts faced no differentially adverse local labor market conditions at graduation, including during the 1990s Finnish recession. Second, there are no differential trends in school funding, and the experiment involved multiple teachers, ruling out resource- or teacher-specific channels. Third, while peer reinforcement within the treated cohorts is a plausible complementary channel, it is itself a downstream consequence of the classroom exposure we study, and we find no spillovers to adjacent cohorts or to other experimental grades in the same school.

²The original Perry Preschool experiment, for instance, comprised 58 treated children (Heckman, Pinto, and Savelyev 2013). This comparison illustrates that small treated samples need not preclude informative long-run evidence, especially when outcomes can be followed with little attrition. At the same time, Perry was randomized, whereas our setting is quasi-experimental. Our design therefore relies on the assumption that adjacent Pirkkala cohorts and comparison municipalities provide a credible counterfactual for the exposed cohorts. We consequently place particular emphasis on pre-treatment balance, pre-trends, placebo exercises, and inference procedures designed for settings with few treated units.

Contributions and related literature Ideology shapes and has historically shaped the content of education ([Bowles and Gintis 1976](#); [Lott 1999](#); [Paglayan 2022](#); [Neundorf et al. 2024](#)), but we have an imperfect understanding of how ideological education affects the economic outcomes of the exposed children. It is particularly important to grasp the long-term impacts of socialist education, given its widespread implementation during the 20th century.³

This paper presents the first long-term follow-up of the socialist Pirkkala school experiment. The experiment was cut short, its findings were never published, and no long-term follow-ups were conducted. The only existing documentation is the final report by [Tutkijaryhmä Peto \(1975\)](#), in which the researchers lament the incomplete nature of the experiment; [Leskinen \(2016\)](#) provides a detailed history but does not examine its potential consequences. By tracing the long-run impacts of the experiment, we contribute to a literature that follows the lasting effects of childhood interventions into adulthood ([Heckman, Pinto, and Savelyev 2013](#); [Chetty et al. 2011](#); [Gertler et al. 2014](#); [García, Heckman, and Ronda 2023](#)). We add to the body of work exploring the persistent effects of socialist and Soviet ideas ([Alesina and Fuchs-Schündeln 2007](#); [Schaewitz, Wang, and Rieger 2022](#); [Fuchs-Schündeln and Schündeln 2020](#); [Lippmann, Georgieff, and Senik 2020](#)), and to the scant evidence that taught ideas themselves can have long-term consequences (e.g., [Ash, Chen, and Naidu 2022](#); [Bai, Jia, and Wang 2023](#)).

Closely related and important contributions by [Costa-Font, García-Hombrados, and Nicińska \(2024\)](#) and [Fuchs-Schündeln and Masella \(2016\)](#) examine labor market impacts of socialist education in different contexts, but this existing work cannot discern whether the observed effects are due to other concurrent changes in the socialist systems. As such, whether socialist classroom indoctrination itself matters for individuals’ long-term economic outcomes remains largely an open question. [Costa-Font, García-Hombrados, and Nicińska \(2024\)](#) study the removal of Marxist–Leninist content from school curricula in 1950s Poland. They find modest increases in educational attainment and labor force participation among individuals born early in the year and not exposed, relative to those born later in the year and exposed. The authors account for season-of-birth effects using data from other cohorts. However, the studied reform also coincided with changes to the grading system, which placed more emphasis on scholarly achievement rather than rewarding participation in communist youth organizations, and the authors do not have

³The former Soviet Union and its satellite states are obvious examples ([Wojdon 2023](#)); other examples range from socialist Sunday schools in the United States ([Teitelbaum 1990](#)) to the Zimbabwean social studies curriculum inspired by socialism ([Ndebele and Tshuma 2014](#)).

geographical within-cohort variation to tackle the identification challenges.⁴ Fuchs-Schündeln and Masella (2016) exploit German reunification as a natural experiment to study the labor market effects of socialist education. They show that longer exposure to the East German educational system, which included ideological indoctrination but also non-meritocratic central planning, had persistent effects on men’s labor market outcomes.

Costa-Font, García-Hombrados, and Nicińska (2024) and Fuchs-Schündeln and Masella (2016) observe effects operating through formal educational attainment or institutional features of socialist schooling. By contrast, we show that ideological *content* alone (stripped of the institutions socialism normally travels with) can have large and lasting economic effects. Our experimental setting therefore isolates a dimension of socialist education—ideological persuasion itself—that existing studies, by design, cannot separate from broader institutional change. Finnish administrative data further allow us to examine a wider set of outcomes over the life cycle and to include a richer set of controls than the earlier work. Importantly, we also study the long-term economic effects of socialist education in a different, non-socialist and democratic, context.

These features of our study further distinguish it from the prior work on the influence of ideologically driven education on ideology and attitudes. For instance, Cantoni et al. (2017) leverage a staggered reform in Chinese schools to study how an authoritarian curriculum shapes ideology, and Voigtländer and Voth (2015) show that Nazi education policies led to enduring anti-Semitic attitudes in the affected cohorts. We complement these studies by leveraging an experimental setting and examining the long-term economic impacts of classroom indoctrination.⁵

Finally, the Pirkkala school experiment resembles the dissemination of propaganda, as ideologically biased information was presented to schoolchildren with the aim of shaping their views. Thus, our paper contributes to the broader literature on the effects of propaganda and persuasion more generally (DellaVigna and Gentzkow 2010). Much of this research focuses on propaganda disseminated through mass media (Yanagizawa-Drott 2014; Adena et al. 2015;

⁴At the same time, there were also other broader cohort-specific shifts in the Polish education system, such as the expansion of education (Hiscocks 1959) and removal of preferential treatment of people coming from workers’ or farmers’ families in university admissions (Balicki 1964).

⁵Economists have examined how education shapes students’ views from various perspectives. Aspachs-Bracons et al. (2008) and Clots-Figueras and Masella (2013) investigate how language instruction in Spain influences identity formation. Arold (2024) examines the impact of U.S. creationism education on students’ occupational choices. Chen, Lin, and Yang (2023) analyze how curriculum reforms in Taiwan foster national identity, while Carlitz et al. (2024) show how the Tanzanian Ujamaa policies enhanced national identity through political education. Studies by Alesina, Giuliano, and Reich (2021) and Bandiera et al. (2019) also discuss the role of education in nation-building. Additionally, research by Fouka (2020) and Mitrinen (2020) examines the effects of assimilation policies through education during the Age of Mass Migration on student outcomes.

Blouin and Mukand 2019; Grosfeld et al. 2024). We extend this body of work by investigating the individual-level economic impacts of propaganda embedded in the educational system.

2 The Pirkkala School Experiment

The socialist school experiment took place in the municipality of Pirkkala during the years 1973-1975. This section describes the experiment, relying in particular on Leskinen (2016) and the original teaching material by Penttilä (1974).⁶

2.1 Pilot Experiment in Pirkkala

The school experiment in Pirkkala started from a seemingly harmless initiative: The Finnish Ministry of Education and Finnish education scholars had been concerned about the lack of social studies and international education in Finnish schools. The Finnish UN Association proposed organizing a 3-4-week project on international education in 1-3 municipalities. Pirkkala, among several other Finnish municipalities, had been involved in developing a new elementary school system since 1961 and was chosen to host the project.

A group of psychologists and educational scientists from the University of Tampere named *Peruskoulun toimivan opetussuunnitelman projekti* (Peto) had already received funding to help develop a new social studies curriculum and were tasked with running an educational experiment in the municipality of Pirkkala, located near Tampere.⁷ In addition to designing the new curriculum, the scholars were interested in studying how worldviews are formed in schools.

In 1973, the Peto group conducted a two-week pilot on global education in Pirkkala. The two-week pilot program aimed to compare socialist and capitalist systems, as well as rich and poor countries. Pupils in Pirkkala were initially taught about the disadvantages of capitalism, followed by lessons on the advantages of socialism. In addition to traditional classroom instruction, the course included presentations from visitors associated with the

⁶The Pirkkala school experiment occurred during an era when the Finnish primary school system underwent a comprehensive reform. This reform abolished the old two-track system and replaced it with a uniform nine-year comprehensive school (Pekkarinen, Uusitalo, and Kerr 2009; Kerr, Pekkarinen, and Uusitalo 2013), which children start in the fall of the year they turn seven years old. We ensure that this reform does not confound our empirical analysis by comparing identically affected municipalities. Further information on the Finnish school system and the comprehensive school reform is available in Online Appendix A.

⁷According to Leskinen (2016, 79), Pirkkala was chosen because it was “considered to be suitable for the experiment.” The meeting minutes do not provide any further justification for the choice, but it was likely due to Pirkkala being close to Tampere and included in the first wave of the school reform that was simultaneously being rolled out. In our empirical setting, we compare adjacent cohorts within Pirkkala against cohorts in municipalities facing the same school reform.

Finland–Soviet Union Society. On one occasion, students began their day by listening to excerpts from *The Communist Manifesto* broadcast over the central radio, followed by *The Internationale* (Leskinen 2016, p. 115).

The pilot also included various practical exercises. For instance, in a developing country simulation, the children were divided into poor and rich classes in a hypothetical capitalist developing country. During the school lunch, the groups were served different food, and the poor had to use their recesses to build slum housing (Panel A of Figure 1). In another exercise depicted in Panel B of Figure 1, the students organized a demonstration against the capitalist society. Here, the signs demand more jobs and better housing.

The children shown in the photographs were in the third grade at the time of the pilot experiment. Two years later, they became subjects of the main experiment, to which they were subjected for the entire school year.



A. A poverty simulation: building slum housing in a capitalist developing country.

B. A demonstration against capitalism.

Figure 1. Exercises during the pilot.

Sources: Archives of Helsingin Sanomat/Lehtikuva and Yle.

2.2 Curriculum Experiment in 1973-1975

The Peto group started implementing its curriculum experiment on a larger scale during the 1973-1974 school term. The research group tasked with implementing and evaluating the experiment was particularly interested in learning if the experiment could bolster socialist views among the students. Professor Annika Takala, one of the leading figures of the Peto group, believed that only a Marxist view of history could give the students a good understanding of how the Finnish capitalist class society based on deprivation was born and how it would evolve into a socialist society (Leskinen 2016, p. 217).

The Peto group planned a five-year study in which curriculum was changed in first-grade environmental studies, fifth-grade history and social studies, and ninth-grade social studies. In their study, only the contents of the targeted subjects were altered, and the time allocation between different subjects remained unchanged. For all other subjects, the students in Pirkkala followed the national curriculum from 1970 that dictates the contents of teaching and time allocation to different subjects.

Our primary focus is on the fifth-grade students who, in the end, were the only students fully exposed to the socialist curriculum experiment. The first-grade environmental studies teaching was less explicit in its Marxist content and, according to the Peto group, the experiment failed because the material was too complicated for the first graders to comprehend (Leskinen 2016, p. 307). The ninth-grade experiment again was unsuccessfully implemented: teaching materials arrived late, and the teacher used a conventional textbook instead of refusing to comply with the Marxist experiment (Leskinen 2016, p. 271). Similar problems did not arise with the experiment with the fifth graders which we describe in detail below. These partially implemented experimental components also provide useful placebo tests: if the baseline estimates reflected a general disruption from the experiment or a broader Pirkkala school shock, we would expect similar effects for these other experimental grades.

To prepare for the history classes using the new teaching material, the teachers were given instruction seminars where they were asked by the Peto group to follow the material (Leskinen 2016, p. 215). Teachers also obtained a detailed study plan which included additional examples that the teachers were encouraged to use in the class. Parents were not asked for permission or even informed about the educational experiment and its central goals in advance.⁸

5th-grade teaching material The fifth-grade teaching material—the so-called Pirkkala handout, officially *History: 5th grade: general features of the development of mankind from the oldest times to the present day* in English (Penttilä 1974)—is generally viewed as the centerpiece of the Pirkkala school experiment. The handout was used in history and social studies classes to teach a Marxist view of history, emphasizing social class structures and presenting the world history as a class struggle. The 95-page handout was severely pro-Soviet in nature; it was largely copied from a Finnish history textbook printed in Petrozavodsk,

⁸This factor coupled with the fact that children were assigned to nearby schools in their home municipalities lends credibility to our empirical set-up. That is to say, it is unlikely that parents would have reacted to the experiment by moving to another municipality in order for their children to attend a school where Marxist indoctrination was not part of the curriculum.

the Soviet Union, in the 1950s.⁹ In that regard, the Pirkkala experiment also resonates with the type of indoctrination that occurred in the Soviet Union.

According to teachers, the Pirkkala handout and the curriculum differed substantially from the more commonly used textbook by Lehtonen and Huttunen (1971), *Primary school history 1, for the fifth grade* (Leskinen 2016, p. 246). Besides the biased teaching material, the schooling experiment included other components not included in the conventional study plan—although there was significant overlap in terms of themes covered (see Online Appendix Table A.1).¹⁰ For instance, the foreword of the Pirkkala handout encourages students to “use these lectures to ponder how some became rich and others poor, and how some of the population did all the hard work while others only enjoyed themselves.” (Penttilä 1974, p. 2). This presents a stark contrast with the standard textbook used elsewhere in Finland which encouraged children to use critical thinking (Lehtonen and Huttunen 1971, p. 3): “[...] you must carefully examine which narratives are true, that is to say, what really happened.”

To illustrate some key differences in the teaching materials, Table 1 shows the word counts for different keywords. The teaching material used in Pirkkala has a significantly higher frequency of words related to work and social classes. Words such as “revolution”, “fascism”, and “socialism” were not even mentioned in the standard fifth-grade textbook. Some of these terms appeared in the material as underlined keywords that the Peto group instructed should be paid special attention to. In the first panel of Figure 2, we present one example related to *strikes* (the third underlined word) which are described as *the most powerful weapon* (the fourth underlined term) of workers (Penttilä 1974, p. 72).

Panel A of Figure 3 similarly showcases the differences in the content. We use a pre-trained, multilingual large language model to classify the content of each paragraph into various predefined topics (see Burst et al. 2023 for further information). We then compute the probability that a randomly drawn paragraph in each document discusses a certain topic and compute the difference of these probabilities. The figure plots five categories in which the differences are largest and smallest.¹¹ In the Pirkkala handout, there is more text that is supportive of labor groups, advocates for expanding education, opposes imperialism, promotes social justice and equality, and endorses Marxist-Leninist ideology. In contrast, in

⁹The handout is publicly available at <https://agricolaverkko.fi/vintti/julkaisut/julkaisusarja/pirkkalan-moniste/> (in Finnish; accessed April 28, 2025).

¹⁰Both curricula covered the core elements of the fifth-grade syllabus, including early culture and society in the Near East, the Greeks, the Romans, ancient Finnish society, and medieval Europe and Finland. The main difference is that the Pirkkala handout reallocated some time away from ancient and medieval history to additional modules on topics like colonialism, the Finnish Civil War, the Russian Revolution, the founding of the Soviet Union, and World War II.

¹¹Other categories can be found in Online Appendix B where we also discuss the data and methods in further detail.

the standard textbook, the text is more supportive of national pride and patriotism, upholds traditional morality and religious values, emphasizes the importance of technological and infrastructure development, advocates for democracy and citizen involvement in politics, and calls for the importance of cultural and leisure activities.

We provide further evidence of the ideological differences between the teaching materials in Panel B of Figure 3 which shows the (document-level) cosine similarities between these texts and selected leftist writings: *The Communist Manifesto* by Marx and Engels and party platforms of the socialist Finnish People’s Democratic League (1973) and the Social Democratic Party (1978). The bias in the Pirkkala handout is visible in the figure.¹²

Table 1. Prevalence of selected words.

Word	Detected Word in Finnish	Count in Pirkkala	Count in Core Curriculum	Difference in Frequency (%)
work	työ	345	56	776
worker	työläis/työvä	138	10	1862
owning	omista	71	7	1342
peasant	talonpoik	45	19	237
class	luok	35	5	895
capitalism/capitalist	kapital	29	0	∞
revolution	vallankum	27	0	∞
corporation	yrity/ylhtiö	20	0	∞
fascism/fascist	fasis	18	0	∞
Soviet Union	neuvostol	14	1	1916
United States	yhdysval	12	0	∞
ruling class	hallitsev luok	12	3	472
socialism/socialist	sosialis	8	0	∞
Total word count		15242	21672	

Notes: We scanned the original textbooks (Lehtonen and Huttunen 1971; Penttilä 1974) and used OCR on Python to convert the text into data. From these data, we first count the prevalence of selected words using the roots of the Finnish words to account for inflection. The table reports these word counts. Difference in frequency (%) is calculated as the percentage difference relative to the core teaching material: $(f_{\text{Pirkkala}} - f_{\text{Core}})/f_{\text{Core}} \times 100$, where frequencies are normalized by total word counts in each text. ∞ indicates zero occurrences in the core teaching material.

Figure 2 provides selected examples of the material that focused on the working classes or was pro-Soviet that was taught to the Pirkkala fifth graders. The handout goes into great detail discussing workers’ rights (Penttilä 1974, p. 73), as shown in Panel B.

“The workers realized that they must establish their own organizations. These organizations are called trade unions. [...] By collective action, the workers achieved improvements: [...] child labor laws, shorter work days, Sundays off, right-to-strike, freedom of speech and assembly, vote their own members in elections. [...] The workers did not achieve any of these reforms for free, they are the results of a hard struggle.”

¹²We also examine paragraph-level cosine similarities in Online Appendix B. On average, the Pirkkala handout is more similar to these three texts than the standard textbook, and the distributions of paragraph-level cosine similarities are statistically significantly different from each other.

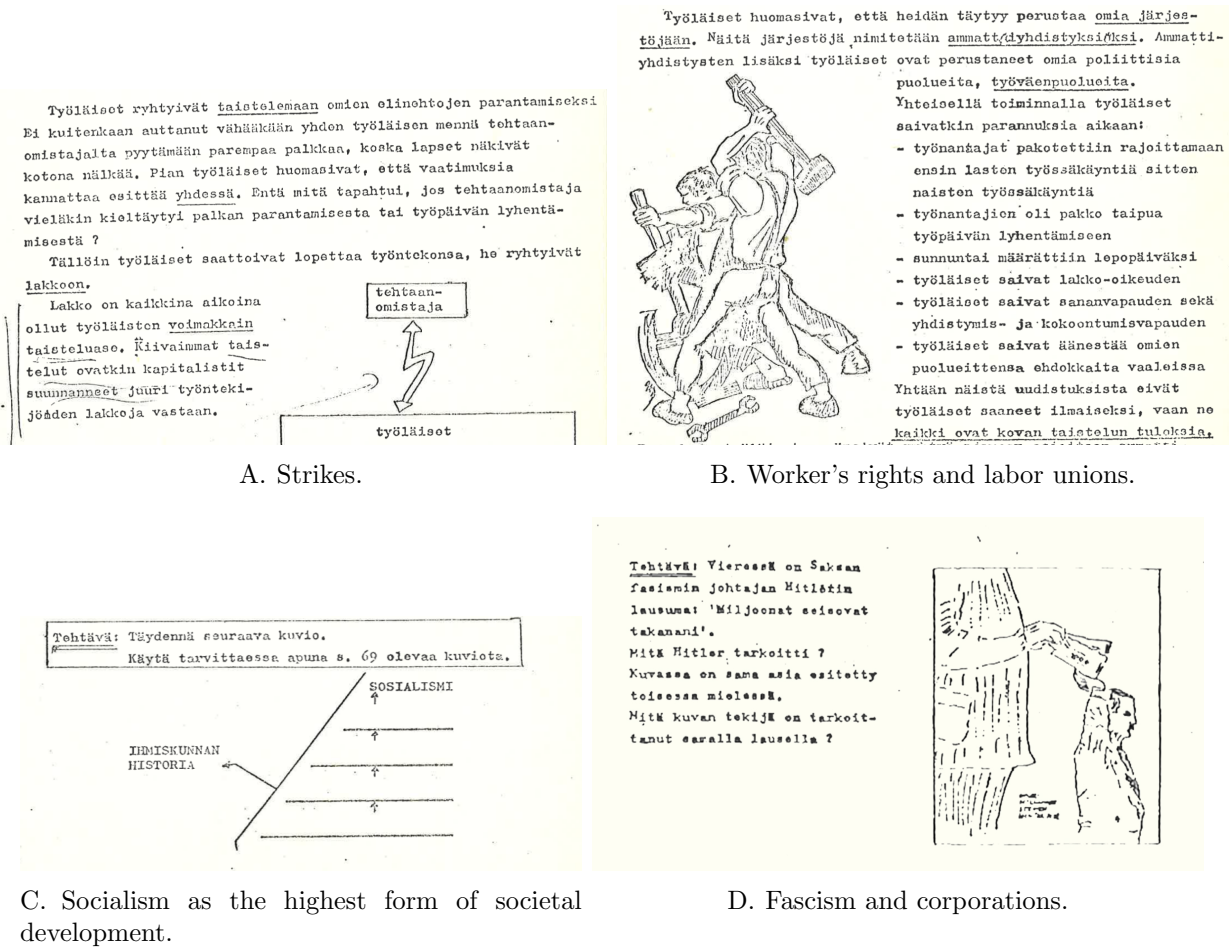
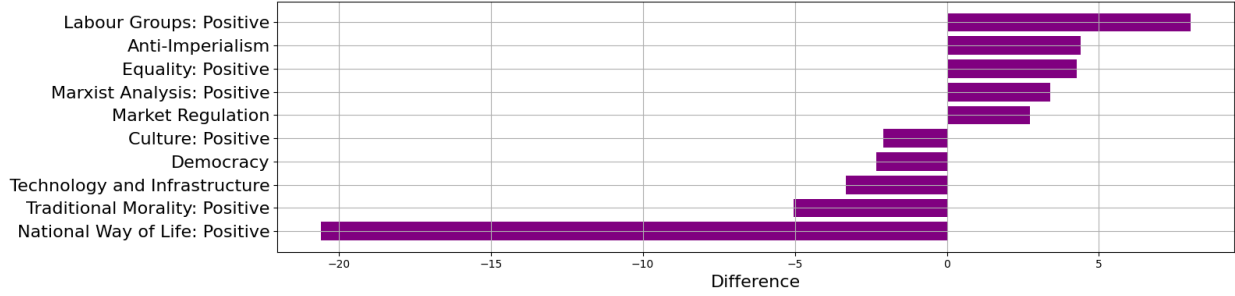


Figure 2. Examples of the content in the Pirkkala handout (Penttilä 1974).

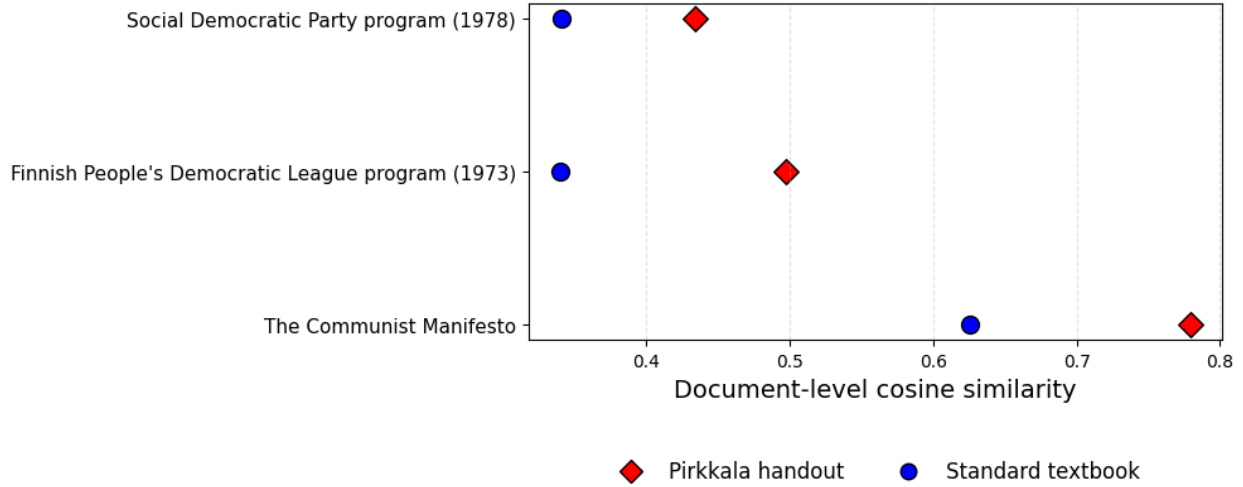
While this information might be true, it is worth noting that nothing similar was taught to fifth graders in other municipalities.¹³ When discussing the Soviet Union, the handout goes even deeper. The Soviet Union was born and socialism was achieved when a “clear majority, more than nine out of ten” of Russians successfully revolted against the bourgeoisie, and now the workers and peasants were in control (Penttilä 1974, p. 85). The state that was born as a consequence is described as follows (Penttilä 1974, p. 87):

“What is this new socialist state like? Means of production were transferred to the society—meaning workers and peasants. [...] Everyday life was no longer governed by large corporations and citizens were in charge of production. [...] This type of economy is called a planned economy where unemployment and

¹³The Cold War and Finlandization did shape the Finnish education policy and school curricula, but this was manifested as a selective coverage of politically sensitive topics or approaching such topics in multiperspectival terms rather than including the type of material that was covered in the Pirkkala handout (Holmén 2006; Ahonen 2008).



A. Differences in the prevalence of different topic categories.



B. Cosine similarity with selected leftist texts.

Figure 3. Contents of the Pirkkala handout and the standard textbook.

Notes: The figures provide a numerical characterization of the contents of the fifth-grade history teaching materials (Lehtonen and Huttunen 1971; Penttilä 1974). We scanned the original textbooks and used OCR on Python to convert the text into data. To construct the differences in Panel A, we use the pre-trained *Manifestoberta* model and classify the content of each paragraph into various topics (Burst et al. 2023). We compute the probability that a paragraph in each document discusses a certain topic and take the difference of these probabilities. Panel B shows cosine similarity distributions between either the Pirkkala handout or the standard textbook and three selected texts. The cosine similarities measure the degree of similarity between the document embeddings, indicating how closely related the contents of the documents are in terms of their semantic meaning. For further details, see Online Appendix B.

recessions are unknown. Similarly, everybody has access to food, housing and clothing or the primary conditions for living [...] in addition to free schooling and medical care. [...] All citizens are equal. As no one can get rich out of others' work, income differences are very small.”

In the exercise below this excerpt, shown in Panel C of Figure 2, the students were asked to fill in the stages of societal development. The end point, “socialism,” was already filled in (Penttilä 1974, p. 87).

While the teaching material spoke highly of the Soviet Union and other socialist countries, it had a strong bias against the United States and the capitalist system. Problems of capitalism were discussed thoroughly in the handout. For example, the material describes the power of private corporations as follows (Penttilä 1974, p. 71):

“The Rockefeller family in the U.S. owns a massive oil company known in Finland as Esso. This company subdued in its rule almost all other U.S. oil companies. [...] After conquering oil, it started to subdue in its rule gas and electric industry, and copper, silk, and lead factories. [...] At the moment, the company has placed its own man, Nelson Rockefeller, as the U.S. vice president.”

Besides the student handout, teachers received an instruction booklet that contained guidelines for successfully teaching the new material. The guide instructed them to emphasize the close link between capitalism and fascism which was seen as an anti-Soviet movement. For instance, the relations between Hitler and private corporations are described as follows:

“Hitler made big speeches and promised to keep the working population in check. So the German large coal and gas corporations gave 6 million Marks to Hitler’s party since the 1930s. Hitler also received money from the American car manufacturer Ford and the Dutch oil company Shell. Hitler’s movement and other similar movements are called fascists.”

The fourth panel of Figure 2 shows an exercise with a picture of the German dictator Adolf Hitler receiving money from large corporations. The task reads: “What did Hitler mean when he said that millions stand behind me?” (Penttilä 1974, p. 95).

In addition to the handout, the fifth graders were required to read novels and comic books that had socialist biases (Leskinen 2016, p. 256). For instance, the French-Belgian comic book *Asterix* was used as an example of a justified revolt of oppressed people against the owning class (Leskinen 2016, p. 259).

In all, the experiment constitutes a clear case of ideological indoctrination. Instead of focusing on noncontroversial subject matter or objectively presenting alternatives in matters where the majority opinion is not settled, the teaching took sides in many controversial matters with the goal of persuading the students and changing their ways of thinking. Educational experts later characterized material used as “highly erroneous,

incomplete, misleading, and completely biased” and giving “a misleadingly black-and-white picture of human history” (Kojo 1975, p. 48).

A year after the onset of the experiment, the Peto group tested 26 exposed fifth graders.¹⁴ The group reported that “the students’ knowledge of history can be considered good” and “there are few biases in the world views” of the tested children (Leskinen 2016, 268; Tutkijaryhmä Peto 1974, 56). This suggests that, at least from the point of view of the executive psychologists, the school experiment was successful in impacting the world views in the desired direction.¹⁵

In a 2020 interview, one person who attended the fifth grade at the time of the experiment said that “it was brainwashing, and not even small-scale.” He does admit, however, that his opinion was formed only when he saw a copy of the teaching material later as an adult. Another person who was part of the pilot experiment notes that she still thinks positively about the Soviet Union. “I am a girl from a working-class home, but I believe that the experiment also affected this a little bit,” she said in the interview.¹⁶

2.3 Termination and Aftermath of the Experiment

The initial plan of the Peto group was to run a five-year trial in the municipality of Pirkkala. There were even plans of adopting the Pirkkala curriculum in Finland more broadly and expanding the ideological teaching to all school subjects (Leskinen 2016, p. 207). In the end, neither of these things happened.

In the spring and summer of 1975, the one-sided history teaching in Pirkkala gained widespread media attention after a copy of the Pirkkala handout made its way to Kullervo Rainio, a professor of social psychology and a member of the parliament representing the conservative National Coalition Party. Immediately after receiving a copy of the handout, on April 4th, 1975, Rainio prepared a written question to the government of Finland addressing the troubling issue of “the feeding of gross propaganda to 11-year-old students” (Written question submitted to the Finnish Parliament No. 110/75). The parliamentary inquiry

¹⁴One question asked how the transition to socialism comes about, with four options: (1) through armed civil war, (2) through a revolution carried out by the working class, (3) through parliamentary elections, and (4) through the intervention of foreign powers. The answer deemed correct by the Peto research group was revolution (Leskinen 2016).

¹⁵According to the measures, the first graders did not understand the material and did not change their views of socialist economy (Leskinen 2016, p. 307). Similarly, the psychologists saw that the material taught to ninth graders had been orthodox but the experiment was perhaps not sufficiently long to alter their worldviews (Tutkijaryhmä Peto 1974)—without knowing that the students were actually taught using a standard textbook (Leskinen 2016, p. 271).

¹⁶See an article in the *Seura* magazine at <https://seura.fi/asiat/tutkivat/pirkkalassa-tehtiin-1970-luvulla-salattu-marxilainen-koulukokeilu-josta-nousi-suuri-skandaali-nyt-puhuvat-entiset-oppilaat-olihan-se-aivopesua-eika-niin-pientakaan/> (accessed April 28, 2025).

was undersigned by Rainio and 25 other members of the parliament. Their key concern is articulated below.

“In all, the teaching material leaves students completely ignorant of cultural achievements and political development, but instead creates a completely twisted and one-sided political attitude in the pupils. When this happens through official school teaching and using the school’s authority while children are isolated from the rest of the environment, it can be called outright political brainwashing in a scientific sense.”

Rainio shared photocopies of the Pirkkala handout with Finnish media, and a scandal broke out. But more than teachers or the psychologists behind the rogue experiment, the media went after the government authorities that allowed the experiment to happen with state funding. The higher-ups in the Ministry of Education and the Board of Education (many of whom were part of the Social Democratic Party) denied that they had any knowledge of what was being taught, as the Peto group acted completely independently. They also argued that due to the large-scale school reform that was ongoing, the oversight was not at a sufficient level. Professor Annika Takala, one of the researchers behind the experiment, did not understand the outcry and said it was only a witch hunt orchestrated by the right-wing parties (Leskinen 2016, p. 56). This was also the view of the Communist Party of Finland that saw the opposition as an attack towards freedom of expression. Other political parties widely condemned the partisan description of human history (Küng 1976, p. 273).

The experiment was then abruptly cut short after two years. The scandal subsided rather quickly and was over by the summer of 1975. There was a police investigation initiated by the attorney general, but no criminal charges were issued in the end. The attorney general merely gave a verbal warning for lax oversight to the bureaucrats who were responsible for supervising the experiment. In the following school year, the Pirkkala schools returned to the normal curriculum.

2.4 Potential for Long-Term Influences

It seems plausible that the Pirkkala school experiment could have had long-term effects on the exposed children. Childhood is broadly acknowledged as a crucial time for forming human capital and diverse attitudes (Fehr, Bernhard, and Rockenbach 2008). Schooling plays an important role in the development of these traits (Cappelen et al. 2020), and the preferences formed early in life have predictive power for life trajectories and later-life achievements (Golsteyn, Grönqvist, and Lindahl 2014). Long-run follow-ups of childhood interventions

consistently uncover sizable effects on adult labor market and life outcomes, well after the intervention itself has ended (Heckman, Pinto, and Savelyev 2013; Chetty et al. 2011; Gertler et al. 2014; García, Heckman, and Ronda 2023).

The Pirkkala experiment was substantial in multiple ways; it lasted the course of an entire school year, and the biased content was imparted by trusted authority figures. According to its critics, the experiment had many hallmarks of brainwashing—to which the pupils in Pirkkala were subjected for a full year and several hours each week. The Pirkkala experiment was inspired by experimental psychology work done in the Soviet Union and East Germany (Tutkijaryhmä Peto 1974, 1975). One central aspect of the experiment was the repetition of certain concepts and issues to bolster student learning and aiding the development of a “socialist personality” (Leskinen 2016, p. 251).¹⁷ Oftentimes, young children do not have strong schemas of topics such as those covered in the Pirkkala experiment, meaning that aggressive introduction to one side of the argument with emotionally appealing imagery could shape the worldview significantly. Once such a schema is formed, it is harder to shake with new information (Taylor 2017, p. 185).

Research in education and developmental psychology highlights the importance of early classroom experiences in the formation of occupational aspirations which can then contribute to actual occupational choice in later life (Trice 1991; Trice and McClellan 1993; Watson and McMahon 2005). One recurrent theme in the material was the role and importance of the working classes in the society. This echoes the Soviet education where one important aim of education was to, besides developing a collectivist mentality, build up an appropriate attitude toward labor—especially respect for manual and socially useful work (Zajda 1979; Tudge 1991). If the experiment influenced labor preferences, occupational preferences and later occupational choice in a systematic way, we expect to see large impacts on adulthood incomes of the exposed children. Occupation itself explains a great deal (up to half) of the variation in incomes (Acemoglu and Autor 2011; Kambourov and Manovskii 2009).

