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Lasting scars: The long-term effects of school closures on earnings

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Almost all countries closed schools and universities during the Covid-19 pandemic, affecting more than 90 per cent of the world's learners. A growing literature aims to estimate the effects of these school closures on learning outcomes, future earnings and GDP. We contribute to this literature by estimating the impact of school closures on earnings in the long term using a natural experiment. In particular, we estimate the effects of school closures due to the 1999 NATO bombing of Serbia on earnings 20 years later. Our results point to substantial and lasting effects: those in first grade at the time of the shock earn about 6-7 per cent less 20 years after the shock than unaffected cohorts just younger than them. Impacts are larger for those in first grade than those in higher grades and for those in the bottom half of the income distribution. Impacts persist despite affected cohorts staying in school longer, being more likely to work for the public sector and having open-ended contracts.

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1. Introduction

Almost all countries closed schools and universities during the Covid-19 pandemic, affecting more than 90 per cent of the world's learners, over 1.5 billion children and young people (UNESCO). On average, students missed 49 days of school in 2020 alone (IMF 2021).

A growing literature aims to estimate the effects of these school closures on learning outcomes, future earnings and GDP (e.g. Adams 2020, ADB 2021, Andresen, Bensnes and Lokken 2020, Azevedo et al. 2020, Dorn et al. 2020, Hanushek and Woessmann 2020, Kaffenberger 2021, Kuhfeld et al. 2020, The DELVE Initiative 2020, Woessmann 2020). These estimates are typically based on extrapolations from returns to schooling or estimates from missed education due to teacher absenteeism or natural disasters. Most of these have focused on relatively short-term effects.

We contribute to this literature by estimating the impact of school closures on earnings in the long term using a natural experiment. In particular, we estimate the effects of school closures due to the 1999 NATO bombing of Serbia on earnings 20 years later. These school closures were the result of an exogenous shock, their timing (2.5 months) was comparable to school closures during the pandemic in many economies, and recent labour force surveys allow us to look at effects on earnings 20 years later.

Our identification comes from birth dates—comparing the earnings of the cohort that was just old enough to be in the first grade of primary school at the time of the school closures with those born some months later, who were not in first grade yet. While the literature on absenteeism deals with potentially endogenous choices of teachers, and studies on school closures due to natural disasters typically only examine short-term effects, we hope to contribute to these literatures by looking at the long-run impact of a plausibly exogenous shock.

Our results point to significant lasting effects: those in first grade at the time of the shock earn about 6-7 per cent less than unaffected cohorts just younger than them, even 20 years later. Impacts are larger for those in first grade than those in higher grades and those in the bottom half of the income distribution. Selection into lower-paying occupations and sectors explains about a fifth of the overall effect. However, the negative effect of education disruption persists despite affected cohorts staying in school longer, being more likely to work for the public sector and having open-ended contracts.

The paper is structured as follows: section 2 provides an overview of related literatures, including on the effects of Covid-19 related school closures and on the returns to education. Section 3 discusses the identification strategy and section 4 provides an overview of the labour force survey used. Section 5 presents the results, section 6 summarizes robustness tests and section 7 concludes with implications for the potential impact of Covid-19 related school closures.

2. Literature

Our paper relates to several literatures, including recent work on the effects of school disruptions during Covid, on returns to education and the effects of time in school on learning outcomes, and a handful of papers on the long-term effects of education on earnings.

There is a rapidly growing literature on the impact of Covid-19 related school closures on academic achievement and earnings. Hammerstein et al. (2021) provided an overview highlighting significant negative effects of school closures on student achievement. The magnitude of such losses is substantial: a learning loss of up to 38 points on the Programme for International Student Assessment (PISA) scale, corresponding to up to 0.9 school years.

A number of papers have also estimated potential impacts on earnings. Azevedo et al. (2020) simulated the impact of 3, 5 and 7 months of school closures and different levels of mitigation effectiveness and found that students from the current cohort could face a 2-8 per cent annual expected earning loss.

Similarly, Fuchs-Schundeln (2022) estimated that school closures in the US until the summer of 2021 could lead to a reduction of lifetime earnings of 1.7 per cent for affected children.

