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ABSTRACT

Schools as Safety Nets: Break-Downs and Recovery in Reporting of Violence against Children*

Schools are a key channel in formal reporting of violence against children, but this channel broke down with the onset of the COVID-19 pandemic. We study how widespread such reporting declines are, and to what extent they were recovered once re-openings begin. Examining the universe of all criminal reports of violence against children in Chile, we observe sharp declines in reporting of all types of violence (psychological, physical, and sexual), and that full recovery in reporting had not occurred, even nearly 2 years following initial school closures. Our estimates suggest that school closure and incomplete re-opening resulted in around 2,800 'missing' reports of intra-family violence against children, 2,000 missing reports of sexual assault, and 230 missing reports of rape against children, equivalent to between 10-25 weeks of reporting at baseline. In the post-school closure period, we find that greater school attendance encourages faster and more complete recovery in reporting of violence against children, pointing to important non-cognitive costs of both school closure, and school absence.

JEL Classification: D10, I28, I18, K42

Keywords: violence against children, reporting, school closure, school attendance, COVID-19

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1 Introduction

Violence against children has devastating impacts on children's well-being, life satisfaction, and physical and mental integrity (Hillis et al., 2016). Long-term consequences have been convincingly documented in terms of physical health (Lippard and Nemeroff, 2020), mental health (Widom et al., 2007), educational attainment, earnings (Currie and Widom, 2010), and antisocial behaviors (Smith and Thornberry, 1995; Thornberry et al., 2001; Currie and Tekin, 2012). Self-reported figures suggest that, worldwide, 127 of every 1,000 individuals have experienced sexual abuse during childhood, while over 20% report having experienced physical and emotional abuse (Stoltenborgh et al., 2015). As a major threat to child welfare, child maltreatment brings significant cost for the victims, and for society (Currie and Tekin, 2012).

The early detection of maltreatment can partially mitigate these detrimental effects by promoting timely interventions which may be more effective at altering abusive behavior (Fitzpatrick et al., 2020). Schools play a key role in this regard, identifying early signs of abuse and maltreatment and channeling these cases into the justice and child protection systems (Fitzpatrick et al., 2020; Puls et al., 2021). This key bridging role played by teachers and educational professionals was disrupted with the global pandemic caused by COVID-19, and the resulting public policy and public health responses. Schools were closed as an initial response to control the spread of the virus (Hale et al., 2021), with research documenting significant declines in reporting following school closure (Baron et al., 2020; Takaku and Yokoyama, 2021; Padilla-Romo and Cabrera-Hernández, 2020).

Importantly, this decline came despite concerns about a potential rise in victimization of children and adolescents, given the confluence of risk factors associated with child maltreatment, such as parental unemployment and economic stress, parental burnout, limited sources of social support (Bullinger et al., 2022; Pereda and Díaz-Faes, 2020; Lindo et al., 2018), and a significant increase in the time spent confined together (Lindo et al., 2018; Fitzpatrick et al., 2020). Indeed, surveys conducted among parents (Rodriguez et al., 2021), teachers (Vermeulen et al., 2022), and social-service professionals (Bullinger et al., 2022) report an increase in family conflict, harsh parenting, and child neglect during the first months of the COVID-19 pandemic. Thus, undetected cases may have increased substantially above baseline levels, with long-term consequences for the victims should these cases remain undetected over time.

In this paper, we focus on rates of reporting of violence against children, as a relevant and consequential indicator of social well-being. Specifically, we seek to understand how formal educational systems first lost and then recovered a specific function—that of identifying and reporting violence against children—with declines and recoveries of face-to-face learning. We extend the relatively nascent literature on the negative consequences of school closures in a number of ways. Specifically, by analyzing *i)* how school closure contributed to a decline in reporting over a two-year time period, distinguishing between different types of violence including physical, sexual and psychological violence, and *ii)* determining whether that decline was reverted once schools re-opened, and if so over what time-frame and under what conditions. By analyzing the recovery of reporting, we are able to shed light on the relevance of in-person interactions between children and educational professionals for the identification of maltreatment.

While a number of existing studies have clearly documented reporting declines with school closure (Baron et al., 2020; Barboza et al., 2021; Prettyman, 2021b; Bullinger et al., 2021; Rapoport et al., 2021; Takaku and Yokoyama, 2021; Padilla-Romo and Cabrera-Hernández, 2020), what is new here is an extension into a considerably broader range of classes of violence including sexual violence; consideration of a longer period extending into pandemic recovery; the first consideration of the subsequent impact of school re-openings; the use of complete and nationally comparable administrative data which resolves a number of substantial measurement concerns noted in the literature (Bullinger et al., 2021); and bounds estimation to incorporate potential increases in violence above baseline rates. Moreover, we study these questions in Chile, a well-suited context for several reasons. Firstly, we have generated rich micro-data covering all reported crimes against children in a number of dimensions, school closures, attendance, and other relevant factors over an extended period of time, which allows use to analyze the long-term cost and the potential recovery of the policies adopted during the pandemic. Chile was exposed to long school closures compared with other regions of the world (The Economist, 2021; UNICEF, 2021), and our data cover the pre-closure, closure, and re-opening periods. Therefore, this setting allows us to provide a more comprehensive evaluation of the impact of school closure and reopenings on rates of violence reporting. Beyond our main aims of understanding the effects of these policies, we seek to contribute to the scant research on child abuse in Latin America overall (Stoltenborgh et al., 2015), and specifically to the consequences of COVID in one of the continents more affected by the pandemic and its policy responses (United Nations, 2020). Secondly, these data and the context allows us to consider results at a national scale using comparable reporting measures.[1] Finally, but importantly, the nature of the pandemic response in Chile allows us to rule out a number of competing explanations for violence reductions such as the existence of parallel lockdowns, given considerable geographic and temporal variation in COVID infection and formal lockdown and similar public health policies.

Our results suggest, firstly, that school closures generate sharp declines in reporting not only in rates of violence against children, as previously documented (Baron et al., 2020; Padilla-Romo and Cabrera-Hernández, 2020), but also in rates of sexual abuse and rape against children. What's more, we find evidence that rates of reporting of abuse are slow to recover, not reaching baseline levels even nearly two years following original school closures. Turning to magnitudes, in aggregate, our results suggest that delays in re-opening and in recovering reporting channels were *as damaging* if not more damaging than school closures themselves. For example, when considering reports of violence against children, our preferred counterfactual projections suggest that there were approximately 1,500 'missing' reports during periods of school closure versus around around 2,500 missing cases once schools were permitted to re-open. Suggestive results based on a partial records of attendance suggests that lack of student attendance is an important impediment to violence reporting, pointing to the importance of policies to bolster school attendance, which has been also affected by the pandemic (McDonald et al., 2022).

In what remains of this paper, in Section 2 we provide background on the importance of schools to protect children's well-being as well as the educational context and school closures in Chile more specifically. We describe the range of micro-data generated for this study in Section 3. In Section 4 we layout methods and identifying assumption. Section 5 documents all results, and Section 6 provides discussion and conclusions.

[1] Data challenges in existing literature have meant that studies of school attendance and violence reporting are often limited to smaller areas such as the State of Florida Baron et al. (2020) or Mexico City Padilla-Romo and Cabrera-Hernández (2020). Two exceptions are the study of Fitzpatrick et al. (2020) and Puls et al. (2021), both based on national-level data in the US, though in a pre-COVID setting.

2 Background

2.1 Schools and Child Wellbeing

Despite the detrimental and long-term consequences of child maltreatment, there is a persistent challenge in identifying those at risk of neglect and abuse. Estimated prevalence rates based on informants (i.e., medical professionals, child protection workers, or teachers) suggest rates of physical, sexual and emotional abuse around 1% (Stoltenborgh et al., 2015; Font and Kennedy, 2022).[2] In contrast, self-report studies provide prevalence rates that go from 7.6% and 18% for sexual abuse among boys and girls, respectively, to 22.6% for physical abuse and 36.3% for emotional abuse (Stoltenborgh et al., 2015). While these gaps in rates partially reflect differences in the period of report covered (Stoltenborgh et al., 2015; Font and Kennedy, 2022), they also evidence the fact that official data only reveal the "tip of the iceberg" in cases of maltreatment (Currie and Tekin, 2012; Hillis et al., 2016).

In fact, observing child maltreatment requires not only the existence of the behavior, but also its recognition as abuse by either the victim or a third party, and the process of reporting it to others, particularly–in the case of administrative data–those agencies responsible for child protection (Prettyman, 2021a). Increasing the likelihood of early detection and reporting are fundamental steps to respond to this mostly silent problem. School personnel have a privileged position to identify signs of abuse as they have an almost daily contact with children and access to their parents, can observe changes in behavior, and are trusted figures to their students (Krase, 2013; Cerezo and Pons-Salvador, 2004). Not surprisingly, teachers and other educational professionals account for over 20% of the reports investigated by child protective services in United States (Puls et al., 2021).

Increasing time at school can thus increases reporting of violence against children, given the nature of the interactions between educational professionals and children. Puls et al. (2021) show a decrease of 16% in reporting when schools were closed, which were not matched with the increases observed during the first two weeks following the closure period. Similarly, Fitzpatrick et al. (2020) show that additional time in schools leads to an increase in reports of child maltreatment, using two

[2] The cumulative risk estimated based on administrative data is, however, higher and more consistent with self-reported studies of prevalence. For example, Kim and Drake (2019) show that, by age 12, 1 in 3 children have at least one maltreatment report.

different identification strategies (child's eligibility for Kindergarten and school calendars). Both studies confirm the relevant role that schools play on identifying and reporting child maltreatment, and closing the gap between prevalence and response to maltreatment.

However, one of the earlier and most expanded responses to the COVID-19 pandemic worldwide was the closure of schools (see Online Appendix Figure A1), with undesired consequences in terms of learning loss (Agostinelli et al., 2022; Engzell et al., 2021), early child development (Abufhele et al., 2022), female labor market participation (Hansen et al., 2022), increased inequalities (Agostinelli et al., 2022), and mental health issues among children and adolescents (Viner et al., 2022). Among the relatively less-explored consequences is that which school closure may have had in terms of identifying and reporting maltreatment. Using a counterfactual design, Baron et al. (2020) observed an almost 30% decrease in the number of allegations in the two first months following school closure in Florida, US. Similar results are reported by Prettyman (2021b) for Colorado, Bullinger et al. (2020) for Georgia and by Padilla-Romo and Cabrera-Hernández (2020) for Mexico City.

These studies confirm the broken link between school closure and reporting during the first months of the pandemic when most educational settings were mandatorily closed at all levels. However, while schools began to resume their in-person activities during the Fall of 2020 in most countries of the global north, they remained closed for particularly long periods in Latin America (The Economist, 2021; UNICEF, 2021), potentially increasing extant inequalities and interrupting three decades of educational improvement in the continent (World Bank, 2021). In March 2021, close to 60% of Latin American school-age children were still affected by school closure (UNICEF, 2021), having lost more days of schools than any other region in the world (World Bank, 2021). Similarly, in Chile, schools closed nationally on March 16th, 2020, and started to open–although very gradually–during August of 2020. By March 2021 only 25% of all schools had some kind of in-person education, increasing to 98% by December of 2021, the end of the school year. Student attendance, however, remained under 50% of the total number of students, which allows us to better understand the contribution of in-person acitivities to the reporting of violence against children. In the following section we discuss in detail the specific context of this study.

2.2 Policy Responses to COVID-19 and Educational Context

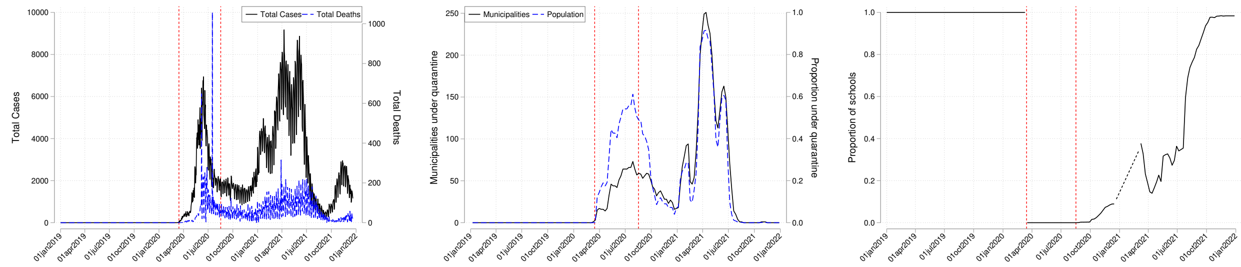
The first COVID case was identified in Chile on March 3, 2020. While initially arriving to Chile from Europe and Asia, COVID cases expanded quickly with local transmission, with around 7,000-8,000 cases per day in mid-June, 2020 at the peak of the first-wave of the pandemic in Chile (see Figure 1, panel (a)). Since the arrival of the first cases, the Chilean government adopted a number of policies to increase social distance, such as an early ban of any large gatherings, the closure of schools and universities, the mandatory use of face-masks in public, and the implementation of formal lock-downs (Tariq et al., 2021).[3]

The first mandatory lockdown was put in place on March 28, 2020 (Tariq et al., 2021). The particularity of Chile is that lock-downs were defined at the national level by the national Ministry of Health (MoH), though implemented at the municipal level (Figure 1, panel (b)). Chile is divided into 346 municipalities, which in urban settings are smaller than a city, though in rural settings can cover various towns. Therefore, two neighboring municipalities may have had different lockdown statuses based on the MoH assessment of the need for lockdown. The assessment was mostly based on the case growth and the risk of contagion, although there was no declared metric, making the exact timing of lockdown hard to predict for a specific municipality (Bhalotra et al., 2021; Lee et al., 2021).[4] Moreover, formal lockdowns were strictly enforced, with police personnel conducting spot checks and citizens–with the exception of essential workers–allowed to leave their house only twice a week for three hours with a specific permit. In fact, the implementation of lockdowns led to a sharp drop in mobility (about 35%) (Bhalotra et al., 2021), beyond the decline already observed after schools were closed (Bennett, 2021). Similarly, the decision to lift lockdowns was determined by the MoH.

In contrast, all K-12 schools nationwide were mandated to close on March 16, 2020 (Figure 1, panel (c)). Schools moved to online education. However, data collected by the Ministry of Education make clear the unequal access to education: only 31% of the parents surveyed report their child had a personal electronic device with which to connect to classes and a similar percentage reported that they had a good internet connection.

[3]Additional discussion of Chile's pandemic response is provided in broader literature, see for example (Mena et al., 2021; Gil and Undurraga, 2020; Tariq et al., 2021; Bhalotra et al., 2021; Castillo et al., 2021; Li et al., 2022).

[4]A video on the dynamic nature of lockdown imposition in the Metropolitan Region of Santiago is available here: https://www.damianclarke.net/resources/quarantines.gif.



(a) COVID Cases (b) Lockdowns (c) School Closure/Reopening

Figure 1: Contextual Details – Epidemiological Measures and School Closure

Notes to Fig. 1: Each panel presents weekly measures of COVID cases and deaths (panel (a)), formal lockdown measures (panel (b)), and the proportion of schools open (panel (c)). Trends are reported between Jan. 1, 2019-Dec. 31, 2021, and are all based on official administrative records maintained by the Ministry of Education, Ministry of Health, or Ministry of Science (further described in Section 3). In panel (b) the number of municipalities under lockdown (left-hand axis) can be at most 346 (the total number of municipalities), while the proportion of the Chilean population under lockdown is plotted on right-hand axis. The first vertical dashed line indicates the day which schools were ordered shut by the central government, while the second vertical dashed line indicates the day which schools were officially allowed to re-open.

In July 2020, the government implemented a "step by step" strategy (in Spanish, the *Paso a Paso* policy) that considered five phases of gradual opening of each municipality–from full lockdown to no restrictions (Tariq et al., 2021). Under this scheme, schools located in municipalities that were not under full lockdown ("Phase 1") were allowed–though not mandated–to resume in-person education in August 2020. In order to do so, they needed to follow the protocols established by the Ministry of Education. For example, students needed to wear masks, and classrooms should guarantee physical distancing between students, along with adequate ventilation. In municipalities which were not classified as Phase 1 (and hence not under complete lockdown), the decision to open and how to resume in-person activities was a school-level decision. For example, some schools adopted a gradual return, establishing shifts with in-person vs. remote schooling, or opening only some days during the week. Attendance remained voluntary up to March 2021. By December 2020, only 10% of schools had had some in-person activity, although this was mostly part time and with low attendance (Claro et al., 2021). In March 2021, with the start of a new school year, 32% of the schools resumed some in-person activities, with all schools being required by the Ministry of Education to develop a plan for a safe but ideally in-person academic year. Early vaccination of teachers and educational personnel was prioritized as part of the plan. However, as the cases increased and full lockdowns were put back in place (see Figure 1, panel (b)), most schools remained or moved back into remote education, with a steady increase in in-person activities starting with the spring semester, which started in July, 2021. That same month, the Ministry of Health allowed schools based in municipalities under Phase 1 to re-open, given the start of the vaccination process among school children (Jara et al., 2021). By the end of the academic year of 2021, 98% of schools had some in person activity in place (Claro et al., 2021). Most of the schools, however, opened either in shorter school days or alternating days/weeks among the students in order to fulfill the requirements of social distancing.