Income differences between occupations can also stem from differences in labor supply across occupations (Erosa et al. 2024). Costa-Font, García-Hombrados, and Nicińska

¹⁷Taylor (2017, xiii) characterizes brainwashing by so-called ICURE techniques: isolating subjects, controlling information, creating uncertainty about previous beliefs or knowledge, repetition, and leveraging both positive and negative emotions. The Pirkkala school experiment applied many of these methods to some degree. Research in social psychology highlights the role of repetition in shaping beliefs and attitudes. Repeated exposure to information increases familiarity and perceived truthfulness, even if the information was not actually truthful, a phenomenon known as the *illusion of truth effect* (Hasher, Goldstein, and Toppino 1977). Similarly, the *mere exposure effect* suggests that familiarity through repetition fosters preference for ideas or stimuli (Zajonc 1968). These mechanisms are central to processes of persuasion, propaganda, and even coercive indoctrination, as detailed in studies of thought reform and brainwashing (Lifton 1961; Schein, Cooley, and Singer 1961). These ideas exemplify the potential impact of repetition in reinforcing messages and shaping long-term attitudes or behaviors.

(2024) argue that while Marxism-Leninism does not advocate for decreased labor force participation, it does repress individual agency and promote more collectivist attitudes which may further have detrimental consequences for economic activity (Gorodnichenko and Roland 2011a,b). Furthermore, research on formerly communist countries finds that longer time under socialism dampens work ethic (Linz and Chu 2013), shifts attributions of success away from personal effort (Alesina and Fuchs-Schündeln 2007; Bondar and Fuchs-Schündeln 2023), and shapes capitalist-market behaviors such as financial risk-taking long after the regime ends (Laudenbach, Malmendier, and Niessen-Ruenzi 2018).

3 Data and Empirical Approach

Our empirical analysis focuses on how the experiment affected exposed pupils’ long-term labor market outcomes. We combine Finnish administrative register data with a generalized difference-in-differences approach that exploits variation in exposure across cohorts and municipalities. This section outlines our data and identification strategy.

3.1 Finnish Administrative Register Data

The full Finnish census data (*FOLK Census 1970-1985*) allow us to identify individuals’ municipality of residence and cohort in the year 1970 which we use to define our treatment and control groups.¹⁸ We match these individuals with their adulthood labor market and educational outcomes using the *FOLK Perus 1995-2015* module of Statistics Finland. An important advantage of the administrative register data is that they allow us to follow the exposed individuals without attrition and use a large control group; the link rate between these administrative datasets using encrypted social security numbers is around 95%. We observe each individual’s adulthood income, educational attainment, employment status, and occupation. Furthermore, the data contain information on various background characteristics, including parents’ 1970 characteristics, which we can use as control variables and to assess the validity of our empirical approach.

3.2 Empirical Strategy

Our empirical analysis exploits two sources of variation. First, we use variation across cohorts in exposure to the school experiment (see Panel A of Figure 4). The cohorts most influenced

¹⁸Importantly, the 1970 census allows us to assign treatment status before the experiment had started. We do not have data on the municipality of residence at the time of the experiment, but the municipality of residence in 1970 likely reflects where an individual went to school. For example, in our own survey which we discuss in detail later, virtually all individuals who were born in Pirkkala also went to school in Pirkkala.

by the experiment were those in fifth grade during 1973–1975, i.e., individuals born in 1962 and 1963. Second, we leverage geographical variation in exposure, as only children living in Pirkkala were affected. To avoid confounding our estimates with the potential effects of the broader education reform, we restrict our control group to municipalities that implemented the comprehensive school reform in the same wave as Pirkkala. These control municipalities are shown in the map in Panel B of Figure 4.

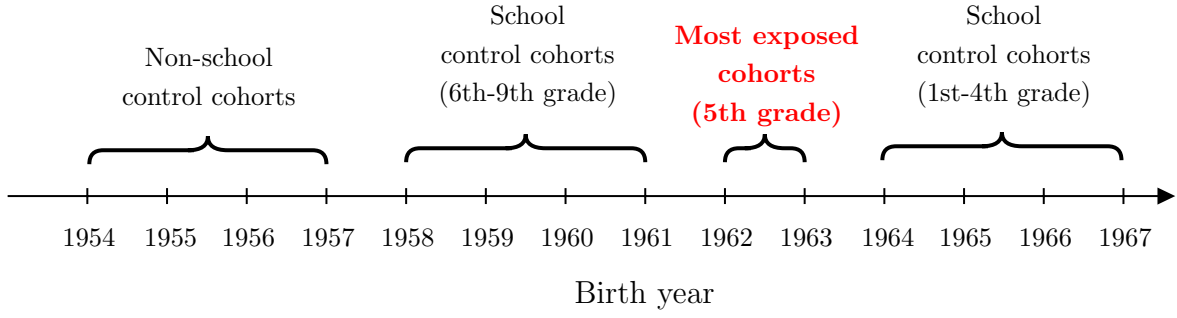
Treatment is assigned by residence in Pirkkala and birth cohort corresponding to fifth-grade exposure. We assign exposure using 1970 municipality of residence to avoid sorting into or out of Pirkkala due to the school experiment.¹⁹ Individuals residing in municipalities (as of 1970) with a similar primary school system but who were not exposed to the socialist experiment serve as a natural control group. The exposed group, fifth graders in Pirkkala, consists of 221 children (distributed across various class groups with typically around 20 pupils) that we can track over time in the data. The administrative data do not include school or class identifiers, so the treatment indicator does not necessarily correspond to direct exposure. We interpret our estimates as the average effects for the entire exposed cohorts, who may have been influenced either directly or indirectly by the school experiment.

Our identification strategy does not hinge on the similarity of the compared municipalities (as we compare within municipalities). Nevertheless, we also conduct our estimations using a sample where the control group is composed of individuals residing in municipalities similar to Pirkkala. We choose these municipalities from the group of municipalities that implemented the comprehensive school reform in the same wave as Pirkkala using propensity score matching. We match Pirkkala with the five most similar municipalities using pre-exposure 1970 characteristics: population, average levels of income, share of university-educated inhabitants, and socialist party vote shares. The selected municipalities (Kemijärvi and Tornio in Northern Finland, and Masku, Kaarina, and Dragsfjärd in Southern Finland) are also shown in Panel B of Figure 4.²⁰

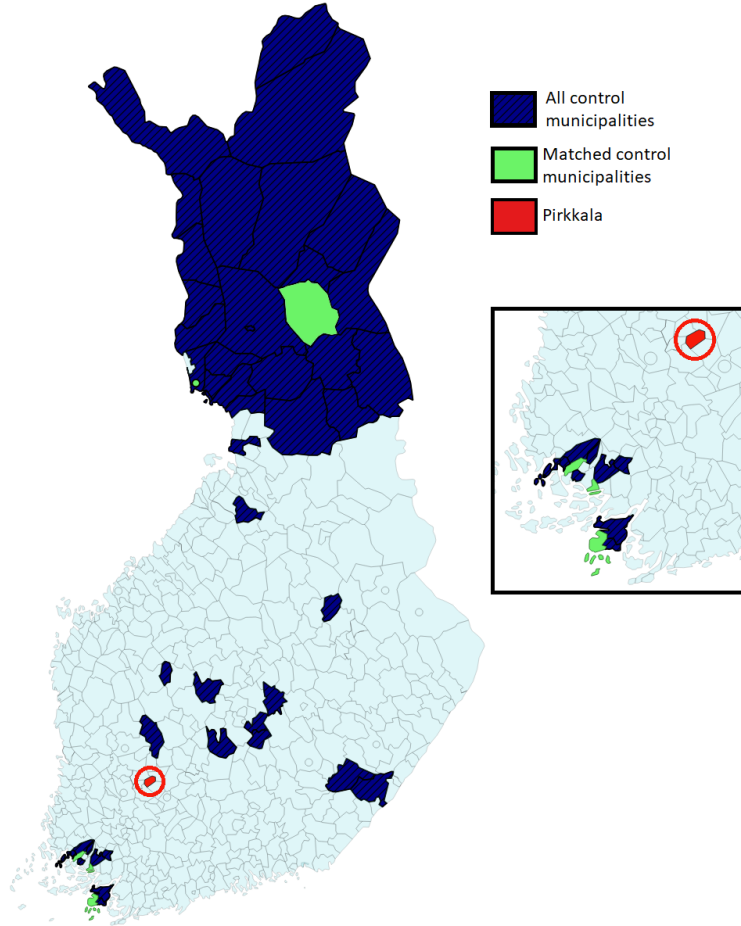
Specification We estimate a generalized difference-in-differences (or a two-way fixed effects) specification that relates adult labor market outcomes with exposure to the experiment as a child. The identifying assumption is that, absent the experiment, cohort differences in Pirkkala would have followed the same pattern as in comparison municipalities. The main threat is therefore an unobserved shock affecting exactly the treated Pirkkala cohorts, but not adjacent Pirkkala cohorts or the same cohorts elsewhere.

¹⁹Our sample excludes very small municipalities with less than 2000 people in 1970.

²⁰We choose five control municipalities in the baseline as this gives balance in the matching variables, but the income estimates are not sensitive to the number of matched municipalities. We show this in Online Appendix Table D.4.



A. Cohort variation.



B. Geographical variation.

Figure 4. Identifying variation.

Notes: The map in Panel B shows the municipality exposed to the socialist experiment (Pirkkala), the other municipalities that had the comprehensive school reform at the same time (plausible control municipalities), as well as five control municipalities that are chosen by propensity score matching as the most alike municipalities to Pirkkala. The matched municipalities are Kemijärvi and Tornio in Northern Finland and Masku, Kaarina, and Dragsfjärd in Southern Finland. Comprehensive school reform data are drawn from [Pekkarinen, Uusitalo, and Kerr \(2009\)](#).

Our main specification takes the following form:

$$Y_{imc} = \beta \mathbf{1}[5th\ grader\ 1973 - 1975_c \times Pirkkala_m] + X'_m \gamma_c + X'_i \sigma + \lambda_m + \delta_c + \varepsilon_{icm}. \quad (1)$$

Here, β is the coefficient of interest. It tells us the impact of the Marxist indoctrination on the exposed cohorts. $\mathbf{1}[5th\ grader\ 1973 - 1975_c \times Pirkkala_m]$ indicates whether the individual was in the fifth grade during the Pirkkala school experiment and also lived in the municipality of Pirkkala in 1970. λ_m represent the municipality fixed effects, and δ_c are the cohort fixed effects. Some specifications include additional covariates: $X'_i \sigma$ includes sex fixed effects and a full set of parent controls obtained from the 1970 census (logarithm of household income, an indicator for owning a house, and indicators for the household head being unemployed, university-educated, having graduated from high school, and working in a managerial or a blue-collar worker occupation), and $X'_m \gamma_c$ captures municipality 1970 characteristics (logarithms of average income and population) interacted with cohort fixed effects. ε_{icm} is the error term.

Inference We cluster our standard errors at the cohort-municipality level as standard in the literature with such a setting (Cantoni et al. 2017; Fouka 2020).²¹ There are 630 clusters in total in the full sample (fourteen cohorts and 45 municipalities) and 84 in the matched sample (fourteen cohorts and six municipalities). Two of the clusters are exposed, and the remaining clusters belong to the control group.

Our data structure, few treated clusters and many controls, is common in applied economics but is known to pose challenges for statistical inference (Dustmann, Schönberg, and Stuhler 2017; Cunningham and Shah 2018; Mastrobuoni 2020). We address this challenge by following state-of-the-art approaches for settings with few treated clusters and report p -values obtained through randomization inferences and wild cluster bootstrap p -values with 999 draws (MacKinnon and Webb 2020; MacKinnon, Nielsen, and Webb 2023; Conley and Taber 2011).

Identifying assumption The identifying assumption is that $cov(\mathbf{1}[5th\ grader\ 1973 - 1975_c \times Pirkkala_m], \varepsilon_{icm} | \lambda_m, \delta_c) = 0$. In other words, we can estimate the causal impact of the Pirkkala school experiment on students' later-life labor market outcomes if the exposed children were not different before the experiment or otherwise differentially exposed conditional on cohort and municipality fixed effects.

²¹Following Abadie et al. (2023), the relevant level is that at which treatment is assigned. Exposure varies at the cohort–municipality level, not at the level of Pirkkala as a whole, since only 5th grade Pirkkala cohorts were exposed.

The fixed effects in our specification take into account several key identification concerns. If Pirkkala was chosen for the test municipality for some reason like political or industrial structure (although there is no evidence for this) the municipality fixed effect would take such worries into account by constraining the comparison to only within municipality. Similarly, if the fifth graders are somehow differently affected everywhere by, for example, labor market fluctuations, the cohort fixed effects would account for this.

The administrative data allow us to provide indirect support for our identifying assumption by regressing pre-determined covariates on the treatment variable. We do this in Table 2, where we consider a number of pre-exposure characteristics of the household or household head in 1970: income, owning a house, employment status, having a university or a high school degree, and working in a managerial or blue-collar occupation.

We find little evidence of pre-exposure differences suggesting that the exposed cohorts were not different beforehand, conditional on cohort and municipality fixed effects. Thus, there is no reason to expect that the outcomes of these treated cohorts would develop differently, which gives us confidence in our estimation strategy. We further show in an event-study estimation that there was no observable differential cohort pre-trend in incomes for the children in the exposed municipality (see Figure 7). Similarly, there is no pre-trend in labor supply which we study as one of the mechanisms (see Online Appendix Figure D.4).

Table 2. Balance of household characteristics in 1970.

	ln(Household Income)	Owns House	Unemployed	University	High School	Manager	Worker
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. Full sample							
Pirkkala $\times$ 5th grade	-0.039 (0.029)	-0.043 (0.032)	-0.004 (0.006)	-0.002 (0.007)	-0.001 (0.039)	0.005 (0.008)	-0.009 (0.019)
N	86147	88485	88485	89129	89129	88481	88481
Y mean	7.629	0.690	0.028	0.043	0.209	0.060	0.281
Randomization inference p	0.587	0.178	0.671	0.861	0.984	0.719	0.797
Wild bootstrap p	0.320	0.321	0.678	0.761	0.983	0.587	0.719
B. Matched sample							
Pirkkala $\times$ 5th grade	-0.019 (0.044)	-0.037 (0.029)	-0.013** (0.006)	0.006 (0.010)	-0.025 (0.045)	0.008 (0.013)	-0.008 (0.022)
N	8018	8201	8201	8245	8245	8201	8201
Y mean	8.093	0.552	0.013	0.058	0.315	0.086	0.370
Randomization inference p	0.760	0.361	0.127	0.779	0.483	0.734	0.841
Wild bootstrap p	0.731	0.439	0.151	0.658	0.707	0.662	0.734

Notes: The table reports $\hat{\beta}$ estimated using $Y_{ime} = \beta \mathbf{1}[5th\ grader\ 1973 - 1975_c \times Pirkkala_m] + X'_m \gamma_c + X'_c \sigma + \lambda_m + \delta_c + \varepsilon_{icm}$. The dependent variables are measured for the household or household head in 1970. All specifications control for municipality and cohort fixed effects. The estimation sample includes 1954-1967 cohorts. Panel A uses the full set of control municipalities and Panel B only the matched control municipalities. Standard errors clustered by municipality $\times$ cohort are shown in parentheses. There are 630 clusters in total in the full sample (fourteen cohorts and 45 municipalities) and 84 in the matched sample (fourteen cohorts and six municipalities). *, ** and *** denote statistical significance at 10%, 5% and 1%, respectively.

4 Main Results

In this section, we study the impacts of the childhood exposure to the Marxist experiment in Pirkkala on long-term incomes. We find sizable adverse income effects. This negative income effect can be attributed to decreased labor supply and occupational choice in particular. We further explore other potential mechanisms.

4.1 Impacts on Long-Term Income

Our main outcome variable of interest is the logarithm of the total taxable income the person gained between 1995-2005 in thousands of 2020 euros. This measure is from the administrative data and available for all individuals in Finland. We use the sum of annual incomes as our preferred measure, as this takes into account the incomes of the treated and control individuals in their prime earning ages (28-51 years old), without relying on any specific year or age.²² This way we can use one variable to assess the long-term, life-time income impacts of the school experiment. However, we stress that the results are robust to various other definitions of the outcome (see Online Appendix Table D.2).

Raw differences We illustrate our main result in Figure 5 which compares raw means of adult incomes (summed over the years 1995–2005) between the exposed and non-exposed cohorts and municipalities. We see that the cohorts in Pirkkala that were exposed to the socialist experiment have around 10% lower incomes, on average, than those cohorts that were not exposed to it (Panel A). At the same time, Panel B shows no major level change between the treatment and the control cohorts in other municipalities in Finland. We further match Pirkkala with observably similar municipalities based on pre-treatment characteristics and compare individuals within this restricted group (Panel C), leading to a similar conclusion.

Baseline estimates We present estimates that we obtain using the specification (1) in Table 3. For the full sample of municipalities (columns 1-3), the exposure to the Marxist school experiment is estimated to lead to a sizable, around 10% reduction in long-run income. The results are robust to adding a battery of household controls and to flexible municipality controls. The results remain highly similar when the control municipalities are limited to those selected via propensity score matching (columns 4-6). Furthermore, we do not find

²²The taxable income includes benefits and we take the sum over ten years, so the number of people without any taxable income over this time period is negligible. We examine the longer ten-year period also due to the recession of Finland in the early 1990s, the recovery from which took until the late 1990s.

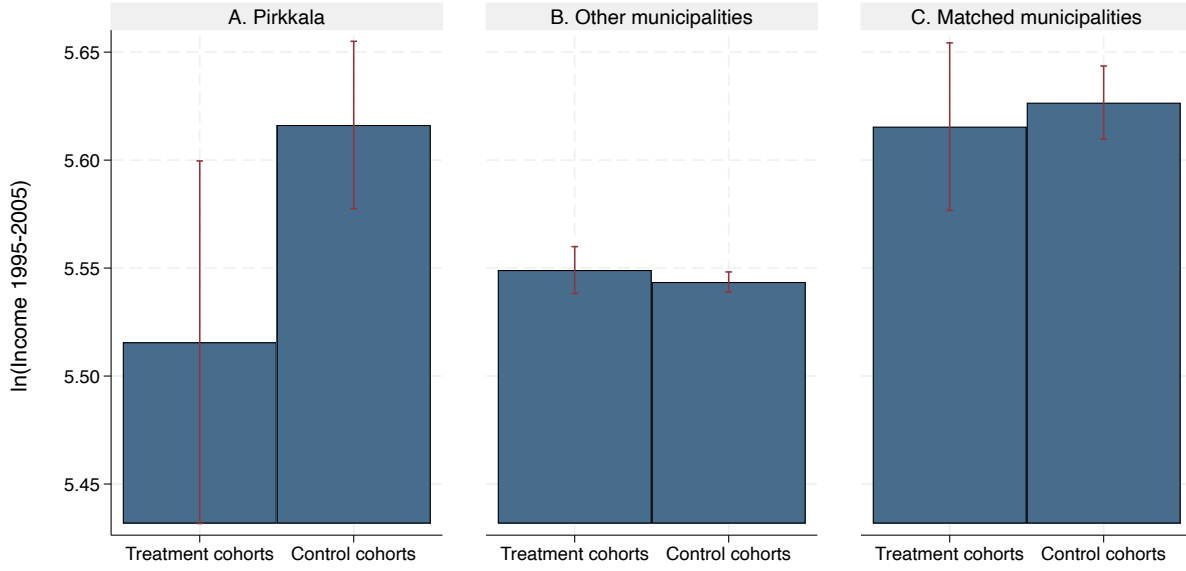


Figure 5. Income differences between treated and control groups.

Notes: The figure presents averages of the logarithm of income over the years 1995–2005 by cohort and municipality. The experiment primarily affected the fifth graders in Pirkkala (treated cohorts). The matched municipalities (Kemijärvi, Tornio, Masku, Kaarina, and Dragsfjärd) are selected based on observable characteristics (see Section 3.1 for further information). In our data, 221 individuals belong to the treated Pirkkala cohorts. We also show 95% confidence intervals.

any effects on students who were attending other grades in the municipality of Pirkkala at the time of the experiment when using a specification that includes also untreated groups (columns 3 and 6), lending support for our empirical design.

The estimates of the effect of exposure to the Pirkkala school experiment in the fifth grade are statistically significant with $p < 0.01$. They remain significant at conventional levels of significance even if we use the more conservative wild bootstrap p -values or randomization inference p -values that account for the uncertainty arising from the small number of treated clusters (MacKinnon and Webb 2020; MacKinnon, Nielsen, and Webb 2023).

We also take a more conservative approach to inference and treat Pirkkala as a single treated cluster. For such settings, standard cluster-robust procedures can be unreliable because they rely on asymptotics requiring many treated clusters. We therefore follow the inference method of Conley and Taber (2011), which is designed for difference-in-differences designs with very few treated units (even just one) but many control units.²³ This approach yields a p -value of 0.02, denoting the share of more extreme values.

²³The key idea is that while the fixed-effects estimator for Pirkkala is not consistent when the number of treated municipalities is fixed at one, the sampling distribution of its noise component can be consistently estimated from the large pool of control municipalities. Operationally, we construct an empirical reference

Full permutation test To understand how likely it is that such long-term results are recovered by chance, we construct a full permutation test in which we re-estimate the model treating each possible pair of cohort-by-municipality cells as exposed, across all 198,134 possible alternatives. This is similar to the randomization-inference p -values reported in the tables, except that here we cover the entire distribution. As Figure 6 shows, the Pirkkala point estimate of -0.129 falls within the most extreme 1.2% of the placebo estimates, giving a p -value of 0.012. This indicates that an estimate of this magnitude is unlikely to arise by chance or to reflect a generic cohort–municipality shock.

Magnitude Our evidence is comparable to estimates of the labor-market effects of socialist education in the prior literature (Fuchs-Schündeln and Masella 2016; Costa-Font, García-Hombrados, and Nicińska 2024). To illustrate the effect magnitude further, let us benchmark it with some other estimates from the literature. The estimated effect roughly corresponds to the economic return to an additional year of schooling in Finland (Uusitalo 1999) and other contexts (Gunderson and Oreopolous 2020). The magnitude is also similar to graduating during a recession and facing a one standard deviation higher unemployment rate in Finland (Päällysaho 2017), increasing class size by ten pupils in Sweden (Fredriksson, Öckert, and Oosterbeek 2012), or reducing spending per pupil by slightly more than 10% for 12 school years in the U.S. (Jackson, Johnson, and Persico 2015).

Event-study estimation The key identifying assumption is that, absent the experiment, outcomes would have evolved similarly for the Pirkkala cohorts and those in the control municipalities. We cannot test this directly, but we can assess it indirectly by checking for parallel trends in the non-exposed cohorts prior to the experiment. This helps ensure that the estimates in Table 3 are not driven by a pre-existing downward trend in income specific to the exposed cohorts.

To assess the parallel-trends assumption, Figure 7 plots estimates of a flexible version of model (1) that interacts exposure with cohort fixed effects, relative to the non-exposed 1960–1961 cohort. We find no differential trends in the cohorts preceding the exposed ones, and no statistically significant effects in the later, non-exposed cohorts. The absence of an observable impact on Pirkkala cohorts on either side of the exposed ones gives us further confidence that we are estimating a causal effect of exposure to the school experiment rather than a spurious pattern.

distribution by re-estimating the treatment effect under placebo assignments that apply Pirkkala’s treatment timing to each control municipality.

Table 3. Effects of the school experiment on long-term income.

	ln(Income 1995–2005)					
	(1)	(2)	(3)	(4)	(5)	(6)
Pirkkala $\times$ 5th grade	-0.115*** (0.019)	-0.129*** (0.022)	-0.148*** (0.029)	-0.109*** (0.023)	-0.114*** (0.027)	-0.120*** (0.039)
Pirkkala $\times$ 1st–4th grades			-0.049 (0.042)			-0.024 (0.042)
Pirkkala $\times$ 6th–9th grades			-0.006 (0.033)			0.007 (0.035)
N	88980	84326	84326	8228	7816	7816
Y mean	5.545	5.545	5.532	5.621	5.621	5.601
Randomization inference p	0.014	0.010	0.005	0.022	0.093	0.089
Wild bootstrap p	0.092	0.063	0.030	0.075	0.064	0.093
Additional controls		✓	✓		✓	✓
Sample municipalities	All	All	All	Matched	Matched	Matched

Notes: The table reports $\hat{\beta}$ estimated using $Y_{imc} = \beta \mathbf{1}[5th\ grader\ 1973 - 1975_c \times Pirkkala_m] + X'_m \gamma_c + X'_i \sigma + \lambda_m + \delta_c + \varepsilon_{icm}$. All specifications include municipality and cohort fixed effects. In columns (2), (3), (5), and (6) we augment the specification with parent controls and municipality controls interacted with cohort. The estimation sample includes 1954–1967 cohorts. Matched control group includes only individuals who resided in Kemijärvi, Tornio, Masku, Kaarina, or Dragsfjärd in 1970. Standard errors clustered by municipality $\times$ cohort are shown in parentheses. There are 630 clusters in total in the full sample (fourteen cohorts and 45 municipalities) and 84 in the matched sample (fourteen cohorts and six municipalities). *, **, and *** denote statistical significance at 10%, 5% and 1%, respectively.

Income dynamics We turn to explore how the exposed individuals’ incomes change over their careers relative to their non-exposed peers. To do so, we run a separate regression using the logarithm of income in each year observed in our data (1980, 1985, and 1993–2010) as the outcome. The estimates and their 95% confidence intervals are shown in Figure 8. The point estimates for prime earning ages are negative and statistically significant. At the peak year, 2003, the income reduction is around 20%, although the point estimates are largely statistically indistinguishable from one another. While the patterns are interesting, the estimates are not perfectly comparable, owing to differences in macroeconomic conditions and in the ages of the comparison sample. The estimates over the baseline period 1995–2005 are by far the most reliable, as the entire comparison sample is still in prime working age and the Finnish economy is not in recession.²⁴

²⁴The over-time variation in the estimated income effects may reflect life-cycle dynamics, in which the consequences of early occupational sorting—such as selecting into lower-paying or less stable occupations—

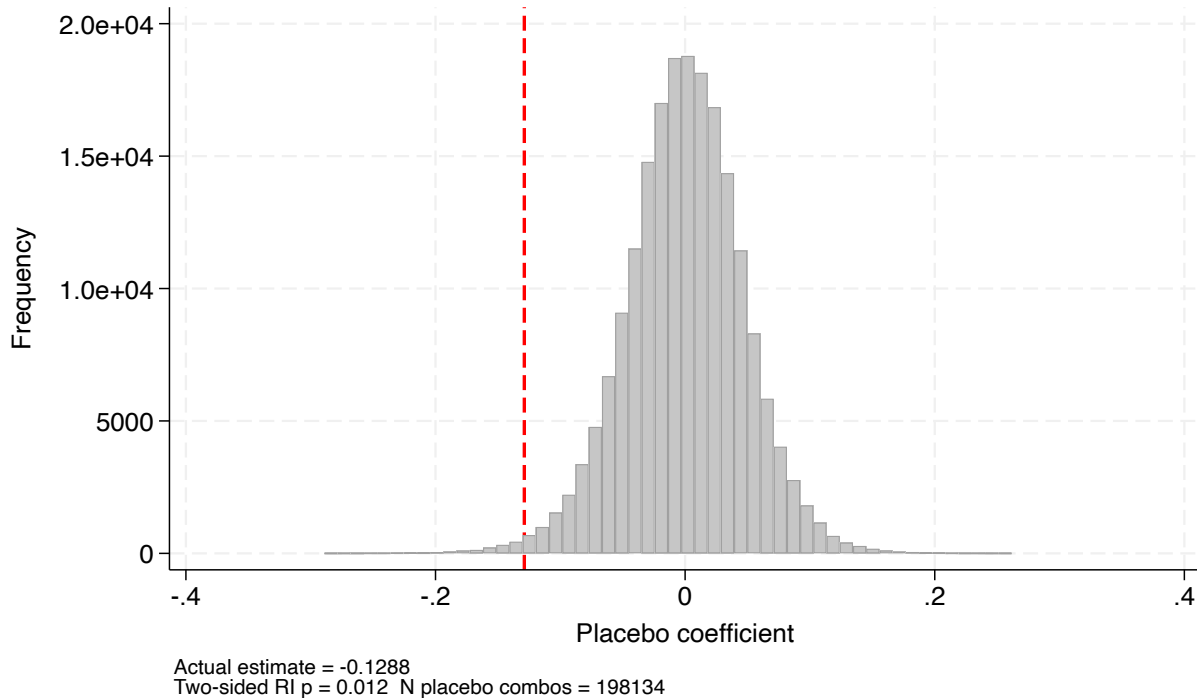


Figure 6. Full permutation test

Notes: The figure plots point estimates from specification $Y_{imc} = \beta_c \mathbf{1}[Cohort_c \times Pirkkala_m] + X'_m \gamma_c + X'_i \sigma + \lambda_m + \delta_c + \varepsilon_{icm}$ where two municipality-cohorts are selected as exposed at a time over the 198,134 possibilities. All estimations include municipality and cohort fixed effects, parent controls, and municipality controls interacted with cohort. The vertical line marks the exposed baseline estimate.

4.2 Robustness Checks

We perform a series of robustness checks to explore the stability of our baseline income findings, which are detailed in the Online Appendix D.2. These analyses support that long-term incomes of the exposed children were negatively impacted by the socialist Pirkkala school experiment.

Online Appendix Table D.2 tests the robustness of results to different income measures; sum of wages and mean income from 1995-2005, levels instead of logs, income at age 40, and total income from longer 1993-2010 period, all showing consistent results. Online Appendix Table D.3 examines alternative specifications and samples. The results are robust to (i) including municipality-specific cohort trends to account for time-varying municipality-specific factors (only identifying the jump in the exposed cohorts); (ii) adding

could compound over the career. In addition, macroeconomic conditions likely interact with these occupational choices, amplifying income disparities during downturns, when less-secure jobs are more exposed to shocks.

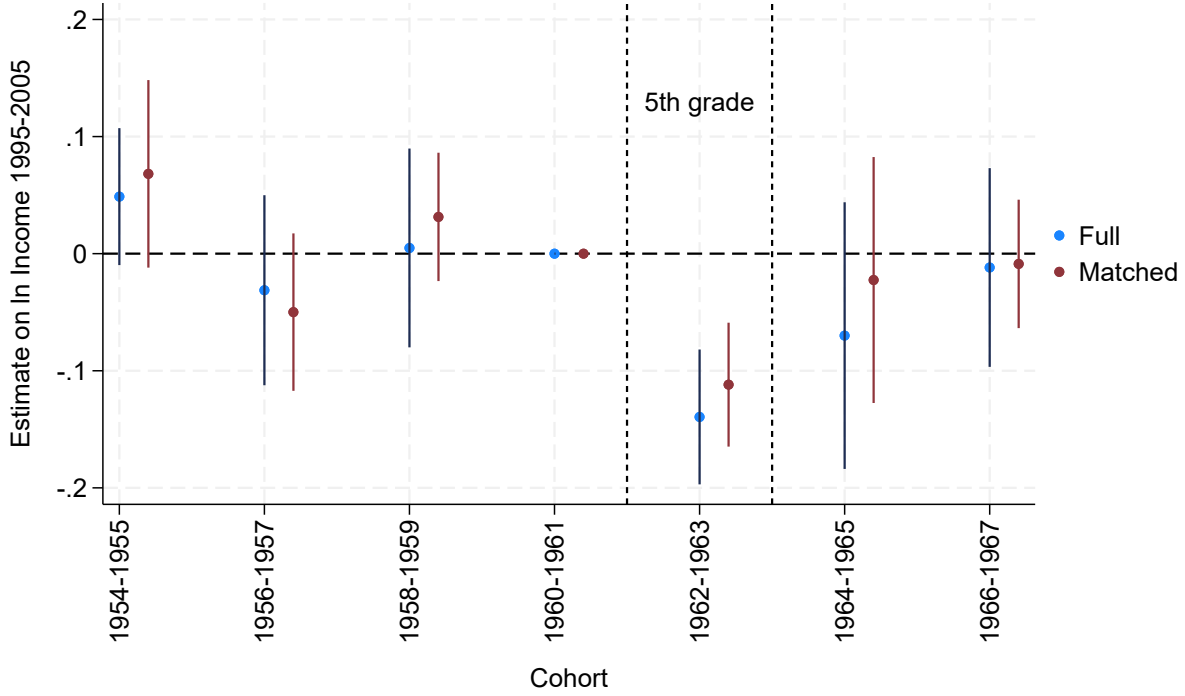


Figure 7. Estimated income effects by cohort.

Notes: The figure plots point estimates of cohort-specific effects (relative to the 1960-1961 cohort) from an event-study specification $Y_{imc} = \beta_c \mathbf{1}[Cohort_c \times Pirkkala_m] + X'_m \gamma_c + X'_i \sigma + \lambda_m + \delta_c + \varepsilon_{icm}$. We use 1960-1961 cohorts as the base group. All estimations include municipality and cohort fixed effects, parent controls, and municipality controls interacted with cohort. The vertical line marks the exposed cohorts (cohorts born 1962-1963). Matched control group includes only individuals who resided in Kemijärvi, Tornio, Masku, Kaarina, or Dragsfjärd in 1970. We also show the 95% confidence intervals constructed using standard errors clustered at the municipality-cohort level. There are 630 clusters in total in the full sample (fourteen cohorts and 45 municipalities) and 84 in the matched sample (fourteen cohorts and six municipalities).

finer urban area fixed effects which restrict the comparisons to within-neighborhood level; (iii) using alternative cohort samples (1958-1970, 1950-1970, 1962-1966, and 1961-1964); (iv) including additional control variables from the 1970 household data; (v) removing possible influential outliers from the estimation sample via winsorization; and (vi) including additional municipality-level controls for the socialist party vote share in parliamentary elections, seat share of socialists in the local council, and school spending in 1972 (all interacted with cohort fixed effects). Online Appendix Table D.4 shows that our findings remain similar when we vary the number of matched control municipalities, with results also robust to having only one matched comparison municipality with a balanced data structure.