Many of these projections draw on estimates from the literature on rates of return to schooling. Psacharopoulos and Patrinos (2018) provide a review of this literature and point to an average rate of return of 9.5 per cent based on the most recent estimate for each country, with higher returns to primary education, general curricula and in poorer economies. Popov (2013) estimates a 10 per cent rate of return for Serbia.

Most studies also point to widening socioeconomic gaps, with younger and lower performing students and those from poorer families or parents with lower levels of education being more affected (Clark et al. 2021; Engzell et al 2021; Hammerstein et al. 2021; Maldonado and De Witte 2022; Meinck et al. 2022; Tomasik et al. 2020). Haeck and Lefebvre (2020) estimate that socioeconomic achievement gaps could increase by up to 30 per cent. In particular, larger effects in younger children can result in additive learning losses in subsequent school years (Grewenig et al. 2021; Haeck and Lefebvre 2020; Kaffenberger 2021; Pensiero et al. 2020). Learning disruptions have also been more severe in poorer economies (World Bank 2022).

Some studies point to additional long-term effects through changes in curricula (Meinck et al. 2022), lower demand for vocational programs relative to academic programs, in particular service-oriented programs (Aalto et al. 2022) and young people remaining in education longer (The Economist 2022).

We hope to inform this literature by focusing on a historical exogenous event and exploiting rich individual-level data two decades later. This allows us to look at the long run effects of the shock. We also distinguish effects on those in the first year(s) of primary school from those in higher grades. Effects for young children are likely to be larger, as learning losses add up in subsequent school years; however, so far relatively few studies have focused on them.

An earlier literature examines the effects of time in school on learning outcomes based on cross-sectional differences in learning time (Carlsson et al. 2015; Lavy 2015), variation in learning time due to teacher absenteeism (Aucejo and Romano 2016; Gershenson et al. 2017; Goodman 2014; Liu et al. 2020) or natural disasters (Andrabi, Daniels and Das 2020; Di Pietro 2016; Gibbs et al. 2019; Goodman 2014; Sacerdote 2012; Wilkinson et al. 2013). While cross-sectional differences could be picking up other factors, absenteeism could be affected by self-selection and the studies looking at natural disasters typically focus on short-run effects. We hope to contribute to these literatures by looking at the long-term effect of an exogenous shock.

Our paper is most closely related to a handful of papers examining the long-term effects of education on earnings. Ichino and Winter-Ebmer (2004) found that Austrian and German individuals who were 10 years old during the Second World War received less education than comparable individuals from countries not at war, such as Switzerland and Sweden, and experienced a sizable earnings loss even 40 years after the war. Meng and Gregory (2002) documented that children whose education was interrupted by the Chinese Cultural Revolution had lower educational attainment, with the largest negative impacts for children whose parents had lower educational achievement and lower occupational status. Meng and Zhao (2021) even find intergenerational effects, showing that children of parents, whose education was disrupted, also left school earlier. We contribute to this literature by focusing on the long-term effects of an education disruption that is comparable in length to school closures during the Covid-19 pandemic in many economies. We are also able to focus on a narrow group of affected and unaffected students, who are similar in terms of observable characteristics and differ only by birth date. Furthermore, detailed individual-level data allows us to examine heterogeneity across different groups.

Our identification strategy, discussed in detail in the following section, is most closely related to that used by Angrist and Krueger (1991) in their seminal paper examining how compulsory school attendance affects schooling and earnings, exploiting variation in the season of birth.

3. Identification

We examine the long-run effects of school closures during the 1999 NATO aerial bombing campaign of the Federal Republic of Yugoslavia during the Kosovo War on earnings 20 years later. The air strikes lasted from the 24th of March 1999 to the 10th of June 1999. The bombing resulted in significant disruptions to everyday life and substantial infrastructure damage. The government closed schools at the start of the bombing, with schools remaining closed until the end of the school year. These school closures were uniform across the country, exogenous, and in timing similar to the length of Covid-19 related disruptions in many economies.

A rich labour force survey, discussed in the following section, allows us to examine effects on earnings in 2018 and 2019, and to look at heterogeneity across individuals.