Low-income schools were less likely to open and opened, on average, for less days, but the school's administration type[5] was the most significant predictor of opening earlier during 2021 and explained all the observed socioeconomic differences in opening probabilities (Canales et al., 2022). Particularly, schools administrated by the municipality were the least likely to open, regardless of the political affiliation of the mayor. And, while rates of re-opening among different school types converged by September 2021, large differences remained between private schools and other schools (public and state-subsidized private schools) in terms of percentage of days open and rates of attendance. For example, as reported by Claro et al. (2021), in November, 2021, while about 70% of the students in private education attended at least one day a week in person, only 40% of those studying in public and private subsidized schools did so.

A key element of school opening decisions, at least for the analysis in this paper, is that school opening decisions seem highly unlikely to be related to changes in rates of DV or sexual violence against children. As we lay out further in Section 4, an identifying concern would occur if schools which opted to re-open were those which were in municipalities in which systematically different changes in rates of DV or sexual violence were occurring, for example if schools were more likely to open when rates of reporting were increasing, or were more likely to open when rates of reporting were decreasing. In practice the precise moment of school re-opening for each school appeared to depend more on the school's abilities to meet the required criteria indicated by the Ministry of Education.

[5]The Chilean school system comprises private schools, public schools (administered by municipalities until 2017, where some public schools have been transferred to a new governmental organization (Canales et al., 2022)), and private state-subsidized schools (Bellei and Munoz, 2021). The later represents over 50% of the total number of students, while another 35% are enrolled in public schools.

3 Data

We collect administrative data from a number of national Ministries in Chile covering a range of factors. These data, which are generally available at the individual or municipal$\times$day level, are aggregated consistently to the level of municipality$\times$week, covering each of Chile's 346 municipalities over the period of (at least) January 2019 to December 2021. We additionally hand-compiled daily data on municipal-level lockdown status from public announcements made over the period. We describe these variables and their sources below.

Crime Reporting Reports of Violence Against Children come from police information that is reported to Chile's Ministry of the Interior. A single observation is provided for each victim, along with demographic characteristics and details of the crime, such as municipality of occurrence and the type of crime. We requested information from the Ministry of the Interior on all victims of crimes reported to the police between January 2010–December 2021. Specifically, we requested full data for those victims that *i)* were under 18 years old at the time of the offense, and *ii)* had been victims of crimes classified as intra-familiar violence, sexual abuse, or rape. Regarding the former, the police distinguish between psychological violence, moderate physical violence, and serious physical violence. The data also provided information about the victim's age and sex, and more detailed classification of the offense. As the specific date of the crime is recorded, those dates were used to generate weekly rates of reports for each type of offense in each municipality. Rates were generated for each class of violence (intra-family violence, sexual abuse, and rape), as well as in sub-groups by age and by sex. Rates are consistently generated using populations by municipality which are available from Chile's National Statistics Agency (INE) for each age and sex.

These principal data contain one line for each victim of crime, and so in certain circumstances may include more than one victim for a single crime event. In data on victimhood, administrative records do not contain information on the exact context of the crime (i.e., inside the house, or in a public place). While in our main analysis we wish to determine rates of victimization, and as such work with data on all crime victims known to police, we also work with another administrative database with a single line for each crime known to police (irrespective of the number of victims). While this event-based database allows us to explore the precise location of the crime (specifically as occurring inside the house, or in a public place), it does not account for all the potential victims involved in one crime, nor provides precise demographic information about the victims (such as sex and exact age), and as such we simply use this in supplementary analyses.

Educational Information From the Ministry of Education we obtain administrative data on records of dates of school closures and reopening for each of the 10,847 schools in the country in 2020 and 10,875 schools in the country in 2021. This covers all schools (both public and private) excluding those which only provide adult education. These data provide a weekly record of whether the school was officially open to receive students for in person instruction. The data are publicly available and cover the months of October 2020-December 2021, and are supplemented by two months of records provided in transparency requests from the Ministry of Education for the months of August and September, 2020, which are not available in public repositories.

The Ministry also provided a database with the number of children attending in-person education every month per school, recorded in four categories: 1) at least once during that month; 2) between one and five days; 3) between six and ten days; 4) More than ten days. While school-level reopening data is available over the entire period under study, school-level information on attendance was only available in a consistent format between July and December, 2021, and, as such, analysis based on school level attendance measures is conducted only over this limited time period, so external validity may be limited. When working with attendance, we calculate proportional measures of attendance in each school, which can be generated from the attendance database, as well as an additional publicly available administrative record of the number of children registered in each school in each academic year.

Public Health and Epidemiological Factors Data of the dates of entry and exit of the confinement of each municipality were prepared by hand by the authors based on daily televised reports made by the Ministry of Health, and open repositories of the Ministry of Science of Chile. The data recorded the exact date when mandatory lockdown started and the date when it was formally lifted. These data are available from March 2020 to December 2021. Official records from the Ministry of Science were maintained from approximately August 2020. Earlier records were announced in highly viewed public announcements made by the Minister of Health or Under-Secretary of Health. Thus, official records available from the Ministry of Science are complemented by our hand-collected records of every lockdown status in all periods from March-August 2020.[6]

Data on COVID-19 infection, all PCR tests, PCR test positivity and COVID-related deaths come from open repositories from the Chilean Ministry of Science. The data provide information for each of the 346 municipalities, and are generally recorded at a frequency of at least weekly, generally with multiple records in each municipality and week. These are consistently measured, and in principal analyses we aggregate these to municipal by week records of test positivity (proportion of PCR tests with a positive result), number of PCR tests per total population, and number of COVID-19 cases per total population.

Descriptive statistics of all dependent variables, independent variables, and covariates are presented in Table 1. These are displayed over the full period of study, from January 1, 2019–December 31, 2021. Municipality by year cells document substantial variation in rates of Intra-family violence against children and sexual abuse and rape against children, all of which are observed to have substantial standard deviations. Of all violence types, reports of Intra-family violence are highest, at 3.93 per 100,000 children per municipality by week, followed by values of 2.94 per 100,000 in the case of sexual abuse, and 0.6 per municipality per week in the case of rape. Panels B and C document variation in measures of school closure, reopening and attendance, and the epidemiological situation of each municipality by week cell.

The geographic variation in school reopening is displayed in Appendix Figure A2 (for all of Chile), and Appendix Figure A3 for the Metropolitan region of Santiago. Here we observe considerable variation of initial school re-opening dates. This is also observed across municipalities *within* the capital of Santiago, despite the fact that contagion rates were generally quite similar, particularly in the central municipalities.

Finally, Figure 2 documents trends in reporting of intra-family violence, sexual assault, and rape against children.[7,8] These are displayed during the pre-pandemic period, initial arrival, and later pandemic; specifically, January 1, 2019-December 31, 2021. The complexity of the COVID-19

[6] We provide these hand-collected data publicly at https://github.com/Daniel-Pailanir/Cuarentenas.

[7] Longer trends in each of these variables are documented in Appendix Figure A4.

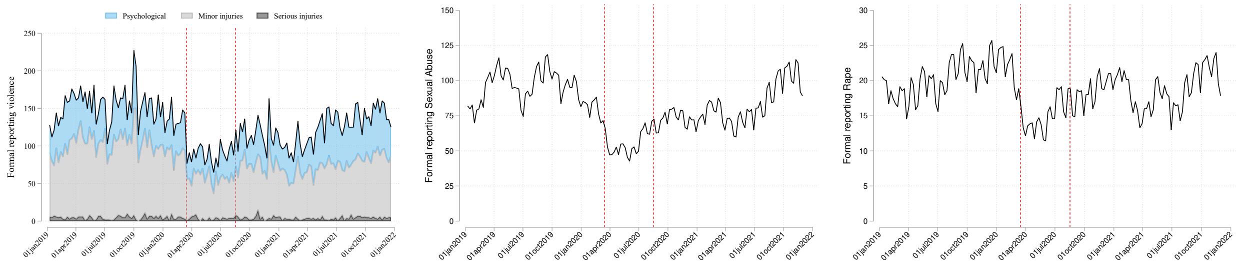
[8] In the case of sexual assault and rape, spikes in reporting generally occur given inexact dates of reporting within each month. Where the precise date is not recorded, the crime is recorded in microdata on the 1st day of each month. In Appendix Figure A5, both original data, as well as smoothed data used in the body of the paper are displayed. In appended results discussed below, we document that estimates are not sensitive to using original (unsmoothed) data.

Table 1: Summary Statistics of Principal Variables

	Observations	Mean	Std. Dev.	Min.	Max.
Panel A: Violence Against Children					
Intra-family Violence	54321	3.93	18.91	0.00	2325.58
Physical Violence (serious)	54321	0.13	2.20	0.00	116.82
Physical Violence (moderate)	54321	2.44	12.62	0.00	1086.96
Psychological Violence	54321	1.35	13.81	0.00	2325.58
Sexual Abuse	53283	2.94	14.09	0.00	1020.41
Rape	53283	0.60	2.18	0.00	80.71
Panel B: Schools					
School Closure	54321	0.31	0.46	0.00	1.00
School Reopening (Binary)	54214	0.29	0.45	0.00	1.00
School Reopening (Continuous)	54214	0.19	0.35	0.00	1.00
Attendance (1 day)	45802	0.54	0.47	0.00	1.00
Attendance (1-5 days)	45802	0.49	0.48	0.00	1.00
Attendance (6-10 days)	45802	0.49	0.48	0.00	1.00
Attendance (10+ days)	45802	0.50	0.48	0.00	1.00
Panel C: COVID/Other Measures					
Quarantine	54321	0.10	0.30	0.00	1.00
COVID-19 Cases per 1,000	54321	0.68	1.88	0.00	233.58
PCR Testing per 1,000	54321	8.10	10.69	0.00	179.53
PCR Test Positivity	54321	5.27	9.66	0.00	100.00
Population between 5-18 years	54321	10222.90	15866.86	9.00	127390.00

Notes to Table 1: Summary statistics are displayed across all municipal by week cells for the period of January 2019–December 2021. Panel A documents mean outcomes of violence against children measured as weekly reports per 100,000 children in each municipality. Panel B documents principal measures of school closure and re-opening, as well as attendance figures for periods in which attendance is available. Panel C documents epidemiological controls.

pandemic, and in particular the implications of school lockdowns, can be clearly observed even graphically. With school closures, criminal complaints of violence against children in the country *immediately* fell from around 150 cases per week to around 75 cases per week (panel (a)). Similar proportional changes are observed in both cases of sexual assault, and rape against children (panels (b) and (c)). That these declines are observed immediately with school closures, and not with formal lockdowns or rates of COVID infection (refer to details documented in Figure 1) suggests a key role of schools in this process. Following school re-opening, while rates of reporting are observed to gradually recover, it is not clear if and when rates of reporting recover their prior trajectory. We take



(a) Intra-family Violence (b) Sexual Assault (c) Rape

Figure 2: Temporal Trends – Crimes Reported Against Children

Notes to Fig. 2: Each panel presents weekly measures of crime reporting against children across all crime classes observed. Trends are reported between Jan. 1, 2019-Dec. 31, 2021, and are all based on official administrative records maintained by the Ministry for the Interior (refer to Section 3). The first vertical dashed line indicates the day which schools were ordered shut by the central government, while the second vertical dashed line indicates the day which schools were officially allowed to re-open.

these questions forward in this paper.

4 Methods

We have two principal aims in this study. The first is to estimate changes in rates of reporting of crimes against children owing to school closure and reopening, and the second is to estimate counterfactual outcomes in the absence of school closures. We lay out methods in each case in the sub-sections below.

4.1 Two-way Fixed Effect Models

We estimate the impact of school closure and school reopening in a two-way fixed effect model. We consider school closure and school reopening which vary by municipality and by time, and separately include fixed effects for all time-specific factors common in the entire country (e.g., regular fluctuations in reporting within the year), and all municipal-specific time-invariant factors, or factors which evolve slowly enough that they are unlikely to vary under our study period of 2019-2021 (e.g., demographic factors which may affect rates of violence). To quantify effects of school closures and reopenings, we begin by estimating the following two-way fixed effect model based on a balanced panel at the municipality$\times$week level:

$$\text{Reporting}_{mt} = \alpha + \beta\text{School Closure}_{mt} + \gamma\text{Schools Reopen}_{mt} + \mu_{WoY} + \phi_m + \boldsymbol{X}'_{mt}\boldsymbol{\Gamma} + \varepsilon_{mt}. \quad (1)$$

Here, Reporting_{mt} refers to crime reporting of violence against children (individuals aged under 18 years), and is consistently expressed as reporting per 100,000 minors. $\text{School Closure}_{mt}$ and $\text{Schools Reopen}_{mt}$ refer to the status of schools in municipality m and week t, which are consistently coded relative to the pre-school closure period. That is, $\text{School Closure}_{mt}$ moves from 0 to 1 sharply at the week schools are closed in the country (week of March 16, 2020), and then remains at 1 until schools reopen in each municipality m, at which point it moves back to 0. And the measure $\text{Schools Reopen}_{mt}$ is set at zero for the entire pre-closure, and closure period, and switches to 1 (or in alternative specifications, to the proportion of students with schools reopen) precisely when schools reopen in the municipality. Thus, given that in the post-school closure period, either $\text{School Closure}_{mt} = 1$, or $\text{Schools Reopen}_{mt} = 1$, but never both, coefficients β and γ are both interpreted as changes in rates of reporting compared to the *pre-school closure period*. Below we discuss the formal interpretation of these coefficients, and identifying assumptions.

In equation 1, $\text{School Closure}_{mt}$ switches sharply from 0 to 1 in a specific week t. We thus include 52 week of year fixed effects, as μ_{WoY}, these are separately identified, and allow us to capture all common factors associated with particular weeks of years in all years under study. Municipality-specific fixed effects are included as ϕ_m for each municipality in the country. The vector of time-varying controls $\boldsymbol{X}_{mt}$ is discussed below, and standard errors are estimated such that the unobserved stochastic error term can be arbitrarily correlated within each municipality over time, allowing, for example, for temporal dependence in municipal-level shocks (Cameron et al., 2008). Regressions are consistently weighted by the population of individuals under the age of 18 in the municipality, allowing us to conduct inference assigning equal weight to individuals, rather than assigning equal weights to municipalities which have considerable variation in populations.

The estimands of interest on $\text{School Closure}_{mt}$ and $\text{Schools Reopen}_{mt}$ are interpreted as the observed changes in rates of reporting, holding constant week of year and municipal fixed effects. This is, then, the mean change in outcomes with school closure, when comparing with rates in the same municipality and week of year in years in which the school indicator is equal to 0 (pre-COVID periods).[9] Causal identification requires that—conditional on factors specific to each week of the year, and each municipality—additional unobserved factors are not correlated with school closure and re-opening. We note that this is equivalent to a 'parallel trends' assumption, given the nature of the two-way FE model. In the absence of school closing and reopening decisions, rates of violence reporting in each municipality would have followed parallel trends to rates occurring in the same moment in previous years. The key identifying concern is that estimated impacts of school closures may actually owe to epidemiological factors related to COVID-19, or other policy changes such as lockdowns. When examining school closures, this concern is minimized given that closures occurred prior to sharp increases in rates of infection, and prior to the announcement of formal lockdowns, however this is likely more relevant in the case of municipal level reopenings.[10] For this reason, time-varying controls $\boldsymbol{X}_{mt}$ are included in equation 1, which consist of rates of COVID infection, rates of COVID testing, and test positivity. Similarly, we control for the existence of a formal lockdown in each period. As already noted, a key aspect of this study is that we observe considerable temporal and spatial variation in the degree to which municipalities were affected by COVID, were under lockdown, and had mobility changes. We are interested in examining the common shock of school closure, and staggered re-opening which occurred throughout the country in municipalities with considerably different epidemiological and public health responses. In all cases, we document models with and without the inclusion of controls, as movement of coefficients upon the inclusion of controls allows us to consider the likelihood that unobservable factors may actually account for

[9] Formally, in the case of school closure, the estimate simply captures:

$$\widehat{\beta} = E[Reporting_{mt}|School\ Closure_{mt} = 1, \boldsymbol{W}_{mt}] - E[Reporting_{mt}|School\ Closure_{mt} = 0, \boldsymbol{W}_{mt}]$$

where conditioning variables $\boldsymbol{W}_{mt}$ include aforementioned fixed effects as well as, potentially, time-varying controls $\boldsymbol{X}_{ct}$ discussed below. Similarly, in the case of school reopening, when considering binary measures of reopening, $\widehat{\gamma}$ captures estimated mean differences conditional on week of year and municipal FEs, and any time-varying controls:

$$\widehat{\gamma} = E[Reporting_{mt}|Schools\ Reopen_{mt} = 1, \boldsymbol{W}_{mt}] - E[Reporting_{mt}|Schools\ Reopen_{mt} = 0, \boldsymbol{W}_{mt}].$$

In the case of continuous models in School reopening, γ refers to marginal changes in rates of reporting compared with rates in the same municipality and week of year, conditional on time-varying controls:

$$\widehat{\gamma} = \frac{\partial E[Reporting_{mt}|\boldsymbol{W}_{mt}]}{\partial Schools\ Open_{mt}}.$$

[10] We also consider a test which examines results stratifying by whether a municipality was ever under lockdown, further dismissing such concerns.

observed effects (Altonji et al., 2005).