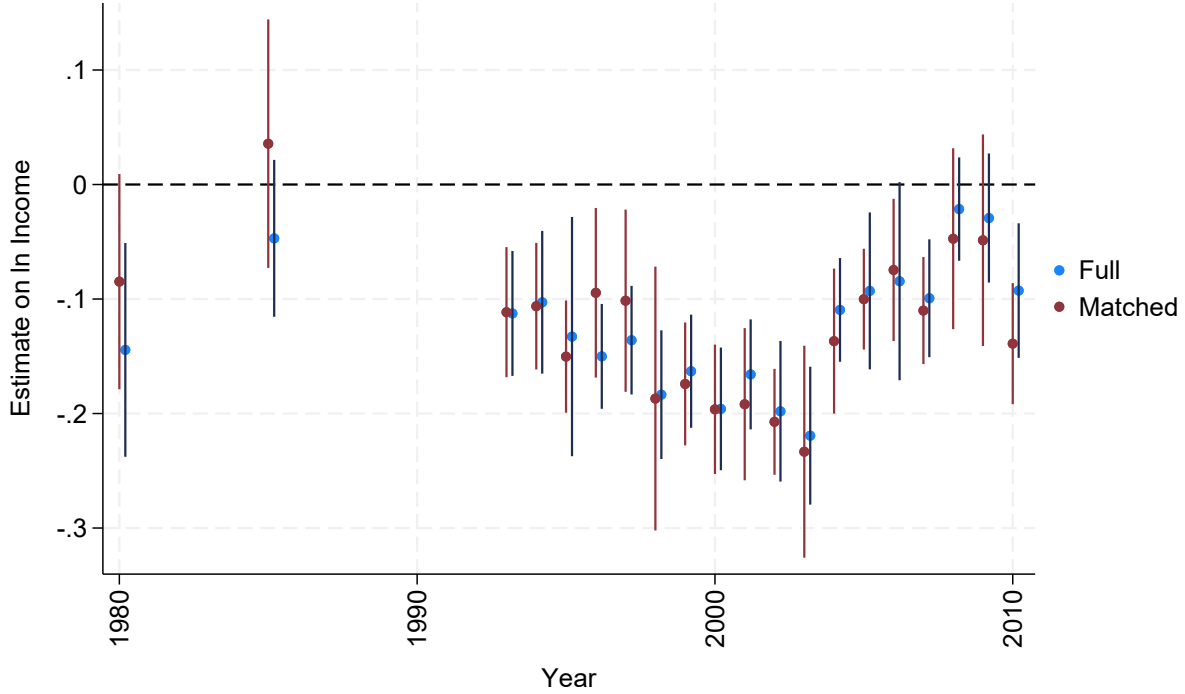


Figure 8. Estimated income effects over life cycle.

Notes: The figure plots $\hat{\beta}$ estimated separately for each year using $Y_{imc} = \beta 1[5th\ grader\ 1973 - 1975_c \times Pirkkala_m] + X'_m\gamma_c + X'_i\sigma + \lambda_m + \delta_c + \varepsilon_{icm}$. All estimations include municipality and cohort fixed effects, parent controls, and municipality controls interacted with cohort. Matched control group includes only individuals who resided in Kemijärvi, Tornio, Masku, Kaarina, or Dragsfjärd in 1970. We also show the 95% confidence intervals constructed using standard errors clustered at the municipality-cohort level. There are 630 clusters in total in the full sample (fourteen cohorts and 45 municipalities) and 84 in the matched sample (fourteen cohorts and six municipalities).

4.3 Alternative estimation strategies

We verify that the negative income effect survives four alternative estimation strategies.

Individual-level matching In Online Appendix Table D.5, for each individual in Pirkkala—in both the treated and control cohorts—we use propensity-score matching on baseline 1970 family characteristics to find the closest individual in the same cohort residing in a control municipality.²⁵ We then re-estimate our baseline specification (1) on the matched sample, adding match (pair) fixed effects, so that identification comes from comparing observably similar individuals. The individual-level matching estimates on

²⁵The 1970 household characteristics are the logarithm of household income, an indicator for owning a house, and indicators for the household head being unemployed, university-educated, having graduated from high school, and working in a managerial or blue-collar occupation.

long-run income presented in Table D.5 remain negative, statistically significant, and nearly identical in magnitude to our baseline estimates (a reduction of roughly 10%). The estimates are similar when using only the matched control municipalities as the pool. An additional benefit of the individual-level matching strategy is that here the Pirkkala cohorts and the cohorts in the comparison municipalities are of equal size, ensuring that differences in group size are not driving the results.²⁶

Synthetic control and synthetic difference-in-differences Because Pirkkala is the only exposed municipality, we benchmark it against a data-driven counterfactual. After netting out the individual household controls and aggregating income by municipality and two-year cohorts, the synthetic control method (Abadie 2021) produces a clearly visible drop of around 15% in the incomes of the treated Pirkkala cohorts relative to a synthetic Pirkkala cohorts constructed from the donor municipalities (presented in Online Appendix Figure D.1). The synthetic difference-in-differences estimator (Arkhangelsky et al. 2021) delivers a comparable, statistically significant decline over the clean pre-post 1954-1963 window presented in Online Appendix Figure D.2. The caveat of using the synthetic control, and the reason it is not our preferred method, is that it does not function when treatment turns on and off, as in our case, and it requires a balanced panel, meaning we cannot fully utilize the individual-level data with individual controls as in our baseline specification. The synthetic difference-in-differences estimator shares these limitations in our setting, which is why we restrict it to the 1954-1963 window.

Difference-in-discontinuities As exposure to the experiment was determined by birth cohort in Pirkkala, we exploit the sharp date-of-birth cutoffs that governed eligibility to implement a difference-in-discontinuities design in Online Appendix D.2. We compare the discontinuity in adult income at the eligibility cutoffs in Pirkkala with the corresponding discontinuities in the control municipalities, within a one-year bandwidth. The running variable is the distance in days between a child’s birth date and the relevant eligibility cutoff, and we pool the two cutoffs that bound the exposed cohorts (January 1, 1962 and December 31, 1963).

The illustrative discontinuity Figure 9 (and Online Appendix Figure D.3) shows a clear downward jump in long-term incomes in Pirkkala around the pooled eligibility cutoffs, with no comparable discontinuity visible in the control municipalities. The corresponding difference-in-discontinuities estimate is -0.20 ($p < 0.01$), implying a long-term income

²⁶Note that we do not have an individual-level panel (i.e., incomes both before and after treatment), which constrains the individual-level matched difference-in-differences design we can perform.

decline of about 18%. The effect holds at each eligibility cutoff separately and is robust to controlling for 1970 parental characteristics, adding cutoff fixed effects, and restricting the control group to the matched municipalities in Online Appendix Table D.6.²⁷ Because this design identifies the effect using jumps at the birth-date cutoffs, it is more demanding than our baseline TWFE specification in a setting with a relatively limited exposed group. We therefore treat the difference-in-discontinuities estimates as additional evidence rather than as our baseline specification.

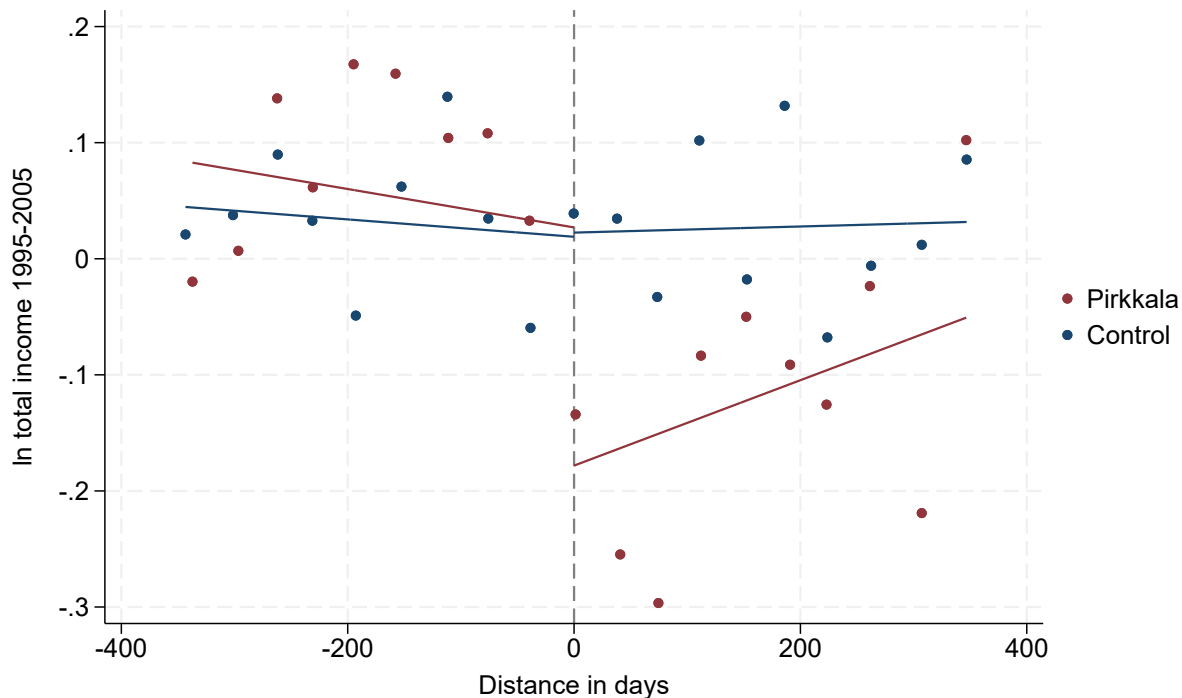


Figure 9. Descriptive discontinuities around the eligibility cutoff.

Notes: The figure plots the discontinuity in long-term income at the pooled birthdate cutoffs (January 1, 1962, and December 31, 1963), estimated separately on each side of the cutoff for Pirkkala and the matched control municipalities. The x-axis denotes the distance to the eligibility cutoff in days, with positive values indicating exposure to the experiment. Baseline household controls are netted out. The matched control group includes only individuals who resided in Kemijärvi, Tornio, Masku, Kaarina, or Dragsfjärd in 1970.

4.4 Mechanisms

We leverage the rich register data to examine the possible mechanisms that could be behind the sizable reduction in long-term incomes. We find evidence that the childhood

²⁷Although this estimate is somewhat larger than our two-way fixed-effects estimate, it attenuates toward the baseline as the bandwidth narrows, while remaining systematically negative.

exposure especially reduced work time as an adult as well as impacted occupational choice towards occupations that could be characterized as more civic-minded and valued by socialists. We find less clear effects on later education or cognitive ability and economically valuable personality traits.

Labor supply We first analyze whether reduced labor market participation or increased unemployment could explain the negative income effects. The Marxist class content could have, for example, reduced the willingness to work in the capitalist system or affected exposed individuals' values so that they became less materialist. To measure labor supply decisions, we use the register data to construct the average of months worked each year over the years 1997-2005, the average of months unemployed each year over the years 1995-2005, and the logarithm of total months worked 1997-2005.²⁸

Panel A of Table 4 shows a substantial reduction in labor supply. The school experiment led to the exposed individuals working nearly one ($0.69 - 0.84$) month less each year, a 16% reduction in total months worked, and being unemployed for around 0.3 more months each year. Relative to the mean of 7.4 months, the effect size of the reduction in yearly months worked is sizable and can explain a large part of the observed income differences.²⁹ Notably, the estimates for unemployment are only one third of the estimate for labor supply, suggesting that the majority of the labor supply effect comes from other sources, such as reduced work time. The result on decreased labor supply also holds up in an event-study estimation framework (Online Appendix Figure D.4). Furthermore, the year-specific estimates closely mirror the results we show for the income effects over the life cycle (Online Appendix Figure D.5).

This result lines up with the broader evidence from formerly socialist economies, where prolonged exposure to communist rule is associated with diminished work ethic (Linz and Chu 2013) and weaker beliefs in personal agency over economic outcomes (Alesina and Fuchs-Schündeln 2007; Bondar and Fuchs-Schündeln 2023). The labor market consequences of socialist education are documented most directly by Fuchs-Schündeln and Masella (2016) and Costa-Font, García-Hombrados, and Nicińska (2024); our finding shows that ideological classroom content alone, absent the broader institutional features of socialism, can generate similar labor supply responses.

Furthermore, consistent with this channel, descriptive cross-sectional evidence from the Finnish samples of the World Values Survey shows that more left-leaning respondents hold systematically weaker pro-work attitudes (Online Appendix Figure D.6), though we note

²⁸1997 is the first year for which the months worked information is available in the administrative data.

²⁹In the mediation analysis of Table 5, we show that the reduced work time indeed explains much of the observed income effect.

this evidence comes from the general Finnish population rather than the Pirkkala-exposed individuals themselves and should be read as illustrating a plausible channel rather than directly testing it within our sample. Here, self-placement on the political left rather than the right is associated with weaker agreement with the statements “people who don’t work turn lazy”, “work is a duty toward society”, “it is humiliating to receive money without having to work for it”, and “work, rather than leisure, makes life worth living.”

Occupational choice A plausible way in which changed values resulting from exposure to experiment could manifest is through occupational choice. Individuals may opt for jobs with more free time or more societal impact, or occupations that adhere better to the socialist ideals, even if they come with lower salaries.

To examine this effect, we take a data-driven approach and identify the occupations that exposed individuals select more frequently relative to the average prevalence of the occupations. We use socio-economic groupings of occupations in the year 2000 as provided by Statistics Finland and create dummy variables for each group for employed individuals. This is the mid-year of our main sample and also matches with prime earning ages of 37–38. We then estimate the effect size separately for each of the 13 groups using specification (1). To allow compatibility of the effects, we report estimates relative to the group mean.³⁰

Figure 10 presents the ranking of these estimated descriptive occupational effects. We see the largest positive impact of the socialist school experiment for the occupational groups “senior officials in education” and “other lower-level employees.” In Online Appendix Tables C.2 and C.3, we show the occupational distribution for the entire country within the socioeconomic groups with the most sizable effects. The first group is mostly composed of teachers, and the second group contains professions that “share the objective of ensuring people’s immediate well-being, safety, and cultural or entertainment needs,” according to Statistics Finland’s classification. 65% of individuals in this category are nurses.

We also see strong effects of the school experiment on the likelihood of choosing agricultural and manufacturing blue-collar work, whereas children exposed to the experiment were much less likely to hold management positions or serve as senior officials or supervisors in adulthood.³¹ The occupational choice effects are present even in the

³⁰Due to Statistics Finland disclosure rules, we cannot report the raw means for each fine occupation group separately by exposed and non-exposed and have to rely on regression estimates for larger socioeconomic groupings.

³¹The largest occupation within the “senior officials in planning” is business professionals (see Online Appendix Table C.4).

absence of discernible effects on the probability of acquiring university or high school education, as we show below.

In Online Appendix Table D.10, we document that the chosen occupations exhibit somewhat lower average labor supply and incomes. Thus, some of the effect we have documented thus far may plausibly run through occupational choice, and it is unlikely that we would be observing a mere labor supply effect.³²

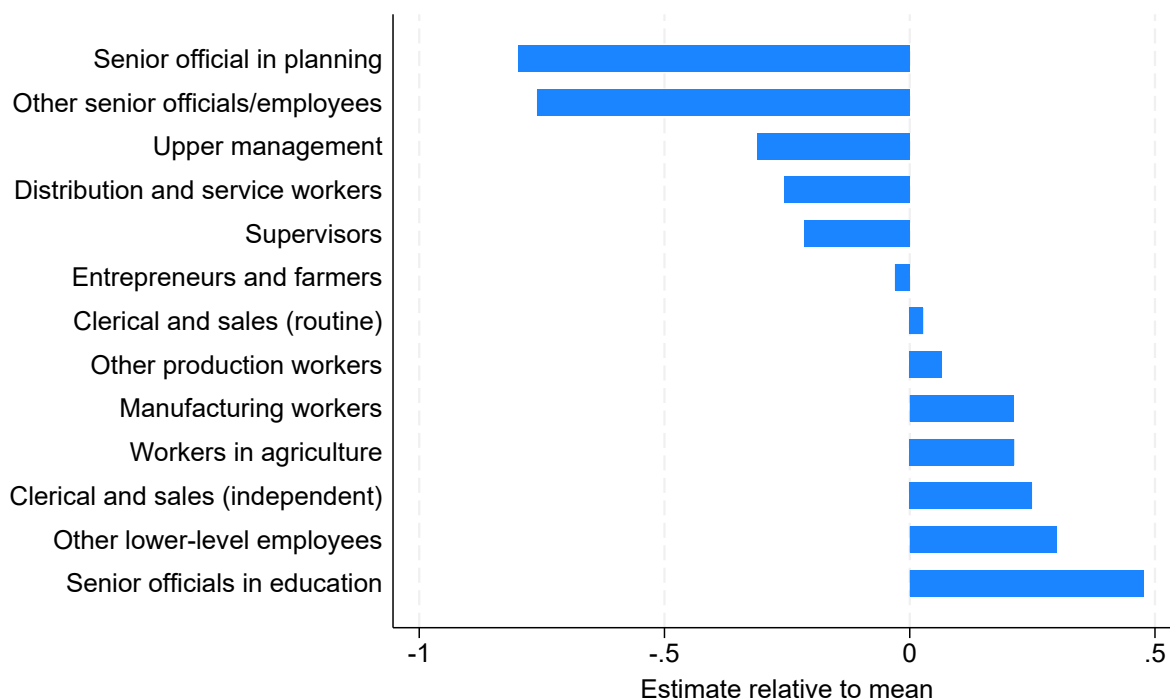


Figure 10. Descriptive evidence on school experiment and occupational choice.

Notes: The figure ranks the estimates relative to the mean resulting from the specification $Y_{imc} = \beta_1[5th\ grade\ 1973 - 1975_c \times Pirkkala_m] + X'_m\gamma_c + X'_i\sigma + \lambda_m + \delta_c + \varepsilon_{imc}$. The outcome variables are dummies for each of the 13 broader occupational groups in 2000. We estimate the model using 1954-1967 cohorts. For descriptions of all occupational groups, see Online Appendix C.1.

Mapping occupations to political views The descriptive results in Figure 10 show that exposed children were more likely to choose proletarian and civic-minded professions, such as teaching and nursing, and to opt out of managerial positions in adulthood. But are these chosen occupations truly “more socialist?” This aspect is difficult to quantify directly, as it can vary across time and context.

³²The differences in occupational choice may also drive at least some of the differences in labor supply, for instance, if temporary work contracts are more prevalent in the occupations chosen by the treated individuals.

To examine whether the occupational choices broadly align with more socialist views in Finland around the time the occupational choice is observed, we use an occupation appreciation survey conducted by the Finnish magazine *Suomen Kuvalehti*. Survey respondents were asked to rank occupations based on their perceived appreciation and also to indicate which political party they support. This survey provides us with data on which political party supporters appreciate a certain occupation the most and least, for approximately 200 occupations.³³

We match the occupation appreciation scores to the occupation codes found in 2000 in the administrative data and then estimate the equation (1). The outcome is now an indicator of whether the person’s occupation in 2000 was most appreciated or valued by the left-wing or the right-wing respondents (supporters of the Left Alliance or the National Coalition Party, respectively). The estimates shown in Panel B of Table 4 reveal that the students exposed to the school experiment were substantially more likely to select occupations appreciated by the left and less likely to select occupations appreciated by the right.³⁴

Educational attainment Education differences could be a plausible channel to explain the negative income impacts. For example, the missed schooling in the fifth-grade history that was replaced by the Marxist content could have impacted later learning if the children fell behind. Such missed schooling could have accumulated over time and led to worse education outcomes that show up in worse labor market performance. We find limited evidence that reduced educational attainment plays a major role in the observed income effects.

We measure the person’s educational attainment in the year 2000 by three metrics available in the administrative registers: years of education (imputed from the highest academic degree that a person holds), and dummies for having a university degree and graduating high school. Panel C of Table 4 presents the estimation results.

There is some evidence of around a 0.2–0.3 year reduction in years of education. However, the magnitude of these impacts is too small to explain the substantial income losses. As we show in the mediation analysis of Table 5, controlling for the years of education does not affect our income effect estimates substantially. This indicates that the observed income differences are not largely driven by differences in educational attainment. The estimated effects for having a university degree and graduating high school are statistically insignificant and small in magnitude.

³³Further information on the survey can be found in Online Appendix C.2.

³⁴Online Appendix Table D.11 further shows that the exposed individuals are less likely to select occupations less valued by socialists and more likely to select occupations less valued by conservatives. Additionally, in Online Appendix Table D.10, we combine our data with survey-based measures of voting intentions by occupation to assess the inherent political orientation of the chosen occupations. We see that the exposed individuals choose occupations where voting for the socialist party is more common.

Table 4. Evidence on mechanisms.

A. Labor market outcomes						
	Yearly months worked		Yearly months unemployed		ln(Total months worked)	
	(1)	(2)	(3)	(4)	(5)	(6)
Pirkkala $\times$ 5th grade	-0.691*** (0.159)	-0.843*** (0.103)	0.277*** (0.081)	0.334*** (0.105)	-0.169** (0.067)	-0.178*** (0.062)
N	84468	7833	84468	7833	84440	7830
Y mean	7.411	7.672	1.470	1.117	4.100	4.161
Randomization inference p	0.011	0.042	0.137	0.152	0.042	0.149
Wild bootstrap p	0.125	0.013	0.116	0.098	0.152	0.144
B. Appreciation of chosen occupations						
	Most appreciated by left		Most appreciated by right			
	(7)	(8)	(9)	(10)		
Pirkkala $\times$ 5th grade	0.064*** (0.023)	0.104*** (0.022)	-0.037** (0.016)	-0.081*** (0.021)		
N	54741	4848	54741	4848		
Y mean	0.609	0.557	0.065	0.090		
Randomization inference p	0.164	0.146	0.106	0.088		
Wild bootstrap p	0.134	0.055	0.157	0.085		
C. Educational attainment						
	Years of education		University degree		HS graduate	
	(11)	(12)	(13)	(14)	(15)	(16)
Pirkkala $\times$ 5th grade	-0.218* (0.122)	-0.340*** (0.101)	-0.004 (0.010)	0.002 (0.014)	-0.033 (0.046)	-0.048 (0.053)
N	84468	7833	84468	7833	84468	7833
Y mean	12.513	12.636	0.099	0.127	0.296	0.377
Randomization inference p	0.203	0.287	0.954	0.888	0.409	0.571
Wild bootstrap p	0.224	0.084	0.725	0.869	0.708	0.683
Sample municipalities	All	Matched	All	Matched	All	Matched

Notes: The table reports $\hat{\beta}$ estimated using $Y_{ime} = \beta_1[5th\ grader\ 1973 - 1975_c \times Pirkkala_m] + X'_m\gamma_c + X'_i\sigma + \lambda_m + \delta_c + \varepsilon_{icm}$. In Panel A, yearly months worked or unemployed is measured as an average over 1997-2005 and total months worked as the sum over 1997-2005. In Panel B, the outcomes are indices based on occupation appreciation survey conducted by the magazine *Suomen Kuvalehti*. *Most appreciated by Left* refers to an occupation being most appreciated by people supporting the socialist Left Alliance, and *Most appreciated by Right* refers to an occupation being most appreciated by people supporting the conservative National Coalition Party. In Panel C, years of education, university degree, high school graduate indicator are measured in 2000. Years of education is an imputed measure based on education codes. All specifications include municipality and cohort fixed effects, parent controls, and municipality controls interacted with cohort. The estimation sample includes 1954-1967 cohorts. Matched control group includes only individuals who resided in Kemijärvi, Tornio, Masku, Kaarina, or Dragsfjärd in 1970. Standard errors clustered by municipality $\times$ cohort are shown in parentheses. There are 630 clusters in total in the full sample (fourteen cohorts and 45 municipalities) and 84 in the matched sample (fourteen cohorts and six municipalities). *, ** and *** denote statistical significance at 10%, 5% and 1%, respectively.

Military service We study the experiment's impact on military service. We use data from tests administered by the Finnish Defense Forces to all conscripts, usually at age 18, to evaluate their leadership abilities. One limitation is that these test data begin with the cohort born in 1962, so we lack a pre-trend analysis. The data are available only for men who entered military service; linked to the administrative data, they let us identify who entered the service.

Figure 11 shows a sizable negative impact (around 10%) on military service. While military service was compulsory in Finland, individuals in the cohorts we study could instead

apply to perform fourteen months of civil service. The negative impact is consistent with a lower willingness to serve in the military, which in Finland has historically been associated with more conservative ideology (Lehtinen 1996). Because this outcome is measured early in life, around age 18, it suggests that whatever made these cohorts different had emerged before then. It also makes it less likely that our economic results are driven by a later idiosyncratic shock hitting the same individuals in adulthood.

Cognitive ability and personality traits We further examine whether the experiment affected the cognitive ability or personality traits of the exposed children, using military tests administered to evaluate leadership abilities. These data allow us to assess whether the experiment impacted students' learning or their ability to learn, which could in turn depress adult incomes.³⁵

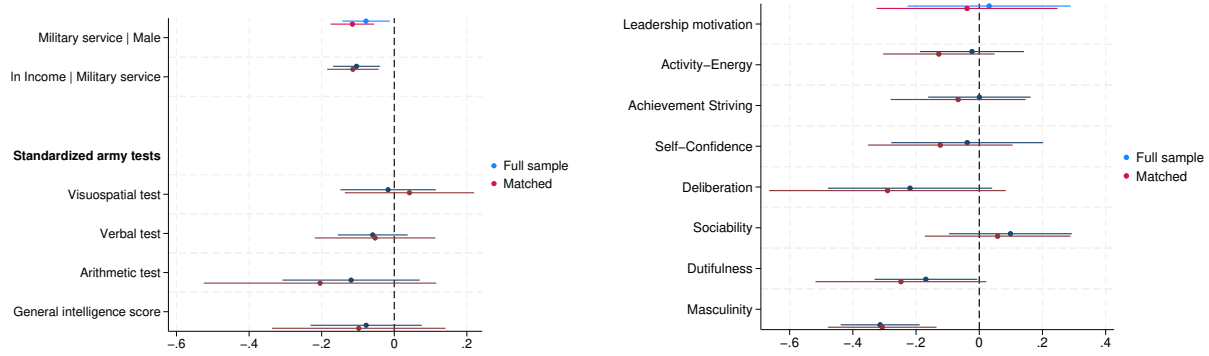
As shown in Figure 11, the experiment had no statistically or economically meaningful impact on cognitive ability. The adverse income effects, however, remain visible and similar in magnitude to the baseline estimates when we condition on completing service (Figure 11), so selection into service does not drive the results, and the tests provide a meaningful basis for assessing whether reduced cognitive skills (for example, from less or lower-quality teaching) could explain the observed income effects.

Similarly, there is no observable impact on economically valuable personality traits such as achievement striving (Figure 11). Exposure to the experiment could plausibly make individuals less inclined to work hard for what they want, leading to lower incomes; we find little evidence for such a mechanism. This is consistent with the exposed cohorts having similar educational attainment, as shown in Table 4.

Two traits show some signs of statistical significance. The first is dutifulness, the extent to which a person follows social norms, where the estimate for the full sample is significant at the 5% level. The second is masculinity, which measures the extent to which a person has occupational and recreational interests traditionally considered masculine rather than feminine. This aligns with our finding on differences in occupational choice.

Overall, we find limited evidence that the experiment impacted cognitive ability or economically valuable personality traits. This suggests that the exposure worked more through changes in other margins, such as preferences for leisure or for socially meaningful and civic-minded work.

³⁵The military assessment consists of two distinct tests: a cognitive ability test and a personality test. The cognitive ability test measures three dimensions of intelligence: verbal, arithmetic, and visuospatial reasoning. The personality test captures eight economically valuable personality traits. See Online Appendix C.3 for further information on the tests.



A. Military service, income conditional on military service, and cognitive ability.

B. Personality traits.

Figure 11. Effects of the school experiment on cognitive ability and personality traits.

Notes: The figure presents the estimation results using outcomes from the Finnish Defense Forces enlistment tests and the specification $Y_{imc} = \beta 1[5th\ grader\ 1973-1975_c \times Pirkkala_m] + X'_m \gamma_c + X'_i \sigma + \lambda_m + \delta_c + \varepsilon_{icm}$. All regressions include municipality fixed effects, cohort fixed effects, and all controls except for sex, as the data only cover men (born after 1962). Matched control group includes only individuals who resided in Kemijärvi, Tornio, Masku, Kaarina, or Dragsfjärd in 1970. The figures also report 95% confidence intervals constructed using standard errors clustered by municipality $\times$ cohort. There are 270 clusters in total in the full sample (six cohorts and 45 municipalities) and 36 in the matched sample (six cohorts and six municipalities).

Survey evidence We conducted a follow-up survey to capture potential long-term effects of the school experiment on individuals' views, as these are not observable in the administrative data. These new survey data were collected by Taloustutkimus between late 2024 and early 2025. The survey targeted 7,265 individuals who, according to the Finnish Population Registry, resided in Pirkkala (the treatment municipality) or in matched Finnish-speaking control municipalities either in 1970 or at the time of the survey. Invitations were mailed based on registry data; however, treatment status is assigned based on respondents' self-reported birth year and municipality, as access to registry-based information on these variables was restricted. Our final analysis sample consists of 817 respondents who were born in Pirkkala or one of the control municipalities, 18 of whom are classified as treated.

Although the response rate was low and the sample size limits precision, the pre-registered survey outcomes in Figure 12 provide suggestive evidence supporting the key argument that the experiment had lasting effects on those exposed.³⁶ We report difference-in-differences estimates for a number of indices constructed based on the survey responses. The indices fall into five broader categories: job characteristics (security and

³⁶The pre-analysis plan is available at <https://osf.io/jufnk/>. It contains additional information on the survey such as the invitation letter and pre-registered hypotheses. Further information on the survey as well as additional results can also be found in Online Appendix E.

stability, flexibility, compensation and career, workplace atmosphere), work values (intrinsic and extrinsic), work ethic (effort and persistence, commitment and attitude), career satisfaction, and economic attitudes (beliefs about poverty and wealth/own effort versus external factors, redistributive preferences, pro-market attitudes, zero-sum thinking). Higher values on these indices indicate stronger endorsement of the underlying concept.

Exposure to the Pirkkala experiment appears to have negatively affected career satisfaction (significant at the 1% level) and may have reduced the importance individuals place on workplace atmosphere and the intrinsic value of work. On the one hand, this may seem counterintuitive, given the content of the indoctrination experiment. On the other hand, Marxism typically views labor under capitalism as exploitative and alienating. For other indices, we do not find any economically or statistically significant differences, making the results on work-related outcomes inconclusive.

In terms of economic attitudes, exposure is associated with weaker pro-market views and increased zero-sum thinking, which aligns with the central lessons of the teaching material used in the Pirkkala experiment. Given the small number of exposed respondents, however, we emphasize that these survey findings are suggestive.

Additional outcomes We additionally examined long-run wealth and property ownership and find negative effects on these outcomes (Online Appendix Table D.7). These patterns are consistent with both the anti-capitalist orientation of the teaching material and the sizeable income losses documented above.

4.5 Effect Heterogeneity

In Online Appendix Table D.8, we document remarkably similar effects for the two treated cohorts (1962 and 1963), for men and women, and for children from high- and low-income households, indicating that the results are unlikely to reflect gender- or socioeconomic-specific shocks in Pirkkala during the 1970s. By contrast, we find larger effects for individuals with below-median cognitive ability according to the (possibly endogenous) military tests, consistent with an instructional mechanism that disproportionately affected lower-ability students.

5 Discussion

The negative effect of the socialist Pirkkala school experiment on the exposed students' long-term incomes is sizable and robust. In this section, we assess the relative importance of the

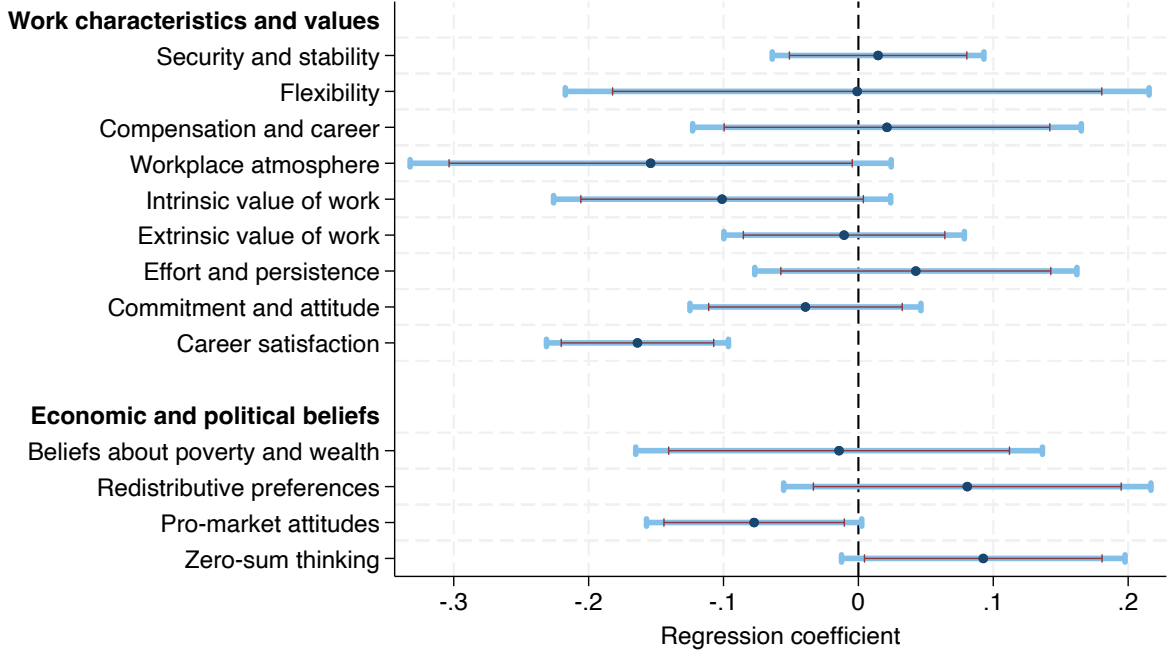


Figure 12. Differences in survey responses.