Our identification strategy is based on school enrolment rules based on birth dates. Children born in the same calendar year start school in the same academic year. In particular, everyone born in 1991 would start school in September 1998, hence would be in first grade at the time of the bombings. In contrast, those born in 1992 would only start school in September 1999, hence would be unaffected by school closures, but would still experience other effects of the bombing, including its effects on parents' employment and income, damage to infrastructure and psychological impact. Because one's birthday is unlikely to be correlated with personal attributes other than age at school entry, birth dates generate exogenous variation in being in education that can be used to estimate the impact of school closures on earnings (see also Lam and Miron 1987). There is substantial variation in education choices and labour market entry dates even among those born in the same calendar year.

Our identification strategy is closely related to that used by Angrist and Krueger (1991), who compare students born across different quarters of a year. Unfortunately, small sample sizes prevent us from exploiting within-year variation. Our baseline specifications thus compare those born across different calendar years, explicitly controlling for age. Comparing those born in the first half of 1991 (affected by the school closures) with those in the second half of 1992 (unaffected by the school closures) points to a similar negative effect of education disruption as our baseline estimates, though given small sample sizes this is not statistically significant.[1]

Our baseline specification compares those who were in the first year of primary school at the time of the bombing-related school closures (the cohort born in 1991) with those who are a year younger (born in 1992) and hence would not be in school yet at the time of the bombing ($affected$ is a dummy variable). The dependent variable throughout is log income in dinars two decades later. Specifications control for a range of individual characteristics $\boldsymbol{X}$ (age, gender, urban/rural location).

$$\ln(income) = \beta_0 + \boldsymbol{\beta}_1 \boldsymbol{X} + \beta_2 affected + \varepsilon$$

In order to examine robustness to larger sample sizes, we also use alternative control groups: comparing those who were in the first year of primary school at the time of the school closures to those born 1-2, 1-3 and 1-4 years later than the cohort in first grade at the time of the school closures. Furthermore, we examine whether effects differ by age—comparing those who were in the first four grades of primary

[1] If anything, the effects of compulsory education laws documented by Angrist and Krueger (1991) would bias our specifications against finding an effect of the education disruption as those born later in the year (affected in our specifications) tend to have more education and higher earnings than those born earlier in the year (unaffected in our specification).

school with those in the last four years, as well as all those in primary school at the time of the school closures. While they are still compared to cohorts that are too young to be in school at the time of the bombing, we control for age as well as the year in which they completed their education to pick up any potential time trends.

Finally, we turn to mediation analysis to examine which variables may be channelling some of the impact of the shock. We look at the effects of highest level of completed education, choice of sector and occupation, years worked, public or private sector of employment and type of contract.

Throughout, in order to mitigate the effects of potential education or work experience abroad, we restrict the sample to those born in Serbia who did not leave the country for more than one year.

4. Data

Our analysis is based on the Serbian Labour Force Surveys for 2018 and 2019. The surveys are primarily aimed at tracking economic and social trends in Serbia and obtaining estimates of the number of employed, unemployed and economically inactive persons. However, the surveys also collect a range of demographic and socio-economic indicators. The sampling frame is based on the 2011 census using a two-stage stratified panel sample (enumeration areas are stratified into urban and rural settlements and by administrative districts). The annual sample size is approximately 68,000 households.

Our baseline specification focuses on a pooled sample of the 2018 and 2019 surveys to increase sample sizes. Earlier waves are not used as many in our cohort of interest were still in education then; later surveys would be affected by the Covid-19 crisis.

5. Results

This section examines the effects of education disruption on earnings, providing descriptive statistics (Section 5.1), documenting average effects (Section 5.2), turning then to heterogeneity across different cohorts and groups (Section 5.3) and finally drawing on mediation analysis to disentangle the effects of education disruptions into impacts channelled through differences in highest levels of completed education, sectoral and occupation choices, and direct effects (Section 5.4).

5.1 Descriptive statistics

Table 1 summarizes key characteristics for those who were in the first year of education in spring 1999 and hence affected by school closures, and the cohort just below them (who would only start school in September 1999 and are hence unaffected by the school closures). The older, affected cohort is statistically significantly more likely to have a higher level of completed education. While the affected cohort are up to a year older than the unaffected cohort, they have less than one year more of work experience, consistent with staying in school for longer. The affected cohort are also more likely to be on open-ended (as opposed to fixed term) contracts. (Patterns for larger control groups, those born 1-2, 1-3 and 1-4 years after the affected cohort are similar and shown in Annex Table 1).