As school reopenings occurred in a time-varying fashion, estimates of the effects of school re-opening may fail to recover average treatment effects (Goodman-Bacon, 2021; de Chaisemartin and D'Haultfœuille, 2020). This will occur if treatment effects are heterogeneous over time given that already treated units act as control units in periods in which their treatment status does not change (Goodman-Bacon, 2021; de Chaisemartin and D'Haultfœuille, 2020). In Appendix Results we provide the decomposition proposed by Goodman-Bacon (2021), allowing us to document the relatively low concern of this bias in this particular setting.

Interactions with School-level Attendance We additionally wish to test whether rates of reporting increase more upon school re-opening when rates of attendance are higher. If in-person contact with educational staff is key for violence to be detected and reported, criminal complaints would be expected to recover as rates of attendance increase. To test this, we estimate interactive two-way FE models, as laid out below.

$$\begin{aligned}\text{Reporting}_{mt} \quad &= \quad \alpha + \beta \text{ School Closure}_{mt} + \gamma \text{ Schools Reopen}_{mt} \\ &\quad + \delta(\text{Schools Reopen}_{mt}) \times \text{Attendance}_{mt} + \mu_{WoY} + \phi_m + \boldsymbol{X}'_{mt}\boldsymbol{\Gamma} + \upsilon_{mt}.\end{aligned} \tag{2}$$

All elements of this model follow those in equation 1, however here we additionally include an interaction between school reopening, and rates of attendance. This model can only be estimated for a shorter time period in which attendance data is consistently available (refer to section 3), and as such, when estimating this model, it is consistently displayed alongside a version which omits the interaction term to consider baseline effects in this particular sub-sample. All other estimating details follow those laid out above in equation 1.

Event Study Methods As a supporting test of assumptions, and given the importance of considering dynamics in this setting (Goodman-Bacon and Marcus, 2020), we provide as supporting results event study methods, which consider rates of reporting in the lead up to, and following changes in, school closure or school reopening policies. Such methods allow for the consideration of whether observed changes in policies actually *do* emerge only following the implementation of said policies, in which case leads to the event (pre-event estimates) act as placebo tests, and lags to the event (post-event estimates) allow for the consideration of the emergence of dynamics (Autor, 2003; Clarke and Tapia-Schythe, 2021). This consists of estimating:

$$\text{Reporting}_{mt} = \alpha + \sum_{j=2}^{J} \beta_j (\text{Lead}_j)_{mt} + \sum_{k=0}^{K} \gamma_k (\text{Lag}_k)_{mt} + \mu_{WoY} + \phi_m + \boldsymbol{X}'_{mt}\boldsymbol{\xi} + \eta_{mt}, \quad (3)$$

where leads and lags are binary variables indicating that a given municipality was a given number of periods away from the event of interest in the respective time period. Specifically, in the case of school closure:

$$\begin{aligned}
(\text{Lead}_J)_{mt} &= 1[t \leq \text{Schools Close}_{mt} - J] \\
(\text{Lead}_j)_{mt} &= 1[t = \text{Schools Close}_{mt} - j] \; \forall \; j \in \{2, \ldots, J-1\} \\
(\text{Lag}_k)_{mt} &= 1[t = \text{Schools Close}_{mt} + k] \; \forall \; k \in \{0, \ldots, K-1\} \\
(\text{Lag}_K)_{mt} &= 1[t \geq \text{Schools Close}_{mt} + K].
\end{aligned}$$

Event studies for school closure and school re-opening are estimated separately, where in the case of school re-opening, identical lags and leads are considered relative to re-opening, rather than closure. All other details of this event study follow equation 1. In each case, event studies are estimated at the level of the week, and the omitted baseline category refers to one week prior to the adoption of school closure or re-opening. When considering event studies for school closure, we consider up to J=60 leads (60 weeks prior to school closure) and K=20 lags, given that after 20 lags, school reopenings begin to occur, which potentially contaminate further lags. Inversely, in the case of re-openings, we consider J=20 leads (20 weeks prior to re-opening), as this covers periods in which schools were entirely closed, and K=40 weeks post reopening, as the majority of municipalities are observed over the entirety of this time horizon. Given that final leads and lags accumulate for all periods greater than this time, in the case of the 20$^{\text{th}}$ lead in school re-opening event studies, this will include pre-school closure periods, and similarly, the 20$^{\text{th}}$ lag in school closure event studies will include post-school reopening periods. Once again, standard errors are clustered by municipality, and population weights are consistently used.

4.2 Counterfactual Projections

While we wish to consider aggregate changes in rates surrounding closure and re-opening, a key consideration is how trends in reporting would have evolved in the absence of school closures and reopenings. Based on such counterfactual projections, we can consider the differences between actual and projected reporting rates, and additionally, what proportion of these differences can be explained by the school closure and reopening channels.

Using data on reporting incidence per 100,000 individuals aged under 18 for each of the three outcomes discussed above, we estimate such counterfactual trends from observable (pre-COVID) data as:

$$\widehat{\text{Reporting}}_{mt}^{post} = \widehat{\alpha}^{pre} + \widehat{\mu}_{WoY}^{pre} + \widehat{\phi}_{m}^{pre} + \widehat{f(t)}^{pre}, \tag{4}$$

where projected reporting in the post school closure and re-opening period in municipality m and week t, denoted $\widehat{\text{Reporting}}_{mt}^{post}$ is estimated by projecting pre-closure averages in each municipality and week of year, as well as flexible temporal trends $\widehat{f(t)}^{pre}$. Estimated week of year fixed effects $\widehat{\mu}_{WoY}^{pre}$ allow us to capture cyclical (within year) variation, while flexible time trends allow us to capture secular changes in reporting over time. We discuss the selection of prediction periods and modeling of secular trends below. A key factor of this counterfactual projection is that it estimates all coefficients and fixed effects entirely off pre-COVID data, allowing for the projection of such trends into the post-COVID period, abstracting from the actual effects of COVID and school closure on violence reporting.

Based on real and projected trends, we calculate differences between real and 'expected' reporting rates in the absence of COVID and school closures. This is simply:

$$\text{Difference}_{mt}^{post} = \text{Reporting}_{mt}^{post} - \widehat{\text{Reporting}}_{mt}^{post} \tag{5}$$

Note that here, we can calculate a difference *for each* municipality and week, thus allowing a fine-grained consideration of how school re-opening allows for recovery, or lack thereof, to reporting trends in the pre-COVID world. The quantity $\text{Difference}_{mt}^{post}$ thus captures the reporting shortfall (or excedent if positive), measured in cases per 100,000 individuals aged under 18, as the actual rate of reporting observed, compared with the expected rate of reporting based on counterfactual projections. In the body of the paper, we consider total differences in reporting measured as *absolute case* differentials at a national level in each time period t by converting these per capita reporting differentials into total reporting differentials, and aggregating over the entire country. This is:

$$\text{Reporting Differential}_t = \sum_{m=1}^{346} \text{Difference}_{mt}^{post} \times \frac{Population_{mt}}{100,000}. \tag{6}$$

As we calculate the Reporting Differential at each period t, we can observe in a dynamic way how this evolves, considering both pre- and post-school reopening periods.

Finally, while these projections allow us to consider reporting differentials between real and counterfactual reporting, they do not allow us to determine the contribution of school closures and openings to this observed differential. Thus, to consider this differential, we re-estimate counterfactual reporting, however now controlling additionally for the channel of school closures and re-openings. This is, we calculate a counterfactual reporting projection, based on secular and cyclical trends in the pre-COVID period, but also accounting for the decline in reporting owing to school closures. This is calculated as:

$$\widehat{\text{Reporting}}_{mt}^{post}\Big|_{SO} = \widehat{\alpha}^{pre} + \widehat{\mu}_{WoY}^{pre} + \widehat{\phi}_{m}^{pre} + \widehat{f(t)}^{pre} + \widehat{\delta}\text{School Opening}_{mt}, \tag{7}$$

where all details follow equation 4, but we additionally control for School Opening$_{mt}$ (SO), which takes the value of 1 while schools are fully open, 0 while schools are fully closed, and then the proportion of students whose school is re-open upon school re-opening periods. We follow identical procedures in calculating reporting differentials from equation 6, however now conditional on School Opening channels, and document relative movements when calculating the unconditional counterfactual in equation 4 and the conditional counterfactual in equation 7 to estimate the proportional contribution of school closures to reporting differentials. In Section 5 we document robustness to the inclusion of time-varying epidemiological and lockdown controls discussed previously.

Model Selection Model selection following equation 4 requires the specification of the secular time term $f(t)^{pre}$, and additionally the selection of periods over which pre parameters should be estimated. In the case of secular time trends, we consider three alternative cases. The first is a case with no trend, equivalent to specifying $f(t)^{pre} = 0$, thus simply using week of year fixed effects to capture temporal dynamics. The second is a case with a linear trend in time equivalent to specifying $f(t)^{pre} = \alpha t^{pre}$, and projecting forward any pre-existing trends when calculating $\widehat{\text{Reporting}}_{mt}^{post}$. And the third is a case with a quadratic trend in time, namely $f(t)^{pre} = \alpha_1 t^{pre} + \alpha_2 (t^{pre})^2$, projecting forward any pre-existing non-linear trends when calculating $\widehat{\text{Reporting}}_{mt}^{post}$.

As we observe pre-COVID data over a long time horizon (refer to Appendix Figure A4), we can estimate $\widehat{\text{Reporting}}_{mt}^{post}$ using a range of time windows. We consider a number of options, beginning in 2015, 2016, 2017 or 2018. The benefit of using a shorter time horizon is that it may provide us a more adequate estimation of cyclical trends in working with week of year fixed effects in years closer to the time period of interest, while the benefit of working with longer timer horizon is that it may provide a more adequate estimation of secular trends in observing macro changes in reporting rates over a longer time horizon.

While we consider projections using each time period as an estimation window, and each functional form to capture secular trends, we report as our main model that which provides the best fit over the final pre-COVID period of 2019, and January, February of 2020. This model is chosen (for each of the three outcomes studied) as the models which minimizes the following Root Mean Squared Prediction Error (RMSPE), where for ease of notation we write 2019, whereas in practice we additionally use the first two months of 2020: $RMSPE = \sqrt{\left(\text{Reporting}_{mt}^{2019} - \widehat{\text{Reporting}}_{mt}^{2019}\right)^2}$. Later in the paper, we provide full results documenting the sensitivity of reporting differentials to each of the potential alternative models, additionally reporting the RMSPE in each case.

Testing Sensitivity to Changes in Violence This counterfactual activity assumes that we can infer projections based on levels and trends in reporting prior to the arrival of COVID-19 and associated school closures. However, a broad stream of literature (Evans et al., 2020; Bullinger et al., 2020; Bhalotra et al., 2021; Erten et al., 2022; Pereda and Díaz-Faes, 2020) suggests that violence may have in fact increased, suggesting that counterfactuals may actually under-estimate the true expected reporting if rates of reporting had been maintained constant. We thus conduct additional sensitivity testing, where Reporting Differentials are calculated under less conservative assumptions, where we project that violence, and hence violence reporting, would have actually increased in the post COVID period. This consists of increasing counterfactual reporting by fixed rates, for example by 10%, as below:

$$\text{Difference}_{mt}^{post,\Delta 10\%} = \text{Reporting}_{mt}^{post} - \left(\widehat{\text{Reporting}}_{mt}^{post}\right) \times 1.10. \tag{8}$$

We consider a wide range of sensitivity values ranging from 0 to as much as a 40% increase. We display Reporting Differentials following equation 4 under these alternative sensitivity assumptions.

Inference Finally, in conducting inference on these projections, we must take account of the fact that counterfactual outcomes are estimated based on observed data, and hence are subject to sampling uncertainty inherent in this estimation procedure. To conduct inference, we undertake a block bootstrap procedure, resampling over Chile's 346 municipalities to maintain the time-series dependence within each municipality.

5 Results

5.1 Impacts of School Closure and Reopening on Violence Reporting

Table 2 reports estimates from equation 1. Columns (1), (4) and (7) document models which simply capture changes in the time series rates of reporting, while columns (2)-(3), (5)-(6) and (8)-(9) report estimates from two-way fixed effect (FE) models with or without controls, which capture all time-invariant municipal-specific factors as municipality FEs, and cyclical components as week of year FEs. As laid out in Section 4, identifying assumptions to causally estimate effects are that conditional on included controls, no other relevant events occur in affected municipalities at precisely the same moment as policy changes (parallel-trend style assumptions). We point to suggestive evidence in favour of such assumptions below.

Focusing on baseline two-way FE models in Panel A, we estimate that school closure results in declines in reporting of intra-family violence by approximately 1.6 per 100,000 children per week, compared to a baseline 4.3 cases. This is in line with the sharp declines observed graphically (Figure 1). Once schools reopen, cases are observed to decline by 'only' 0.84 cases per 100,000 children. This suggests that cases sharply decline upon school closure and increase upon school reopening,[11]

[11] We formally test the difference between coefficients on closure and re-opening, and find clear evidence to suggest that even though reporting is lower than in the baseline period, it is considerably higher than during the period of full closure (p-value<0.01 in table footer).

but that this increase upon reopening is not sufficient to recover baseline rates of violence reporting. Similar patterns, albeit it with different magnitudes, are observed in the case of sexual abuse, and rape against children. In the case of sexual abuse, initial declines are estimated as 0.95 fewer cases per 100,000, while post-opening declines are more moderate, at 0.35 fewer cases per 100,000 children, while in the case of rape, these values are estimated at declines of 0.10 (closure) and 0.05 (re-opening). In each case, these values are substantial when compared with baseline rates. In the case of intra-family violence and sexual abuse against children, substantively similar results are observed even when conditioning on each municipality's lockdown status as well as rates of COVID infection, rates of COVID testing, and test positivity, suggesting that these results do not simply capture changes owing to municipal circumstances beyond school closures. In the case of rape, which is the most infrequent outcome and hence least powered outcome, while we still observe reductions in rates of criminal reporting both during school closure and re-opening, we can no longer conclude that reporting rates *increase* compared to closure periods when moving from closure to re-opening.

Results documented in Panel A of Table 2 are based on binary measures of first school reopening, however this may underplay the importance of re-opening, particularly in municipalities with many schools where reopening occurred only gradually. Panel B thus re-estimates with a measure of the continuous proportion of students whose school were re-opened, which varies between 0 (no students with open schools) to 1 (all students with an open school). It is important to note here that all coefficients on Schools Reopening are thus cast as the effect of moving all children back into school. In reality, this occurred only substantially after first reopening.[12] Here, in the case of intra-family violence reporting we observe that if school reopening does indeed become complete, rates of reporting are estimated to no longer be statistically significantly below baseline rates. Estimates fully conditioning on time-varying controls in column (3) suggest that while complaints would still be 0.26 per 100,000 children lower than in baseline periods, we cannot formally rule out that the confidence interval of this estimate contains 0, or a return to pre-closure rates of reporting.[13] As in panel A, we consistently observe sharp reporting declines with initial school closures.

[12]For example, by September 2021, 84.4% of students' schools were reopened, by October 2021, this value had reached 96.6%, and by December 2021 this value had reached 98.6%.

[13]The results between panels A and B are consistent, in that panel A refers to a binary measure of municipal reopening (regardless of the proportion of students whose schools had reopened), while panel B refers to continuous measures of students whose schools had re-opened, and estimates are interpreted as the impact of moving from 0 (full closure) to 1 (full opening), with few municipal by week cells observed with full re-opening.

Table 2: Modelled Impacts of School Closure and Re-opening on Reporting of Violence Against Children

	Intra-family Violence			Sexual Abuse			Rape		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Binary Re-opening Measure									
School Closure	-1.393***	-1.589***	-1.342***	-0.819***	-0.949***	-1.014***	-0.114***	-0.099***	-0.086**
	(0.104)	(0.126)	(0.145)	(0.067)	(0.080)	(0.095)	(0.024)	(0.027)	(0.037)
School Reopening	-0.771***	-0.843***	-0.892***	-0.306***	-0.352***	-0.622***	-0.047*	-0.050*	-0.085*
	(0.127)	(0.131)	(0.195)	(0.070)	(0.073)	(0.112)	(0.027)	(0.028)	(0.044)
Test of $\beta = \gamma$ (p-value)	0.000	0.000	0.002	0.000	0.000	0.000	0.006	0.082	0.964
Observations	54,214	54,214	54,214	53,179	53,179	53,179	53,179	53,179	53,179
Baseline Mean	4.302	4.302	4.302	2.677	2.677	2.677	0.582	0.582	0.582
Panel B: Continuous Re-opening Measure									
School Closure	-1.202***	-1.432***	-0.940***	-0.692***	-0.855***	-0.691***	-0.104***	-0.088***	-0.048
	(0.093)	(0.119)	(0.114)	(0.060)	(0.075)	(0.077)	(0.021)	(0.025)	(0.030)
School Reopening	-0.556***	-0.725***	-0.261	-0.014	-0.189**	-0.048	-0.041	-0.038	-0.024
	(0.174)	(0.171)	(0.199)	(0.093)	(0.095)	(0.117)	(0.038)	(0.039)	(0.046)
Test of $\beta = \gamma$ (p-value)	0.000	0.000	0.000	0.000	0.000	0.000	0.078	0.198	0.598
Observations	54,214	54,214	54,214	53,179	53,179	53,179	53,179	53,179	53,179
Baseline Mean	4.302	4.302	4.302	2.677	2.677	2.677	0.582	0.582	0.582
Municipal & WoY FEs		Y	Y		Y	Y		Y	Y
Lockdown & Epidemiological controls			Y			Y			Y

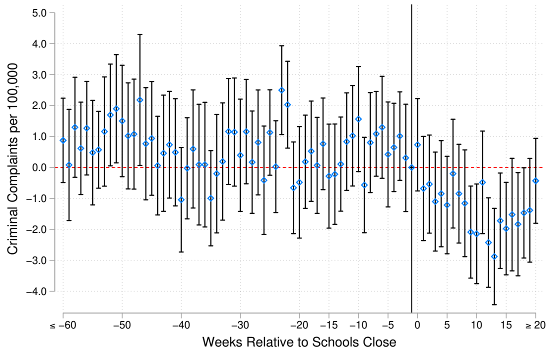
Notes to Table 2: Each column presents coefficients and standard errors from separate weighted linear regression models with alternative sets of covariates. Outcomes are consistently measured as the number of violence reports per 100,000 children per municipality and week, for each week between January 1, 2019, and December 31, 2021. Each column estimates equation 1, where School Closure is a binary indicator for periods in which schools are closed due to national decree, and Schools Reopening is a measure capturing the schools having reopened. Panel A measures re-opening as an indicator of at least one school in a municipality being open, while panel B measures re-opening as the proportion of students in the municipality whose school has re-opened. 'Test of $\beta = \gamma$' refers to coefficients in equation 1, ie the equality of estimates on School Closure and Schools Reopening. Cluster-robust standard errors allowing for arbitrary correlations of unobservable shocks over time within each municipality are presented in parentheses below coefficient estimates. *** $p< 0.01$; ** $p< 0.05$; * $p< 0.10$.