Notes: The figure presents the estimation results using indices constructed based on our survey and the specification $Y_{imc} = \beta \mathbf{1}[5th\ grader\ 1973 - 1975_c \times Pirkkala_m] + \mathbf{1}[Pirkkala_m] + \mathbf{1}[5th\ grader\ 1973 - 1975_c] + \varepsilon_{icm}$. In Online Appendix Figure E.2, we show that the results are robust to using the same two-way fixed effects specification that we use in our main analyses. Higher index values indicate stronger endorsement of the underlying concept. The estimation sample consists of survey respondents born in Pirkkala or one of the control municipalities and belonging to the relevant cohorts. The figure also reports 95% and 90% confidence intervals (thick and thin lines, respectively) constructed using standard errors clustered at the birth municipality $\times$ cohort level.

plausible mechanisms as well as alternative explanations to our findings. We emphasize that any competing explanation would need to generate a cohort- and municipality-specific shock that matches the timing, location, and persistence of the effects we document. We view such scenarios difficult to reconcile with the evidence; the full set of empirical checks consistently points to the classroom intervention itself explaining our results.

5.1 Descriptive Decomposition of Mechanisms

To shed light on the relative importance of the plausible mechanisms, we conduct a naïve mediation analysis in Table 5. Because education, labor supply, and occupation are post-treatment outcomes, these specifications should not be interpreted as causal mediation estimates. They are intended to show which observed variables account mechanically for the income gap. Controlling for the years of education (column 2) makes

the point estimates slightly smaller. In columns (2)-(5), we show that labor supply is particularly important. Controlling for different measures of labor supply reduces the estimates substantially. Controlling for occupation (column 6) similarly reduces the magnitude of the point estimates but not by as much as controlling for labor supply does. Thus, the effect is not solely due to differential occupational choice (for example, teachers working fewer hours). In column (7), we jointly control for all the variables. We still observe a negative direct effect, although it is smaller in magnitude and statistically insignificant in the matched sample.

Table 5. Mediation analysis.

	ln(Income 1995-2005)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. Full sample							
Pirkkala $\times$ 5th grade	-0.129*** (0.022)	-0.104*** (0.020)	-0.036** (0.018)	-0.060*** (0.019)	-0.095*** (0.021)	-0.083*** (0.016)	-0.047*** (0.013)
Years of Education		0.108*** (0.001)					0.043*** (0.001)
Yearly Months Worked			0.124*** (0.001)				0.052*** (0.003)
ln(Total Months Worked)				0.338*** (0.003)			0.025 (0.017)
Yearly Months Unemployed					-0.124*** (0.001)		-0.065*** (0.001)
<i>N</i>	84326	84326	84326	84300	84326	66289	66286
<i>Y</i> mean	5.532	5.532	5.532	5.532	5.532	5.532	5.532
B. Matched sample							
Pirkkala $\times$ 5th grade	-0.114*** (0.027)	-0.080** (0.031)	0.001 (0.031)	-0.042 (0.040)	-0.066*** (0.019)	-0.075*** (0.023)	-0.029 (0.018)
Years of Education		0.100*** (0.004)					0.044*** (0.003)
Yearly Months Worked			0.130*** (0.003)				0.052*** (0.009)
ln(Total Months Worked)				0.360*** (0.010)			0.050 (0.043)
Yearly Months Unemployed					-0.143*** (0.002)		-0.074*** (0.005)
<i>N</i>	7816	7816	7816	7813	7816	6460	6460
<i>Y</i> mean	5.601	5.601	5.601	5.601	5.601	5.601	5.601
Occupation						✓	✓

Notes: The table reports $\hat{\beta}$ estimated using $Y_{imc} = \beta 1[5th\ grader\ 1973 - 1975_c \times Pirkkala_m] + X'_m \gamma_c + X'_i \sigma + \lambda_m + \delta_c + \varepsilon_{icm}$. All specifications control for municipality and cohort fixed effects, and the full set of covariates. The estimation sample includes 1954-1967 cohorts. Panel A uses data from all municipalities in our sample, and Panel B restricts the sample to the matched municipalities sample. The matched control group includes only individuals who resided in Tornio, Kemijärvi, Masku, Kaarina, or Dragsfjärd in 1970. Standard errors clustered by municipality $\times$ cohort are shown in parentheses. *, ** and *** denote statistical significance at 10%, 5% and 1%, respectively.

5.2 Alternative Explanations

Role of omitted variables The main remaining concern is an unobserved shock affecting exactly the treated Pirkkala cohorts. Such a shock would need to be unusually large and cohort-specific. Our findings imply that any omitted variable generating bias in our estimate of the exposure effect must: (i) operate within the municipality, turning on and then off so as to affect only the two exposed Pirkkala cohorts, without affecting adjacent cohorts or generating differential trends (as shown in the event-study estimates); (ii) be similar in magnitude for the two adjacent exposed cohorts; (iii) be negative enough to fall in the most extreme 1% of permutations that assign two random municipality-cohort shocks (randomization inference); (iv) be orthogonal to initial parental household characteristics (household balance); (v) create a discontinuous jump observed around the eligibility birthdates (as in the difference-in-discontinuities analysis); and (vi) produce occupational choices in line with the content taught in the experiment without materially affecting education or cognitive ability. We believe that plausible confounders are unlikely to satisfy all of these conditions. The permutation exercise in Figure 6, which uses the distribution over the *universe* of realized placebo shocks, shows that the likelihood of two cohort-municipality cells exhibiting a shock of this magnitude at random is roughly 1%.

Can the observed impacts be explained by school or teacher effects, funding, or changes in time allocation? Although these things matter in general (Card and Krueger 1992; Rose and Betts 2004; Chetty, Friedman, and Rockoff 2014), we argue that the answer is “no” in the particular case that we study. Since Pirkkala is a small municipality with only one major school, any potential school-specific effects would be largely absorbed by the municipality fixed effects. The finer urban area fixed effects which we include in Online Appendix Table D.3 would almost certainly capture school-specific effects. Again these effects would need to turn on and off for the specific cohorts to explain the effects.

In Online Appendix Figure D.7, we further show that schooling expenditures did not follow differential trends in Pirkkala and the control municipalities around the experiment. This suggests that there are no shocks to investments in public education that could explain our findings (see, e.g., Jackson, Johnson, and Persico 2015) and it would be unlikely to impact only these specific cohorts.³⁷

Furthermore, the teachers who participated in the experiment were already employed at the school and continued to be so after the experiment, and multiple instructors were involved in teaching history during the Pirkkala school experiment. Therefore, teacher-specific effects

³⁷Our results are robust to controlling for education spending in 1972, as we show in Online Appendix Table D.3.

would need to be exceptionally large and alternate across different cohorts, as these educators likely instructed various students in the same municipality at different times. The absence of adverse effects in other experimental grades further reduces the likelihood that teacher effects would explain away the observed effects (see Online Appendix Table D.1).

We stress that there were no major changes in time allocation between subjects; the primary modification was in the content delivered within the same time structure. Note also that the effect size of the experiment on income is around the same magnitude as the income increase associated with one additional year of schooling (see Table 5). Even if there were changes in the time allocation or total instruction time, the estimated magnitudes are too sizable to be entirely explained by missed schooling. It also appears implausible that missed schooling would generate effects on occupational choice that are in line with the content taught in the school experiment without as substantial impacts on education outcomes.

Can the observed impacts be explained by stigma or labor-market discrimination? A distinct concern is that our estimates reflect not the content of the teaching but the notoriety of the experiment itself. The Pirkkala experiment became a national scandal in 1975, and one might worry that being identifiable as a Pirkkala-experiment child carried a reputational penalty in the labor market.

First, nobody blamed the children; they were viewed as victims, and the blame was placed entirely on the researcher in charge of the experiment and the officials, owing to their lack of oversight (Leskinen 2016). There is no reason to expect the victims to be discriminated against decades later.

Second, the scandal was short-lived. The controversy broke in the spring and summer of 1975 and subsided within months, after which the experiment largely disappeared from public view. The main national newspaper, *Helsingin Sanomat*, did not mention the experiment again until a retrospective piece in 2000 recalling events of twenty-five years earlier. A stigma mechanism requires that the exposed children remained identifiable as Pirkkala-experiment subjects throughout their working lives; the rapid and near-complete disappearance of the episode from public discourse makes a sustained, decades-long reputational penalty implausible.

Third, and most tellingly, stigma is hard to reconcile with the sharp boundaries of the effect across cohorts and grades. A reputational mechanism attaches to being from Pirkkala at the time of the school experiment; it has no reason to respect the fifth-grade eligibility cutoff. Yet we find no effects for the adjacent Pirkkala cohorts or for the other experimental grades who attended the same schools during the same period but where the experiments

failed (Online Appendix Table D.1). Children who shared the municipality’s notoriety but not the fifth-grade socialist teaching show no comparable adverse effects.

Can the observed impacts be explained by differential local labor market exposure? Given that we compare nearby cohorts within a small municipality, it is unlikely that a local labor market shock (like any other idiosyncratic shock of a similar nature) could be concentrated enough to only impact the cohorts exposed to the experiment and not cohorts just prior or after. The permutation exercise in Figure 6 puts the likelihood random adverse shock of this magnitude impacting on two specific municipality-cohorts to be around 1%. Furthermore, the substantial 10% reduction in the probability of military service (see Figure 11) strongly suggests that whatever caused these exposed individuals to behave differently took place before the age of 18.

However, as notable work has shown cohort-specific effects of graduating into a recession and initial labor market conditions in general (Oreopoulos, von Wachter, and Heisz 2012; von Wachter 2020), we explore this alternative by calculating the employment and unemployment rates of a municipality in the year the person graduated with the highest degree according to the administrative data and matching these to the person’s municipality of residence in 1970.³⁸ We then estimate the baseline difference-in-differences equation (1), where the dependent variable is the local employment rate or unemployment rate in the year of graduation. In the estimates presented in Online Appendix Table D.12, the students exposed to the socialist experiment had, if anything, comparatively better local labor market conditions when graduating. There is no statistically observable difference in the matched sample. In sum, there is no empirical evidence to support differential cohort impact to a local labor market shock at the time of graduation.³⁹

Can the observed impacts be explained by peer or parental influences? A natural question is whether the long-run effects we document might reflect peer dynamics or parental reactions rather than classroom exposure. We think that reinforcement within the treated cohorts is a plausible complementary channel: exposed students remained classmates in subsequent grades and may have mutually reinforced the worldview introduced during the experiment. Parents could have discussed more of these topics at home if the children had

³⁸We can observe the local employment in 1970, 1980, 1985, and 1987-2010 in the administrative data. The year of graduation with the highest degree is matched to the closest year.

³⁹It is also worth noting that those exposed to the school experiment are not the primary graduating cohorts impacted by the Finnish recession of the 1990s; some 85% had already completed their highest degree before the recession. There is also little evidence of differential local exposure to the recession in Pirkkala, as seen in Online Appendix Figures D.8 and D.9 which plot local employment and unemployment rates over time.

questions. Still these are outcomes of the initial socialist classroom exposure impacting the students, documenting the long-term effects of which is the aim of this paper.

6 Concluding Remarks

This paper uses administrative data to conduct a long-term follow-up of the Pirkkala school experiment, which subjected two cohorts of fifth graders to radical Marxist-Leninist classroom content. We find that the experiment had enduring economic effects, reducing the adulthood incomes of the exposed children by around 10%. One substantial channel was reduced labor supply, a result consistent with previous findings from socialist economies ([Alesina and Fuchs-Schündeln 2007](#); [Linz and Chu 2013](#); [Fuchs-Schündeln and Masella 2016](#)) and with descriptive evidence from the Finnish samples of the World Values Survey, in which more left-leaning respondents hold systematically weaker pro-work attitudes. We further find that the exposed children selected into occupations that were more civic-minded and more highly valued by socialists, though the reduced labor supply is observable within occupations as well. The exposed male children were also less likely to serve in the military at age 18. We find less substantial and less clear effects on later education, cognitive ability, or personality traits. Finally, our follow-up survey, although considerably less powered than our administrative-data results, offers suggestive evidence that the exposed cohorts are less satisfied with their careers, more inclined toward zero-sum views, and less favorable toward free markets.

Our empirical strategy leverages the fact that exposure to the experiment turns on and then off, affecting only specific cohorts in Pirkkala. Municipality fixed effects absorb common municipality-wide factors, cohort fixed effects account for cohort-specific influences, and a rich set of parental controls accounts for initial differences in family background. As alternative identification strategies, we apply individual-level matching (with balanced sample size), synthetic control methods, and a difference-in-discontinuities design at the birth-date eligibility cutoff, all yielding consistent estimates. The results survive a battery of robustness checks.

The design has limitations. The experiment occurred in a single municipality and affected only two cohorts. The estimates should therefore be interpreted as the long-run effect of this radical socialist classroom intervention, rather than as an average effect of more bundled ideological education in general. The strength of the setting is the sharp, localized, and well-documented experimental treatment, separate from broader exposure to a socialist system or to socialist educational content, which allows us to pinpoint the impact of the socialist classroom content itself on long-term economic outcomes.

The natural residual concern, given our relatively limited exposed group, is that some other idiosyncratic shock affected exactly these cohorts in Pirkkala. The full permutation exercise in Figure 6 speaks directly to this concern. The estimated income decline for the exposed Pirkkala cohorts lies in the extreme lower tail of the placebo distribution: only around 1% of all possible placebo assignments generate an estimate at least as negative. Thus, idiosyncratic shocks of this magnitude are highly unusual among municipality-cohort comparisons in the data.

For an idiosyncratic shock to overturn our interpretation, it would therefore have to be more than merely adverse. An additional shock from worse teachers, worse schools, poor labor or marriage markets, or the like would itself have to be unusually large and strike exactly the two exposed cohorts in Pirkkala (turning on and then off), while leaving adjacent Pirkkala cohorts and the same birth cohorts in comparison municipalities unaffected. Moreover, it would have to reproduce the pattern of results we observe across outcomes: lower income and labor supply, changes in occupational choice and military service consistent with the classroom content, but limited effects on educational attainment and measured ability. Combined with the absence of any documented cohort-specific shock in Pirkkala, this pattern makes an additional idiosyncratic municipality-cohort shock a considerably less plausible explanation than the experiment itself.

Several distinctive features of the setting likely explain the substantial and lasting effects. The content was intentionally biased, delivered intensively across an entire school year at an impressionable age, and taught by trusted authority figures. Moreover, because the country was not communist, children lacked first-hand experience with the system’s failures and thus had fewer reasons to question the material.

We provide the first empirical exploration of the long-term impacts of the Pirkkala socialist school experiment, contributing to the important work on long-term follow-ups of childhood experiments (Heckman, Pinto, and Savelyev 2013; Gertler et al. 2014; García, Heckman, and Ronda 2023). We further add to the literature on the enduring effects of socialist education by tackling previously unaddressed identification challenges. We can plausibly rule out contemporaneous policy changes, cohort effects, and broader political-system changes, distinguishing our study from previous work (Fuchs-Schündeln and Masella 2016; Costa-Font, García-Hombrados, and Nicińska 2024) — enabling us to isolate the effect of ideological classroom content itself. We leverage high-quality Finnish administrative data to document the long-term economic impacts of a socialist classroom experiment in a non-communist, democratic country.

References

- Abadie, Alberto. 2021. "Using Synthetic Controls: Feasibility, Data Requirements, and Methodological Aspects." *Journal of Economic Literature* 59 (June): 391–425.
- Abadie, Alberto, Susan Athey, Guido W Imbens, and Jeffrey M Wooldridge. 2023. "When should you adjust standard errors for clustering?" *The Quarterly Journal of Economics* 138 (1): 1–35.
- Acemoglu, Daron, and David Autor. 2011. "Skills, Tasks and Technologies: Implications for Employment and Earnings." In *Handbook of Labor Economics*, ed. David Card and Orley Ashenfelter. Vol. 4. Elsevier.
- Adena, Maja, Ruben Enikolopov, Maria Petrova, Veronica Santarosa, and Ekaterina Zhuravskaya. 2015. "Radio and the Rise of the Nazis in Prewar Germany." *Quarterly Journal of Economics* 130 (4): 1885–1939.
- Ahonen, Sirkka. 2008. "Finnish History Textbooks in the Cold War." In *The Cold War and the Politics of History*, ed. Juhana Aunesluoma and Pauli Kettunen. Helsinki: Edita.
- Alesina, Alberto, and Nicola Fuchs-Schündeln. 2007. "Good-Bye Lenin (Or Not?): The Effect of Communism on People's Preferences." *American Economic Review* 97 (4): 1507–1528.
- Alesina, Alberto, Paola Giuliano, and Bryony Reich. 2021. "Nation-Building and Education." *Economic Journal* 131 (638): 2273–2303.
- Arkhangelsky, Dmitry, Susan Athey, David A. Hirshberg, Guido W. Imbens, and Stefan Wager. 2021. "Synthetic Difference-in-Differences." *American Economic Review* 111 (December): 4088–4118.
- Arold, Benjamin W. 2024. "Evolution vs. Creationism in the Classroom: The Lasting Effects of Science Education." *Quarterly Journal of Economics* 139 (06): 2331–2375.
- Ash, Elliott, Daniel L. Chen, and Suresh Naidu. 2022. "Ideas Have Consequences: The Impact of Law and Economics on American Justice." NBER Working Paper No. 29788.
- Aspachs-Bracons, Oriol, Irma Clots-Figueras, Joan Costa-Font, and Paolo Masella. 2008. "Compulsory Language Educational Policies and Identity Formation." *Journal of the European Economic Association* 6 (2-3): 434–444.
- Bai, Ying, Ruixue Jia, and Runnan Wang. 2023. "Ideas Mobilize People: The Diffusion of Communist Ideology in China." NBER Working Paper No. 30947.
- Balicki, Stanislaw W. 1964. *Twenty Years of the Polish People's Republic*. Warsaw: Warszawa Państwowe Wydawnictwo Ekonomiczne.
- Bandiera, Oriana, Myra Mohnen, Imran Rasul, and Martina Viarengo. 2019. "Nation-building Through Compulsory Schooling during the Age of Mass Migration." *Economic Journal* 129 (617): 62–109.

- Blouin, Arthur, and Sharun W. Mukand. 2019. "Erasing Ethnicity? Propaganda, Nation Building, and Identity in Rwanda." *Journal of Political Economy* 127 (3): 1008–1062.
- Bondar, Mariia, and Nicola Fuchs-Schündeln. 2023. "Good Bye Lenin Revisited: East–West Preferences Three Decades after German Reunification." *German Economic Review* 24 (1): 97–119.
- Bowles, S., and H. Gintis. 1976. *Schooling in Capitalist America: Educational Reform and the Contradictions of Economic Life*. Routledge & Kegan Paul.
- Bowles, Samuel. 1998. "Endogenous Preferences: The Cultural Consequences of Markets and Other Economic Institutions." *Journal of Economic Literature* 36 (1): 75–111.
- Burst, Tobias, Pola Lehmann, Simon Franzmann, Denise Al-Gaddooa, Christoph Ivanusch, Sven Regel, Felicia Riethmüller, Bernhard Weßels, and Lisa Zehnter. 2023. "manifestoberta. Version 56topics.sentence.2023.1.1." Wissenschaftszentrum Berlin für Sozialforschung/Göttinger Institut für Demokratieforschung. Available online at <https://doi.org/10.25522/manifesto.manifestoberta.56topics.sentence.2023.1.1>.
- Bursztyn, Leonardo, Alessandra L. González, and David Yanagizawa-Drott. 2020. "Misperceived Social Norms: Women Working Outside the Home in Saudi Arabia." *American Economic Review* 110 (10): 2997–3029.
- Cantoni, Davide, Yuyu Chen, David Y. Yang, Noam Yuchtman, and Y. Jane Zhang. 2017. "Curriculum and Ideology." *Journal of Political Economy* 125 (2): 338–392.
- Cappelen, Alexander, John List, Anya Samek, and Bertil Tungodden. 2020. "The Effect of Early-Childhood Education on Social Preferences." *Journal of Political Economy* 128 (7): 2739–2758.
- Card, David, and Alan Krueger. 1992. "Does School Quality Matter? Returns to Education and the Characteristics of Public Schools in the United States." *Journal of Political Economy* 100 (1): 1–40.
- Carlitz, Ruth, Ameet Morjaria, Joris Mueller, and Philip Osafo-Kwaako. 2024. "State Building in a Diverse Society." *Review of Economic Studies*: rdae116.
- Chen, Wei-Lin, Ming-Jen Lin, and Tzy-Ting Yang. 2023. "Curriculum and National Identity: Evidence from the 1997 Curriculum Reform in Taiwan." *Journal of Development Economics* 163: 103078.
- Chetty, Raj, John N. Friedman, and Jonah E. Rockoff. 2014. "Measuring the Impacts of Teachers II: Teacher Value-Added and Student Outcomes in Adulthood." *American Economic Review* 104 (9): 2633–79.
- Chetty, Raj, John N. Friedman, Nathaniel Hilger, Emmanuel Saez, Diane Whitmore Schanzenbach, and Danny Yagan. 2011. "How Does Your Kindergarten Classroom Affect Your Earnings? Evidence from Project STAR." *Quarterly Journal of Economics* 126 (4): 1593–1660.

- Clots-Figueras, Irma, and Paolo Masella. 2013. "Education, Language and Identity." *Economic Journal* 123 (570): F332–F357.
- Conley, Timothy G., and Christopher R. Taber. 2011. "Inference with "Difference in Differences" with a Small Number of Policy Changes." *The Review of Economics and Statistics* 93 (1): 113–125.
- Corno, Lucia, Eliana La Ferrara, and Justine Burns. 2022. "Interaction, Stereotypes, and Performance: Evidence from South Africa." *American Economic Review* 112 (12): 3848–3875.
- Costa-Font, Joan, Jorge García-Hombrados, and Anna Nicińska. 2024. "Long-lasting effects of indoctrination in school: Evidence from the People's Republic of Poland." *European Economic Review* 161: 104641.
- Cunningham, Scott, and Manisha Shah. 2018. "Decriminalizing Indoor Prostitution: Implications for Sexual Violence and Public Health." *The Review of Economic Studies* 85: 1683–1715.
- DellaVigna, Stefano, and Matthew Gentzkow. 2010. "Persuasion: Empirical Evidence." *Annual Review of Economics* 2 (1): 643–669.
- Doepke, Matthias, and Fabrizio Zilibotti. 2008. "Occupational Choice and the Spirit of Capitalism." *Quarterly Journal of Economics* 123 (2): 747–793.
- Dustmann, Christian, Uta Schönberg, and Jan Stuhler. 2017. "Labor Supply Shocks, Native Wages, and the Adjustment of Local Employment." *Quarterly Journal of Economics* 132: 435–483.
- Erosa, Andrés, Luisa Fuster, Gueorgui Kambourov, and Richard Rogerson. 2024. "Wage and earnings inequality between and within occupations: The role of labor supply." *Journal of Monetary Economics*: 103553.
- Fehr, Ernst, Helen Bernhard, and Bettina Rockenbach. 2008. "Egalitarianism in young children." *Nature* 454 (7208): 1079–1083.
- Fouka, Vasiliki. 2020. "Backlash: The Unintended Effects of Language Prohibition in U.S. Schools after World War I." *Review of Economic Studies* 87 (1): 204–239.
- Fredriksson, Peter, Björn Öckert, and Hessel Oosterbeek. 2012. "Long-Term Effects of Class Size." *Quarterly Journal of Economics* 128 (11): 249–285.
- Fuchs-Schündeln, Nicola, and Matthias Schündeln. 2020. "The Long-Term Effects of Communism in Eastern Europe." *Journal of Economic Perspectives* 34 (2): 172–191.
- Fuchs-Schündeln, Nicola, and Paolo Masella. 2016. "Long-Lasting Effects of Socialist Education." *Review of Economics and Statistics* 98 (3): 428–441.

- García, Jorge Luis, James J. Heckman, and Victor Ronda. 2023. “The Lasting Effects of Early-Childhood Education on Promoting the Skills and Social Mobility of Disadvantaged African Americans and Their Children.” *Journal of Political Economy* 131 (6): 1477–1506.
- Gertler, Paul, James Heckman, Rodrigo Pinto, Arianna Zanolini, Christel Vermeersch, Susan Walker, Susan M. Chang, and Sally Grantham-McGregor. 2014. “Labor market returns to an early childhood stimulation intervention in Jamaica.” *Science* 344 (6187): 998–1001.
- Ghosh, Arkadev, Prerna Kundu, Matt Lowe, and Gareth Nellis. 2025. “Creating Cohesive Communities: A Youth Camp Experiment in India.” *Review of Economic Studies*: rdaf026.
- Golsteyn, Bart H.H., Hans Grönqvist, and Lena Lindahl. 2014. “Adolescent Time Preferences Predict Lifetime Outcomes.” *Economic Journal* 124 (580): F739–F761.
- Gorodnichenko, Yuriy, and Gerard Roland. 2011a. “Individualism, innovation, and long-run growth.” *Proceedings of the National Academy of Sciences* 108 (4): 21316–21319.
- Gorodnichenko, Yuriy, and Gerard Roland. 2011b. “Which Dimensions of Culture Matter for Long-Run Growth?” *American Economic Review* 101 (3): 492–498.
- Grosfeld, Irena, Etienne Madinier, Seyhun Orcan Sakalli, and Ekaterina Zhuravskaya. 2024. “Independent Media, Propaganda, and Religiosity: Evidence from Poland.” *American Economic Journal: Applied Economics*: forthcoming.
- Gunderson, Morley, and Philip Oreopolous. 2020. “Returns to education in developed countries.” In *The Economics of Education (Second Edition)*, ed. Steve Bradley and Colin Green. Second edition ed. Academic Press.
- Hasher, Lynn, David Goldstein, and Thomas Toppino. 1977. “Frequency and the conference of referential validity.” *Journal of Verbal Learning and Verbal Behavior* 16 (1): 107–112.
- Heckman, James J., Rodrigo Pinto, and Peter Savelyev. 2013. “Understanding the Mechanisms through Which an Influential Early Childhood Program Boosted Adult Outcomes.” *American Economic Review* 103 (6): 2052–2086.
- Hiscocks, Richard. 1959. “Education in Poland.” *International Journal* 14 (4): 259–271.
- Holmén, Janne Sven-Åke. 2006. *Den politiska läroboken: Bilden av USA och Sovjetunionen i norska, svenska och finländska läroböcker under Kalla kriget*. Uppsala: Uppsala University, Faculty of Arts, Department of History.
- Jackson, C. Kirabo, Rucker C. Johnson, and Claudia Persico. 2015. “The Effects of School Spending on Educational and Economic Outcomes: Evidence from School Finance Reforms.” *Quarterly Journal of Economics* 131 (10): 157–218.
- Jokela, Markus, Tuomas Pekkarinen, Matti Sarvimäki, Marko Terviö, and Roope Uusitalo. 2017. “Secular rise in economically valuable personality traits.” *Proceedings of the National Academy of Sciences* 114 (25): 6527–6532.

- Kambourov, Gueorgui, and Iouri Manovski. 2009. "Occupational Mobility and Wage Inequality." *Review of Economic Studies* 76 (2): 731–759.
- Kerr, Sari Pekkala, Tuomas Pekkarinen, and Roope Uusitalo. 2013. "School Tracking and Development of Cognitive Skills." *Journal of Labor Economics* 31 (3): 577–602.
- Kojo, Pauli. 1975. *Mitä-Missä-Milloin, Kansalaisen vuosikirja 1976*. Helsinki: Otava.
- Küng, Andres. 1976. *Mitä Suomessa tapahtuu*. Helsinki: Kirjayhtymä.
- Laudenbach, Christine, Ulrike Malmendier, and Alexandra Niessen-Ruenzi. 2018. "The long-lasting effects of propaganda on financial risk-taking." Working paper.
- Lehtinen, Lasse Juhani. 1996. *Kokoomus vai konservatismi*. Kärppä-julkaisut.
- Lehtonen, Kai R., and Veikko Huttunen. 1971. *Peruskoulun historia 1*. Porvoo: WSOY.
- Leskinen, Jari. 2016. *Kohti sosialismia! Pirkkalan peruskoulun marxilainen kokeilu 1973–75*. Helsinki: Siltala.
- Lifton, Robert Jay. 1961. *Thought Reform and the Psychology of Totalism: A Study of "Brainwashing" in China*. New York, NY: Norton.
- Linz, Susan J., and Yu-Wei Luke Chu. 2013. "Work Ethic in Formerly Socialist Economies." *Journal of Economic Psychology* 39: 185–203.
- Lippmann, Quentin, Alexandre Georgieff, and Claudia Senik. 2020. "Undoing Gender with Institutions: Lessons from the German Division and Reunification." *Economic Journal* 130 (629): 1445–1470.
- Lott, John R., Jr. 1999. "Public Schooling, Indoctrination, and Totalitarianism." *Journal of Political Economy* 107 (S6): S127–S157.
- MacKinnon, James G., and Matthew D Webb. 2020. "Randomization inference for difference-in-differences with few treated clusters." *Journal of Econometrics* 218 (2): 435–450.
- MacKinnon, James G., Morten Ørregaard Nielsen, and Matthew D. Webb. 2023. "Fast and reliable jackknife and bootstrap methods for cluster-robust inference." *Journal of Applied Econometrics* 38 (5): 671–694.
- Mastrobuoni, Giovanni. 2020. "Crime is Terribly Revealing: Information Technology and Police Productivity." *Review of Economic Studies* 87: 2727–2753.
- Mitrunen, Matti. 2020. "Can You Make an American? Compulsory Patriotism and Assimilation of Immigrants." Working paper.
- Ndebele, Clever, and Reuben Tshuma. 2014. "Ideology and the Curriculum: How did Socialist Curriculum Development and Implementation in Zimbabwe from 1980 to 2004 Take Place Through the Social Studies Curriculum?" *The Anthropologist* 18 (1): 227–239.

- Neundorff, Anja, Eugenia Nazrullaeva, Ksenia Northmore-Ball, Katerina Tertytchnaya, and Wooseok Kim. 2024. “Varieties of Indoctrination: The Politicization of Education and the Media around the World.” *Perspectives on Politics*: forthcoming.
- Oreopoulos, Philip, Till von Wachter, and Andrew Heisz. 2012. “The short-and long-term career effects of graduating in a recession.” *American Economic Journal: Applied Economics* 4 (1): 1–29.
- Paglayan, Agustina S. 2022. “Education or Indoctrination? The Violent Origins of Public School Systems in an Era of State-Building.” *American Political Science Review* 116 (4): 1242–1257.
- Pekkarinen, Tuomas, Roope Uusitalo, and Sari Kerr. 2009. “School tracking and intergenerational income mobility: Evidence from the Finnish comprehensive school reform.” *Journal of Public Economics* 93 (7-8): 965–973.
- Penttilä, Antti. 1974. *Historia: 5. luokka; ihmiskunnan kehityksen yleispiirteet vanhimmista ajoista nykypäiviin saakka*. Tampereen yliopiston psykologian laitos.
- Päällysaho, Miika. 2017. “The Short- and Long-Term Effects of Graduating During a Recession: Evidence from Finland.” VATT Working Papers No. 174.
- Rose, Heather, and Julian R. Betts. 2004. “The Effect of High School Courses on Earnings.” *Review of Economics and Statistics* 86 (2): 497–513.
- Schaewitz, Johannes, Mei Wang, and Marc Oliver Rieger. 2022. “Culture and Institutions: Long-lasting effects of communism on risk and time preferences of individuals in Europe.” *Journal of Economic Behavior & Organization* 202: 785–829.
- Schein, Edgar H., Jerome W. Cooley, and Margaret T. Singer. 1961. *Coercive Persuasion: A Socio-Psychological Analysis of the “Brainwashing” of American Civilian Prisoners by the Chinese Communists*. New York, NY: Norton.
- Taylor, Kathleen. 2017. *Brainwashing: The Science of Thought Control*. Oxford University Press.
- Teitelbaum, Kenneth. 1990. ““Critical Lessons” from Our Past: Curricula of Socialist Sunday Schools in the United States.” *Curriculum Inquiry* 20 (4): 407–436.
- Trice, Ashton D. 1991. “A Retrospective Study of Career Development: I. Relationship among First Aspirations, Parental Occupations, and Current Occupations.” *Psychological Reports* 68 (1): 287–290.
- Trice, Ashton D., and Nancy McClellan. 1993. “Do Children’s Career Aspirations Predict Adult Occupations? An Answer from a Secondary Analysis of a Longitudinal Study.” *Psychological Reports* 72 (2): 368–370.
- Tudge, Jonathan. 1991. “Education of Young Children in the Soviet Union: Current Practice in Historical Perspective.” *Elementary School Journal* 92 (1): 121–133.

- Tutkijaryhmä Peto. 1974. *Yhteiskunnallisten oppiaineiden opetussuunnitelmien tieteelliseen kehittämiseen tähtäävä projekti: maailmankuvan mittaamisen metodiikan kehittelyä*. 89/1974 Tampere: Department of Psychology, University of Tampere.
- Tutkijaryhmä Peto. 1975. *Yhteiskunnallisten oppiaineiden opetussuunnitelmien tieteelliseen kehittämiseen tähtäävä projekti: Pirkkalan kokeiluun liittyvää aineistoa*. 97/1975 Tampere: Department of Psychology, University of Tampere.
- Uusitalo, Roope. 1999. "Return to education in Finland." *Labour Economics* 6 (4): 569–580.
- Voigtländer, Nico, and Hans-Joachim Voth. 2015. "Nazi indoctrination and anti-Semitic beliefs in Germany." *Proceedings of the National Academy of Sciences* 112 (26): 7931–7936.
- von Wachter, Till. 2020. "The Persistent Effects of Initial Labor Market Conditions for Young Adults and Their Sources." *Journal of Economic Perspectives* 34 (4): 168–194.
- Watson, Mark, and Mary McMahon. 2005. "Children's career development: A research review from a learning perspective." *Journal of Vocational Behavior* 67 (2): 119–132.
- Wojdon, Joanna. 2023. *Communist Propaganda at School: The World of the Reading Primers from the Soviet Bloc, 1949-1989*. Routledge Histories of Central and Eastern Europe London, England: Routledge.
- Yanagizawa-Drott, David. 2014. "Propaganda and Conflict: Evidence from the Rwandan Genocide." *Quarterly Journal of Economics* 129 (4): 1947–1994.
- Zajda, Joseph. 1979. "Education for Labour in the USSR." *Comparative Education* 15 (3): 287–299.
- Zajonc, Robert B. 1968. "Attitudinal effects of mere exposure." *Journal of Personality and Social Psychology* 9 (2): 1–27.

Communism in the Classroom: Long-Run Effects of an Experiment

Online Appendix (Not for Publication)

Jaakko Meriläinen* Matti Mitrunen†

June 24, 2026

*Department of Economics, Stockholm School of Economics. Email: jaakko.merilainen@hhs.se.

†Faculty of Social Sciences, University of Helsinki. Email: matti.mitrunen@helsinki.fi.