Table 1. Descriptive statistics

	In first year of primary school			One year younger			In primary school		
	Obs	*Mean*	*Std. Dev.*	*Obs*	*Mean*	*Std. Dev.*	*Obs*	*Mean*	*Std. Dev.*
Income	509	35385	12415	496	37002	18879	4446	36254	21583
Age	2,220	26.9	0.71	2,224	25.9*	0.70	17,783	30.6	2.45
Male	2,220	0.52	0.50	2,224	0.54	0.50	17,783	0.53	0.50
Lower secondary educ.	2,220	0.19	0.39	2,224	0.20	0.40	17,783	0.23	0.42
Upper secondary educ.	2,220	0.39	0.49	2,224	0.43*	0.50	17,783	0.33	0.47
Tertiary, non-university educ.	2,220	0.04	0.20	2,224	0.03	0.17	17,783	0.05	0.22
University educ.	2,220	0.30	0.46	2,224	0.26*	0.44	17,783	0.27	0.44
Urban	2,220	0.56	0.50	2,224	0.54	0.50	17,783	0.56	0.50
Years worked	1,741	4.14	3.12	1,689	3.61*	2.95	15,560	6.86	4.49
Public sector employment	1,324	0.18	0.39	1,241	0.17	0.38	11,810	0.23	0.42
Open-ended contract	1,154	0.63	0.48	1,077	0.59*	0.49	10,176	0.70	0.46

*Notes: * denotes differences that are statistically significantly different at the 5 per cent level across the two groups. Public sector employment refers to state as opposed to private registered enterprises. Open-ended contract refers to jobs of an unlimited duration as opposed to jobs for a limited duration. Throughout the paper, analysis is restricted to those born in Serbia, who did not leave Serbia for more than a year. While there is significant non-response on exact incomes even among those in paid employment, with incomes more likely to be missing for those with primary education and those in urban areas, non-response does not vary systematically across birth cohorts.*

5.2 Average effects of education disruption on earnings

Table 2 reports the baseline regressions. These aim to identify the effect of being in first grade at the time of the shock on log income in dinars relative to unaffected younger cohorts, while controlling for age, gender and urban/rural location. Variables, which could be channelling some of the impact of educational disruption, such as highest level of education attained or choice of sector or occupation, are examined in Section 5.4.

Table 2. Baseline specifications

	(1)	(2)	(3)	(4)
Age	0.0418***	0.0617***	0.0665***	0.0512***
	(0.0152)	(0.0122)	(0.00832)	(0.00638)
Male	0.162***	0.149***	0.128***	0.120***
	(0.0236)	(0.0210)	(0.0185)	(0.0168)
Urban	0.0414*	0.0535***	0.0508***	0.0264
	(0.0231)	(0.0200)	(0.0179)	(0.0164)
In first year of primary school relative to one year younger	-0.0587**			
	(0.0269)			
In first year of primary school relative to 1-2 years younger		-0.0638**		
		(0.0254)		
In first year of primary school relative to 1-3 years younger			-0.0742***	
			(0.0243)	
In first year of primary school relative to 1-4 years younger				-0.0594**
				(0.0235)
Number of obs.	1,005	1,401	1,755	2,087
Adjusted R-sq.	0.050	0.053	0.059	0.051

*Notes: Robust standard errors in parentheses. * denotes significant at the 10 per cent level, ** at the 5 per cent level, *** at the 1 per cent level. The dependent variable is log income.*

The first four specifications vary the size of the control group from those in the cohort just below the affected group (column 1) to 2 cohorts below (column 2), 3 cohorts below (column 3) and 4 cohorts below (column 4). Column 5 compares all those in primary education at the time of the shock with the

cohort born in 1992 (those not in school yet at the time of the bombing). Results are robust to also controlling for the year in which they completed their education, if anything the magnitude of the effect is slightly larger (Annex Table 2). Covariates have the expected signs, with older individuals, men and those in urban locations earning more.

The results point to significant lasting effects of the shock on income. Those in first grade at the time of the shock earn about 6-7 per cent less than cohorts just younger than them (and hence unaffected) 20 years later. The magnitude of these estimates is comparable to those by Ichino and Winter-Ebmer (2004) for education disruptions experienced during the Second World War—they point to 5 per cent earnings losses relative to younger cohorts.