In the case of sexual abuse and rape, we once again observe sharp declines in reporting upon school closure, and evidence to suggest that re-opening may provide recovery in rates of complaints, at least compared to baseline levels (later in the paper we consider counterfactual outcomes in which rates of violence against children actually increased), once re-opening reaches 100%. In columns (6), (8) and (9) we observe that when schools fully re-opened, we cannot reject that rates of reporting would have been the same as in pre-closure periods, with all point estimates being slightly negative, but again not statistically distinguishable from zero.

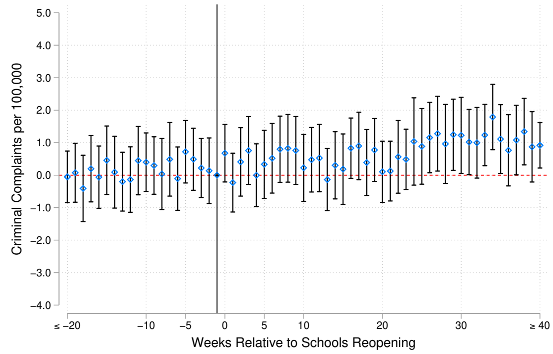
Event Study Models Event study models are displayed in Figure 3.[14] These are displayed for each of the three main outcomes, and for each of school closure and then school opening. Generally speaking, we observe flat pre-event leads prior to closure or opening events, and then changes which are sudden in the case of closures, or more gradual in the case of reopening. This is consistent with a sharp 'switching off' of the school reporting channel when schools close, and a more gradual recovery of the channel, consistent with lags in times between children returning to school, and in interaction with children and educational professionals. In one case, that of sexual abuse and school closure (panel (c)), we note considerable variation in trends in the run-up to school closure. Rather than being consistent with violations of pre-trends, this variation corresponds to cyclical variation in rates of rape-reporting observed clearly in Figure 1 (in line with lower rates of rape reports in winter and summer school vacation periods, even in the pre-school closure period). We note, additionally, that even despite this variation, rates of sexual abuse reporting are observed to be at their lowest immediately following school closure.

Attendance Interactions Even when schools re-opened, not all students went back to in-person activities. For example, by November, 2021, while almost all schools had resumed some form of in-person activities, 30% of students were still observed to not attend even one day per week. If, as we argue and has been suggested by Fitzpatrick et al. (2020), identifying violence requires time spent in school, the lack of attendance will inevitably result in a more limited recovery. We explore these patterns by interacting the school reopening measures with the proportion of attendance of students

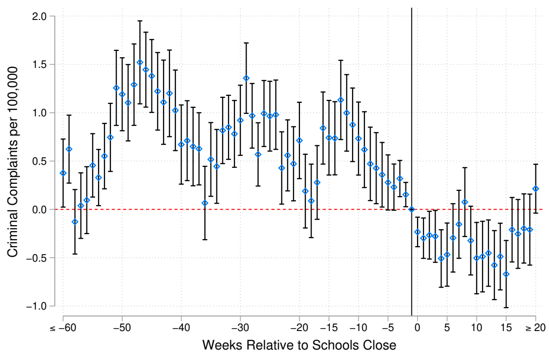
[14] Versions of the models with graduated controls are included as Appendix Figures A6-A7, and are observed to be largely unchanged.



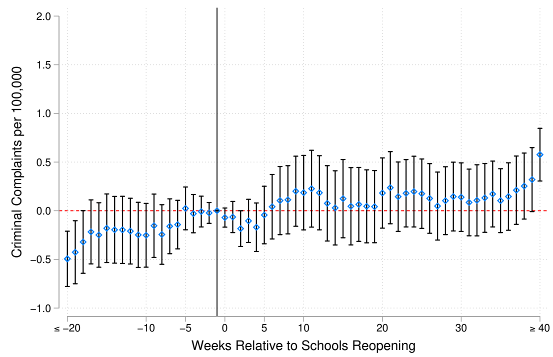
(a) Intra-family violence (closure)



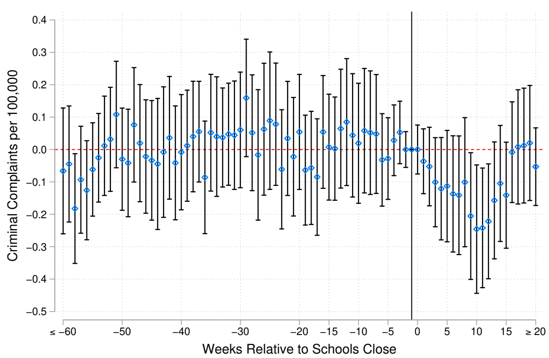
(b) Intra-family violence (opening)



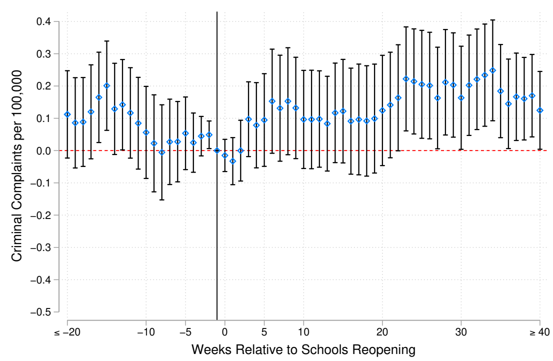
(c) Sexual abuse (closure)



(d) Sexual abuse (opening)



(e) Rape (closure)



(f) Rape (opening)

Figure 3: Event Study Estimates of School Closure and Re-opening on Reporting of Violence Against Children

Notes to Fig. 3: Event studies are documented as described in equation 3. Hollow blue diamonds display point estimates, and error bars denote 95% CIs. Here the 'event' occurring at time 0 refers to school closure in the left-hand panel, and school reopening in the right-hand panel, with period -1 (one week prior to the event) included as the omitted base period. The outcome is cases of each class of violence against minors per 100,000 minors. Pre-reopening leads are included up to 20 weeks pre-reopening, as beyond this point, schools had not yet closed. All other details follow those described in equation 3.

that were attending at least one day a week.[15]

Table 3 presents attendance interaction models described in equation 2. Here coefficients on School Reopening are interpreted as reporting declines when schools reopen, but when attendance is 0, while coefficients on School Reopening $\times$ Attendance document how reporting declines are moderated as attendance increases.[16] Across all tables, we observe evidence consistent with declines in reporting in the post school reopening period (compared with pre-school closure), but these declines become less acute as attendance increases. While these are always significant in baseline models, when including controls, although attendance interactions are consistently positive, these are at times not sufficiently precise to rule out null effects. When examining gradients in attendance in table footers we see that attendance appears to be a relevant mediator, for example in Table 3 column 6, reports of sexual abuse against minors are observed to decline by 1.0 per 100,000 minors when schools close, compared to 0.59 per 100,000 when schools open but attendance is only at the 25$^{\text{th}}$ percentile, 0.33 per 100,000 at the 50$^{\text{th}}$ percentile, 0.14 per 100,000 at the 75$^{\text{th}}$ percentile, and return to baseline rates (or 0.03 per 100,000 above baseline rates) at the 90$^{\text{th}}$ percentile.

Heterogeneity of Policy Impacts Figure 4 documents variation in estimates from Table 2 within different sectors of the population. In each case point estimates are reported along with 95% CIs, based on two-way FE models with time-varying controls (models with graduated controls are documented in Appendix Figure A8). Estimates signalled with hollow diamonds present impacts of school closure, while filled circles present impacts of re-opening. Variation in observed estimates allows us to consider how and where schools act as a safety net in cases of reporting of violence against children, and additionally, in the case of variation in quarantine status, provide a partial test of identifying assumptions as we can observe impacts independent of formal government policy responses.

Consider first the impact by age. In sub-figure (a) we observe a 'backwards J' pattern, in which the impact of school closure is largest in absolute magnitude among children in their mid teenage years, lower among older teens, and lowest among younger children. This pattern is consistent with

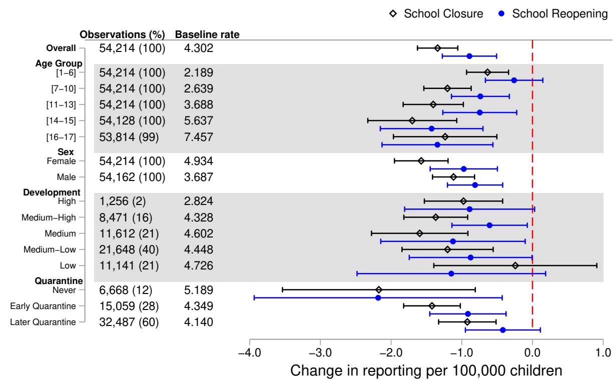
[15]As data on attendance is not observed for all periods (refer to section 3, these models are only estimated for periods in which all data are available.

[16]Table 3 follows Panel A of Table 2 in using a binary measure of school re-opening. Appendix Table A1 documents results with a continuous measure of school re-opening, where substantively similar results are observed.

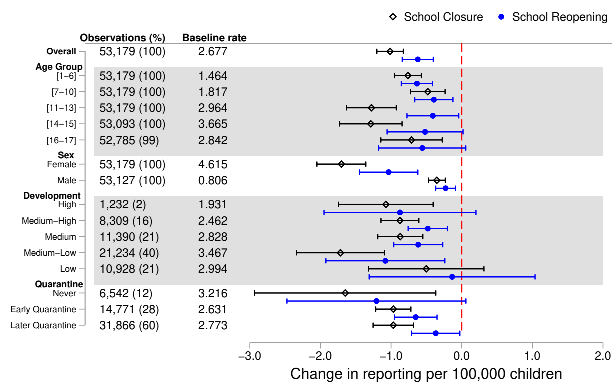
Table 3: Attendance, School Closure and School Reopening

	Intra-family Violence					
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Intra-family Violence						
School Closure	-1.381***	-1.381***	-1.533***	-1.535***	-1.253***	-1.258***
	(0.105)	(0.105)	(0.135)	(0.136)	(0.159)	(0.161)
School Reopening	-0.356**	-1.123***	-0.586***	-0.702**	-0.559**	-0.739**
	(0.156)	(0.247)	(0.182)	(0.291)	(0.246)	(0.362)
School Reopening × Attendance		1.985***		0.304		0.451
		(0.602)		(0.656)		(0.675)
Reopening Effect at Percentile 25 of Attendance		-0.671		-0.633		-0.637
Reopening Effect at Percentile 50 of Attendance		-0.166		-0.555		-0.522
Reopening Effect at Percentile 75 of Attendance		0.211		-0.498		-0.436
Reopening Effect at Percentile 90 of Attendance		0.540		-0.447		-0.361
Baseline Mean	4.302	4.302	4.302	4.302	4.302	4.302
Observations	45,802	45,802	45,802	45,802	45,802	45,802
Panel B: Sexual Abuse						
School Closure	-0.817***	-0.817***	-0.966***	-0.972***	-0.995***	-1.007***
	(0.068)	(0.068)	(0.085)	(0.085)	(0.098)	(0.098)
School Reopening	0.025	-0.847***	-0.199**	-0.536***	-0.415***	-0.823***
	(0.083)	(0.152)	(0.095)	(0.149)	(0.134)	(0.180)
School Reopening × Attendance		2.257***		0.888**		1.022***
		(0.357)		(0.365)		(0.367)
Reopening Effect at Percentile 25 of Attendance		-0.334		-0.334		-0.590
Reopening Effect at Percentile 50 of Attendance		0.240		-0.108		-0.330
Reopening Effect at Percentile 75 of Attendance		0.668		0.061		-0.136
Reopening Effect at Percentile 90 of Attendance		1.043		0.208		0.033
Baseline Mean	2.677	2.677	2.677	2.677	2.677	2.677
Observations	45,802	45,802	45,802	45,802	45,802	45,802
Panel C: Rape						
School Closure	-0.112***	-0.112***	-0.097***	-0.097***	-0.072*	-0.073*
	(0.023)	(0.023)	(0.026)	(0.026)	(0.038)	(0.038)
School Reopening	-0.022	-0.160***	-0.049	-0.074	-0.088	-0.128*
	(0.036)	(0.059)	(0.039)	(0.060)	(0.054)	(0.074)
School Reopening × Attendance		0.359***		0.065		0.100
		(0.133)		(0.140)		(0.142)
Reopening Effect at Percentile 25 of Attendance		-0.079		-0.059		-0.105
Reopening Effect at Percentile 50 of Attendance		0.013		-0.042		-0.080
Reopening Effect at Percentile 75 of Attendance		0.081		-0.030		-0.061
Reopening Effect at Percentile 90 of Attendance		0.141		-0.019		-0.044
Baseline Mean	0.582	0.582	0.582	0.582	0.582	0.582
Observations	45,802	45,802	45,802	45,802	45,802	45,802
Municipal & WoY FEs			Y	Y	Y	Y
Lockdown & Epidemiological controls					Y	Y

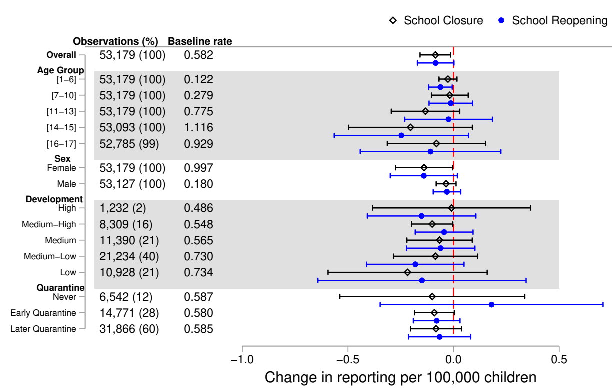
Notes to Tab. 3: Results replicate those of Table 1 from the main text for the outcome of intra-family violence against children, however here additionally interacting School Reopening measures with the proportion of attendance in each municipality by week cell. Attendance data is not observed for all periods (refer to section 3, and as such, these models are only estimated for periods in which all data are available. Columns 1, 3 and 5 present baseline models for the period in which attendance data are available, while columns 2, 4 and 6 additionally include the School Reopening by Attendance interaction. In table footers, linear estimated effects on school re-opening are reported at various margins of attendance, namely percentiles 25, 50, 75 and 90 of attendance from observed data. These percentiles are p25=26.6% attendance, p50=49.9% attendance, p75=68.2% attendance, and p90=84.7% attendance. All other details follow those laid out in Table 1. *** $p< 0.01$; ** $p< 0.05$; * $p< 0.10$.



(a) Intra-family Violence



(b) Sexual Abuse



(c) Rape

Figure 4: Modelled Impact of School Closures and Openings: Demographic and Socio-Economic Variation

Notes to Fig. 4: Estimates (diamonds and circles) and 95% confidence intervals are displayed for models corresponding to sub-groups indicated on the vertical axis. Estimates for each group correspond to coefficients on School Closure (diamonds), and Schools Reopening (circles), following equation 1, with full time-varying controls. In each case, estimates are based on the population or municipality-specific estimation sample, and estimates are consistently weighted by the population of the estimation sample. The total number of municipality by week observations are indicated in "Observations", with the % referring to the percent of the full sample of municipality by week cells. Baseline (pre-2020) rates per 100,000 individuals of each group are displayed as "Baseline rate".

schools acting to channel complaints most frequently for children above 6 who are most connected to school systems, and the fact that infants may have better access to other channels, such as the health system (refer to Appendix Table A2 for a descriptive example of this). The role of schools is also reduced when children are older (16-17), and potentially more empowered to make their own complaints (Ortiz et al., 2021). Broadly similar impacts of closure by age are observed in cases of sexual violence in panels (b) and (c). In the case of re-opening, we observe largest recoveries in rates of reporting among younger children, in line with increases in attendance patterns in these groups (Appendix Figures A9-A10). Indeed, significant increases in reporting are observed only among individuals aged 13 and under in the case of intra-family violence, and are concentrated among younger individuals in the case of sexual abuse (Appendix Figure A11).