Contents

A	Brief Background on the Finnish School System	OA-3
A.1	Finnish School System	OA-3
A.2	Standard History Curriculum	OA-4
B	Text Similarity	OA-5
B.1	Topics	OA-5
B.2	Cosine Similarities	OA-11
C	Data	OA-14
C.1	Occupational Classes	OA-14
C.2	Measuring Occupation Appreciation	OA-18
C.3	FDF Data	OA-19
D	Additional Results	OA-22
D.1	Income Effects for Different Grades	OA-22
D.2	Robustness Checks	OA-23
D.3	Additional Outcomes	OA-32
D.4	Effect Heterogeneity	OA-34
D.5	Further Results on Labor Supply	OA-36
D.6	Occupation Characteristics	OA-41
D.7	Municipal Schooling Expenditures	OA-42
D.8	Initial Labor Market Conditions	OA-42
E	Survey Evidence	OA-45
E.1	Survey Collection	OA-45
E.2	Survey Questions	OA-46
E.3	Aggregation of the Survey Questions	OA-51
E.4	Analysis of the Survey Data	OA-55
E.5	Results	OA-55

A Brief Background on the Finnish School System

In this appendix, we provide a brief description of the Finnish school system during the period of our study. We also describe the contents of the standard history curriculum in the fifth grade.

A.1 Finnish School System

The Pirkkala school experiment occurred during an era when the Finnish primary school system underwent a comprehensive reform, which abolished the old two-track system and replaced it with a uniform nine-year comprehensive school ([Pekkarinen, Uusitalo, and Kerr 2009](#); [Kerr, Pekkarinen, and Uusitalo 2013](#)). The main aim of the reform was to provide equal educational opportunities to all students, regardless of their place of residence or social background. The reform was implemented in a staggered manner. In order to avoid confounding our empirical analysis with potential impacts of the reform, our empirical analysis only uses data on individuals who lived in municipalities that were at the same stage of the reform implementation as Pirkkala.

Prior to the reform, students entered primary school the year they turned seven years, and after four years, they had to choose between applying to general secondary school or continuing in primary school. Admission to the general secondary school was based on an entrance examination, teacher assessment, and primary school grades. Those who were not admitted continued in the primary school for eight years, with the last two years focusing on vocational skills. The reform introduced a new curriculum, which increased the academic content of education and made one foreign language compulsory for all students.

After the reform, all students followed the same curriculum in the same establishments up to age 16. School starting age did not change. The previous primary schools, civic schools, and junior secondary schools were replaced by nine-year comprehensive schools, and the upper secondary school was separated into a distinct institution. Students could choose between applying to upper secondary school or vocational schools after nine years in the comprehensive school, and admission to both tracks was based solely on comprehensive school grades. The reform did not extend the length of compulsory schooling or change the minimum school leaving age, which had been 16 since 1957.

Virtually all primary education is publicly provided in Finland. There were some private schools in the 1970s, but those also followed the standard, centrally planned curriculum. Similarly, home schooling has historically been rare and heavily regulated. This high degree of institutional standardization ensures that observed variation in ideological exposure does

not reflect differences in school governance or discretion regarding what is taught at the school.

A.2 Standard History Curriculum

Table A.1 lays out the plan (topics and hours dedicated to each topic) for the standard 5th-grade history, as stated in the 1970 national curriculum ([Opetussuunnitelma 1970](#)). There is significant overlap in terms of themes covered between the standard curriculum and the contents of the Pirkkala school experiment. However, as we discuss here and in the main text, the way that these topics were discussed was drastically different. Topics not included in the standard history curriculum but that were touched upon in the Pirkkala school experiment teaching material include the Second World War and colonialism.

Table A.1. 5th grade history curriculum

Topic	Hours
Introduction	10
Early culture and society in the Near East	8
The Greeks	12
The Romans	12
Ancient Finnish society	6
Medieval society and Finland in the Middle Ages	12

B Text Similarity

This appendix provides additional details on the text similarity computations.

B.1 Topics

We use the pre-trained **Manifestoberta** model to classify the content of each paragraph into various predefined topics (Burst et al. 2023). We use this information to compute the probability that a paragraph in each document discusses a certain topic, after pre-processing the text data as before. Although the model is intended to analyze the contents of party manifestos, we believe it can be informative about topics in any text with political and/or societal content. Table B.1 provides a list of the topic categories as well as a brief description of what kind of text falls within the category. For further details, see the Manifesto Project website at <http://manifestoproject.wzb.eu> (accessed April 28, 2025). In Figure B.1, we assign each text excerpt to the most likely category and show the shares of these categories. We see clear differences between the Pirkkala handout and the standard textbook. Figure B.2 plots the differences of the shares.

Table B.1. A simplified summary of the Manifesto Project coding scheme.

Code	Category	Short description
101	Foreign Relationships: Positive	Special Favourable mentions of cooperation with specific countries.
102	Foreign Relationships: Negative	Special Negative mentions of relationships with specific countries.
103	Anti-Imperialism	Negative references to imperialism and advocacy for de-colonisation.
104	Military: Positive	Support for military strength and expenditure.
105	Military: Negative	Criticism of military power and advocacy for disarmament.
106	Peace	Advocacy for peaceful solutions and negotiations.
107	Internationalism: Positive	Support for international cooperation and global governance.
<i>Continued on next page.</i>		

Code	Category	Short description
108	European Community/Union: Positive	Favourable mentions of the European Union and its expansion.
109	Internationalism: Negative	Support for national sovereignty and opposition to internationalism.
110	European Community/Union: Negative	Criticism of the European Union and its policies.
201	Freedom and Human Rights	Advocacy for personal freedom and civil rights.
202	Democracy	Support for democracy and democratic institutions.
203	Constitutionalism: Positive	Support for maintaining the constitution and constitutional arguments.
204	Constitutionalism: Negative	Calls for constitutional amendments or changes.
301	Federalism	Support for decentralisation and autonomy of sub-national levels.
302	Centralisation	Support for unitary government and centralisation of power.
303	Governmental and Administrative Efficiency	Advocacy for efficiency and economy in government.
304	Political Corruption	Need to eliminate political corruption and abuses of power.
305	Political Authority	References to the competence and stability of the government.
401	Free Market Economy	Favourable mentions of free market and capitalism.
402	Incentives	Support for supply-side economic policies and business incentives.
403	Market Regulation	Advocacy for creating a fair and open economic market.

Continued on next page.

Code	Category	Short description
404	Economic Planning	Favourable mentions of government economic planning.
405	Corporatism/Mixed Economy	Support for cooperation between government, employers, and trade unions.
406	Protectionism: Positive	Support for protection of internal markets.
407	Protectionism: Negative	Advocacy for free trade and open markets.
408	Economic Goals	General economic statements not related to specific goals.
409	Keynesian Demand Management	Support for demand-side economic policies and government stimulus.
410	Economic Growth: Positive	Advocacy for measures to facilitate economic growth.
411	Technology and Infrastructure	Importance of modernisation and public spending on infrastructure.
412	Controlled Economy	Support for government control of the economy.
413	Nationalisation	Favourable mentions of government ownership of industries.
414	Economic Orthodoxy	Advocacy for economically healthy government policies and traditional economic institutions.
415	Marxist Analysis: Positive	Positive references to Marxist-Leninist ideology.
416	Anti-Growth Economy: Positive	Support for sustainable development and opposition to harmful growth.
501	Environmental Protection: Positive	Advocacy for environmental protection and climate change policies.
502	Culture: Positive	Support for state funding of cultural and leisure facilities.
503	Equality: Positive	Advocacy for social justice and fair treatment of all people.
504	Welfare State Expansion	Support for expanding public social services and social security schemes.
505	Welfare State Limitation	Limiting state expenditures on social services.
506	Education Expansion	Need to expand and improve educational provision at all levels.

Continued on next page.

Code	Category	Short description
507	Education Limitation	Limiting state expenditure on education.
601	National Way of Life: Positive	Support for national pride and patriotism.
602	National Way of Life: Negative	Opposition to national pride and patriotism.
603	Traditional Morality: Positive	Support for traditional moral values and religious institutions.
604	Traditional Morality: Negative	Opposition to traditional moral values.
605	Law and Order: Positive	Support for strict law enforcement and internal security.
606	Civic Mindedness: Positive	Appeals for national solidarity and public spiritedness.
607	Multiculturalism: Positive	Support for cultural diversity and plurality.
608	Multiculturalism: Negative	Advocacy for cultural integration and homogeneity.
701	Labour Groups: Positive	Support for labour groups, trade unions, and fair treatment of employees.
702	Labour Groups: Negative	Negative references to labour groups and trade unions.
703	Agriculture and Farmers: Positive	Support for agriculture and farming practices.
704	Middle Class and Professional Groups	Favourable references to the middle class and professional groups.
705	Underprivileged Minority Groups	Support for underprivileged minorities not defined economically or demographically.
706	Non-economic Demographic Groups	Support for special interest groups defined demographically.
000	No meaningful category applies	Statements not covered by other categories or devoid of meaning.

Notes: For a more detailed description of the coding scheme, see https://manifesto-project.wzb.eu/coding_schemes/mp_v4 (accessed April 28, 2025).

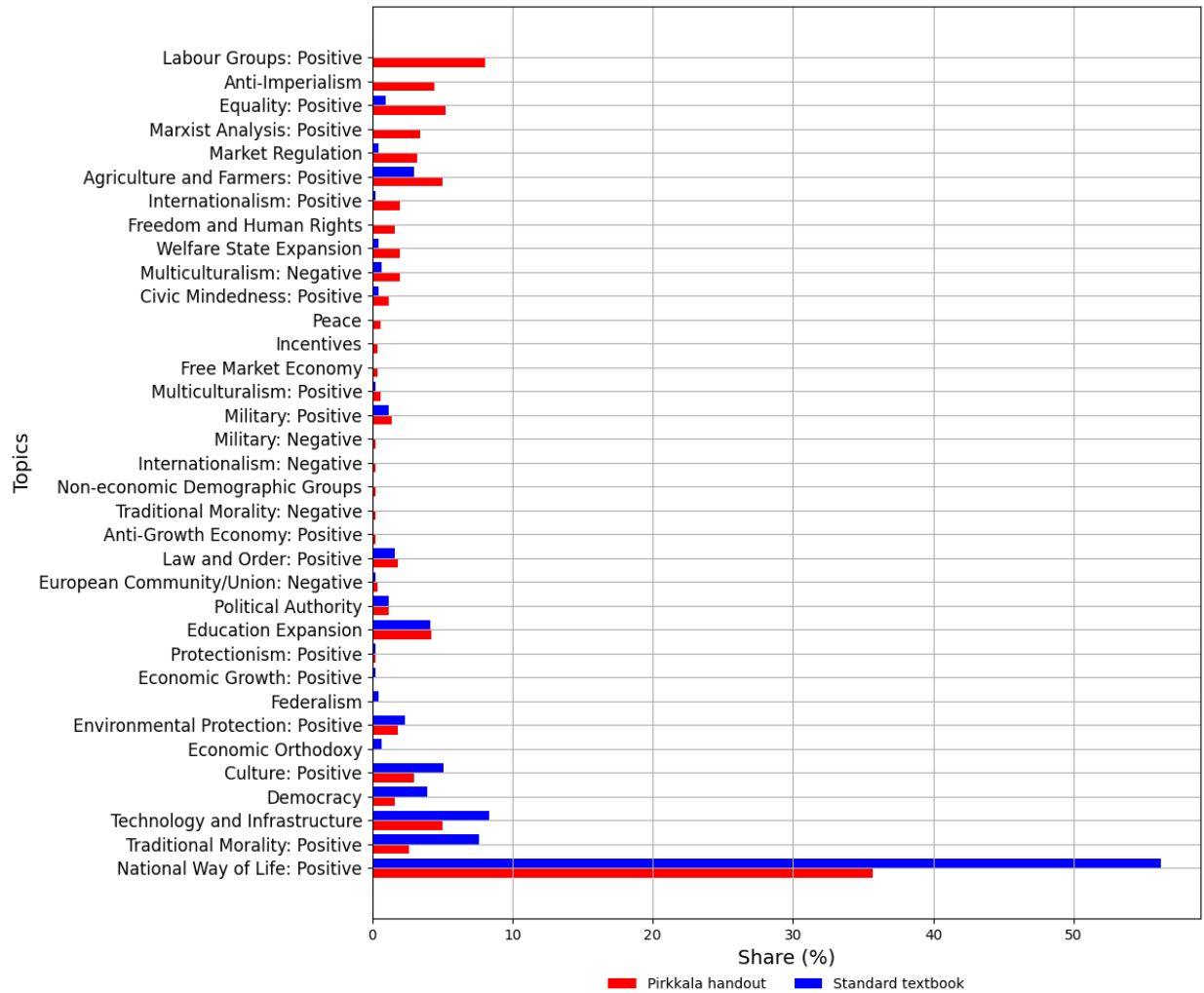


Figure B.1. Share of excerpts classified to each topic.

Notes: We use the pre-trained **Manifestoberta** model to classify the content of each paragraph into various predefined topics (Burst et al. 2023). We use this information to compute the probability that a paragraph in each document discusses a certain topic. The figure shows the share of text excerpts classified to each topic for each text.

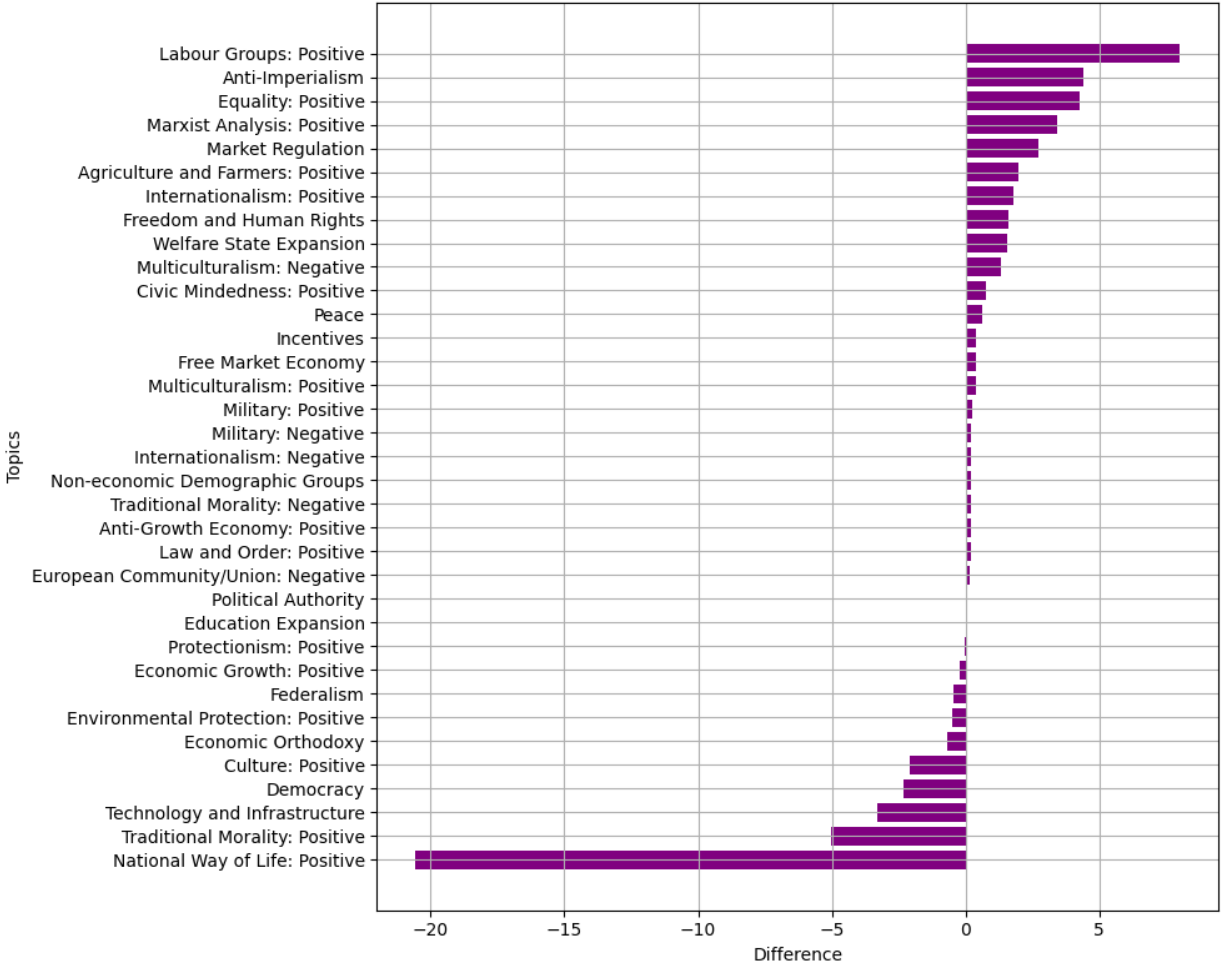


Figure B.2. Difference in share of excerpts classified to each topic.

Notes: We use the pre-trained **Manifestoberta** model to classify the content of each paragraph into various predefined topics (Burst et al. 2023). We use this information to compute the probability that a paragraph in each document discusses a certain topic and compute the difference of these probabilities.

B.2 Cosine Similarities

To characterize the contents of the materials, we leverage natural language processing techniques to compute semantic similarity between documents. We begin by preprocessing the text, removing Finnish stopwords and tokenizing the text into words. Each cleaned paragraph is then encoded into a dense vector representation using the pre-trained SentenceTransformer model `paraphrase-multilingual-MiniLM-L12-v2`, which captures semantic information in a high-dimensional embedding space.

We compare both teaching materials to three texts with a leftist slant: *The Communist Manifesto* by Marx and Engels and the 1970s party programs of two major Finnish left-wing parties—the Social Democratic Party and the Finnish People’s Democratic League.

The manifesto was retrieved from https://fi.wikisource.org/wiki/Kommunistisen_puolueen_manifesti_suomeksi (accessed April 28, 2025), and the party programs were obtained from the Pohtiva database (<https://www.fsd.tuni.fi/pohtiva/ohjelmalistat>, accessed April 28, 2025).

We construct a document-level embedding for each text as follows. Let d index documents and let $p = 1, \dots, P_d$ index the paragraphs within document d . Each paragraph is encoded into a vector $\mathbf{v}_{dp} \in \mathbb{R}^K$ using the pre-trained SentenceTransformer model `paraphrase-multilingual-MiniLM-L12-v2`. The document-level embedding is defined as the mean of its paragraph embeddings,

$$\mathbf{E}_d = \frac{1}{P_d} \sum_{p=1}^{P_d} \mathbf{v}_{dp}.$$

The vector $\mathbf{E}_d$ summarizes the central semantic content of document d in a K -dimensional embedding space.

To quantify the semantic proximity between two documents d and d' , we compute the cosine similarity between their document-level embeddings,

$$\text{sim}(d, d') = \frac{\mathbf{E}_d \cdot \mathbf{E}_{d'}}{\|\mathbf{E}_d\| \|\mathbf{E}_{d'}\|}.$$

Cosine similarity takes values in $[-1, 1]$ and increases as the angle between the embedding vectors decreases. Higher values therefore indicate that the average semantic content of documents d and d' is more closely aligned.

In addition to the document-level measure, we also examine the full distribution of paragraph-level similarities between documents. For any two documents d and d' , and for any paragraphs p in d and p' in d' , let $\mathbf{v}_{dp}$ and $\mathbf{v}_{d'p'}$ denote their respective paragraph

embeddings. The cosine similarity between these paragraphs is given by

$$\text{sim}(dp, d'p') = \frac{\mathbf{v}_{dp} \cdot \mathbf{v}_{d'p'}}{\|\mathbf{v}_{dp}\| \|\mathbf{v}_{d'p'}\|}.$$

Computing this quantity for all (p, p') pairs across documents yields a distribution of similarity scores that captures not only the average semantic alignment between two texts, but also the shape and heterogeneity of their overlap. Paragraphs that discuss similar themes or ideas tend to have embeddings that point in similar directions in the high-dimensional space, resulting in higher cosine similarity.

We visualize the distributions of these paragraph-level cosine similarities below in Figure B.3. On average, the Pirkkala handout is more similar to these three texts than the standard textbook, and the distributions of paragraph-level cosine similarities are statistically significantly different from each other.

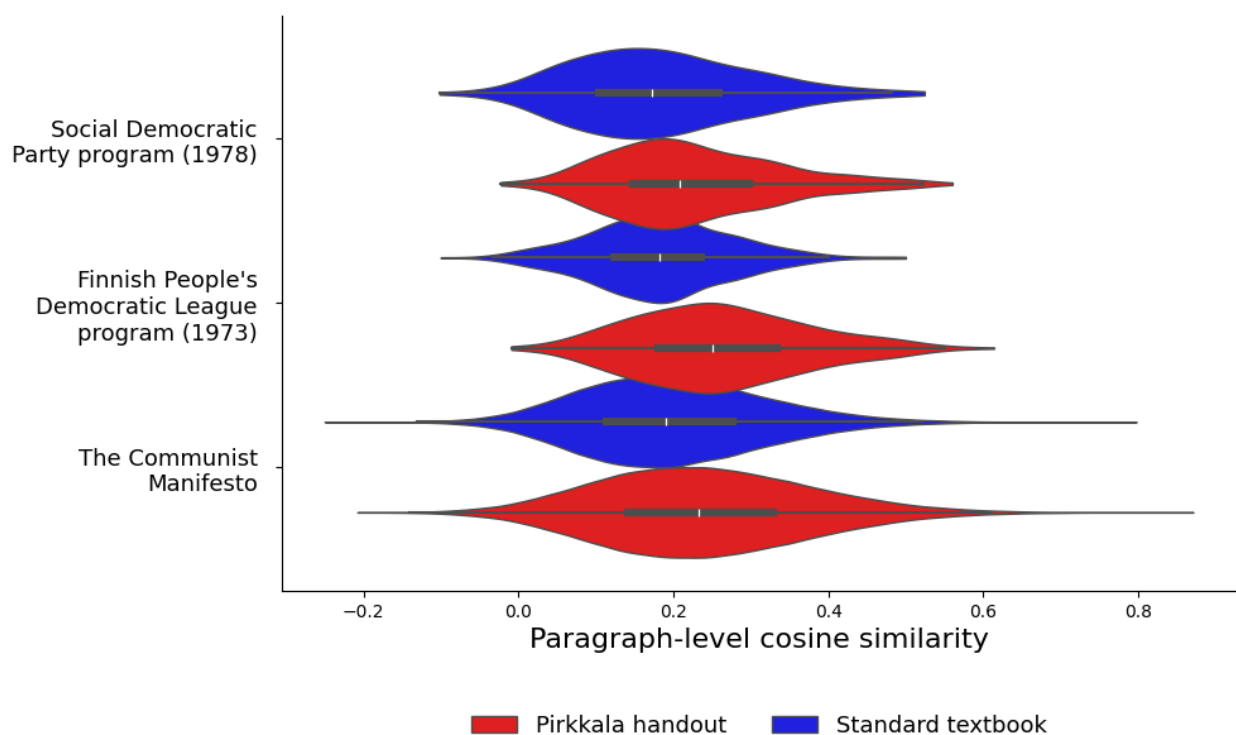


Figure B.3. Paragraph-level cosine similarity with selected leftist texts.

Notes: The figure provides a numerical characterization of the contents of the fifth-grade history teaching materials (Lehtonen and Huttunen 1971; Penttilä 1974). We scanned the original textbooks and used OCR to convert the text into data. We show cosine similarity distributions between either the Pirkkala handout or the standard textbook and three selected texts. The cosine similarities measure the degree of similarity between the document embeddings, indicating how closely related the contents of the documents are in terms of their semantic meaning. The colored shapes represent violin plots, and the boxplot elements indicate the interquartile range and median.

C Data

In this appendix, we provide additional details on some of the auxiliary data we use: the survey data to measure occupation appreciation by ideology and the cognitive and personality trait test data from the Finnish Defense Forces.

C.1 Occupational Classes

We use the 1-digit occupational classification from Statistics Finland to analyze occupational choice in the main text. We provide a description of each occupational class below in Table C.1.

In the main text, we document the largest positive impact of the socialist school experiment for the occupational groups “senior officials in education” and “other lower-level employees.” In Tables C.2 and C.3, we show the occupational distribution for the entire country within the socioeconomic groups with the most sizable effects. The first group is mostly composed of teachers, and the second group contains professions that “share the objective of ensuring people’s immediate well-being, safety, and cultural or entertainment needs,” according to Statistics Finland’s classification. 65% of individuals in this category are nurses.

We also document that children exposed to the experiment were much less likely to hold management positions or serve as senior officials or supervisors in adulthood. The largest occupation within the “senior officials in planning” is business professionals (Table C.4).

Table C.1. Occupational classes.

Occupational class	Short description
Senior officials in education	Educators and teachers who generally require higher education qualifications. Includes principals and school directors, regardless of whether they also teach.
<i>Continued on next page.</i>	

Occupational class	Short description
Other lower-level employees	Covers various occupations not classifiable as clerical, sales, or supervisory work. Many roles focus on welfare, safety, or cultural/entertainment services. Includes transportation and communications workers handling information and safety, as well as others performing direct or interpretive services for consumers. Typically requires vocational training and some autonomy.
Clerical and sales (independent)	Traditional or evolved clerical/sales jobs not reduced to repetitive tasks. Workers handle symbolic content or serve the public, often producing or mediating information and service. Requires vocational training, theoretical knowledge, or relevant experience.
Workers in agriculture	Employees in agriculture and forestry who are not entrepreneurs.
Manufacturing workers	Involved in material handling or processing in manufacturing. Includes those operating, maintaining, or repairing machinery, handling logistics in production chains, or doing installation and commissioning tasks on behalf of the manufacturer.
Other production workers	Work in preparatory or supporting roles in production that do not involve office tasks. Includes industrial workers outside manufacturing sectors, domestic and institutional production workers, launderers, ironers, pressers, and various helpers.
Clerical and sales (routine)	Occupations that involve routine, simplified, often repetitive tasks, especially in office or sales work. These roles are generally low-autonomy and easily replaceable. Comparable to assembly line work but in service or office settings.

Continued on next page.

Occupational class	Short description
Entrepreneurs	Individuals who run their own business, farm owned or leased land, or practice a liberal profession. They may be sole proprietors or employers with one or more employees. Family members assisting without pay are classified similarly. Also includes owner-entrepreneurs in small limited liability companies where they or their family own over half of the company's share capital.
Supervisors	Mainly cover roles in supervisory and technical positions (e.g., foremen, technicians, masters). Also includes managers of collective or cooperative enterprises not classifiable under upper management unless clearly workers.
Distribution and service workers	Operate vehicles, store, package, or distribute goods outside of production, or perform cleaning, sanitation, beauty, and hygiene work. Includes uncategorized service occupations not primarily focused on symbolic work or direct care for wellbeing/safety.
Upper management	Leaders and managers of large organizations such as public administrations, businesses, factories, or NGOs. Their central duties involve decision-making and organizational leadership, typically in high-ranking administrative positions within the private sector.
Other senior officials/employees	Specialists in occupations applying specialized theoretical knowledge, not classifiable in prior senior categories. Tasks are typically indivisible and not easily delegated except for routine components. Includes creative or performing professionals in arts, culture, and entertainment.
Senior official in planning	Officials working closely with leadership, producing services for decision-making, participating in decision-making processes, or serving oversight and development roles. Leadership tasks may be involved but are not central. Often work directly under senior management or at the middle-management level in large organizations.

Notes: The classification follows the coding used by Statistics Finland.

Occupation code	Share (%)	Occupation name
231	8.13	College, university and higher education teaching professionals
232	48.86	Secondary education teaching professionals
233	31.05	Primary and pre-primary education teaching professionals
234	4.35	Special education teaching professionals
235	7.61	Other teaching professionals

Table C.2. Senior officials in education.

Occupation code	Share (%)	Occupation name
011	0.86	Legislators, senior officials and managers
313	1.23	Optical and electronic equipment operators
314	0.22	Ship, aircraft and harbour traffic controllers and technicians
321	3.41	Life science technicians and related associate professionals
322	5.58	Health associate professionals (except nursing)
323	24.27	Nursing and related associate professionals
334	0.41	Traffic instructors and other teaching associate professionals
341	1.36	Finance and sales associate professionals
342	1.15	Business services agents and trade brokers
343	0.36	Administrative associate professionals
344	1.65	Customs inspectors, frontier and coast guards, government tax officials and related associate professionals
346	3.40	Social instructors and related associate professionals
347	2.14	Artistic, entertainment and sports associate professionals
348	2.40	Religious associate professionals
414	2.33	Library, mail and related clerks
422	3.14	Client information clerks
511	1.00	Travel attendants and related workers
513	40.73	Practical nurses, childminders and home care assistants
514	1.26	Other personal services workers
516	3.06	Protective services workers
521	0.03	Fashion and other models
615	0.00	Fishery workers, hunters and trappers

Table C.3. Other lower-level employees.

Occupation code	Share (%)	Occupation name
011	1.15	Legislators, senior officials and managers
222	21.22	Health professionals (except nursing)
223	5.56	Matrons and ward sisters
241	21.24	Business professionals
242	6.55	Legal professionals
243	4.37	Archivists, librarians and related information professionals
244	15.04	Social science and related professionals
245	15.33	Writers and creative or performing artists
246	2.32	Parish clergy and other religious professionals
314	2.51	Ship, aircraft and harbour traffic controllers and technicians
322	0.91	Health associate professionals (except nursing)
341	2.26	Finance and sales associate professionals
347	1.54	Artistic, entertainment and sports associate professionals

Table C.4. Senior officials in planning.

C.2 Measuring Occupation Appreciation

The occupation survey data was drawn from the website of the Finnish magazine *Suomen Kuvalehti* in May 2023 (see <https://suomenkuvalehti.fi/kotimaa/onko-ammattisnousussa-vai-laskussa-katso-mita-ammattaja-suomi-arvostaa-ja-mita-ei/>). Each occupation holds an appreciation rank as well as which party appreciates the occupation group the most. We assign the right-appreciated occupations as those appreciated most by voters of the conservative National Coalition Party (*Kokoomus*) and the left-appreciated as those appreciated the most by voters of the socialist Left Alliance (*Vasemmistoliitto*).

Figure C.1 provides an example. Economists are ranked 181st, being most appreciated by voters of the conservative National Coalition Party and least appreciated by voters of the socialist Left Alliance. This occupation would be coded as an occupation most appreciated by the right.

We assign each occupation an occupational code using Statistics Finland’s occupational classification, and then use these codes to link the data to the register data from Statistics Finland. Note that not all occupations in the survey show up in the Statistics Finland classification, and not all occupations in the classification were part of the survey. This reduces the sample size to around 70% of what we have in the income analyses.

The survey by the magazine was conducted from May 2nd to May 7th, 2018, using the Kantar TNS Gallup Forum respondent panel, with data collected online from 2,000 Finnish individuals aged 15-74, excluding the Åland Islands. Respondents were randomly divided into four groups of 500, each evaluating an alphabetically ordered list of 94-95 professions on a scale of one to nine. The respondents were categorized by gender, age, place of residence, and type of housing. The study analyzed responses from supporters of six political parties: the National Coalition Party, the Centre Party, the Social Democratic Party, the Green Party, the Left Alliance, and the Finns Party, excluding those who supported other parties or did not disclose their political affiliation.

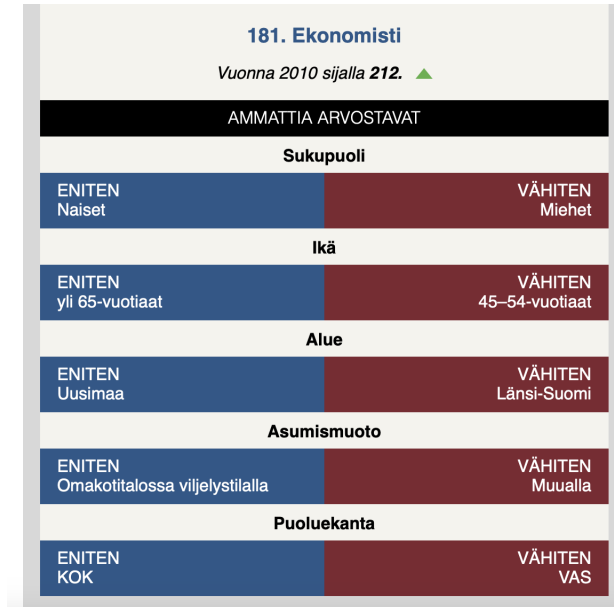


Figure C.1. Example from the survey.

Notes: The figure shows an example of the survey data from *Suomen Kuvalehti*. The profession of economists is ranked 181st. The occupation was most appreciated by voters of the National Coalition Party and least appreciated by voters of the Left Alliance, so the occupation is coded as an occupation appreciated by the right and unappreciated by the left. *Source:* <https://suomenkuvalehti.fi/kotimaa/onko-ammattisiusnousussa-vai-laskussa-katso-mita-ammattiteja-suomi-arvostaa-ja-mita-ei/> (accessed July 2, 2025).

C.3 FDF Data

We utilize the cognitive ability test (*Peruskoe 1*) and personality test (*Peruskoe 2*) provided by the Finnish Defense Forces. The contents of these tests are detailed in Table C.5 below.

All conscripts are required to take both the cognitive ability and personality tests early in their military service. A significant strength of our data is that they encompass a large and stable portion of Finnish men over a long period.¹ This allows us to record test scores for a considerable portion of men at around age 18.

The Finnish Defense Forces use these test scores to select conscripts for military officer training. Consequently, the tests are designed to identify high-quality military leaders and likely capture skills relevant to political leaders as well. The tests' validity and predictive power for successful military service have been assessed in several internal reports (Nyman 2007), showing good internal consistency and correlations with other leadership selection criteria such as peer reviews and evaluations by army personnel. Furthermore, evidence

¹The test items remained unchanged during our study period, ensuring score comparability across cohorts. Additionally, test results are not public information; the test details are a military secret, and conscripts are unaware of the scoring criteria, making it harder to game the test.

suggests that test scores positively correlate with desirable outcomes outside the military (Jokela et al. 2017; Izadi and Tuhkuri 2024).

The cognitive test comprises three subtests measuring visuospatial, verbal, and arithmetic reasoning (40 questions each). The visuospatial subtest is akin to Raven’s Progressive Matrices. The verbal subtest includes tasks such as identifying synonyms and antonyms, categorizing words, finding outliers, and determining word relations. The arithmetic subtest evaluates the ability to create number sequences, solve verbal math problems, tackle simple algebraic questions, and explain numerical relationships.