5.3 Heterogeneity in the effects of education disruption on earnings

While there appears to be a broader effect for all those in primary school at the time, this is of smaller magnitude (around 3 per cent of income) than for those in first grade. This is consistent with the findings of the returns to education literature, which point to higher returns for primary education than secondary or tertiary education (Psacharopoulos 1985; Psacharopoulos and Patrinos 2018).[2]

Examining effects separately for those in the first versus last four years of primary school points to results being significantly larger and statistically significant only for those in lower grades (Table 3).

Table 3. Baseline specifications by grade

	(1)	(2)	(3)
In first four grades of primary school	-0.0838**		
	(0.0391)		
In last four grades of primary school		0.0329	
		(0.0695)	
In primary school			-0.0289
			(0.0327)
Number of obs.	1,206	1,373	2,345
Adjusted R-sq.	0.115	0.101	0.097

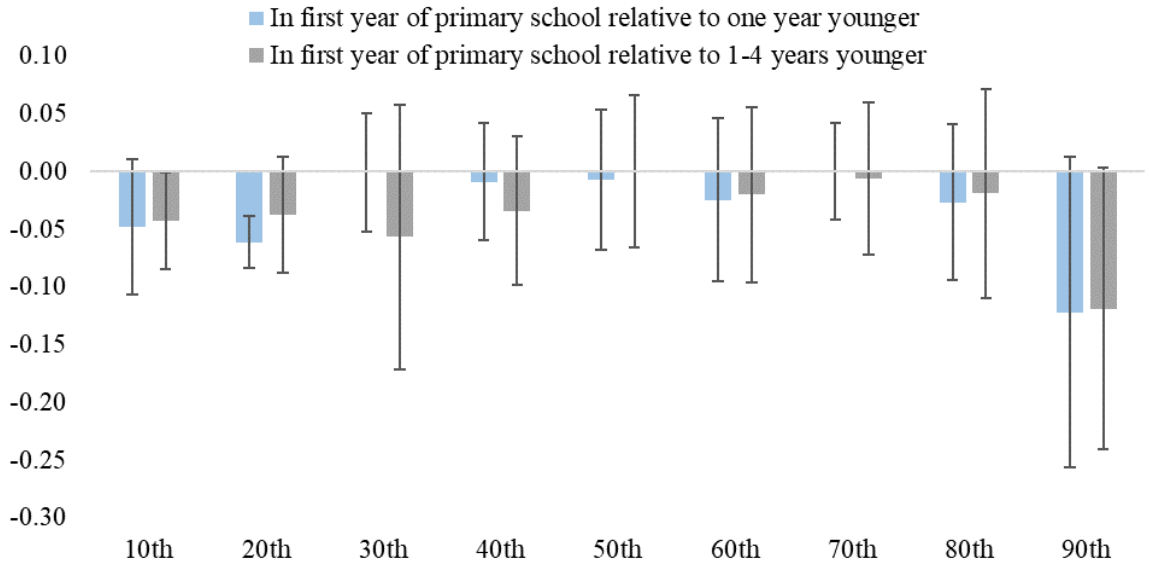
*Notes: Robust standard errors in parentheses. * denotes significant at the 10 per cent level, ** at the 5 per cent level, *** at the 1 per cent level. The dependent variable is log income, all specifications control for age, gender and urban/rural location in 2018/2019.*

Averages also conceal substantial heterogeneity across different groups. Figure 1 illustrates these using quantile regressions: effects are most pronounced in the second decile. Effects are also large at the very top, though imprecisely estimated. These results are consistent with recent findings on the impact of Covid-19 school closures, pointing to an increase in heterogeneity in learning progress among primary school pupils (Tomasik, Helbling and Moser 2020).

Effects are also somewhat larger in rural than in urban areas, though the magnitudes are not statistically significantly different, perhaps reflecting more significant scarring in thinner labour markets.

[2] Ichino and Winter-Ebmer (2004) do not provide separate estimates by grade. Meng and Gregory (2022) look at much longer education disruptions and focus specifically on university entry—they find that missing school at every level reduced the probability of obtaining a formal degree.