We report estimates by each municipality's lockdown status, as an early lockdown (March 16-August 30, 2020), late lockdown (September 1, 2020 or after), or no lockdown area. In each of these three cases we observe sharp declines in rates of complaints for intra-family violence and sexual abuse (though noisier estimate for rape) suggesting that these results do not simply capture reductions in movement owing to lockdowns (or a 'lock-down effect'), but rather transversal effects of school closure on violence reporting, observed across all municipality types. Similarly, with the case of re-opening, we generally observe that declines in reporting are substantially reversed, regardless of a municipality's lockdown status.

Additional Results and Identification Checks A number of additional results are displayed in the Online Appendix to this paper. Firstly, these results hold when eliminating months of summer vacations when schools are closed (Appendix Table A3), and are virtually unchanged if we use raw measures of sexual abuse and rape with over-reporting on the first day of each month rather than smoothed measures (Appendix Table A4). If disaggregating intra-family violence by specific classifications (Appendix Table A5), we observe results are both largest in magnitude and proportion when considering moderate physical violence, followed by psychological violence, and smallest or insignificant when considering serious physical violence, consistent with these more serious cases being captured by authorities even when schools are closed (Loiseau et al., 2021).

In the case of estimated impacts of school re-opening, staggered adoption of re-opening implies that when estimating two-way FE models, units which have re-opened in the past and not changed their re-opening status in a given period will be viewed as equivalent to control units in regression models (Goodman-Bacon, 2021; de Chaisemartin and D'Haultfœuille, 2020). In cases where treatment effects are heterogeneous, this can lead to two-way FE estimates being considerably different to the underlying average treatment effect of interest (no such concern exists for school closure given that adoption occurs at a single moment in time). In Appendix Figure A12 and Appendix Table A6 we consider whether this is likely to be problematic for our global estimates documented in Table 2 of the main paper. In Figure A12 we observe that this does not appear to be a significant issue. We note that here, in general grey "x" marks which capture estimated impacts in a 2×2 DD setting are reasonably closely clustered around the red line indicating the single-coefficient estimate, and, additionally, these units—which compare already treated units with not yet treated units—are those which take the majority of weights in the aggregate estimate. In Appendix Table A6 we present summary values of weights and estimates for each of the two groups which form the aggregate estimate. We observe that nearly 80% of the estimate is generated from comparisons of interest between already treated and not-yet treated municipalities, while only around 20% of the estimate is generated off later-treated to already treated comparisons. In both the cases of intra-family violence against children and rape, effects are observed to be large and negative in the case of the prior estimate, and small or slightly positive in the latter estimate, consistent with the (negative) impact of school closure compared to the baseline period shrinking over time. In the case of sexual abuse, both estimates are observed to be negative, consistent with negative effects of school closures compared with pre-closure periods. When considering the decomposition proposed by de Chaisemartin and D'Haultfœuille (2020), we find that no units are assigned a negative weight, which is where concerns may be most serious given that treatment effects may be mis-signed. All told, these results suggest that in the case of school re-opening where adoption is staggered, the two-way FE estimates do not suffer from substantial problems flagged in de Chaisemartin and D'Haultfœuille (2020); Goodman-Bacon (2021).

Finally, we use data from a Children's Rights Protection Office (OPD for its initials in Spanish) of a large municipality in the capital city of Santiago to examine how different institutional channels changed during this period.[17] Descriptive results show that the number of referrals coming from

[17]OPDs are the institutions in charge of the prevention and early detection of children's rights violations, and either refer cases or provide interventions when a violation has been identified (Stutzin Vallejos, 2018). This local OPD collects data on all cases which they cover, including information about the type of right that has been violated, the gender and age of the victim, and–importantly for the analysis here–the institution that reported the case to the OPD. They shared this information with us subject to a privacy agreement, and we harmonized these data for the period of 2019-2021, for

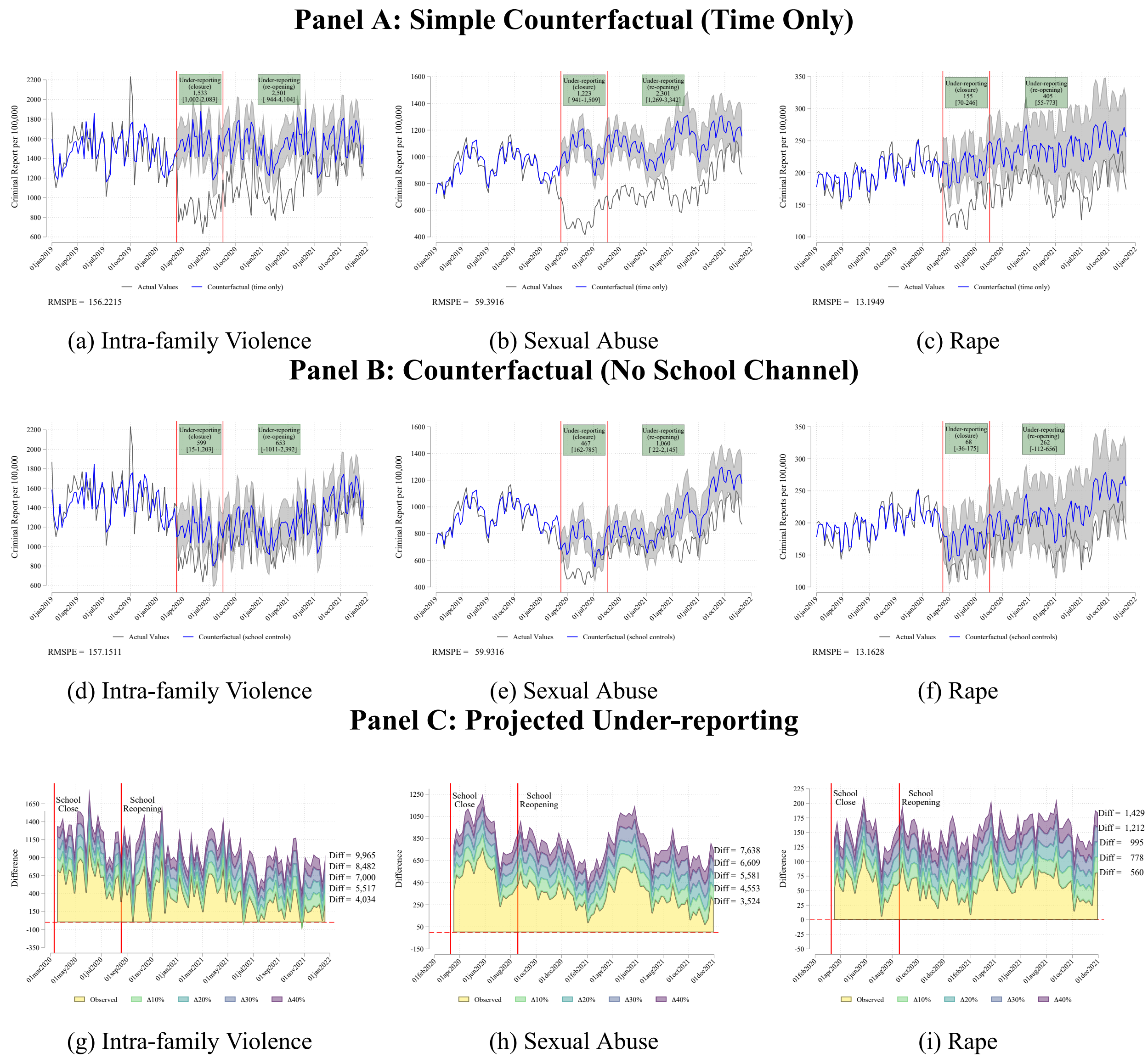
schools sharply decrease once schools closed, and they recovered, but not to pre-pandemic levels, during the second semester of 2021 (Appendix Figure A13 and Appendix Table A7). We also observed a decrease in cases reported by the health system, particularly during the first months of the pandemic.

5.2 Estimated under-reporting based on counterfactual projections

To understand the impact of school closure, as well as the dynamics of recovery, we conduct counterfactual projections which are presented in Figure 5. In panel A we document simple projections: how would complaints of violence against children perform if simply projecting optimally-chosen cyclical (week of year) and temporal trends forward estimated off the pre-pandemic period. In each case, we observe that such projections perform well in predicting in-sample (2019) and out-of-sample (2020), up until the week of school closure. We then observe sharp declines when comparing actual complaints (thin grey line), to counterfactual predictions (thick blue line). We observe that over time, these lines nearly converge, with actual reporting nearly reaching counterfactual projections, though this convergence is slow, only approaching predicted levels by around the fourth quarter of 2021, over a year after the first schools were re-opened. Indeed, when mapping estimated differences between real and projected complaints, we estimate that in the school closure period 1,533 (95% CI: 1,002–2,083) cases of intra-family violence against children were not reported, 1,223 (95% CI: 941–1,509) cases of sexual abuse against children were not reported, and 155 (95% CI: 70–246) cases of rape against children were not reported. Somewhat similar values are observed in the post-reopening period, but these are estimated over a much longer period. In weekly terms, we estimate that reductions are generally much larger during school closure than school reopening, at 64 versus 36 per week in the case of intra-family violence, 51 versus 33 in the case of sexual abuse, and 7 versus 6 in the case of rape.

Counterfactual projections in Panel A are simply based on optimally chosen historical cyclical and temporal trends, however we extend these in a number of ways. In Panel B, we consider alternative projections, however now 'turning off' the school reporting channel. This is, we consider the counterfactual outcomes based on the projections from panel A, but also concentrating out effects of

use in a number of descriptive supplementary analyses to examine in a specific case how institutional channels changed surrounding the time of school closures in this particular context.



(a) Intra-family Violence (b) Sexual Abuse (c) Rape

(d) Intra-family Violence (e) Sexual Abuse (f) Rape

(g) Intra-family Violence (h) Sexual Abuse (i) Rape

Figure 5: Reporting, Projected Reporting, and Under-reporting Under Various Counterfactual Assumptions

Notes to Fig. 5: Subfigures (a)-(c) document actual reporting (grey line) and projected counterfactuals (blue line), with 95% bootstrap confidence intervals for (a) intra-family violence, (b) sexual abuse, and (c) rape against children, following equation 4. Counterfactuals are estimated using optimal temporal trends (linear in subfigure (a), and quadratic in (b) and (c), and pre-pandemic prediction periods, with root mean squared prediction errors displayed in the bottom left corner of each panel. Subfigures (d)-(f) document identical counterfactual procedures, but now 'switching off' the school closure and reopening channel, following equation 7. Aggregate differences between real and projected reporting for the closure and reopening period along with bootstrapped 95% CIs are displayed in green squares, and week by week reporting differentials are displayed in subfigures (g)-(i), along with sensitivity testing following equation 8, in which rates of projected violence are allowed to increase in the post-pandemic period.

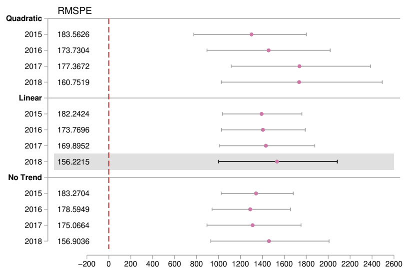
school closure and re-opening (refer to Methods). If school closure accounts for the full difference in reporting, we would expect that these counterfactual and observed trends would now entirely overlap. While we observe large movements, we do not observe that school closure can explain away *the entirety* of under-reported cases in these projections. Comparing panel (a) to panel (d), we observe that school closure can explain away 934 of the 1,533 estimated 'missing' intra-family violence reports during the school closure period, and that partial school closure can account for 1,848 of the 2,501 'missing' intra-family violence reports during the school opening period. In the case of sexual abuse and rape, we observe similar patterns, with school closure explaining between 41% (rape) to 57% (sexual abuse) of the drop in reporting observed over the entire period. In Appendix Figure A14 we document that these results are broadly similar if additionally controlling for COVID case rates, testing and positivity rates, as well as municipality lockdown status, suggesting that school closure plays a substantial and substantive role in crime reporting, which was missing upon closures.

Projections in Figure 5 of the text consider counterfactual projections generated by optimally selecting the number of pre-COVID years on which to estimate temporal trends, as well as the polynomial degree with which to estimate long-term trends. We document robustness to alternative counterfactual modelling processes in Appendix Figures A15-A18. These consider under-reporting across 12 different counterfactual models (using each of 4 potential pre-COVID baseline periods, and using no trends, linear, or quadratic trends). Total estimated under-reporting across each of these scenarios is documented in Figure 6, and shows projections are relatively stable across a range of models.[18] This is particularly clear in the case of intra-family violence against children, where under each of the 12 models considered, point estimates and confidence intervals are similar. The one case where there is slightly more variation is that of sexual abuse, given that sexual abuse appears to be following an upward trend prior to school closures, so if this trend is not included in counterfactual modeling, we observe relatively lower projected under-reporting differentials. Our preferred models (included in the paper), do a considerably better job in explaining pre-school closure trends, though we note that if such a trend had not considered during the school closure and re-opening period, under-reporting would be lower, as summarised in panels (b)-(c) and (e)-(f) of Figure 6.

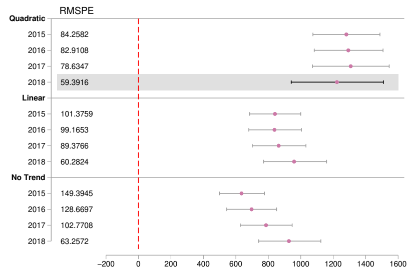
Finally, in panel C of Figure 5 we consider alternative projections where rather than assuming that historical and cyclical trends predict counterfactual (no COVID-19 related school closures) outcomes, we assume that violence may actually have increased. As discussed above and in wider literature, policy responses to the pandemic may conceivably result in increases in the risk of violence against

[18]Similar robustness checks for cases controlling for rates of school opening are provided as Appendix Figure A19.

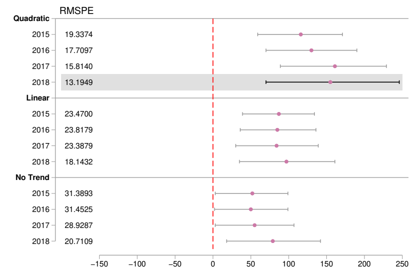
Panel A: Post School Closure and before School Reopening



(a) Intra-family violence (Time only)

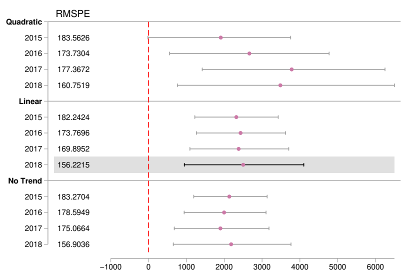


(b) Sexual Abuse (Time only)

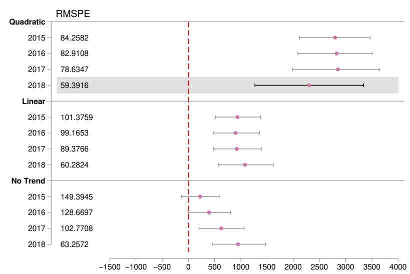


(c) Rape (Time only)

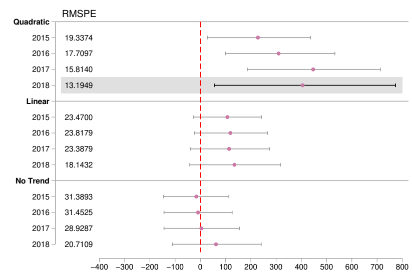
Panel B: Post School Reopening



(d) Intra-family violence (Time only)



(e) Sexual Abuse (Time only)