The personality test assesses eight non-cognitive traits, mostly considered non-cognitive skills, through 218 statements where respondents agree or disagree.

The first trait is leadership motivation, which assesses the preference for leading groups and influencing others. The second trait, activity-energy, measures physical effort in daily activities and the speed of task execution. The third trait, achievement striving, evaluates the drive to perform well and achieve significant goals. Fourth, the self-confidence score gauges self-esteem and beliefs in one’s abilities. Fifth, deliberation is measured by the preference for thinking ahead and planning. Sixth, sociability is assessed through the expressed level of gregariousness and social preferences. Seventh, dutifulness measures honesty and adherence to social norms. The eighth trait is masculinity, based on occupational and recreational interests traditionally viewed as masculine.

Jokela et al. (2017) document that the non-cognitive test traits align with some Big Five personality traits. Specifically, sociability, leadership motivation, activity-energy, and self-confidence correlate with extraversion. Achievement striving, deliberation, dutifulness, and self-confidence correlate positively with conscientiousness. Self-confidence is also negatively correlated with neuroticism. However, agreeableness and openness to new experiences are not well captured by the Finnish Defense Forces’ personality tests.

Table C.5. Components of the cognitive ability and personality tests.

Score	Questions	Explanation
A. Cognitive ability test		
Verbal	40	The participant needs to identify synonyms or antonyms of a given word, select a word that belongs to the same category as a given word pair, choose which word on a list does not belong in the group, and choose similar relationships between two word pairs.
Arithmetic	40	The participant must complete a series of numbers that follow a certain pattern, solve short verbal problems, computing simple arithmetic operations, and choose similar relationships between two pairs of numbers.
Visuospatial	40	The participant needs to decide which of the given alternative figures completes a matrix containing a pattern with one removed part.
B. Personality test		
Leadership motivation	30	The score measures how much the person prefers to take charge in groups and influence other people.
Activity-energy	28	The score measures how much the person exerts physical effort in everyday activities and how quickly the person prefers to execute activities; e.g., whether the person tends to work fast and vigorously and prefers fast-paced work.
Achievement striving	24	The score measures how strongly the person wants to perform well and achieve important life goals; e.g., whether the person is prepared to make personal sacrifices to achieve success.
Self-confidence	32	The score measures the person's self-esteem and beliefs about his abilities; e.g., whether the person feels to be as good and able as others and can meet other people's expectations.
Deliberation	26	The score measures how much the person prefers to think ahead and plan things before acting; e.g., whether the person prefers to spend money carefully.
Sociability	33	The score measures how fond of company a person is and his preference for socializing with other people; e.g., whether the person likes to host parties and not withdraw from social events.
Dutifulness	18	The score measures attitudes and behaviors related to honesty, i.e., how closely the person follows social norms and whether he considers them to be important; e.g., whether the person would return money if he was given back too much change at a store.
Masculinity	27	The score captures the extent to which the person's occupational and recreational interests are traditionally considered as masculine (instead of feminine); e.g., whether the person would like to work as a construction manager.

Source: [Jokela et al. \(2017\)](#)

D Additional Results

This appendix contains additional results: income effects for different grades that were part of the experiment (the 1st and the 9th grade), additional robustness checks, further results on occupation characteristics, and results on the initial labor market conditions upon graduation.

D.1 Income Effects for Different Grades

In Table [D.1](#), we show estimates for the 1st and 9th grade students (besides the 5th grade students) who were also exposed to the Pirkkala school experiment but not to similar material as the 5th graders. We only detect a negative impact for those who were in the 5th grade at the time of the experiment. The estimates for first and ninth graders are small in magnitude and statistically insignificant.

Table D.1. Later income by grade of exposure

	ln(Income 1995–2005)	
	(1)	(2)
Pirkkala $\times$ 5th grade	-0.125*** (0.026)	-0.110*** (0.030)
Pirkkala $\times$ 1st grade	0.003 (0.041)	-0.007 (0.031)
Pirkkala $\times$ 9th grade	0.019 (0.041)	0.033 (0.030)
N	84326	7816
Y mean	5.532	5.601
Birth year FE	✓	✓
Municipality FE	✓	✓
Controls	✓	✓
Sample municipalities	All	Matched
Sample cohorts	1954–1967	1954–1967

Notes: The table reports $\hat{\beta}$ estimated using $Y_{imc} = \beta \mathbf{1}[5th\ grade\ 1973-1975_c \times Pirkkala_m] + X'_m \gamma_c + X'_i \sigma + \lambda_m + \delta_c + \varepsilon_{icm}$. Matched control group includes only individuals who resided in Tornio, Kemijärvi, Masku, Kaarina, or Dragsfjärd in 1970. Standard errors clustered by municipality $\times$ cohort are shown in parentheses. *, ** and *** denote statistical significance at 10%, 5% and 1%, respectively.

D.2 Robustness Checks

We perform a number of robustness checks that confirm our main finding. More specifically, our income results are robust to (1) using alternative income measures, (2) alternative specifications, and (3) alternative numbers of matched control municipalities.

First, Table D.2 uses alternative income measures as the dependent variable. The negative income effect for the exposed students is present when we use a longer time span (columns 1 and 2), the logarithm of the average income between 1995–2005 (columns 3 and 4), the total income in 1995–2005, the logarithm of earnings in 1995–2003 or 1995–2005, and the logarithm of income or income at the age 40 as the dependent variable.

Second, Table D.3 examines robustness to alternative specifications or estimation samples: including municipality trends (column 1) or urban area fixed effects (column 2),

restricting the estimation sample to cohorts born in 1962-1966 (column 3), 1958-1970 (column 4), or 1950-1970 (column 5), adding more household characteristics as control variables (column 6) and using cohorts born in 1952-1970 with these controls (column 7), winsorizing the income variable (column 8), and including additional municipality-level controls—socialist party vote share in the parliamentary election preceding the school experiment, socialist party seat share in the local council following the 1972 local elections, and school expenditures in 1972 (column 9).

Third, we assess the robustness of the analyses where we only use a matched municipality as the control group by varying the number of matched municipalities (Table D.4). We vary the number of matched control municipalities between one and nine, systematically finding similar effects as in the main analyses.

Fourth, we consider alternative estimation strategies that also confirm our key result. The first alternative approach is a matching strategy. We utilize individual-level register data, and for each individual in Pirkkala, either in the treated or control cohorts, we find the closest individual match from the control municipalities in the same cohort using propensity score matching with replacement based on baseline 1970 household characteristics (logarithm of household income, an indicator for owning a house, and indicators for the household head being unemployed, university-educated, having graduated from high school, and working in a managerial or a blue-collar worker occupation). We then estimate Equation 1 for the matched sample, but also include an individual-level match fixed effect δ_p , comparing the matched individuals. We do this also for the sample of matched municipalities as the pool. In the results presented in Table D.5, we find similar results to our baseline analysis. The matched estimates remain negative, statistically significant, and close in magnitude to our baseline (a reduction of roughly 10%). An additional benefit of the individual matching is that the sample is the same size between the Pirkkala cohorts and the cohorts in the comparison municipalities, assuring that the size differences of the compared groups is not driving the results. Note that we do not have an individual-level panel (i.e., incomes before and after the treatment), which constrains the individual-level matched difference-in-differences design we can perform.

As Pirkkala is the sole treated municipality, we assess the robustness of our results using a synthetic control method in Figure D.1 (Abadie 2021). Here, we first net out the individual-level household controls and then use income by municipality–two-year cohort as the outcome. There is a clearly visible drop in the incomes of the treated cohorts in Pirkkala relative to the synthetic alternative. The caveat of using the synthetic control, and the reason it is not our preferred method, is that it does not function well when treatment turns on and off, as in our case, and it requires a balanced panel, meaning we cannot fully

utilize the individual-level data with individual controls as in our baseline specification. Similarly, in Figure D.2, we present the synthetic difference-in-differences following Arkhangelsky et al. (2021). This method has the same core issues as the synthetic control method in our setting, as we cannot utilize the entire dataset and variation. However, for the pre–post 1954–1963 period, we find estimates that are similar to our baseline estimates.

Finally, we exploit our set-up to perform a difference-in-discontinuities exercise in Table D.6 and illustrate the setting in Figure D.3. To perform the exercise more credibly, we use the distance in days from being treated (positive for those treated regardless of birth year, negative for the control group) as the running variable and perform the RDD estimation for the treatment and control group within an ad hoc bandwidth of one year. Our specification takes the following form:

$$Y_{imc} = \beta (D_i \times \text{Pirkkala}_m) + \gamma D_i + \pi_1 r_i + \pi_2 (D_i \times r_i) \\ + \text{Pirkkala}_m \times [\eta + \theta_1 r_i + \theta_2 (D_i \times r_i)] + X_i' \sigma + \varepsilon_{imc} \quad (1)$$

where r_i is the distance of individual i 's birth date to the cutoff between the 1962 and 1963 cohorts, and $D_i = \mathbf{1}[r_i \geq 0]$ indicates the treated-side cohort (5th grade). The terms $\pi_1 r_i + \pi_2 (D_i \times r_i)$ allow the running-variable slope to differ on each side of the cutoff in the control municipalities, and the corresponding θ_1, θ_2 terms allow these slopes to differ further in Pirkkala; Pirkkala_m indicates the treated municipality. The coefficient of interest, β , is the difference-in-discontinuities estimate: the discontinuity in the outcome at the cutoff in Pirkkala net of the corresponding discontinuity in the control municipalities. We restrict the bandwidth to one year.

We visualize findings from this approach in Panels A–C of Figure D.3. Panels A and B focus on the 1961–1962 and 1963–1964 cohorts, respectively, and Panel C pools the two cutoffs (January 1, 1962 and December 31, 1963) together. We observe a clear jump downwards in incomes. This jump is larger than what we find in our TWFE analyses but becomes attenuated as we make the bandwidth smaller (Panel D of Figure D.3). That said, the estimates remain systematically negative and statistically indistinguishable from our main estimates, which is reassuring.

Columns (1)–(3) of Table D.6 estimate the regression discontinuity separately for Pirkkala and for the control municipalities. There is a clear negative discontinuity in the long-term incomes of Pirkkala 5th graders, on the order of 18%, with no comparable discontinuity in the control municipalities. Columns 4–7 present the difference-in-discontinuities estimates, which are robust to adding parent controls, separate cutoff fixed-effect interactions, and restricting the controls to the matched municipalities. Columns 8–9 estimate the difference-

in-discontinuities separately for the two cutoffs, each yielding a substantial negative impact despite the limited sample size.

Because this design identifies the effect using jumps at the birth-date cutoffs, it is more demanding than our baseline TWFE specification in a setting with a relatively limited exposed group. We therefore treat the difference-in-discontinuities estimates as additional evidence rather than as our baseline specification.

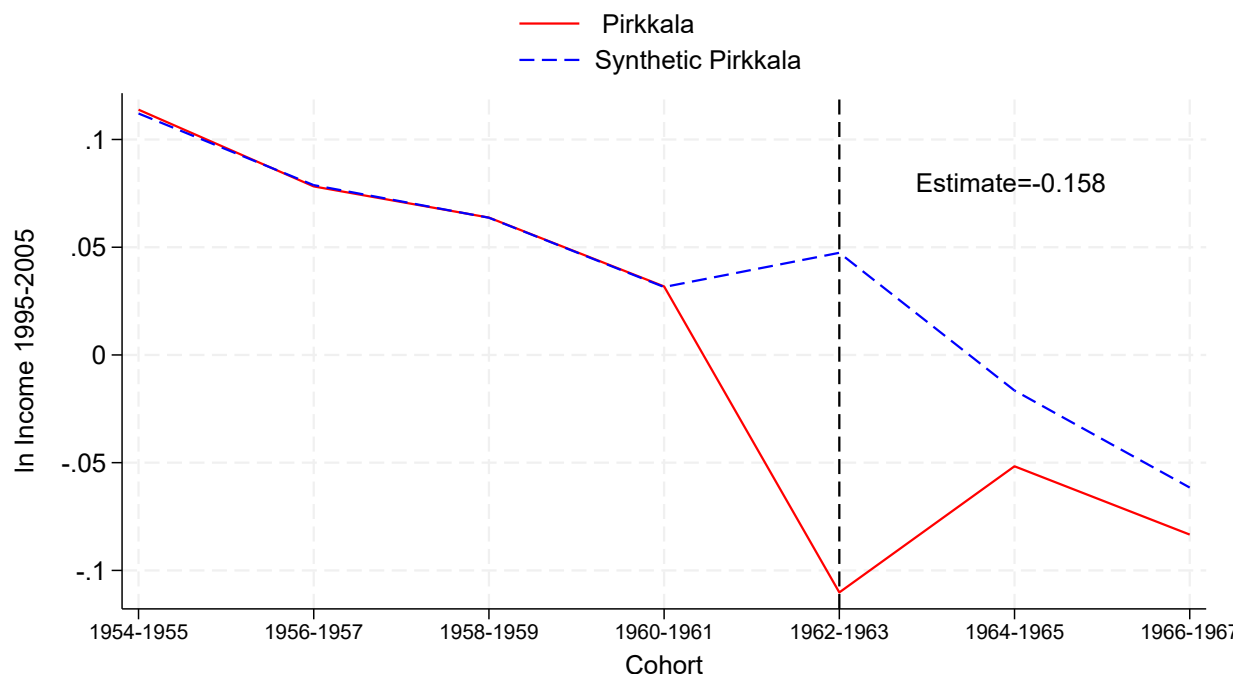


Figure D.1. Visualization of the synthetic control method.

Table D.2. Robustness to alternative ways of measuring adulthood income

	ln(Income 1993–2010)		ln(Mean Income 1995–2005)		Income 1995–2005		ln(Earnings 1995–2005)		Earnings 1995–2005		ln(Income at Age 40)		Income at Age 40	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Pirkkala × 5th grade	-0.107*** (0.027)	-0.103*** (0.020)	-0.128*** (0.020)	-0.123*** (0.028)	-31.536*** (6.332)	-35.759*** (10.173)	-0.226** (0.108)	-0.201*** (0.047)	-40.196*** (7.494)	-44.702*** (10.775)	-0.155*** (0.035)	-0.161*** (0.022)	-13.999*** (3.138)	-17.154*** (5.235)
<i>N</i>	84383	7827	84326	7816	84468	7833	79842	7481	84468	7833	83888	7756	84237	7804
<i>Y</i> mean	6.077	6.146	3.172	3.252	302.855	326.808	5.142	5.309	252.620	282.840	11.652	11.735	139.295	151.793
Birth year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Municipality FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Sample municipalities	All	Matched	All	Matched	All	Matched	All	Matched	All	Matched	All	Matched	All	Matched
Sample cohorts	1954–1967	1954–1967	1954–1967	1954–1967	1954–1967	1954–1967	1954–1967	1954–1967	1954–1967	1954–1967	1954–1967	1954–1967	1954–1967	1954–1967

Notes: The table reports $\hat{\beta}$ estimated using $Y_{inc} = \beta_1[5th\ grade] + \beta_2[5th\ grade] + X'_{inc}\gamma_c + X'_{inc}\gamma_m + X'_{inc}\gamma_{em} + \epsilon_{inc}$. Matched control group includes only individuals who resided in Tornio, Kemijärvi, Masku, Kaarina, or Dragsfjärd in 1970. Standard errors clustered by municipality × cohort are shown in parentheses. *, **, and *** denote statistical significance at 10%, 5% and 1%, respectively.

Table D.3. Robustness of income results.

	Municipality Trends	Urban Area FE	1962- 1966	1958- 1970	1950- 1970	More Household Controls	More Household Controls + 1952-1970	Winsorize	Additional Municipality Controls
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
A. Full sample									
Pirkkala $\times$ 5th grade	-0.118*** (0.025)	-0.159*** (0.034)	-0.108** (0.048)	-0.093*** (0.026)	-0.094*** (0.022)	-0.119*** (0.024)	-0.093*** (0.024)	-0.131*** (0.019)	-0.127*** (0.024)
<i>N</i>	84326	37661	28794	71708	118870	85436	110256	84326	84326
B. Matched sample									
Pirkkala $\times$ 5th grade	-0.106*** (0.026)	-0.151*** (0.028)	-0.112** (0.043)	-0.079*** (0.028)	-0.080*** (0.027)	-0.106*** (0.026)	-0.081*** (0.026)	-0.115*** (0.021)	-0.076*** (0.005)
<i>N</i>	7816	5601	2794	7174	11406	7964	10579	7816	7816
Birth year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Municipality FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes: The table reports $\hat{\beta}$ estimated using $Y_{inc} = \beta_1[5th\ grade\ 1973 - 1975_c \times Pirkkala_m] + X'_m\gamma_c + X'_i\sigma + \lambda_m + \delta_c + \varepsilon_{inc}$. Additional household controls include dummies for housing type, education degree, main type of employment activity, 2-digit industry, dummy for entrepreneur, and number of rooms measured in 1970. Matched control group includes only individuals who resided in Tornio, Kemijärvi, Masku, Kaarina, or Dragsfjärd in 1970. Standard errors clustered by municipality $\times$ cohort are shown in parentheses. *, **, and *** denote statistical significance at 10%, 5% and 1%, respectively.

Table D.4. Robustness to the matched municipality sample selection.

	ln(Income 1995-2005)								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Pirkkala x 5th grade	-0.131*** (0.029)	-0.121*** (0.030)	-0.115*** (0.027)	-0.107*** (0.024)	-0.109*** (0.023)	-0.113*** (0.022)	-0.116*** (0.022)	-0.118*** (0.019)	-0.113*** (0.023)
N	3859	4463	5234	6741	8228	8879	9543	16010	19575
Number of municipalities	2	3	4	5	6	7	8	9	10
Birth year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Municipality FE	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes: The table reports the estimates when using 1-9 best matched municipalities as the control group. In the baseline we use 5. The table reports $\hat{\beta}$ estimated using $Y_{imc} = \beta \mathbf{1}[5th\ grader\ 1973 - 1975_c \times Pirkkala_m] + \lambda_m + \delta_c + \varepsilon_{icm}$. Standard errors clustered by municipality $\times$ cohort are shown in parentheses. *, ** and *** denote statistical significance at 10%, 5% and 1%, respectively.

Table D.5. Matching estimates.

	ln(Income 1995-2005)	
	(1)	(2)
Pirkkala $\times$ 5th grade	-0.117** (0.046)	-0.101** (0.043)
N	2944	2914
Y mean	5.6189	5.6285
Individual match FE	✓	✓
Municipalities	All	Matched

Notes: The table reports $\hat{\beta}$ estimated using $Y_{imc} = \beta 1[5th\ grader\ 1973 - 1975_c \times Pirkkala_m] + X'_m \gamma_c + X'_i \sigma + \lambda_m + \delta_c + \delta_p + \varepsilon_{icm}$. Matched control group includes only individuals who resided in Tornio, Kemijärvi, Masku, Kaarina, or Dragsfjärd in 1970. Standard errors clustered by municipality $\times$ cohort are shown in parentheses. *, ** and *** denote statistical significance at 10%, 5% and 1%, respectively.

Table D.6. Difference-in-discontinuities estimates.

	ln Income 1995-2005								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
5th grade	-0.198** (0.034)	0.012 (0.021)	-0.002 (0.047)						
Pirkkala $\times$ 5th grade				-0.209*** (0.036)	-0.206*** (0.030)	-0.209*** (0.039)	-0.206*** (0.067)	-0.139*** (0.024)	-0.283*** (0.024)
N	454	25104	1924	25558	24374	24374	2272	11850	12524
Parent controls					✓	✓	✓	✓	✓
Cutoff FEs						✓	✓		
Sample municipalities	Pirkkala	Control	Matched Control	All	All	All	Matched	All	All
Cutoff								1962	1963
Model	RDD	RDD	RDD	Diff-in-Disc	Diff-in-Disc	Diff-in-Disc	Diff-in-Disc	Diff-in-Disc	Diff-in-Disc

Notes: The table reports $\hat{\beta}$ estimated regression discontinuity or difference in discontinuities with linear polynomial in birthdate. Matched control group includes only individuals who resided in Tornio, Kemijärvi, Masku, Kaarina, or Dragsfjärd in 1970. Standard errors clustered by municipality $\times$ cohort are shown in parentheses. *, ** and *** denote statistical significance at 10%, 5% and 1%, respectively.

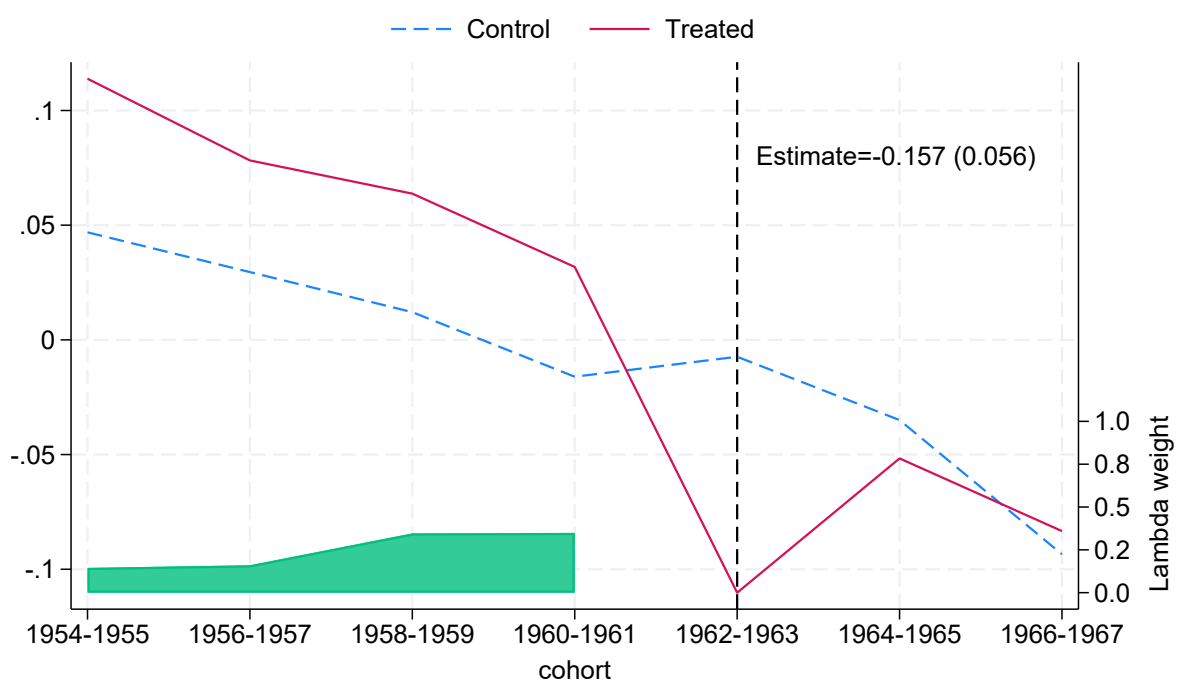


Figure D.2. Visualization of the synthetic difference-in-differences.

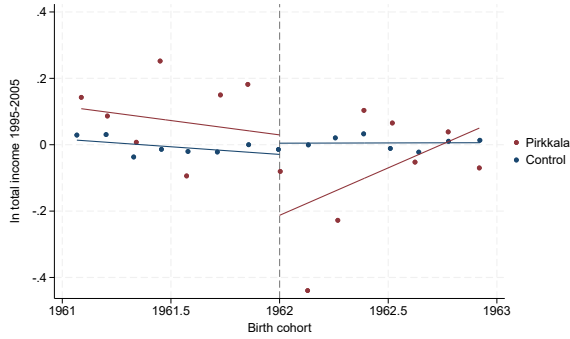
D.3 Additional Outcomes

Table D.7 reports additional evidence on long-run economic outcomes by examining taxable wealth and asset ownership in adulthood. Across both the full and matched samples, individuals exposed to the Pirkkala curriculum in 5th grade exhibit modest but consistently negative differences in accumulated wealth relative to controls.

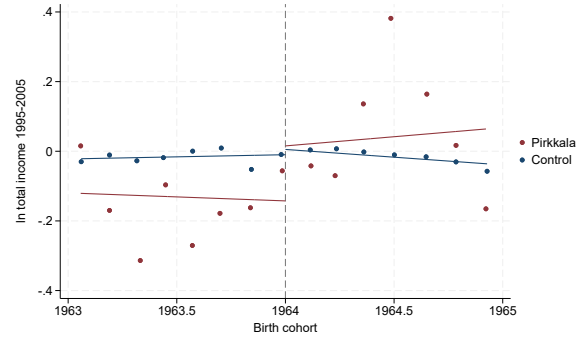
Table D.7. Effects on wealth and ownership of a house or a car.

	ln(Taxable wealth)		Owns apartment or house		Owns car	
	(1)	(2)	(3)	(4)	(5)	(6)
Pirkkala $\times$ 5th grade	-0.218*** (0.057)	-0.203** (0.081)	-0.040* (0.021)	-0.052* (0.029)	-0.028* (0.015)	-0.005 (0.025)
N	58624	5453	84468	7833	84468	7833
Y mean	9.585	9.585	0.861	0.861	0.721	0.721
Sample municipalities	All	Matched	All	Matched	All	Matched

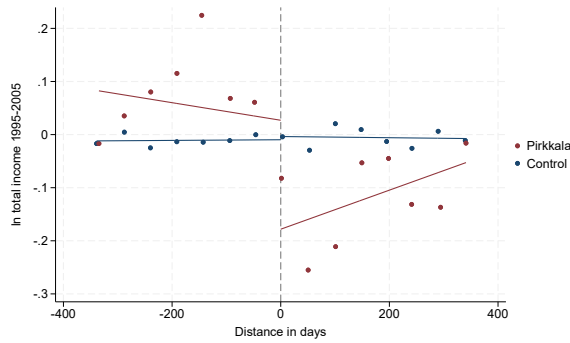
Notes: The table reports $\hat{\beta}$ estimated using $Y_{imc} = \beta \mathbf{1}[5th\ grader\ 1973 - 1975_c \times Pirkkala_m] + X'_m \gamma_c + X'_i \sigma + \lambda_m + \delta_c + \varepsilon_{icm}$. Matched control group includes only individuals who resided in Tornio, Kemijärvi, Masku, Kaarina, or Dragsfjärd in 1970. Standard errors clustered by municipality $\times$ cohort are shown in parentheses. *, ** and *** denote statistical significance at 10%, 5% and 1%, respectively.



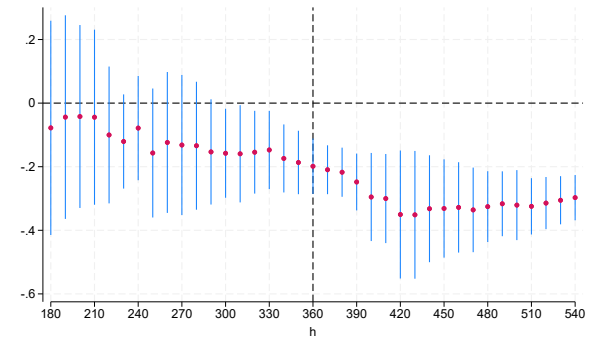
A. RDD by group, 1961 vs 1962.



B. RDD by group, 1963 vs 1964.



C. Pooled RDD by group.



D. Difference-in-discontinuities estimates for varying bandwidths.

Figure D.3. Difference-in-discontinuities.

Notes: The figures illustrate results from the difference-in-discontinuities approach. The estimate obtained by pooling the two cutoffs together and using a bandwidth of one year is -0.20 ($p < 0.01$) in Panel C. Household characteristics from the 1970 census are netted out. Panel D shows estimates obtained using varying bandwidths and their 95% confidence intervals.

D.4 Effect Heterogeneity

In Table D.8, we examine effect heterogeneity. Importantly, the lessons from these analysis support the interpretation that the estimated income effects reflect the consequences of the Pirkkala instructional experiment rather than some other correlated shock.

First, the losses are tightly aligned with the treated cohorts: both the 1962 and 1963 cohorts—those exposed to the curriculum in 5th grade—experience sizable income reductions, while adjacent untreated cohorts do not show comparable declines elsewhere in the analysis. This cohort specificity is difficult to reconcile with any municipality-wide event or economic shift.

Second, the income effects are essentially identical for men and women. A local economic shock or labor-market change would typically produce gender-differentiated impacts given the period’s strong occupational segregation. The absence of such differences is instead consistent with a mechanism operating uniformly in the classroom.

Third, the effects do not vary meaningfully by childhood household income. If the estimates reflected a place-based shock in Pirkkala, one would expect larger consequences for children from low-income households, who are more exposed to local economic fluctuations. Instead, the reductions in long-run income are nearly the same across socioeconomic backgrounds, again pointing toward an educational rather than environmental mechanism.

Finally, the clearest heterogeneity emerges along a dimension that would be expected to shape sensitivity to instructional quality: cognitive ability. Children with below-median IQ experience substantially larger income losses, while those above the median show no detectable effect. This pattern is consistent with the idea that the experimental curriculum altered the learning environment in ways that particularly disadvantaged lower-ability students, and is much harder to explain through external shocks that would affect all children more uniformly.

Table D.8. Heterogeneity.

	ln(Income 1995-2005)			
	(1)	(2)	(3)	(4)
Pirkkala $\times$ 1962 cohort	-0.140*** (0.026)			
Pirkkala $\times$ 1963 cohort	-0.119*** (0.023)			
Pirkkala $\times$ 5th grade $\times$ male		-0.131*** (0.031)		
Pirkkala $\times$ 5th grade $\times$ female		-0.127*** (0.046)		
Pirkkala $\times$ 5th grade $\times$ above median hh income			-0.132*** (0.027)	
Pirkkala $\times$ 5th grade $\times$ below median hh income			-0.109 (0.122)	
Pirkkala $\times$ 5th grade $\times$ above median CA				0.024 (0.079)
Pirkkala $\times$ 5th grade $\times$ below median CA				-0.169*** (0.029)
N	84326	84326	84326	13819
Y mean	5.5453	5.5453	5.5453	5.6844
p -value $\beta_1=\beta_2$	0.4148	0.9534	0.8683	0.0332

Notes: The table reports $\hat{\beta}$ estimated using $Y_{imc} = \beta_1[5th\ grader\ 1973 - 1975_c \times Pirkkala_m] + X'_m\gamma_c + X'_i\sigma + \lambda_m + \delta_c + \varepsilon_{icm}$. Matched control group includes only individuals who resided in Tornio, Kemijärvi, Masku, Kaarina, or Dragsfjärd in 1970. Standard errors clustered by municipality $\times$ cohort are shown in parentheses. *, ** and *** denote statistical significance at 10%, 5% and 1%, respectively.

D.5 Further Results on Labor Supply

In Figure D.4, we estimate the cohort-specific effects of exposure to the school experiment on labor supply, measured as mean yearly months worked in 1997–2005. Similar to the event-study results for our main outcome, income over 1995–2005, we see that an effect is only present for the cohort that was part of the school experiment. The estimates for other cohorts are close to zero and statistically insignificant.

In Figure D.5, we show year-specific estimates for the yearly months worked. The pattern we observe closely reflects what we also find when using income as the dependent variable.

In Table D.9, we show that the labor supply effect is not solely from occupational choice. It is still present when conditioning on occupation (assigned based on the occupation code in the year 2000).

Figure D.6 documents the relationship between political ideology and pro-work attitudes in the Finnish adult population. Using the Finnish samples of the World Values Survey—Wave 5 (2005) for items C037, C038, and C039 ($N \approx 900$) and Wave 3 (1996) for item C008 ($N \approx 840$)—we regress four work-attitude measures on respondents’ self-placement on the standard 1–10 left-right scale (item E033), with robust standard errors. The four work-attitude items, all recoded so that higher values denote stronger pro-work attitudes, are “people who don’t work turn lazy” (C038), “work is a duty toward society” (C039), “it is humiliating to receive money without having to work for it” (C037), and “work, rather than leisure, makes life worth living” (C008).

In all four panels, the estimated slope is positive, indicating that Finns who place themselves further to the right of the political spectrum report systematically stronger pro-work attitudes than those further to the left. Three of the four associations are statistically significant at conventional levels.

We treat these cross-sectional patterns as descriptive, but they nevertheless lend external plausibility to the labor-supply channel in our main analysis: more left-leaning political identification is systematically associated with weaker individual attachment to work.

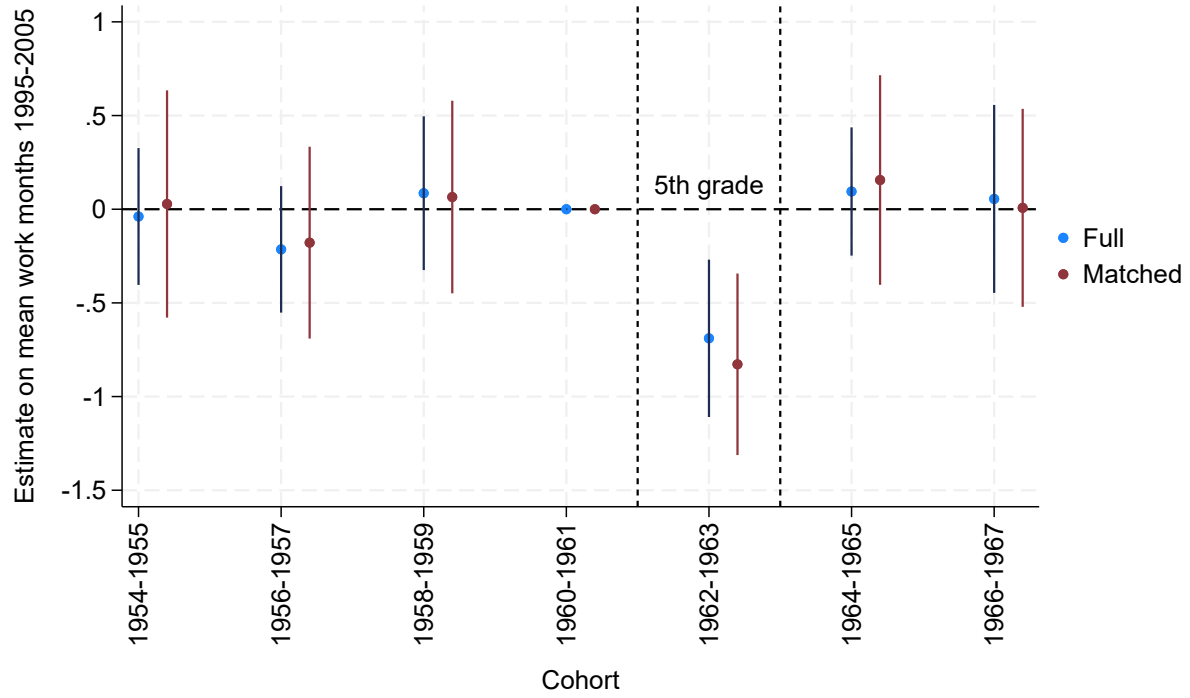


Figure D.4. Estimated labor supply effects by cohort.