Figure 1. Quantile regressions



Notes: Coefficients and 90 per cent confidence intervals from quantile regressions. The dependent variable is log income, specifications control for age, gender and urban/rural location in 2018/2019.

5.3 Mediating variables

Some of the effects of missed education may be channelled through related educational and employment choices, such as selection into particular sectors or occupations, or may be related to employment history more broadly. For instance, students who struggled academically because of exposure to school closures at an early age may drop out of school earlier, and this could explain part of the earnings differential observed later in life. They may also become more risk averse, selecting into more stable jobs, or particular sectors or occupations.

This section thus decomposes the total effect of education disruption on earnings into direct effects (the impact of school closures on earnings leaving out the effects of potential mediator variables) and indirect effects (those that are mediated through other variables). The following estimates rely on the Karlson, Holm and Breen decomposition (Breen, Karlson and Holm 2011; Karlson and Holm 2011; Karlson, Holm and Breen 2010; Kohler, Karlson and Holm 2011).

Table 4 presents the decomposition for a number of potential mediator variables. Those affected by school closures have *higher* educational attainments than those unaffected by school closures and this offsets some of the negative effect. While the total effect of school closures on earnings is around 5 per cent, accounting for differences in educational attainment the effect would be even larger, around 6 per cent (column 1).

Some of the negative effect of schooling disruption on earnings is, however, channelled through sector and occupation selection (columns 2 and 3). Sectoral selection explains around a fifth of the total effect; the indirect effect through occupational choices is smaller.

Differences in years worked have a small indirect effect overall—while older, affected cohorts could have worked more years, this is in part offset by staying in school longer (column 4).

Affected cohorts are more likely to work in the public sector and to be on open-ended contracts—consistent with both higher educational attainment but possibly also higher risk aversion. On average, those working in the public sector and on open-ended contracts earn more than those in private firms

and on fixed term contracts, this selection effect thus offsets some of the negative effect of the schooling disruption (columns 5 and 6).

Column 7 includes all of the above mediator variables: indirect effects account for about a fifth of the total effect as those affected by the school closures may be more risk averse, selecting into lower-paying sectors and occupations. The overall negative effect can, however, be observed despite affected students staying in school longer, being more likely to work for the public sector and on open-ended contracts.

Table 4. Mediation analysis

Mediator:	Education	Sector	Occupation	Years worked	Public sector employment	Open-ended contract	All
Total effect	-0.052	-0.052	-0.052	-0.052	-0.051	-0.048	-0.049
	(0.027)	(0.027)	(0.024)	(0.028)	(0.028)	(0.027)	(0.022)
Effect controlling for mediator	-0.064	-0.043	-0.046	-0.054	-0.055	-0.053	-0.040
	(0.027)	(0.027)	(0.026)	(0.028)	(0.028)	(0.027)	(0.025)
Effect through mediator	0.011	-0.009	-0.006	0.002	0.005	0.005	-0.009
	(0.007)	(0.009)	(0.019)	(0.002)	(0.003)	(0.003)	(0.021)

Notes: Standard errors in parentheses. The dependent variable is log income, all specifications control for age, gender and urban/rural location in 2018/2019. 20 sectors; ISCO 3-digit level occupations.

6. Robustness

Baseline specifications aim to mitigate potential effects of education or employment experience abroad by focusing on those born in Serbia who did not leave the country for more than one year. Results are robust to controlling for citizenship instead of country of birth.

Those living in Vojvodina, an autonomous province of Serbia with a substantial Hungarian minority, may also have been more likely to move abroad (in particular to Hungary) during the NATO bombing. We have thus also examined results separately for Vojvodina and the rest of Serbia. Effects are, as expected, stronger for Serbia excluding Vojvodina.

There was significant geographic variation in the extent of bombing, with bombing targeting key infrastructure (such as bridges) as well as industrial areas. Results are not statistically significantly different across hard hit and less hard hit regions (school closures were universal).

Results are also robust to examining 2019 separately instead of the 2018-2019 pooled sample and to excluding outliers with the highest incomes. Baseline regressions include robust standard errors; results are broadly robust, though in some specifications less significant, when clustering standard errors at the cohort level.