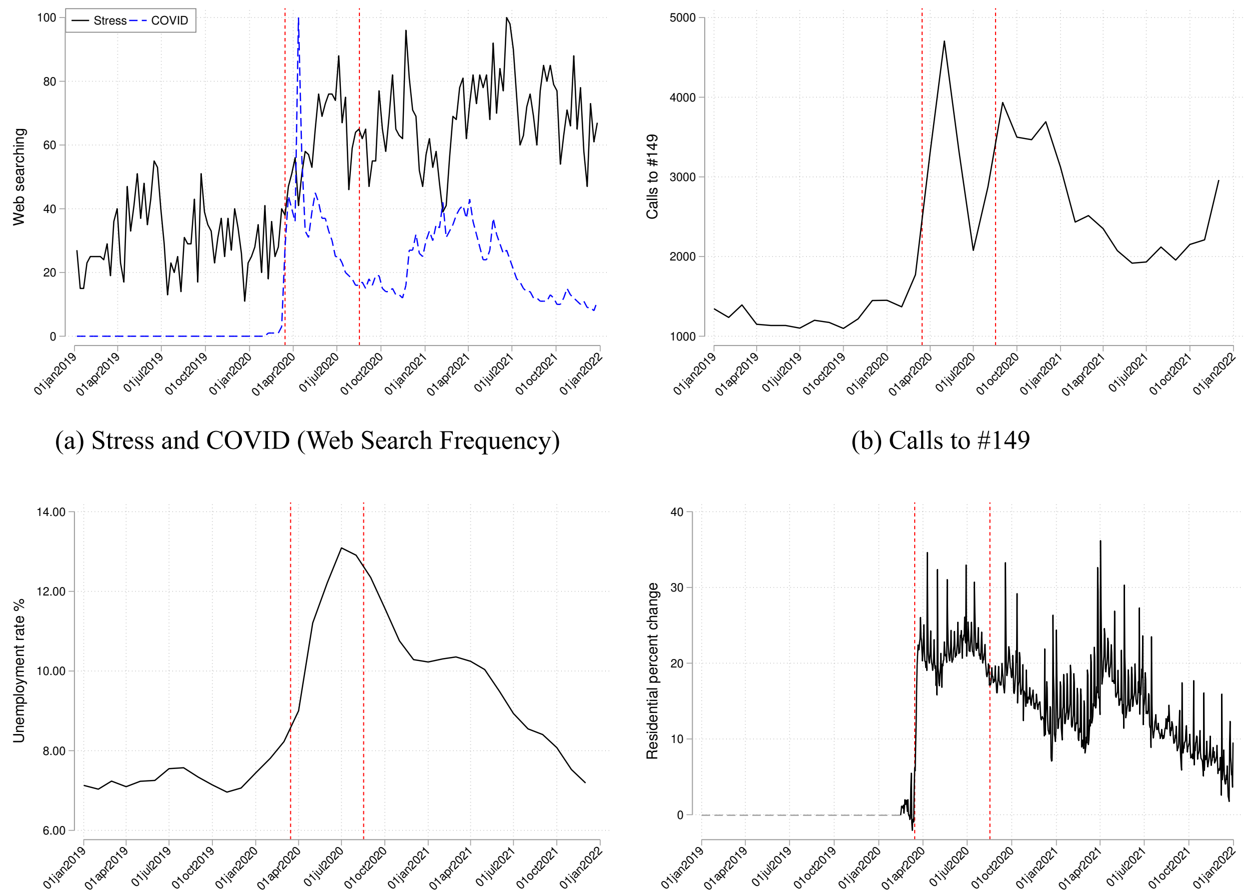
(f) Rape (Time only)

Figure 6: Projected Under-reporting under Various Counterfactual Assumptions

Notes to Fig. 6: Projected values for under-reporting of cases (pink circles), and 95% CIs (error bars) are displayed for alternative counterfactual models. In each case, total under-reporting corresponds to the total estimated "missing cases" reported in green boxes on Figure 3, however here under alternative modelling assumptions. MSE optimal models displayed in Figure 3 are shaded and indicated with thicker error bars, and Root-Mean Squared Prediction Errors are displayed in the left hand margin of each figure. In each case, we consider alternative secular trends (quadratic, linear and no trend), estimated off periods from 2015 onwards, 2016 onwards, 2017 onwards, or 2018 onwards. Panel A documents under-reporting estimates in periods of full school closure, while Panel B documents under-reporting estimates in periods of school re-opening.

children (Pereda and Díaz-Faes, 2020). Children experience higher risk of violence at home, and the most likely perpetrator are parents and other family members (Valenzuela et al., 2022; Fitzpatrick et al., 2020).[19] With stay-at-home orders in place and schools closed, families spent more time together, increasing the opportunities for violence, and decreasing the interactions with non-family members and other sources of social support. Additionally, child maltreatment is more likely to occur under situations of economic strain, financial hardship, mental stress, and family conflict (Bullinger et al., 2022; Lindo et al., 2018; Rodriguez et al., 2021). As displayed in Figure 7, the COVID pandemic increased exposure to all these factors, making an increase in violence against

[19]Our data also confirm this fact: over time, the most likely place where sexual abuse and rape against children occur is at home (Appendix Figure A20). And domestic violence happens, by definition, inside the family environment, with over 80% of the cases taking place at home.



(a) Stress and COVID (Web Search Frequency)

(b) Calls to #149

(c) Unemployment

(d) Home time

Figure 7: Trends in Other Relevant Factors

Notes to Fig. 7: Trends by day (panel (d)), week (panel (a)), or month (panels (b) and (c)) of other relevant factors in Chile are displayed over time. Panel (a) documents relative frequency of online search based on Google trends data in Chile for the term 'stress' (*estrés*) and COVID (for comparison), suggesting increases in searches related to stress following the arrival of COVID to the country. Panel (b) documents the monthly quantity of calls to the Police's Family Help Phone line (*fono familia*, # 149), which are only available until November 2021. Panel (c) documents monthly unemployment rates as reported by the Central Bank of Chile. And Panel (d) documents relative changes in the amount of time which individuals are estimated to spend in residential areas based on Google's Community Mobility Reports in the country. Vertical red lines document the data of first school closure, and first re-opening.

children a most likely hypothesis. Therefore, the models presented in Panels A and B could be read as the lower bound of reports missing due to school closure.

We do not know, however, the magnitude of the increase. A survey conducted with US parents in the first weeks of the pandemic documents that 20% of the parents report hitting or spanking their child in the past 2 weeks, with 5% reporting doing so more often than usual, while 1 in 4 recognizing an increase in conflict during that time (Lee et al., 2021). Similarly estimates accounting for factors associated with violence point to increases of up to 29% in referrals of violence against children in a specific US-setting (Prettyman, 2021b), while an increase of around 10% in the use of women's shelters in Chile was observed as a consequence of lockdowns (Bhalotra et al., 2021), pointing to an increases in violence within the household based on objective measures. In panel C, rather than projecting counterfactual and actual outcomes, we document the difference between counterfactuals and actual reporting under alternative assumptions of increases in underlying violence by {10, 20, 30, 40}%. This can be considered a bounding exercise, given that we do not know by how much true violence rates may have increased. If true rates of violence had actually increased by 10% above trend, rather than 4,034 unreported cases of intra-family violence against children in aggregate, this would rise to 5,517, with broadly similar proportional changes in the case of sexual abuse (from 3,524 to 4,553) and rape (from 560 to 778). Sensitivity of these estimates are displayed in Appendix Figures A15-A18.

6 Discussion and Conclusion

The importance of schools, and the impact of their closure during the COVID-19 pandemic has been noted across a range of outcomes including learning loss (Engzell et al., 2021; Angrist et al., 2021), child mental health (Viner et al., 2022) and inequality (Agostinelli et al., 2022; Van Lancker and Parolin, 2020). However, the results documented here make clear (a) that schools play a substantive role as a safety net in cases of violence against children, and (b) that recovering this channel has required substantial time.

An important contextual detail of this study setting is that in Chile there was significant variation in the implementation of lockdowns, infection rates, and school re-openings. Thus, we observe municipalities with very different epidemiological and public health profiles at the moment of school closures. The fact that sharp declines in reporting are observed in all settings suggests that it is unlikely that these owe to other (non-school) channels. In general, the substantial temporal and geographic variation of school opening allows us to examine the plausibility of identifying causal effects in this observational setting. Across all outcomes considered, we observe substantial changes precisely at the moment of closures and re-opening, rather than prior to policy shifts, suggesting that it is indeed changes in the availability of in person contact between students, teachers and other education professionals which drives large changes in reporting of violence against children.

Limited in-person interaction may also explain the persistent effect of school closure. While schools were mandated to close nation-wide in March, 2020, the decision to open and how to resume in-person activities was a school-level decision. While only 10% of schools had some in-person activity by December 2020, the opening process evolved at an increasing–but still gradual–pace, with 31% of schools opened by the end of the fall semester of 2021 and 98% by the end of that academic year (Figure 1). Beyond the school status, attendance remained voluntary up to March 2022, with only between 35% and 55% of students attending school each day by the end of the academic year of 2021 (Claro et al., 2021). Low attendance rates limit the ability of teachers to detect signs of abuse, and could explain ongoing declines in reporting. These results are in line with previous evidence that show that additional time spent in school leads to an increase in reporting of child maltreatment (Fitzpatrick et al., 2020; Puls et al., 2021).

While school opening results in increased rates of contagion in schools (Tupper and Colijn, 2021), and schools are still being closed as a prophylactic measure in many countries (Hale et al. (2020) and Figure A1), contagion in educational systems can be reduced if taking adequate avoidance measures (Macartney et al., 2020; Tupper et al., 2020). In contrast, the results of this study suggest that continued use of school closures imposes potentially significant costs on child well-being, even if *only* considering reduced rates of violence reporting, and that those costs remain over time, suggesting that such factors should be accounted for when weighing up the costs of school closure decisions.

These results have several policy implications. First, teachers and professionals could be trained and more staff can be hired in order to better identify violence against children, even after time has passed, as schools with more and better trained personnel have higher chances of identifying maltreatment (Baron et al., 2020; Fitzpatrick et al., 2020). Cerezo and Pons-Salvador (2004), for example, show how detection increases following training in educational settings. In the post-COVID era, this decision could promote recovery in schools' capacities to observe and channel victims of violence to legal and child protection systems. Second, the results confirm the relevance of in-person interactions for detecting cases of violence against children. Policies that encourage school attendance or generate alerts in cases of frequent absenteeism, may have an impact not only on the chances of school dropout–with all its negative consequences (Mussida et al., 2019)–but would also result in a higher likelihood of identifying maltreatment. Finally, developing and implementing alternative types of reporting channels for children who experience victimization may help in times when the ‘school reporting channel’ is not available. For example, the use of text messages or other private messaging services may provide children with an accessible and safer way of seeking help, while additionally being robust to weakened ties between children and schooling systems (Ortiz et al., 2021).

All told, these results confirm that schools act as a social safety net for children, detecting and formalising complaints for violence which otherwise may be left undetected. They do so in so far as schools provide opportunities for in-person interactions with teachers and school personnel who are able to identify signals of maltreatment and report such cases to the relevant authorities. Thus, their role in protecting children is likely substantially interrupted as schools remain closed, or attendance remains low. And, while our results suggest the this school reporting channel could be recovered after periods of closure–due to holidays, weather, or future pandemics, this recovery occurs slowly, and certainly does not appear to suggest a spike in reporting upon re-opening which would be consistent with ‘missing’ cases being channeled into the criminal system with a lag.

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Online Appendix

"Schools as Safety-nets: Break-downs and Recovery in Reporting of Violence against Children"

A1 Supplementary Figures

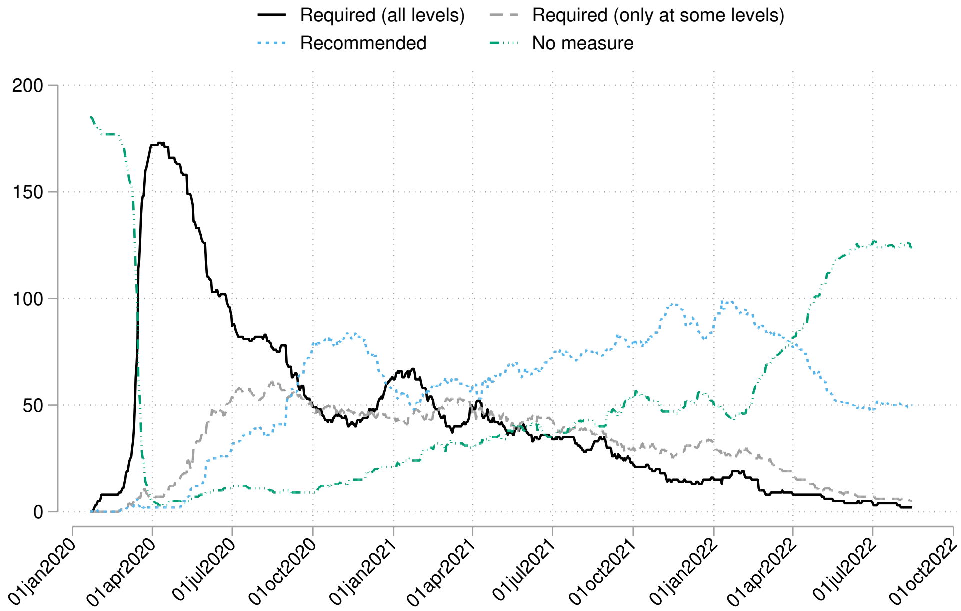


Figure A1: Country-Level School Closure Policies

Country level school closure policies by time are plotted, as classified by Hale et al. (2021).

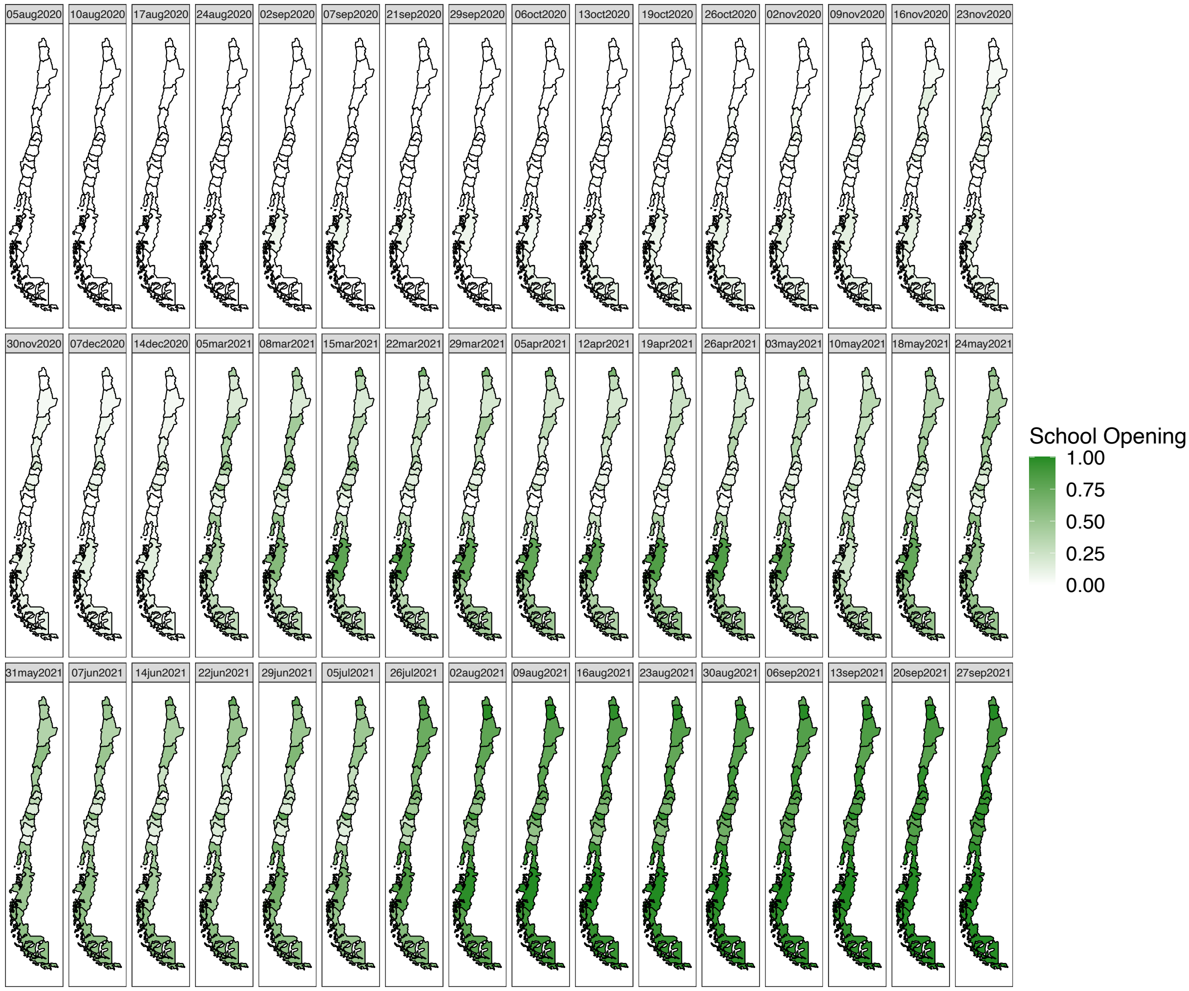


Figure A2: Administrative Records of School Re-opening Across Chile

Notes to Fig. A2: School opening is displayed across the entire of country of Chile for weeks beginning 5 August 2020 (the week of first re-opening is 17 August 2020), up to 27 September 2021. After this date, schools were nearly entirely reopened (see Figure 1). Proportion of schools re-opened are displayed at the regional level for each of Chile's 16 regions, and refers to the proportion of all students whose school is reopened based on administrative records of student enrollment, and school reopenings.