Notes: The figure plots point estimates of cohort-specific effects (relative to the 1960-1961 cohort) from an event-study specification $Y_{imc} = \beta_c \mathbf{1}[Cohort_c \times Pirkkala_m] + X'_m \gamma_c + X'_i \sigma + \lambda_m + \delta_c + \varepsilon_{icm}$. We use 1960-1961 cohorts as the base group. All estimations include municipality and cohort fixed effects, parent controls, and municipality controls interacted with cohort. The vertical line marks the exposed cohorts (cohorts born 1962-1963). Matched control group includes only individuals who resided in Kemijärvi, Tornio, Masku, Kaarina, or Dragsfjärd in 1970. We also show the 95% confidence intervals constructed using standard errors clustered at the municipality-cohort level.

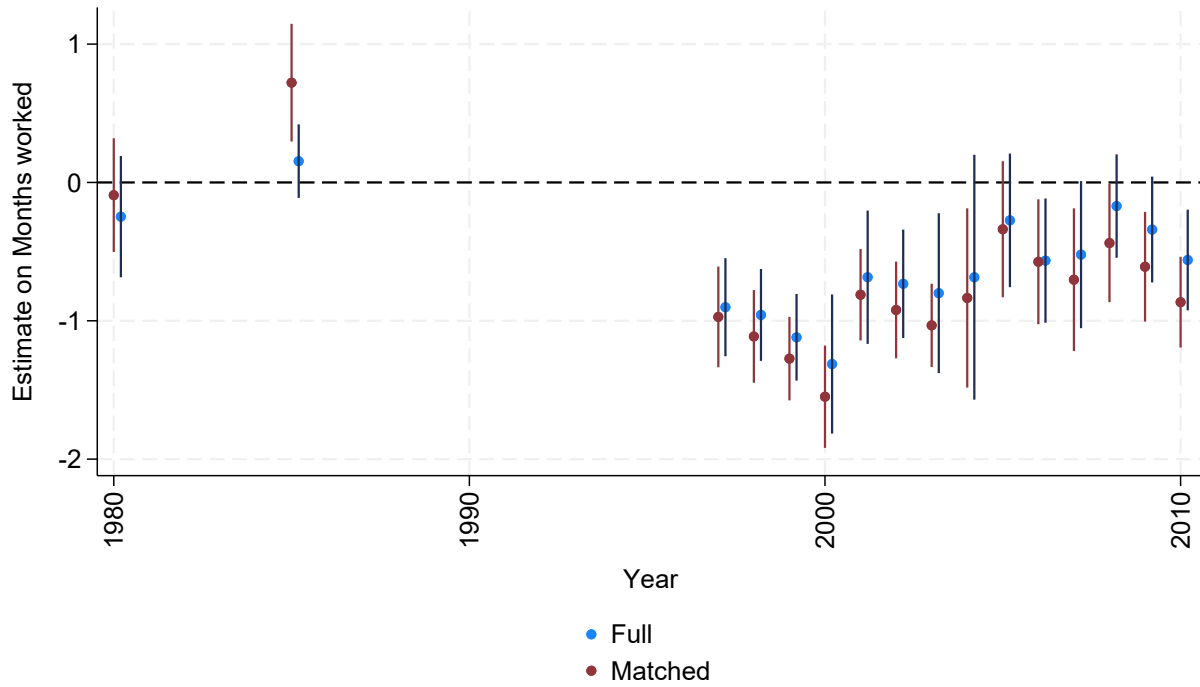


Figure D.5. Estimated labor supply effects over life cycle.

Notes: The figure plots $\hat{\beta}$ estimated separately for each year using $Y_{imc} = \beta \mathbf{1}[5th\ grader\ 1973 - 1975_c \times Pirkkala_m] + X'_m \gamma_c + X'_i \sigma + \lambda_m + \delta_c + \varepsilon_{icm}$. All estimations include municipality and cohort fixed effects, parent controls, and municipality controls interacted with cohort. Matched control group includes only individuals who resided in Kemijärvi, Tornio, Masku, Kaarina, or Dragsfjärd in 1970. We also show the 95% confidence intervals constructed using standard errors clustered at the municipality-cohort level.

Table D.9. Effects of the school experiment on labor market participation conditional on occupation.

	Yearly Months Worked		Yearly Months Unemployed		ln(Total Months Worked)	
	(1)	(2)	(3)	(4)	(5)	(6)
Pirkkala $\times$ 5th grade	-0.476*** (0.091)	-0.560*** (0.087)	0.133 (0.092)	0.115 (0.131)	-0.079*** (0.029)	-0.089*** (0.028)
N	66301	6461	66301	6461	66298	6461
Sample cohorts	1954-1967	1954-1967	1954-1967	1954-1967	1954-1967	1954-1967
Y mean	7.411	7.672	1.470	1.117	4.100	4.161
Birth year FE	✓	✓	✓	✓	✓	✓
Birth municipality FE	✓	✓	✓	✓	✓	✓
Occupation FE	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓
Sample municipalities	All	Matched	All	Matched	All	Matched

Notes: The table reports $\hat{\beta}$ estimated using $Y_{ime} = \beta \mathbf{1}[5th\ grader\ 1973 - 1975_c \times Pirkkala_m] + X'_m \gamma_c + X'_i \sigma + \lambda_m + \delta_c + \varepsilon_{icm}$. Conditional on occupation measured in the year 2000. Months unemployed 1995-2005 and yearly months worked is measured as an average over 1997-2005. All specifications include municipality and cohort fixed effects, parent controls, and municipality controls interacted with cohort. The estimation sample includes 1954-1967 cohorts. Matched control group includes only individuals who resided in Kemijärvi, Tornio, Masku, Kaarina, or Dragsfjärd in 1970. Standard errors clustered by municipality $\times$ cohort are shown in parentheses. There are 630 clusters in total in the full sample (fourteen cohorts and 45 municipalities) and 84 in the matched sample (fourteen cohorts and six municipalities). *, ** and *** denote statistical significance at 10%, 5% and 1%, respectively.

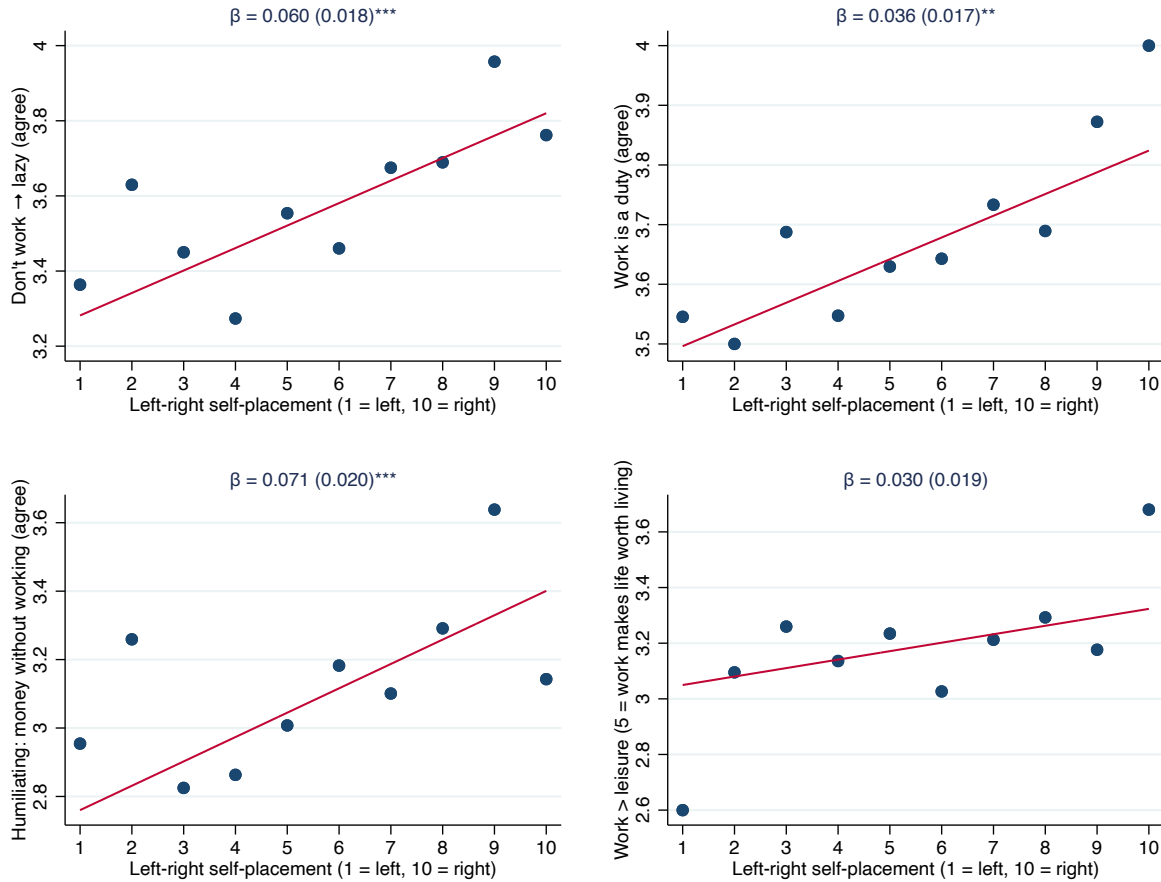


Figure D.6. Pro-work attitudes and left-right self-placement in Finland.

Notes: Binned scatterplots of four work-attitude items against respondents' left-right self-placement (WVS item E033, 1 = left to 10 = right) in the Finnish samples of the World Values Survey. The panels for “people who don't work turn lazy” (C038), “work is a duty toward society” (C039), and “humiliating to receive money without having to work for it” (C037) use Wave 5 (2005, $N \approx 900$); the panel for “work, rather than leisure, makes life worth living” (C008) uses Wave 3 (1996, $N \approx 840$). All work-attitude items are recoded so that higher values denote stronger pro-work attitudes. The figure shows binned averages and a linear fit. We report the OLS slope coefficient with robust standard errors in parentheses. *, ** and *** denote statistical significance at 10%, 5% and 1%, respectively.

D.6 Occupation Characteristics

Table D.10 shows additional estimation results for occupation characteristics. There is weak indication that the occupations chosen by those exposed to the Pirkkala school experiment have less months worked (columns 1 and 2) and lower average wages (columns 3 and 4), although these estimates are weaker in terms of statistical significance and robustness to using the full or matched sample of control municipalities.

We also construct an index of left-wing voting by occupation using data from the Finnish National Election Study 2019. These data record individuals' occupation and voting intentions (however, only for 23 occupation groups). For each occupation, we compute the share of survey respondents who express that they intend to vote for the socialist Left Alliance and then match this information with the administrative register data. We see that preference for the Left Alliance is greater in the occupations chosen by those who were part of the Pirkkala experiment than in the control group (columns 5 and 6).

In Table D.11, we use the occupation appreciation data to code occupations as least appreciated by socialists and conservatives. We see that the occupations chosen by the individuals exposed to the Pirkkala school experiment are more likely to be least appreciated by conservatives (columns 3 and 4). There is also some indication that they are less likely to be least appreciated by socialists, but only when we consider the matched sample.

Table D.10. Effects of the school experiment on occupation characteristics.

	Months Worked		Average Wage		Left Index	
	(1)	(2)	(3)	(4)	(5)	(6)
Pirkkala x 5th grade	-0.045 (0.036)	-0.108*** (0.035)	-0.944* (0.497)	-1.072 (0.810)	0.362* (0.194)	0.429 (0.378)
N	66302	6464	66302	6464	64613	6303
Sample cohorts	1954-1967	1954-1967	1954-1967	1954-1967	1954-1967	1954-1967
Y mean	11.220	11.220	33.384	33.384	6.561	6.561
Birth year FE	✓	✓	✓	✓	✓	✓
Municipality FE	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓
Sample municipalities	All	Matched	All	Matched	All	Matched

Notes: The table reports $\hat{\beta}$ estimated using $Y_{imc} = \beta 1[5th\ grader\ 1973 - 1975_c \times Pirkkala_m] + X'_m \gamma_c + X'_i \sigma_+ \lambda_m + \delta_c + \varepsilon_{icm}$. The outcomes are occupation based indices calculated from the full population or from the Finnish electoral study. Matched control group includes only individuals who resided in Tornio, Kemijärvi, Masku, Kaarina, or Dragsfjärd in 1970. Standard errors clustered by municipality $\times$ cohort are shown in parentheses. *, ** and *** denote statistical significance at 10%, 5% and 1%, respectively.

Table D.11. Unappreciation of the occupations by socialists and conservatives.

	Least Appreciated Left		Least Appreciated Right	
	(1)	(2)	(3)	(4)
Pirkkala $\times$ 5th grade	0.004 (0.030)	-0.048* (0.027)	0.090*** (0.024)	0.118*** (0.026)
N	54741	4848	54741	4848
Y mean	0.617	0.592	0.522	0.494
Randomization inference p	0.939	0.432	0.060	0.085
Wild bootstrap p	0.935	0.320	0.132	0.114
Sample municipalities	All	Matched	All	Matched

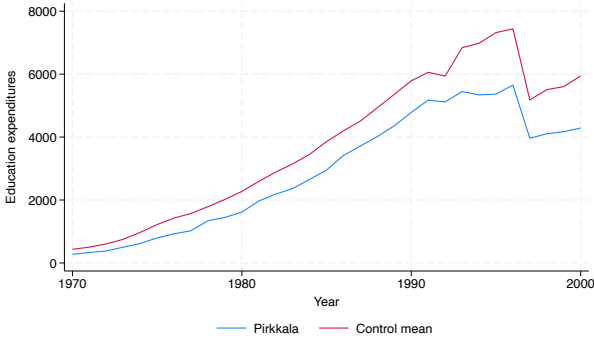
Notes: The table reports $\hat{\beta}$ estimated using $Y_{imc} = \beta 1[5th\ grader\ 1973 - 1975_c \times Pirkkala_m] + X'_m \gamma_c + X'_i \sigma + \lambda_m + \delta_c + \varepsilon_{imc}$. The outcomes are indices based on occupation appreciation survey conducted by the magazine *Suomen Kuvalehti*. *Least appreciated by Left* refers to an occupation being least appreciated by people supporting the socialist Left Alliance, and *Least appreciated by Right* refers to an occupation being least appreciated by people supporting the conservative National Coalition Party. All specifications include municipality and cohort fixed effects, parent controls, and municipality controls interacted with cohort. The estimation sample includes 1954-1967 cohorts. Standard errors clustered by municipality $\times$ cohort are shown in parentheses. *, **, and *** denote statistical significance at 10%, 5% and 1%, respectively.

D.7 Municipal Schooling Expenditures

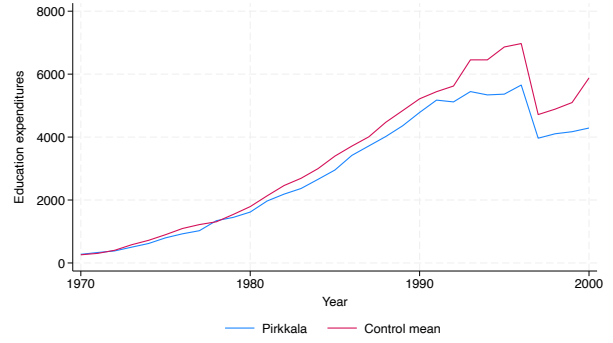
Providing primary schooling is municipalities' responsibility. Although the central government provides them with fiscal grants to finance education provision, there is variation in education expenditures across time and space. In Figure D.7, we show that schooling expenditures do not have significantly diverging trends in Pirkkala and the control municipalities around the experiment (although the levels are somewhat different in the full sample). Thus, it is unlikely that differential investments in schooling that would affect our exposed group would explain the observed effects.

D.8 Initial Labor Market Conditions

One concern is that the labor market outcomes of the individuals exposed to the Pirkkala school experiment are driven by local labor market conditions at the time of graduation rather than being part of the experiment. However, we believe that this is unlikely. To substantiate our claim, we estimate our main specification using the local employment and unemployment rates at the time of graduation as the dependent variable. If anything, there is some evidence that the Pirkkala students graduated in *better* labor market conditions than the control group. These estimates are shown in Table D.12. In Figures D.8 and D.9, we show employment and unemployment rates in Pirkkala and the control municipalities by year. The



A. Full sample.

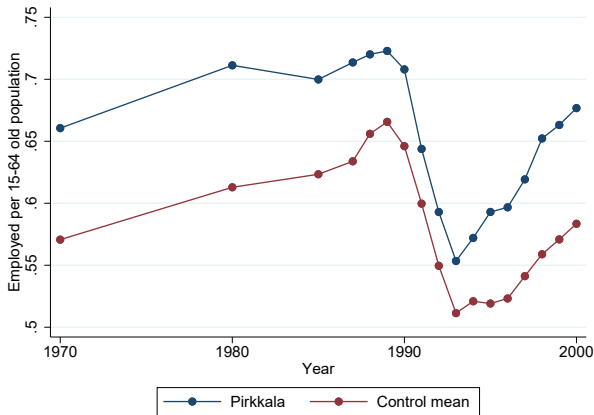


B. Matched municipalities.

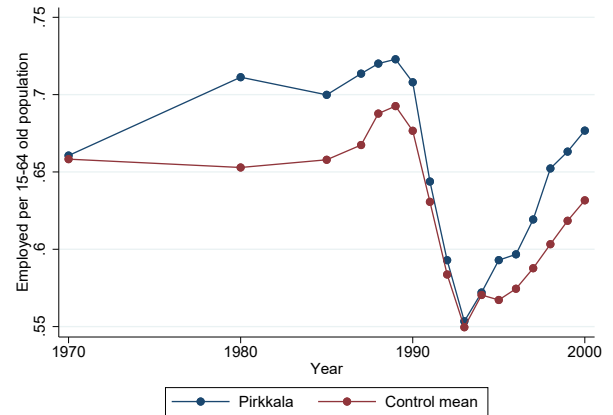
Figure D.7. School expenditures over time.

Notes: School expenditures are measured in thousands of 1970 Finnish Marks per capita.

figures reveal that employment in Pirkkala was higher or equal to and unemployment lower or equal to employment and unemployment in the control municipalities from 1970 to 2000.



A. Full sample.



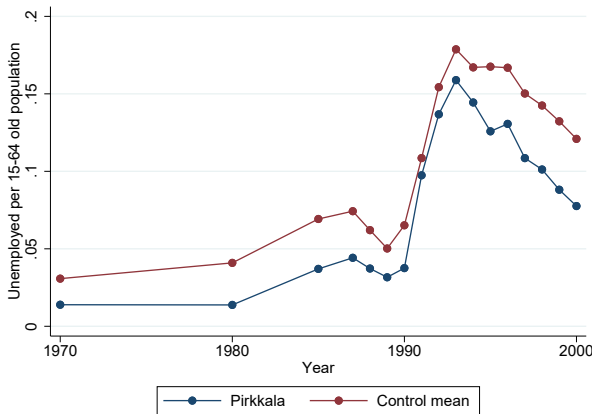
B. Matched municipalities.

Figure D.8. Local employment rates over time.

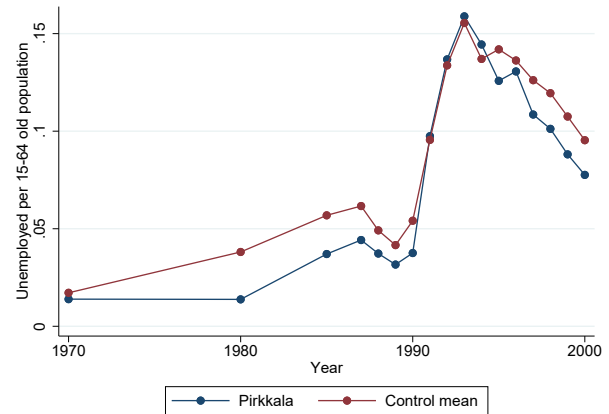
Table D.12. Local employment at the time of graduation.

	Employment rate		Unemployment rate	
	(1)	(2)	(3)	(4)
Pirkkala $\times$ 5th grade	0.005** (0.002)	0.003 (0.003)	-0.005** (0.002)	-0.003 (0.002)
N	71775	6568	71775	6568
Y mean	0.591	0.660	0.077	0.056
Birth year FE	✓	✓	✓	✓
Municipality FE	✓	✓	✓	✓
Controls	✓	✓	✓	✓
Sample municipalities	All	Matched	All	Matched

Notes: The table reports the employment and unemployment rates at the time of graduation by the 1970 municipality of residence. Matched control group includes only individuals who resided in Tornio, Kemijärvi, Masku, Kaarina, or Dragsfjärd in 1970. Standard errors clustered by municipality $\times$ cohort are shown in parentheses. *, ** and *** denote statistical significance at 10%, 5% and 1%, respectively.



A. Full sample.



B. Matched municipalities.

Figure D.9. Local unemployment rates over time.

E Survey Evidence

We complement our analysis of the administrative register with findings from an original survey that we ran at the end of the year 2024 and the beginning of the year 2025. This appendix provides details on the survey collection and analysis thereof. The analysis was pre-registered in January 2025 before we received any of the survey data.² The survey collection was approved by the University of Helsinki Ethical Review Board in Humanities and Social and Behavioral Sciences (Statement 42/2024).

E.1 Survey Collection

The survey was conducted between late 2024 and early 2025 by the Finnish survey company Taloustutkimus. The survey was mailed to randomly selected individuals who resided in the treatment municipality, Pirkkala, or one of matched Finnish-speaking control municipalities (Kaarina, Masku, Tornio, Kemijärvi, Kemiö, Toivakka, Raisio, Nousiainen, Kuru, and Lieto) in the year 1970.

For the treatment group, Taloustutkimus identified individuals who lived in Pirkkala in 1970 and were in the fifth grade between 1973 and 1975. The Finnish Population Registry provided addresses for 158 such individuals, and all were invited to participate in the survey. For the control group, Taloustutkimus retrieved addresses of a random sample of individuals. In total, we received 1,707 responses from 7,265 individuals who received the invitation to participate, indicating a response rate of around 23%.³

Unfortunately, and unexpectedly, the population registry refused to share information on the municipality of residence in 1970 or birth year which were used as the basis of retrieving present-day addresses of the individuals invited to respond to the survey (and which would correspond to the information we use to assign the treatment status of each individual in our register-based analyses). We thus resort to using the birth year and municipality information that we collected in our survey to split the data into treatment and control groups. Following this classification, there are only 18 individuals in our group of individuals exposed to the experiment. Moreover, we only keep individuals who were born in municipalities that were in

²The pre-analysis plan is available at <https://osf.io/jufnk/>. Additional information on the survey such as the invitation letter and pre-registered hypotheses can be found in the pre-analysis plan.

³To improve response rates, we sent up to three reminder letters to non-respondents. The survey followed a “push-to-web” method: participants received an invitation letter by mail with a QR code linking to an online survey platform. Each invitation letter included a unique password allowing for single-use access. To encourage participation, we held a lottery for three gift cards worth 100 euros each.

the same wave of the comprehensive school reform as Pirkkala.⁴ In total, this leaves us with a sample of 817 respondents (11% from the group of individuals who received the invitation letter), 26 of whom were born in Pirkkala but were not part of the 5th-grade curriculum experiment. Given these severe sample size limitations, we stress that the results that follow are rather suggestive in nature.

Let us also note that the municipality of birth is a strong indicator of where a person went to school, which we also asked about in the questionnaire. For example, 43 out of 44 respondents born in Pirkkala went to school in the same municipality. Furthermore, our (register-based) main finding remains unchanged if we use the birth municipality to assign individuals to the treatment and control groups. We are thus not too concerned about this discrepancy in the way the treatment is assigned in our register-based and survey-based analyses.

E.2 Survey Questions

This section details the survey questions (and their answering options) that we analyze here. Some survey questions were repeated in a similar form in different parts of the survey to allow monitoring the accuracy of the responses.

A. The following questions are about working life.

1. How important are the following characteristics of a job/workplace to you? If you are not currently employed (no longer, not yet, or temporarily away), please answer based on your previous experiences and perceptions.

Very important — Important — Somewhat important — Not very important — Not at all important — Don't know

- The job is full-time
- Flexible coordination between work and free time is possible
- The work is meaningful/serves a good and important cause
- The working hours are regular, and no work needs to be taken home
- The job is permanent
- You can express yourself in the job

⁴Although Taloustutkimus targeted the survey to individuals who resided in one of the matched control municipalities, some individuals were born in non-matched control municipalities. We keep these individuals in our estimation sample to maximize the sample size.

- Good performance is encouraged and rewarded with money
- The job is socially respected/has high status
- The workplace is secure and stable
- The work is interesting
- The compensation/salary is high
- The workplace has a good atmosphere and a pleasant working environment
- The job offers good career advancement opportunities
- The working hours are flexible, and the job can be done remotely

2. What does paid work personally mean to you? If you are not currently employed (no longer, not yet, or temporarily away), please answer based on your previous experiences and perceptions.

To a great extent — To a large extent — To some extent — Very little — Not at all — Don't know

- Just a means of earning a living
- A duty that every member of society must fulfill (taxes, etc.)
- The great intrinsic value of work, work is a person's honor
- A necessary evil, a chore that must be endured
- Pleasant activity, something I would engage in even if I didn't need the money
- A privilege to be able to contribute to the common good

3. How important is it for you to succeed in your profession? If you are not currently employed (no longer, not yet, or temporarily away), please answer based on your previous experiences and perceptions.

Very important — Important — Somewhat important — Not very important — Not at all important — Don't know

4. What does paid work personally mean to you? If you are not currently employed (no longer, not yet, or temporarily away), please answer based on your previous experiences and perceptions.

Strongly agree — Somewhat agree — Don't know — Somewhat disagree — Strongly disagree

- I am satisfied with my job at my current employer.

- Work is the most important part of a person's life.
- I would accept any job if the alternative was unemployment.
- It is important to me that my work is socially respected.
- Paid work is just a means of earning a living.
- Paid work is a privilege to contribute to the common good.
- Paid work is a person's honor, with great intrinsic value.
- Paid work is a necessary evil.
- Paid work is an activity I would engage in even if I didn't need the money.

B. The following questions are about your time management and overall life satisfaction.

5. Is your time divided between the following life areas in the way you wish, or would you like to change it in some way?

I would like to spend my time...

More than currently — As much as currently — Less than currently — Don't know

- On paid work
- On household chores
- With family
- With friends and acquaintances
- On hobbies
- On charitable or societal activities

6. How satisfied are you with your current profession? If you are not currently employed (no longer, not yet, or temporarily away), please answer based on your previous experiences and perceptions.

Very satisfied — Fairly satisfied — Fairly dissatisfied — Very dissatisfied — Don't know

7. How satisfied are you with your overall career choice? If you are not currently employed (no longer, not yet, or temporarily away), please answer based on your previous experiences and perceptions.

**Very satisfied — Fairly satisfied — Fairly dissatisfied — Very dissatisfied
— Don't know**

8. How satisfied are you with your life overall at the moment?

**Very satisfied — Fairly satisfied — Fairly dissatisfied — Very dissatisfied
— Don't know**

9. What do you think about the following statements related to values?

**Strongly agree — Somewhat agree — Don't know — Somewhat disagree
— Strongly disagree**

- Hard work leads to a happier life.
- It is important to finish what you start.
- It is important to do as well as possible at work.
- You should demand a lot of yourself at work.
- If you start a task, you should do it as well as possible.

C. The following questions are about your societal values.

10. What do you think about the following statements regarding working life and Finnish society? If you are not currently employed (no longer, not yet, or temporarily away), please answer based on your previous experiences and perceptions.

**Strongly agree — Somewhat agree — Don't know — Somewhat disagree
— Strongly disagree**

- I would be willing to take a pay cut if my working hours were reduced, and I got more free time.
- Success at work largely depends on oneself and one's attitude.
- Being unemployed is not a bad thing as long as one's livelihood is secured.
- Nowadays, people don't need to commit to any interest group or organization.
- I would like to fully commit to a workplace or company and work hard for its success.
- I would accept any job if the alternative was unemployment.

11. What do you think about the following statements about society?

- Everyone in Finland has the opportunity to succeed financially.
Strongly agree — Somewhat agree — Don't know — Somewhat disagree — Strongly disagree
- A person cannot significantly influence their financial success in Finland.
Strongly agree — Somewhat agree — Don't know — Somewhat disagree — Strongly disagree
- What do you think has more influence on why a person is poor in Finland?
Own activity — Circumstances beyond their control — Don't know
- What do you think has more influence on why a person is rich in Finland?
The person has worked harder than others — The person has had better starting conditions than others — Don't know

12. What do you think about the following statements regarding income distribution in Finland?

Strongly agree — Somewhat agree — Don't know — Somewhat disagree — Strongly disagree

- High income taxes reduce my willingness to work.
- The state should do more to reduce income inequality.
- High-income earners should pay more taxes than they currently do.
- It is too easy to live off social security in Finland.
- Large income differences are acceptable to reward differences in talent and hard work.
- If necessary, it would be better to raise taxes than cut social benefits.
- If one income class becomes wealthier, it usually happens at the expense of other classes.
- Most of the wealth of the rich is created without taking from others.
- The continuation of people's well-being can only be based on economic growth.
- Maintaining the welfare state is not possible without strong economic growth.
- The market economy in Finland currently works well for the benefit of all citizens.
- If our country were more positive towards entrepreneurship, it would benefit the entire society.
- The state should pursue much more vigorous policies to secure employment in our country.

E.3 Aggregation of the Survey Questions

When natural, we aggregate the survey data into indices. Questions that are not used in any index are kept as independent items as they do not fit well within any of the categories.

Survey respondents were asked a number of questions related to preferences for work and work characteristics, life and work satisfaction, time use, and political preferences. In the main text, we analyze a set of 13 indices which capture specific dimensions of work-related attitudes, values, and economic beliefs. Each index aggregates binary-coded responses, reverse-coded where necessary, and calculates the mean of relevant questions to form a composite measure that varies between zero and one.

We can categorize our indices in five broader groups. The first set of indices is about job characteristics: *Security and stability*, assessing the importance of permanence, full-time nature, and stability of a job; *Flexibility*, measuring importance of being able to coordinate work with personal life and work remotely; *Compensation and career*, focusing on how much job characteristics such as salary, rewards for performance, and career opportunities matter for an individual; and *Workplace atmosphere*, evaluating to what extent an individual cares about the workplace environment and regularity of working hours. The higher these indices are, the more value an individual places on the job characteristics in question.

Second, we measure the value of work with two indices: *Intrinsic value of work* and *Extrinsic value of work*. The intrinsic value index measures the extent to which individuals emphasize the personal and societal significance of work, and the extrinsic value index captures the extent to which individuals frame work as a means to financial ends or societal obligations.

Third, we aim to capture the survey respondents' work ethic with two indices: *Effort and persistence*, capturing dedication and diligence, and *Commitment and attitude*, focusing on workplace loyalty and personal responsibility.

Fourth, we are also interested in *Career satisfaction*, which evaluates overall job and career contentment.

Fifth and finally, we construct four indices of economic attitudes. *Beliefs about poverty and wealth* assesses views on the causes of economic success and hardship, with higher values indicating a stronger belief that someone's economic circumstances are due to their own effort rather than external factors. *Redistributive preferences* evaluates support for taxation and inequality reduction. A higher score means that an individual is more in favor of redistribution. *Pro-market attitudes* reflects beliefs about the role of growth and welfare policies. The higher values this index obtains, the more pro-market economy and economic

growth an individual is. *Zero-sum thinking* assesses perceptions of wealth distribution. Higher values reflect more zero-sum thinking.

Construction of the indices Below in Table E.1, we list which survey items are included in each index. For items denoted by (*), we reverse-code the answers before aggregation. For each question, we create a binary variable indicating whether a survey respondent picked one of the top two responses, e.g., “strongly agree” or “somewhat agree” (or “somewhat disagree” or “strongly disagree” in the case of reverse coding). For question C11.3, we assign the indicator value 1 if the survey respondent picked “Own activity”, and for question C11.4, we assign the indicator value 1 if the survey respondent picked “The person has worked harder than others.”

After discretizing the survey responses, we aggregate them into indices as follows:

$$\text{Index}_i = \frac{\sum_{i=1}^n d_i}{n}, \quad (2)$$

where n is the number of selected questions in each category, and d_i is defined as detailed above.

Table E.1. Components of the indices.