7. Conclusions

Our results point to significant lasting effects of schooling disruptions on earnings: those in first grade at the time of the shock earn about 6-7 per cent less even 20 years later than unaffected cohorts just younger than them.

Given about 3 months' of school closures, these effects are larger than those estimated in the returns to education literature. This could be consistent with the universal exogenous nature of the shock, as even those who would have chosen more education and had high returns to it were affected (the teacher absenteeism literature is also likely subject to the endogeneity of teacher's choices and selection into schools).

There is, however, significant heterogeneity in the magnitude of the effect across different groups. Impacts are larger for those in first grade than those in higher grades (consistent with studies on higher returns to education for younger children), more pronounced at the bottom of the income distribution (and at the very top), and in thinner labour markets in rural areas. Selection into lower-paying occupations and sectors explains about a fifth of the overall effect. However, the negative effect of education disruption persists despite affected cohorts staying in school longer, being more likely to work for the public sector and on open-ended contracts.

Households had very limited ability to offset the shock at the time (for instance there was no formal remote education), the effects of Covid-19 related school closures would thus likely be smaller, at least in economies with more widespread remote learning, but given unequal access to such resources may be even more uneven than the effects estimated here.

Annex

Annex Table 1. Descriptive statistics, larger control groups

	In first year of primary school			1-2 years younger			1-3 years younger			1-4 years younger		
	Obs	*Mean*	*Std. Dev.*	*Obs*	*Mean*	*Std. Dev.*	*Obs*	*Mean*	*Std. Dev.*	*Obs*	*Mean*	*Std. Dev.*
Income	509	35385	12415	892	35694	18901	1246	34708	18996	1578	34235	17725
Age	2,220	26.9	0.71	4488	25.5*	0.9	6697	25.0*	1.1	9186	24.4*	1.3
Male	2,220	0.52	0.50	4,488	0.5	0.50	6,697	0.5	0.50	9,186	0.5	0.50
Lower secondary educ.	2,220	0.19	0.39	4488	0.18	0.39	6697	0.18	0.38	9186	0.17	0.37
Upper secondary educ.	2,220	0.39	0.49	4,488	0.46*	0.50	6,697	0.50*	0.50	9,186	0.55*	0.50
Tertiary, non-university educ.	2,220	0.04	0.20	4488	0.03*	0.17	6697	0.02*	0.16	9186	0.02	0.15
University educ.	2,220	0.30	0.46	4,488	0.24*	0.43	6,697	0.21*	0.41	9,186	0.18	0.39
Urban	2,220	0.56	0.50	4,488	0.54	0.50	6,697	0.53*	0.50	9,186	0.54	0.50
Years worked	1,741	4.14	3.12	3,207	3.35*	2.83	4,543	3.21*	2.75	5,833	3.03*	2.65
Public sector employment	1,324	0.18	0.39	2,306	0.15	0.35	3,259	0.14	0.35	4,159	0.14	0.34
Open-ended contract	1,154	0.63	0.48	2,011	0.58*	0.49	2,839	0.56*	0.50	3615	0.55*	0.50

*Notes: * denotes differences that are statistically significantly different at the 5 per cent level across the affected cohort and the control group. Public sector employment refers to state as opposed to private registered enterprises. Open-ended contract refers to jobs of an unlimited duration as opposed to jobs for a limited duration.*

Annex Table 2. Baseline specifications, controlling for year in which completed education

	(1)	(2)	(3)	(4)	(5)
In first year of primary school	-0.0721				
relative to one year younger	(0.0472)				
In first year of primary school		-0.0947**			
relative to 1-2 years younger		(0.0443)			
In first year of primary school			-0.103**		
relative to 1-3 years younger			(0.0411)		
In first year of primary school				-0.100**	
relative to 1-4 years younger				(0.0397)	
In primary school					-0.0289
relative to 1-4 years younger					(0.0327)
Number of obs.	*481*	*674*	*847*	*1008*	*2345*
Adjusted R-sq.	*0.086*	*0.063*	*0.063*	*0.063*	*0.097*

*Notes: Robust standard errors in parentheses. * denotes significant at the 10 per cent level, ** at the 5 per cent level, *** at the 1 per cent level. The dependent variable is log income, all specifications control for age, gender, urban/rural location in 2018/2019 and fixed effects for the year in which they completed their education.*

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