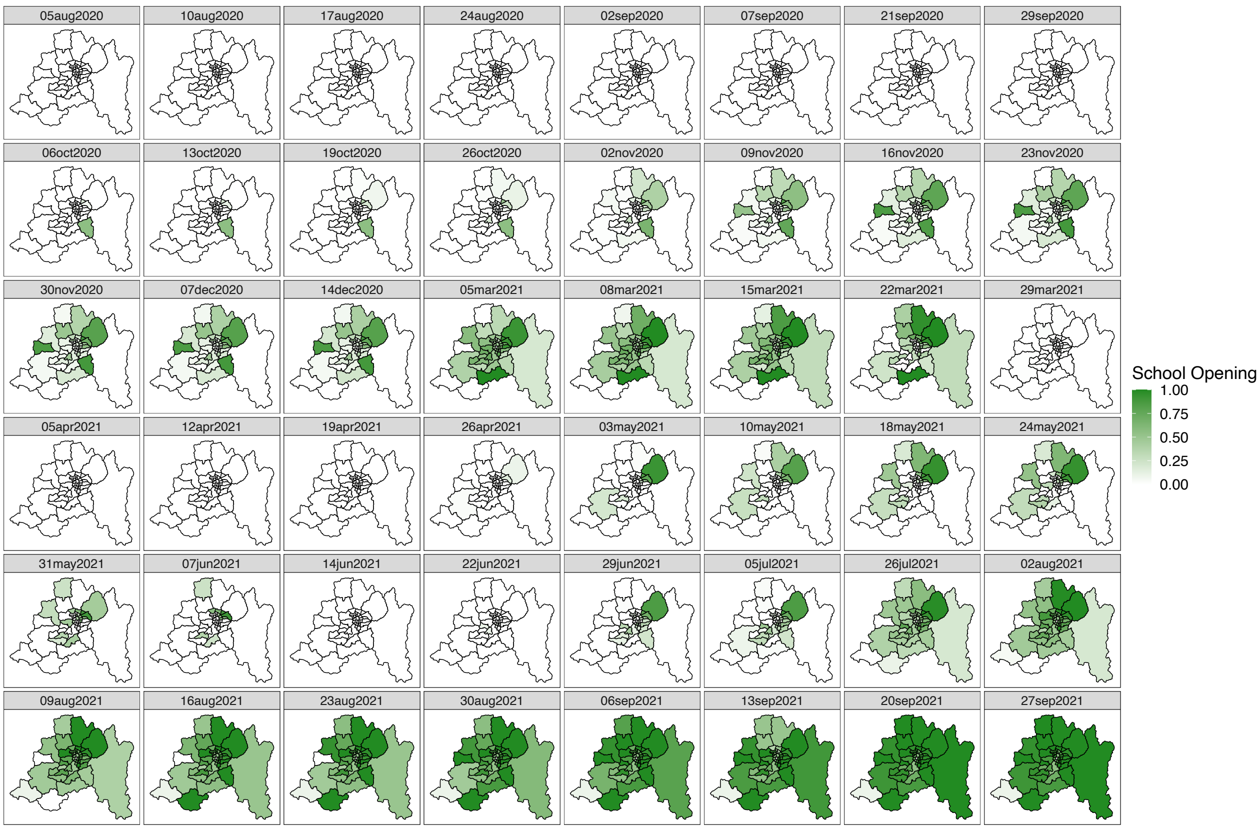
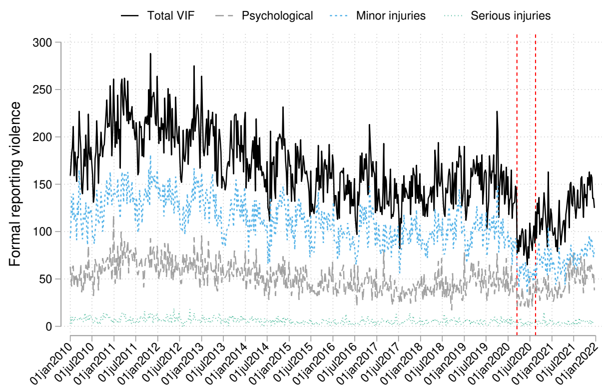
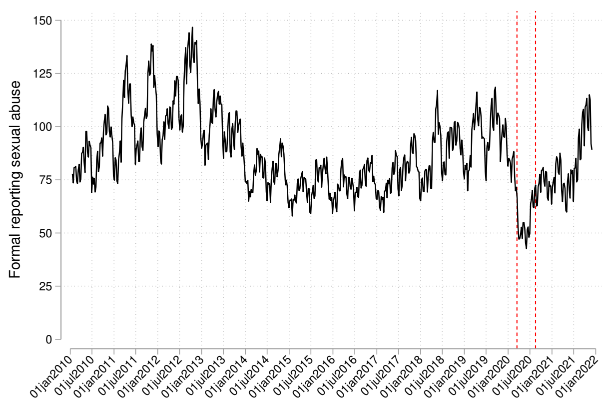


Figure A3: Administrative Records of School Re-opening Across Chile's Metropolitan Region

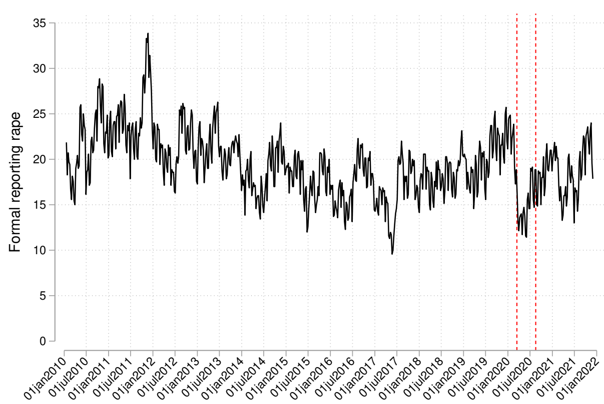
Notes to Fig. A3: School opening is displayed across the Metropolitan Region of Santiago (the capital of Chile) for weeks beginning 5 August 2020 (the week of first re-opening is 17 August 2020), and ending 27 September 2021. After this date, schools were nearly entirely reopened (see Figure 1). Proportion of schools re-opened are displayed at the municipal level for each of the Metropolitan Region's 32 municipalities (of the total of 346 municipalities in the country), and refers to the proportion of all students whose school is reopened based on administrative records of student enrollment, and school reopenings.



(a) Intra-family Violence



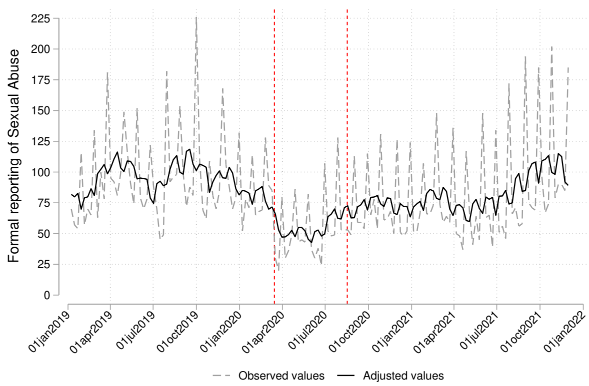
(b) Sexual Abuse



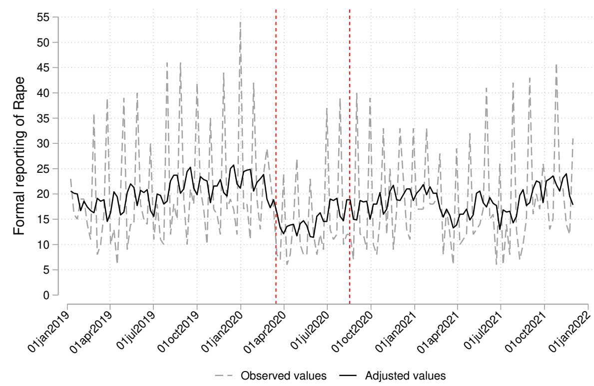
(c) Rape

Figure A4: Extended Trends: Intra-family violence, Sexual Abuse and Rape Against Minors

Notes to Fig. A4: Trends show the weekly number of formal complaints received by police related to intra-family violence (panel (a)), sexual abuse (panel (b)), and rape (panel (c)) against individuals aged under 18 years. Here, longer trends in outcomes are documented, dating from January 1, 2010 to 31 December, 2021. In main analysis, the period of January 1, 2019 to 31 December, 2021 is used. Vertical red lines denote school closures and the date of first reopening. Panel (a) additionally breaks down total intra-family violence (black solid line), into complaints classified as psychological violence, minor injuries, or serious injuries.



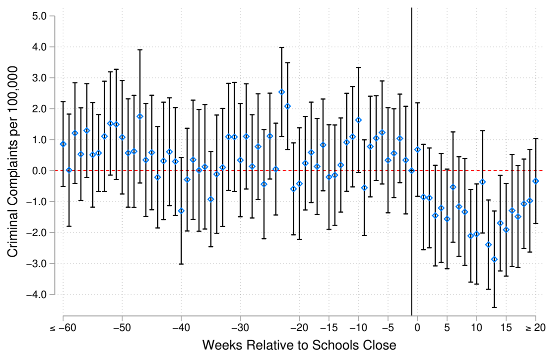
(a) Reporting of sexual assault against minors



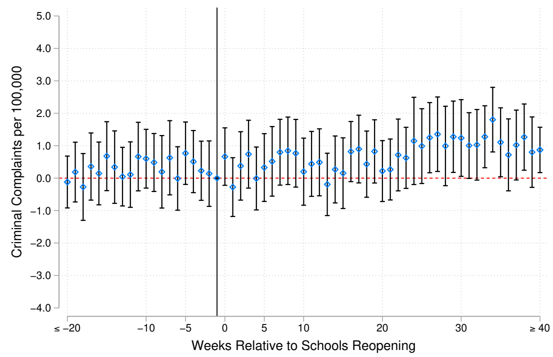
(b) Reporting of rape against minors

Figure A5: Temporal Trends – Sexual abuse and Rape against Minors, Smoothed and Unsmoothed Outcomes

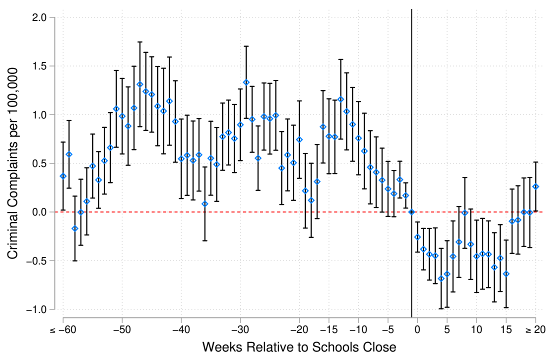
Notes to Fig. A5: Trends show the total number of cases of Sexual assault (panel (a)), and rape (panel (b)) against minors according to original records which tend to over-assign dates as the first day of each month, and smoothed records where these over assigned cases have been uniformly reassigned in each municipality within each month. In principal analysis smoothed values (solid black line) are used, as these are closer to the actual occurrence of crimes. In Appendix Results we document results using original unsmoothed measures.



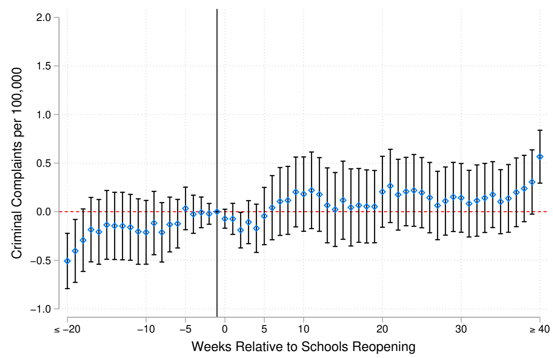
(a) Intra-family violence (closure)



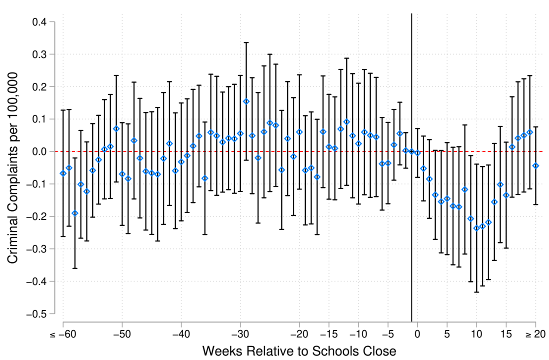
(b) Intra-family violence (opening)



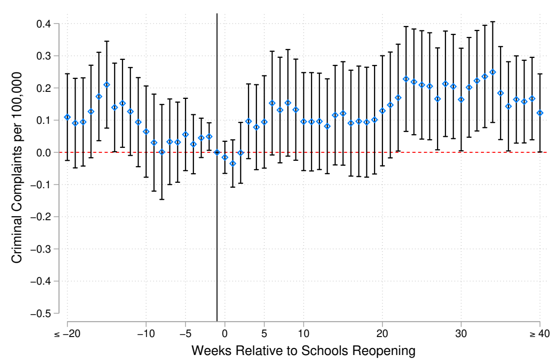
(c) Sexual abuse (closure)



(d) Sexual abuse (opening)



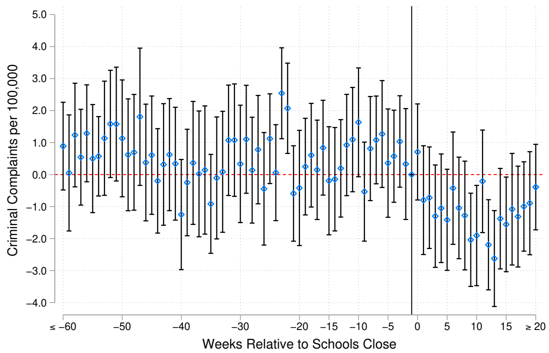
(e) Sexual abuse (closure)



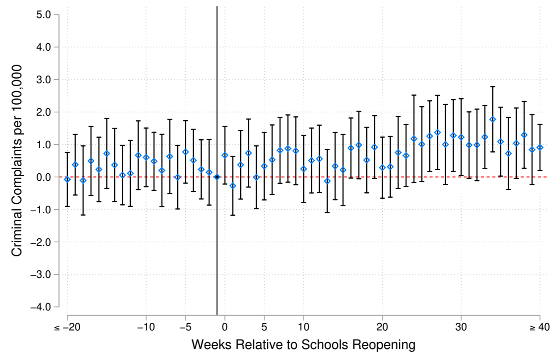
(f) Sexual abuse (opening)

Figure A6: Event Study Estimates of School Closure and Re-opening on Reporting of Violence Against Children

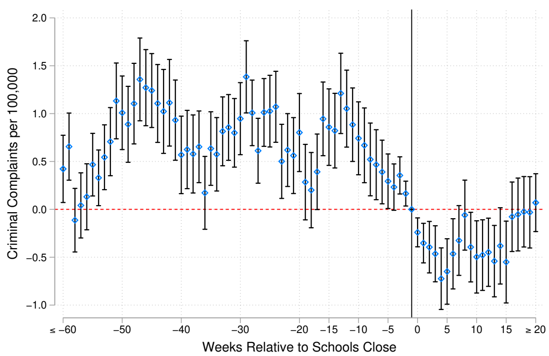
Notes to Fig. A6: Event studies are documented as described in equation 3, and details follow those in Notes to Fig. 3. Here, lock-down controls are consistently included. All other details follow those described in equation 3.



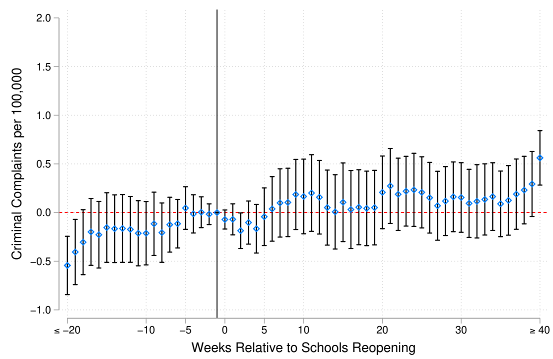
(a) Intra-family violence (closure)



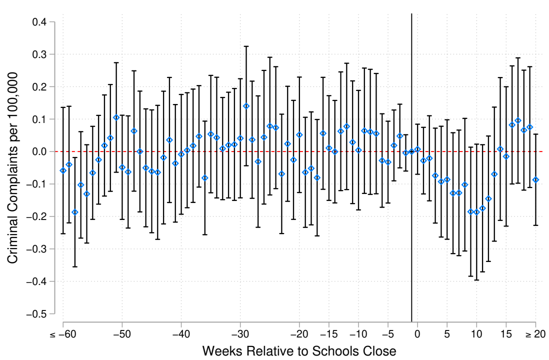
(b) Intra-family violence (opening)



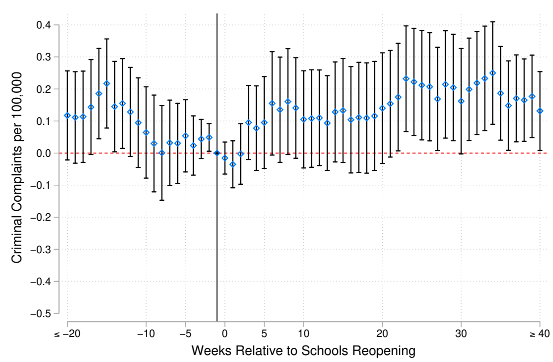
(c) Sexual abuse (closure)



(d) Sexual abuse (opening)



(e) Sexual abuse (closure)



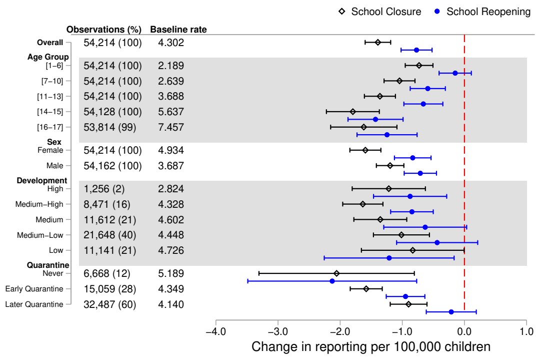
(f) Sexual abuse (opening)

Figure A7: Event Study Estimates of School Closure and Re-opening on Reporting of Violence Against Children

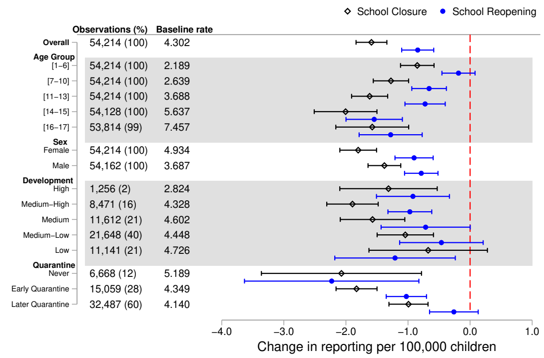
Notes to Fig. A7: Event studies are documented as described in equation 3, and details follow those in Notes to Fig. 3. Here, lock-down and COVID-intensity controls are consistently included. All other details follow those described in equation 3.