Index	Questions/Survey items
Security and stability index	The job is full-time (A1.1); The job is permanent (A1.5); The workplace is secure and stable (A1.9)
Flexibility index	Flexible coordination between work and free time is possible (A1.2); The working hours are flexible, and the job can be done remotely (A1.14)
Compensation and career index	The compensation/salary is high (A1.11); Good performance is encouraged and rewarded with money (A1.7); The job offers good career advancement opportunities (A1.13)
Workplace atmosphere index	The workplace has a good atmosphere and a pleasant working environment (A1.12); The working hours are regular, and no work needs to be taken home (A1.4)

Index	Questions/Survey items
Intrinsic value of work index	The great intrinsic value of work; work is a person's honor (A2.3); Pleasant activity, something I would engage in even if I didn't need the money (A2.5); A privilege to be able to contribute to the common good (A2.6); Paid work is a privilege to contribute to the common good (A4.6); Paid work is a person's honor, with great intrinsic value (A4.7); Paid work is an activity I would engage in even if I didn't need the money (A4.9); The work is meaningful/serves a good and important cause (A1.3); You can express yourself in the job (A1.6); The work is interesting (A1.10)
Extrinsic value of work index	Just a means of earning a living (A2.1); A duty that every member of society must fulfill (A2.2); A necessary evil, a chore that must be endured (A2.4); I would accept any job if the alternative was unemployment (A4.3) (*); Paid work is just a means of earning a living (A4.5); Paid work is a necessary evil (A4.8); It is important to me that my work is socially respected (A4.4); The job is socially respected/has high status (A1.8); I would accept any job if the alternative was unemployment (C10.6) (*)
Effort and persistence index	How important is it for you to succeed in your profession? (A3); Hard work leads to a happier life (B9.1); It is important to finish what you start (B9.2); It is important to do as well as possible at work (B9.3); You should demand a lot of yourself at work (B9.4); If you start a task, you should do it as well as possible (B9.5)
Commitment and attitude index	Work is the most important part of a person's life (A4.2); Success at work largely depends on oneself and one's attitude (C10.2); Being unemployed is not a bad thing as long as one's livelihood is secured (C10.3) (*); I would like to fully commit to a workplace or company and work hard for its success (C10.5)

Index	Questions/Survey items
Career satisfaction index	I am satisfied with my job at my current employer (A4.1); How satisfied are you with your current profession? (B6); How satisfied are you with your overall career choice? (B7)
Beliefs about poverty and wealth index	What do you think has more influence on why a person is poor in Finland? (C11.3); What do you think has more influence on why a person is rich in Finland? (C11.4); Everyone in Finland has the opportunity to succeed financially (C11.1); A person cannot significantly influence their financial success in Finland (C11.2) (*)
Redistributive preferences index	High income taxes reduce my willingness to work (C12.1) (*); The state should do more to reduce income inequality (C12.2); High-income earners should pay more taxes than they currently do (C12.3); It is too easy to live off social security in Finland (C12.4) (*); Large income differences are acceptable to reward differences in talent and hard work (C12.5) (*); If necessary, it would be better to raise taxes than cut social benefits (C12.6)
Pro-market attitudes index	The continuation of people's well-being can only be based on economic growth (C12.9); Maintaining the welfare state is not possible without strong economic growth (C12.10); The market economy in Finland currently works well for the benefit of all citizens (C12.11); If our country were more positive towards entrepreneurship, it would benefit the entire society (C12.12); The state should pursue much more vigorous policies to secure employment in our country (C12.13) (*)
Zero-sum thinking index	If one income class becomes wealthier, it usually happens at the expense of others classes (C12.7); Most of the wealth of the rich is created without taking from others (C12.8) (*)

E.4 Analysis of the Survey Data

Due to sample size limitations, we deviate slightly from the specification used in the analysis of the administrative register data and instead estimate a simple difference-in-differences specification to relate adulthood attitudes with exposure to Marxist education as a child. The specification takes the following form, as specified also in our pre-analysis plan:

$$Y_{imc} = \beta \mathbf{1}[5th\ grader\ 1973 - 1975_c \times Pirkkala_m] + \mathbf{1}[Pirkkala_m] + \mathbf{1}[5th\ grader\ 1973 - 1975_c] + \varepsilon_{icm}. \quad (3)$$

Here, β is the coefficient of interest. It tells us the impact of the Marxist content on the exposed cohorts. $\mathbf{1}[5th\ grader\ 1973 - 1975_c \times Pirkkala_m]$ indicates whether the individual was in the fifth grade during the Pirkkala school experiment and was born in Pirkkala. $\mathbf{1}[Pirkkala_m]$ is an indicator for being born in Pirkkala, and $\mathbf{1}[5th\ grader\ 1973 - 1975_c]$ is an indicator for being in the exposed cohorts. ε_{icm} is the error term. We cluster our inference at the birth municipality $\times$ cohort level.

E.5 Results

Results for the indices We present the means of the indices, splitting the data by birth municipality and cohort (exposed versus non-exposed), in Figure E.1. These graphs to some extent illustrate the noisiness of our survey data—there is perhaps no reason to expect that the 1962–1963 and other cohorts in the control municipalities would be different from each other. Nevertheless, the graphs also point toward some potential effects of exposure to the experiment. These potential effects become more apparent in the main text Figure 9 which presents the difference-in-differences estimates for the indices.

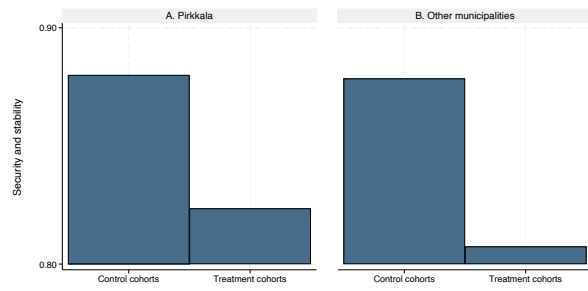
Let us first discuss some of the indices related to work.⁵ There is weak indication that exposure to the Pirkkala school experiment led to lower importance of workplace atmosphere ($p < 0.10$). The exposed group also appears to exhibit lower intrinsic valuation of work, although this estimate is statistically insignificant. While this may appear to be to some extent at odds with the content of the experiment, it could be explained by dissatisfaction with one’s career. Indeed, we see a negative and statistically significant effect on career satisfaction ($p < 0.01$).

⁵Our pre-analysis plan specified five primary hypotheses related to work attitudes: that exposed individuals would score (1) lower on compensation and career orientation, (2) lower or equal on effort and persistence, (3) lower or equal on commitment and attitude toward work, (4) higher on intrinsic value of work, and (5) lower on extrinsic value of work. We do not find strong evidence in favor of—or against—any of these hypotheses.

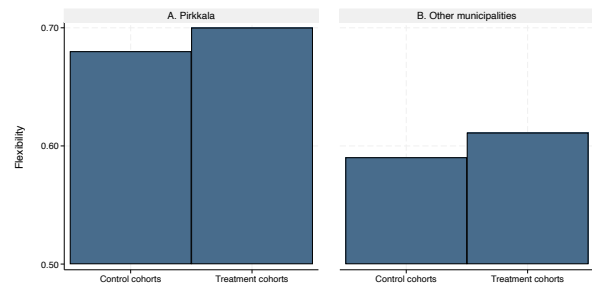
In terms of political preferences, there is some indication of exposure to the experiment leading to stronger preferences for redistribution, albeit this estimate is insignificant at all conventional levels of statistical significance. We also find a negative effect on pro-market attitudes and zero-sum thinking ($p < 0.10$). These results are in line with what we might have expected based on the content of the Pirkkala school experiment.⁶

In our pre-analysis plan, we specified that we would analyze the survey using a conventional difference-in-differences specification with indicators for being born in Pirkkala and being in the 5th grade in 1973–1975, and the interaction of these two. The results are robust to using the same two-way fixed effects specification that we use in the main analyses. However, we did not include this specification in our pre-analysis plan. In Figure E.2, we show the point estimates obtained using this specification.

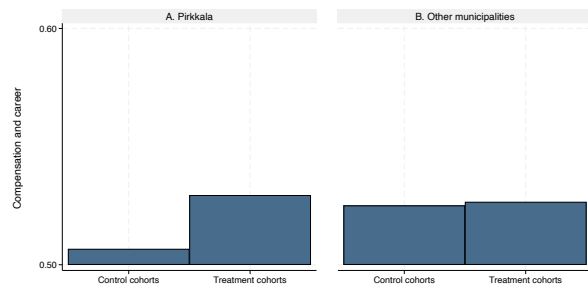
⁶We also specified three related secondary hypotheses in our pre-analysis plan: that exposed individuals would score (1) higher on redistributive preferences, (2) lower on pro-market views related to economic growth and welfare, and (3) higher on zero-sum thinking. We find weak support for these hypotheses.



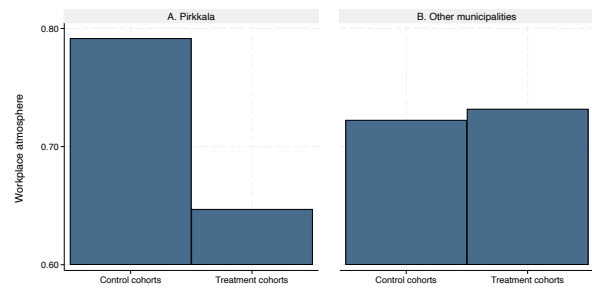
A. Security and stability.



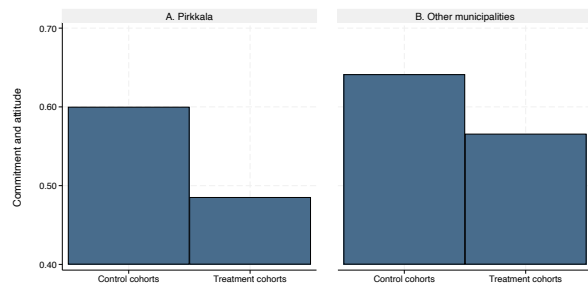
B. Flexibility.



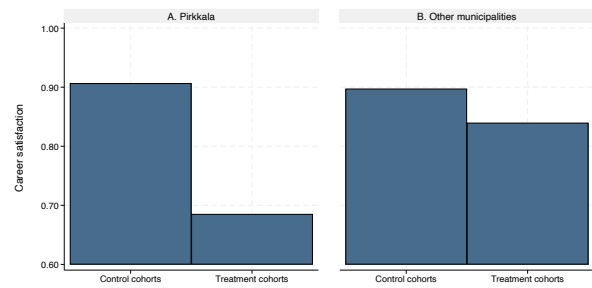
C. Compensation and career.



D. Workplace atmosphere.

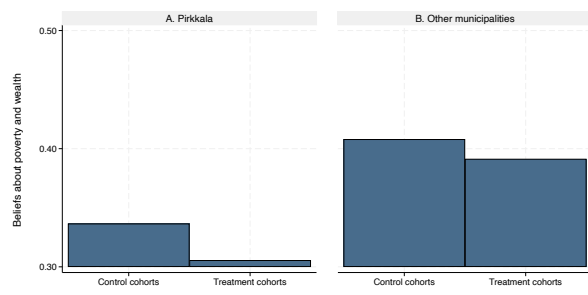


E. Commitment and attitude.

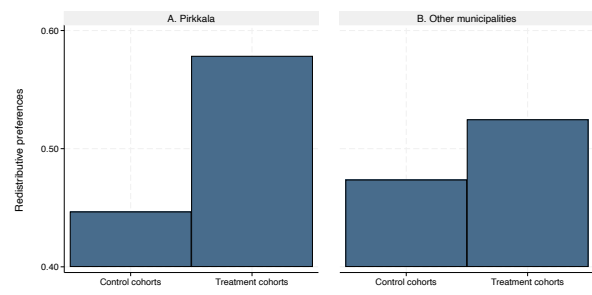


F. Career satisfaction.

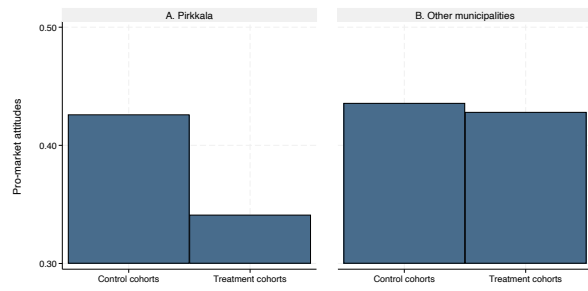
Figure E.1. Means of indices across groups.



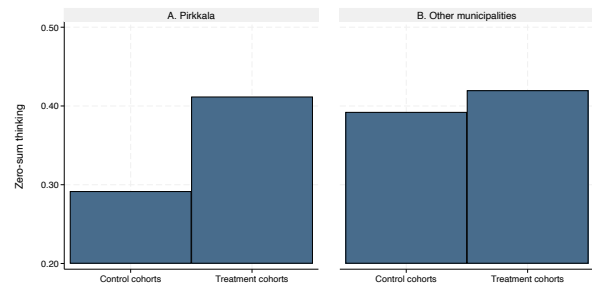
A. Beliefs about poverty and wealth.



B. Redistributive preferences.



C. Pro-market attitudes.



D. Zero-sum thinking.

Figure E.1. (continued) Means of indices across groups.

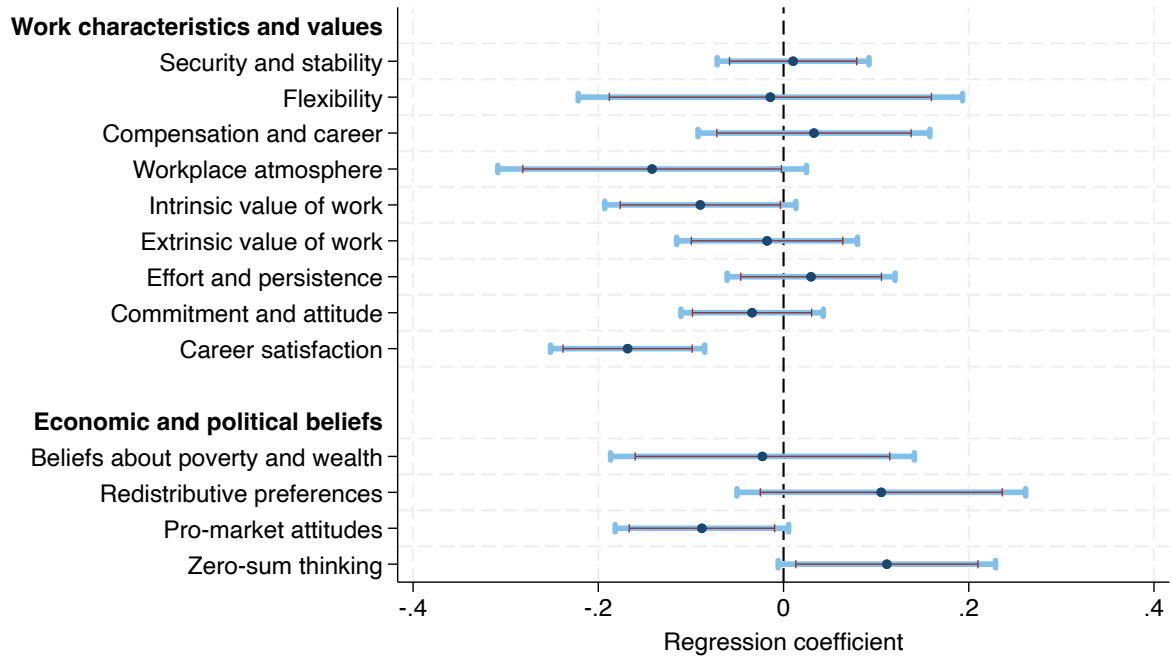


Figure E.2. Differences in survey responses.

Notes: The figure presents the estimation results using indices constructed based on our survey and the specification $Y_{imc} = \beta \mathbf{1}[5th\ grader\ 1973 - 1975_c \times Pirkkala_m] + \lambda_m + \lambda_c + \varepsilon_{icm}$. Higher index values indicate stronger endorsement of the underlying concept. The estimation sample consists of survey respondents born in Pirkkala or one of the control municipalities and belonging to the relevant cohorts. The figure also reports 95% and 90% confidence intervals (thick and thin lines, respectively) constructed using standard errors clustered at the birth municipality $\times$ cohort level.

Minimum Detectable Effects We present the minimum detectable effects for the indices in Table E.2. These minimum detectable effects are computed at the 95% confidence level with 80% power. As indicated by the table, we are underpowered to draw any strong conclusions with the survey data we have at hand, which is not unexpected given the small size of the survey sample especially in the treatment group.

Table E.2. Minimum detectable effects.

Variable	MDE
Security and stability	0.11
Flexibility	0.31
Compensation and career	0.20
Workplace atmosphere	0.25
Intrinsic value of work	0.18
Extrinsic value of work	0.13
Effort and persistence	0.17
Commitment and attitude	0.12
Career satisfaction	0.10
Beliefs about poverty and wealth	0.21
Redistributive preferences	0.19
Economic growth and welfare	0.11
Zero-sum thinking	0.15

Results for each survey item Tables E.3–E.11 report difference-in-differences estimates for all survey questions, following the order in which they were presented to respondents. For each item, we report the OLS estimates using the continuous response score as the dependent variable and further distinguish between “high” and “low” response categories—for example, agreement versus disagreement or satisfaction versus dissatisfaction. These estimates are not adjusted for multiple hypothesis testing, and many differences fall short of conventional levels of statistical significance. We therefore treat this analysis as exploratory and merely indicative rather than definitive.

The first set of questions, shown in Table E.3, relates to job and workplace preferences. We find that respondents exposed to the ideological intervention place greater importance on work being meaningful or serving a good and important cause. This aligns with our occupational choice results. They are also less likely to value regular working hours and a clear separation between work and home—echoing, to some extent, the finding on reduced labor supply. Other differences in job-related priorities—such as self-expression, job security, flexibility, or compensation—are small and not statistically significant.

Table E.4 presents results on how individuals interpret the broader meaning of work. Exposed individuals are less likely to agree that work is a civic duty or a source of personal honor. They are also less likely to describe work as a necessary evil and more likely to see it as something they would do even without financial need. There is some evidence that they are less inclined to view work as a way of contributing to the common good. This perhaps goes against what we might have expected as a consequence of Marxist indoctrination.

Table E.5 focuses on the importance respondents place on professional success. We do not find strong evidence of a difference between exposed and non-exposed individuals. If anything, the responses suggest a modest increase in the importance placed on professional achievement, but the difference is small and not statistically meaningful.

Table E.6 presents results on general attitudes toward work. Exposed individuals are less likely to agree that paid work has great intrinsic value or is a source of personal honor. They also express less agreement with the idea that paid work is something they would do even without needing the money. At the same time, they are more likely to say that work is the most important part of life. We also observe a small increase in job satisfaction, though this difference is relatively modest. Overall, these results suggest a complex pattern: while exposed individuals place slightly more centrality on work in life, they are less inclined to see it as inherently meaningful or honorable in itself.

Table E.7 reports preferences regarding how individuals would like to allocate their time relative to their current situation. We find that exposed individuals express a stronger preference for spending more time with friends and acquaintances. Other differences in time-use preferences—including time spent on paid work, family, hobbies, or household chores—are small and not statistically significant. The overall pattern suggests a modest shift in social orientation, with somewhat greater value placed on informal social interaction.

Table E.8 presents results on satisfaction-related outcomes. Exposed individuals report significantly lower satisfaction with both their current profession and their overall career choice. In contrast, there is no meaningful difference in overall life satisfaction. These findings point to a gap between how individuals feel about their work specifically and their broader life evaluations.

Table E.9 reports on broader values related to work. Exposed individuals are more likely to agree that one should do tasks as well as possible, indicating stronger endorsement of conscientiousness. At the same time, they are less likely to say they would accept any job merely to avoid unemployment and are less willing to trade salary for additional free time.

These responses suggest a more selective approach to work, combining personal standards of effort with a lower tolerance for undesirable or poorly compensated work.⁷

Table E.10 examines beliefs about economic mobility and the causes of inequality. We do not find statistically significant differences between exposed and non-exposed individuals in their views on whether financial success is influenced by individual effort or structural factors. Likewise, responses to attribution-style questions about poverty and wealth—such as whether outcomes reflect personal activity or circumstances—show no clear or consistent pattern.

Table E.11 presents results on attitudes toward redistribution and the market economy. Exposed individuals are less likely to agree that social security is too generous and express more critical views of income inequality, being more inclined to see large income differences as unjustified and to view wealth accumulation as a zero-sum process. At the same time, they show lower support for state intervention in employment policy and are somewhat less likely to view the market economy as functioning well for everyone. In a sense, these findings suggest skepticism toward both market-based and state-led approaches to economic policy.

⁷In our pre-analysis plan, we specified a fourth secondary hypothesis that the individuals exposed to the school experiment would have been less in agreement with the claim that “Nowadays, people don’t need to commit to any interest group or organization.” However, we do not find any evidence in favor or against this secondary hypothesis.

Table E.3. Difference-in-differences estimates for job/workplace characteristics.

Statement/Question	Score			High		Low	
	Mean	Estimate	<i>p</i> -value	Estimate	<i>p</i> -value	Estimate	<i>p</i> -value
The job is full-time	3.40	0.03	0.81	-0.08	0.08	0.00	0.98
Flexible coordination between work and free time is possible	3.05	0.14	0.62	0.00	1.00	-0.02	0.33
The work is meaningful/serves a good and important cause	3.21	0.33	0.05	0.18	0.05	-0.06	0.28
The working hours are regular, and no work needs to be taken home	2.58	-0.51	0.06	-0.22	0.06	0.16	0.13
The job is permanent	3.49	0.04	0.82	0.06	0.26	-0.05	0.21
You can express yourself in the job	2.98	0.09	0.63	0.01	0.88	-0.08	0.13
Good performance is encouraged and rewarded with money	2.85	0.12	0.56	0.16	0.17	-0.08	0.36
The job is socially respected/has high status	2.18	0.31	0.10	0.18	0.07	0.03	0.75
The workplace is secure and stable	3.23	0.20	0.50	0.06	0.39	-0.06	0.35
The work is interesting	3.34	-0.17	0.20	-0.04	0.55	0.05	0.30
The compensation/salary is high	2.53	-0.08	0.71	-0.07	0.44	-0.05	0.58
The workplace has a good atmosphere and a pleasant working environment	3.40	-0.14	0.64	-0.09	0.27	-0.02	0.04
The job offers good career advancement opportunities	2.11	-0.11	0.65	-0.03	0.78	0.05	0.74
The working hours are flexible, and the job can be done remotely	2.25	-0.23	0.36	-0.08	0.50	0.01	0.93

Notes: The table reports difference-in-differences estimates of the effect of exposure to the intervention on the perceived importance of a job or workplace characteristic. A higher score reflects a greater importance of each job characteristic. “High” refers to responding “very important” or “important”; “Low” refers to “not very important” or “not at all important.” *p*-values are constructed using standard errors clustered at the municipality $\times$ cohort level.

Table E.4. Difference-in-differences estimates for the meaning of work.

Statement/Question	Score			High		Low	
	Mean	Estimate	<i>p</i> -value	Estimate	<i>p</i> -value	Estimate	<i>p</i> -value
Just a means of earning a living	2.63	0.03	0.93	-0.05	0.63	-0.08	0.27
A duty that every member of society must fulfill (taxes, etc.)	2.82	-0.55	0.01	-0.14	0.36	0.19	0.00
The great intrinsic value of work, work is a person’s honor	2.48	-0.50	0.00	-0.25	0.01	0.09	0.09
A necessary evil, a chore that must be endured	0.95	0.48	0.05	0.08	0.23	-0.26	0.01
Pleasant activity, something I would engage in even if I didn’t need the money	1.66	-0.16	0.47	-0.02	0.81	0.23	0.00
A privilege to be able to contribute to the common good	2.42	0.47	0.13	0.11	0.48	-0.22	0.01

Notes: The table reports difference-in-differences estimates of the effect of exposure to the intervention on the meaning of work. A higher score reflects a greater agreement with each meaning of work. “High” refers to responding “to a great extent” or “to a large extent”; “Low” refers to “very little” or “not at all.” *p*-values are constructed using standard errors clustered at the municipality $\times$ cohort level.

Table E.5. Difference-in-differences estimates for success in one's profession.

Statement/Question	Score			High		Low	
	Mean	Estimate	<i>p</i> -value	Estimate	<i>p</i> -value	Estimate	<i>p</i> -value
How important is it for you to succeed in your profession?	2.96	0.17	0.49	0.03	0.80	-0.02	0.32

Notes: The table reports difference-in-differences estimates of the effect of exposure to the intervention on the importance of performing well in one's profession. A higher score reflects a higher importance of faring well in one's profession. "High" refers to responding "very important" or "important"; "Low" refers to "not very important" or "not at all important." *p*-values are constructed using standard errors clustered at the municipality $\times$ cohort level.

Table E.6. Difference-in-differences estimates for further work-related attitudes and values.

Statement/Question	Score			High		Low	
	Mean	Estimate	<i>p</i> -value	Estimate	<i>p</i> -value	Estimate	<i>p</i> -value
I am satisfied with my job at my current employer.	3.12	-0.23	0.15	-0.09	0.20	0.11	0.08
Work is the most important part of a person's life.	2.51	-0.39	0.18	-0.18	0.23	0.23	0.02
I would accept any job if the alternative was unemployment.	2.18	-0.27	0.29	-0.14	0.18	0.12	0.20
It is important to me that my work is socially respected.	2.45	0.21	0.32	0.22	0.03	0.01	0.91
Paid work is just a means of earning a living.	1.88	-0.04	0.90	-0.08	0.48	0.00	1.00
Paid work is a privilege to contribute to the common good.	2.75	-0.14	0.41	-0.08	0.32	0.11	0.02
Paid work is a person's honor, with great intrinsic value.	2.46	-0.55	0.00	-0.17	0.13	0.18	0.02
Paid work is a necessary evil.	1.16	0.16	0.63	0.08	0.55	-0.10	0.42
Paid work is an activity I would engage in even if I didn't need the money.	1.73	-0.88	0.00	-0.30	0.02	0.45	0.00

Notes: The table reports difference-in-differences estimates of the effect of exposure to the intervention on work-related attitudes and values. A higher score reflects a stronger agreement with each claim. "High" refers to responding "strongly agree" or "agree"; "Low" refers to "somewhat disagree" or "strongly disagree." *p*-values are constructed using standard errors clustered at the municipality $\times$ cohort level.

Table E.7. Difference-in-differences estimates for time use relative to the current situation.

Statement/Question	Score			High		Low	
	Mean	Estimate	<i>p</i> -value	Estimate	<i>p</i> -value	Estimate	<i>p</i> -value
On paid work	2.54	-0.24	0.06	-0.17	0.16	0.03	0.57
On household chores	3.05	0.02	0.96	-0.09	0.24	0.05	0.19
With family	3.28	0.27	0.11	-0.07	0.11	0.06	0.15
With friends and acquaintances	3.36	0.28	0.06	0.04	0.07	-0.01	0.47
On hobbies	3.39	0.26	0.11	0.02	0.26	-0.01	0.60
On charitable or societal activities	2.72	-0.28	0.38	0.02	0.93	0.20	0.23

Notes: The table reports difference-in-differences estimates of the effect of exposure to the intervention on preferences for time use relative to the current situation. A higher score suggests that a person would like to spend more time on a particular activity than they currently do. "High" refers to responding "more than currently"; "Low" refers to "less than currently." *p*-values are constructed using standard errors clustered at the municipality $\times$ cohort level.

Table E.8. Difference-in-differences estimates for career, job, and life satisfaction.

Statement/Question	Score			High		Low	
	Mean	Estimate	<i>p</i> -value	Estimate	<i>p</i> -value	Estimate	<i>p</i> -value
How satisfied are you with your current profession?	4.27	-0.52	0.03	-0.23	0.00	0.20	0.00
How satisfied are you with your overall career choice?	4.12	-0.25	0.23	-0.19	0.00	0.16	0.00
How satisfied are you with your life overall at the moment?	4.23	0.01	0.96	0.01	0.89	0.04	0.61

Notes: The table reports difference-in-differences estimates of the effect of exposure to the intervention on different satisfaction outcomes. A higher score reflects a greater satisfaction. “High” refers to responding “very satisfied”; “Low” refers to “very dissatisfied.” *p*-values are constructed using standard errors clustered at the municipality $\times$ cohort level.

Table E.9. Difference-in-differences estimates for work-related values.

Statement/Question	Score			High		Low	
	Mean	Estimate	<i>p</i> -value	Estimate	<i>p</i> -value	Estimate	<i>p</i> -value
Hard work leads to a happier life.	1.55	-0.22	0.38	-0.13	0.06	0.12	0.44
It is important to finish what you start.	3.39	-0.10	0.51	0.05	0.19	-0.00	0.74
It is important to do as well as possible at work.	3.13	0.07	0.76	0.05	0.52	-0.09	0.05
You should demand a lot of yourself at work.	2.63	-0.03	0.94	-0.11	0.43	0.07	0.48
If you start a task, you should do it as well as possible.	3.02	0.31	0.26	0.22	0.04	-0.05	0.52
I would be willing to take a pay cut if my working hours were reduced, and I got more free time.	2.13	-0.31	0.22	0.02	0.89	0.21	0.04
Success at work largely depends on oneself and one’s attitude.	2.82	-0.01	0.97	-0.00	0.99	-0.16	0.15
Being unemployed is not a bad thing as long as one’s livelihood is secured.	1.93	0.18	0.59	0.17	0.22	0.10	0.46
Nowadays, people don’t need to commit to any interest group or organization.	2.40	-0.08	0.74	0.02	0.81	0.04	0.73
I would like to fully commit to a workplace or company and work hard for its success.	2.56	-0.05	0.79	-0.07	0.58	-0.01	0.87
I would accept any job if the alternative was unemployment.	2.04	-0.59	0.06	-0.22	0.10	0.21	0.06

Notes: The table reports difference-in-differences estimates of the effect of exposure to the intervention on work-related values. “High” refers to responding “strongly agree” or “agree”; “Low” refers to “somewhat disagree” or “strongly disagree.” *p*-values are constructed using standard errors clustered at the municipality $\times$ cohort level.

Table E.10. Difference-in-differences estimates for statements regarding economic opportunities in Finland.

Statement/Question	Score			High		Low	
	Mean	Estimate	<i>p</i> -value	Estimate	<i>p</i> -value	Estimate	<i>p</i> -value
Everyone in Finland has the opportunity to succeed financially.	1.82	-0.26	0.34	-0.08	0.53	0.01	0.93
A person cannot significantly influence their financial success in Finland.	1.44	0.17	0.42	0.09	0.47	-0.06	0.53
What do you think has more influence on why a person is poor in Finland? Own activity	0.32	0.00	0.97				
What do you think has more influence on why a person is poor in Finland? Circumstances beyond their control	0.58	-0.10	0.58				
What do you think has more influence on why a person is rich in Finland? The person has worked harder than others	0.18	0.07	0.41				
What do you think has more influence on why a person is rich in Finland? The person has had better starting conditions than others	0.71	-0.05	0.60				

Notes: The table reports difference-in-differences estimates of the effect of exposure to the intervention on attitudes regarding economic opportunities in Finland. For the first two claims, a higher score reflects a stronger agreement with the claim. For the questions, the outcomes are defined as binary indicators of choosing a particular response. “High” refers to responding “very important” or “important”; “Low” refers to “not very important” or “not at all important.” *p*-values are constructed using standard errors clustered at the municipality $\times$ cohort level.

Table E.11. Difference-in-differences estimates for preferences for redistribution and pro-market attitudes.

Statement/Question	Score			High		Low	
	Mean	Estimate	<i>p</i> -value	Estimate	<i>p</i> -value	Estimate	<i>p</i> -value
High income taxes reduce my willingness to work.	1.95	-0.18	0.73	0.07	0.69	0.13	0.49
The state should do more to reduce income inequality.	2.54	0.37	0.11	0.28	0.03	0.03	0.78
High-income earners should pay more taxes than they currently do.	2.45	-0.17	0.48	-0.06	0.51	0.14	0.10
It is too easy to live off social security in Finland.	2.44	-0.49	0.00	-0.19	0.03	0.16	0.05
Large income differences are acceptable to reward differences in talent and hard work.	2.00	-0.53	0.03	-0.22	0.05	0.19	0.12
If necessary, it would be better to raise taxes than cut social benefits.	2.38	-0.26	0.34	-0.19	0.10	0.06	0.55
If one income class becomes wealthier, it usually happens at the expense of other classes.	2.07	0.44	0.00	0.12	0.31	-0.08	0.56
Most of the wealth of the rich is created without taking from others.	1.98	0.09	0.79	0.10	0.62	0.08	0.57
The continuation of people's well-being can only be based on economic growth.	2.13	0.02	0.91	-0.12	0.39	-0.11	0.25
Maintaining the welfare state is not possible without strong economic growth.	2.65	-0.14	0.67	-0.11	0.49	0.11	0.17
The market economy in Finland currently works well for the benefit of all citizens.	1.73	-0.37	0.09	-0.14	0.11	0.21	0.14
If our country were more positive towards entrepreneurship, it would benefit the entire society.	2.90	-0.11	0.64	-0.05	0.67	0.04	0.74
The state should pursue much more vigorous policies to secure employment in our country.	3.20	-0.31	0.05	-0.17	0.09	0.04	0.21

Notes: The table reports difference-in-differences estimates of the effect of exposure to the intervention on the preferences for redistribution and market-oriented attitudes. A higher score reflects a stronger agreement with each claim. "High" refers to responding "strongly agree" or "agree"; "Low" refers to "strongly disagree" or "disagree." *p*-values are constructed using standard errors clustered at the municipality $\times$ cohort level.

References

- Abadie, Alberto. 2021. “Using Synthetic Controls: Feasibility, Data Requirements, and Methodological Aspects.” *Journal of Economic Literature* 59 (June): 391–425.
- Arkhangelsky, Dmitry, Susan Athey, David A. Hirshberg, Guido W. Imbens, and Stefan Wager. 2021. “Synthetic Difference-in-Differences.” *American Economic Review* 111 (December): 4088–4118.
- Burst, Tobias, Pola Lehmann, Simon Franzmann, Denise Al-Gaddooa, Christoph Ivanusch, Sven Regal, Felicia Riethmüller, Bernhard Weßels, and Lisa Zehnter. 2023. “manifestoberta. Version 56topics.sentence.2023.1.1.” Wissenschaftszentrum Berlin für Sozialforschung/Göttinger Institut für Demokratieforschung. Available online at <https://doi.org/10.25522/manifesto.manifestoberta.56topics.sentence.2023.1.1>.
- Izadi, Ramin, and Joonas Tuhkuri. 2024. “Evolving Returns to Personality.” *Journal of Labor Economics*: forthcoming.
- Jokela, Markus, Tuomas Pekkarinen, Matti Sarvimäki, Marko Terviö, and Roope Uusitalo. 2017. “Secular rise in economically valuable personality traits.” *Proceedings of the National Academy of Sciences* 114 (25): 6527–6532.
- Kerr, Sari Pekkala, Tuomas Pekkarinen, and Roope Uusitalo. 2013. “School Tracking and Development of Cognitive Skills.” *Journal of Labor Economics* 31 (3): 577–602.
- Lehtonen, Kai R., and Veikko Huttunen. 1971. *Peruskoulun historia 1*. Porvoo: WSOY.
- Nyman, Kai. 2007. “Varusmiesten johtajavalintojen luotettavuus.” Maanpuolustuskorkeakoulun käyttäytymistieteiden laitoksen julkaisusarja nro 1/2007. Available online at <https://www.doria.fi/handle/10024/104690>.
- Opetussuunnitelmakomitea. 1970. *Komiteamietintö II 1970*. Helsinki: Valtion painatuskeskus.
- Pekkarinen, Tuomas, Roope Uusitalo, and Sari Kerr. 2009. “School tracking and intergenerational income mobility: Evidence from the Finnish comprehensive school reform.” *Journal of Public Economics* 93 (7-8): 965–973.
- Penttilä, Antti. 1974. *Historia: 5. luokka; ihmiskunnan kehityksen yleispiirteet vanhimmista ajoista nykypäiviin saakka*. Tampereen yliopiston psykologian laitos.