No controls

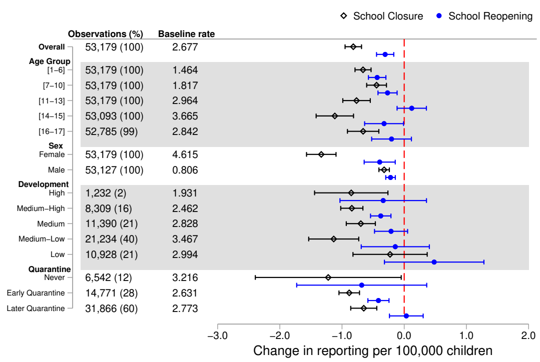
Baseline FEs



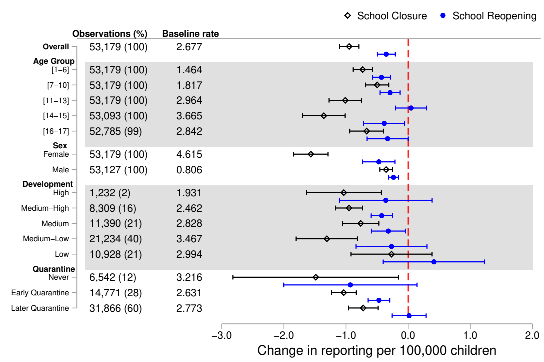
(a) Intra-family Violence (No controls)



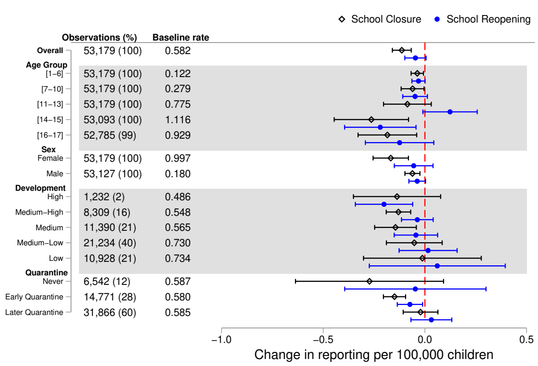
(b) Intra-family Violence (Baseline FEs)



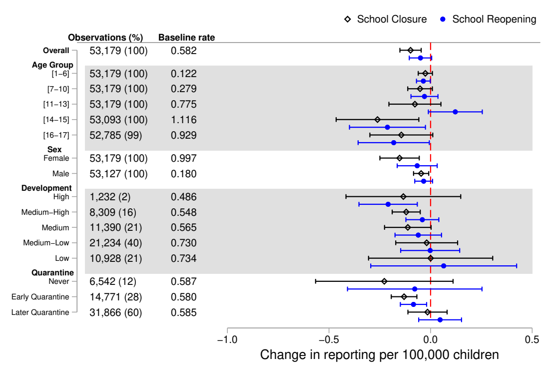
(c) Sexual Abuse (No controls)



(d) Sexual Abuse (Baseline FEs)



(e) Rape (no controls)



(f) Rape (Baseline FEs)

Figure A8: Alternative Specifications of Demographic and Socio-economic Variation in Closure/Reopening

Notes to Fig. A8: Figure replicates results of Figure 4 of the main analysis, however here omitting all controls (left-hand column) and controlling only for municipal and week of year FEs (right-hand column). These estimates correspond to heterogeneity in estimates of columns (1), (4) and (7) of Table 2 (in panels (a), (c) and (e) respectively) and columns (2), (5) and (7) of Table 2 (in panels (b), (d) and (f) respectively). Refer to Notes to Figure 4 for further details.

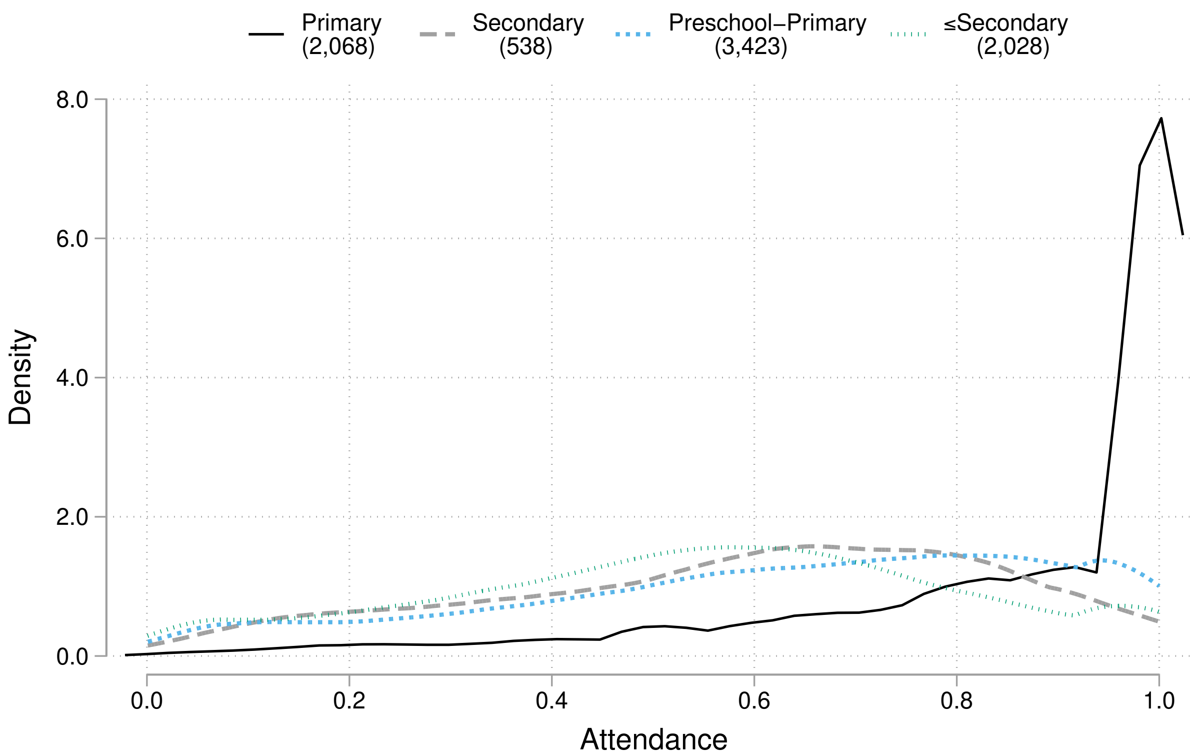
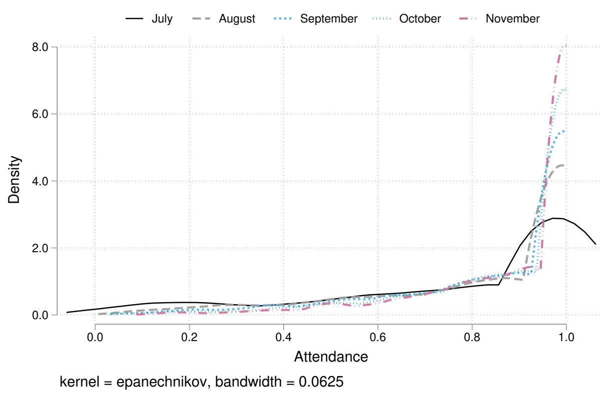
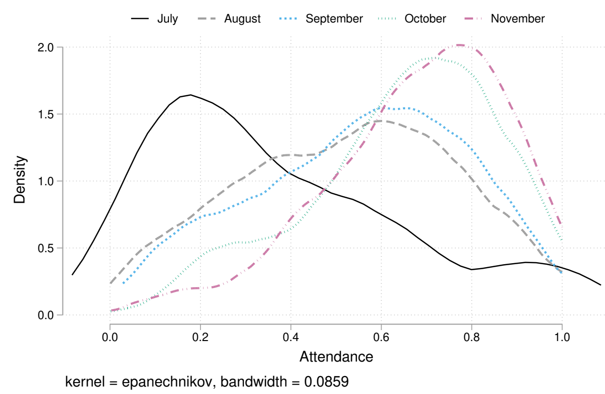


Figure A9: School Attendance Proportions by Education Level in School Reopening Periods

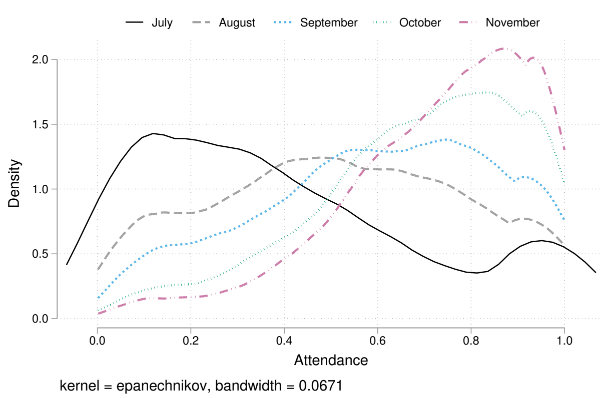
Notes to Fig. A9: Kernel densities of the proportion of individuals observed to attend school at least one day in each month are displayed by school type. Here densities are documented over all schools in each month for which attendance data is available and in which any attendance occurs. Values of 1 imply that all students attend school (at least 1 day) in a given school in a given month. Schools are stratified by level as Primary (only covering primary classes), Secondary (only covering Secondary classes), Preschool-Primary (covering both pre-school and primary, but not secondary), and $\leq$ Secondary refers to schools which have secondary as well as earlier levels (eg secondary and primary levels). Numbers below school types refer to the total number of each type of school in the country (a number of schools are not included here, as these are registered for adult learning, or as special education, and potentially cover all grades).



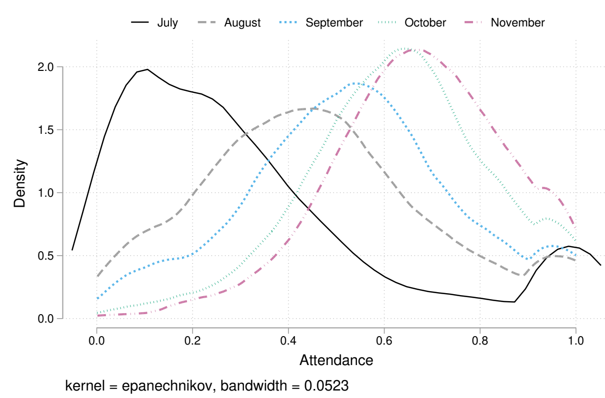
(a) Primary level



(b) Secondary level



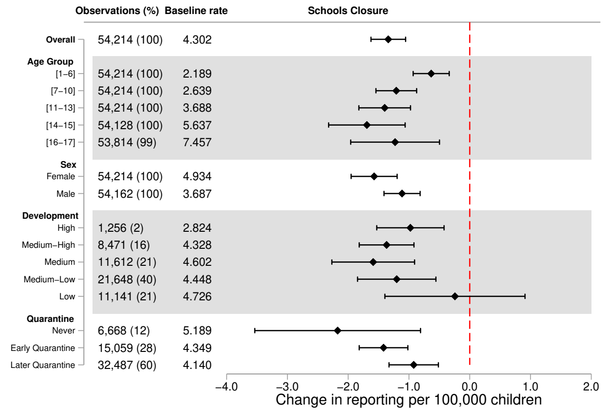
(c) Preschool and primary level



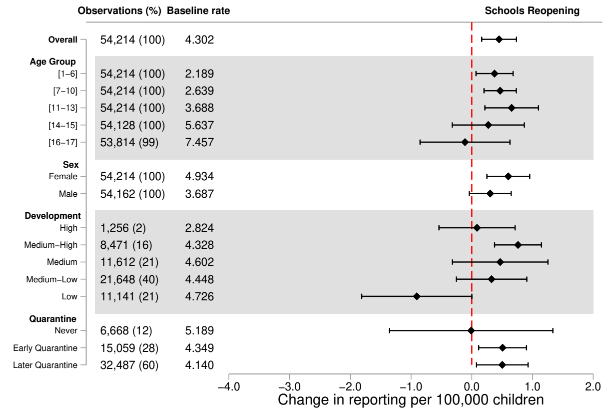
(d) Less than or equal to secondary level

Figure A10: School Attendance Proportion by Education Level and by Month in School Reopening Periods

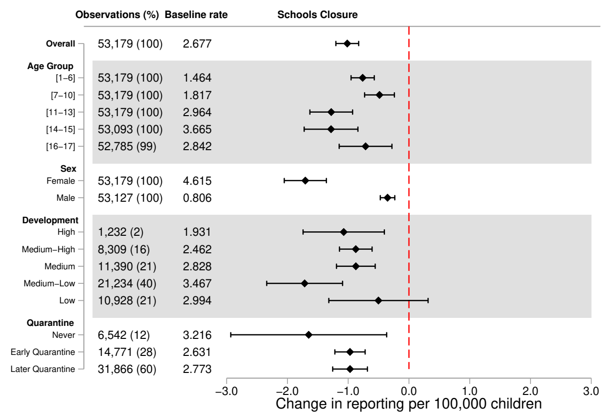
Notes to Fig. A10: Refer to notes to Figure A9. Here similar school attendance frequency distributions are documented, however now a single plot is provided for each education level, where densities are observed over all schools of each type in each re-opening month for which attendance data is available (July–December, 2021).



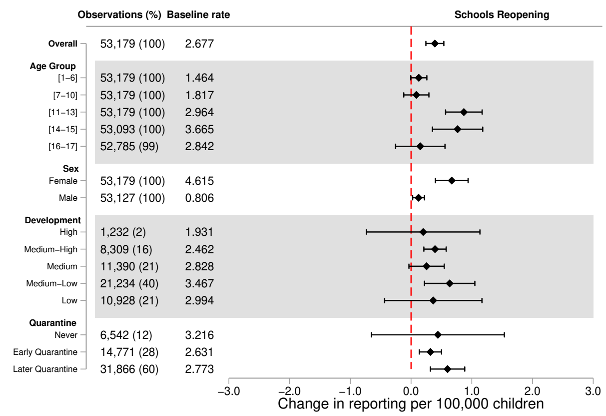
(a) School Closure: Intra-family Violence



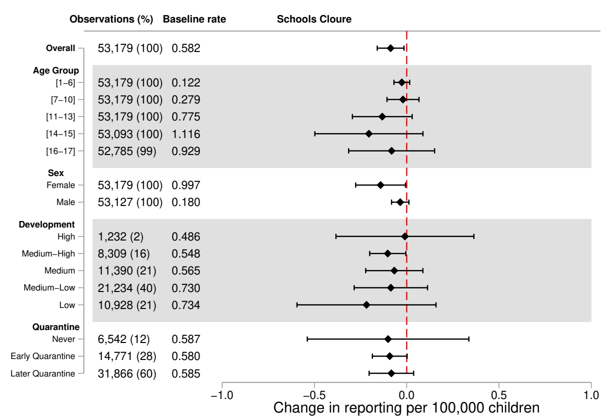
(b) School Opening: Intra-family Violence



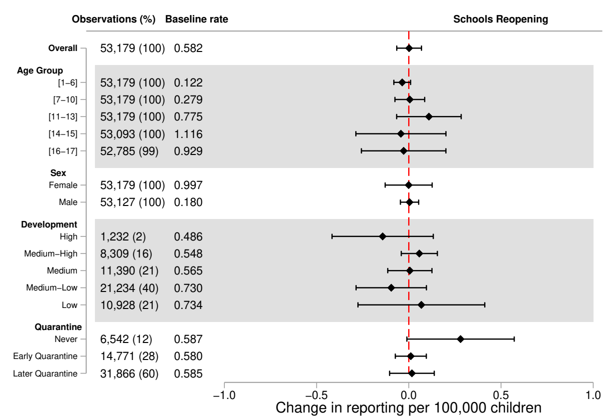
(c) School Closure: Sexual Abuse



(d) School Opening: Sexual Abuse



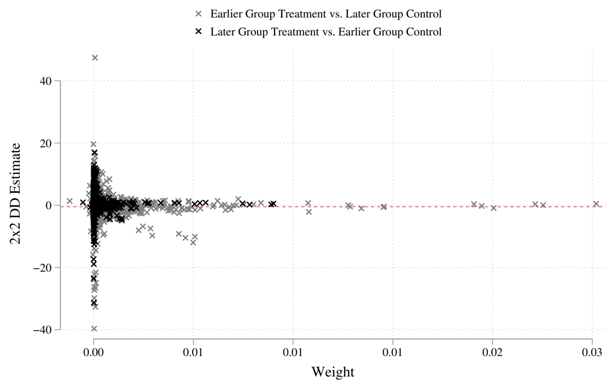
(e) School Closure: Rape



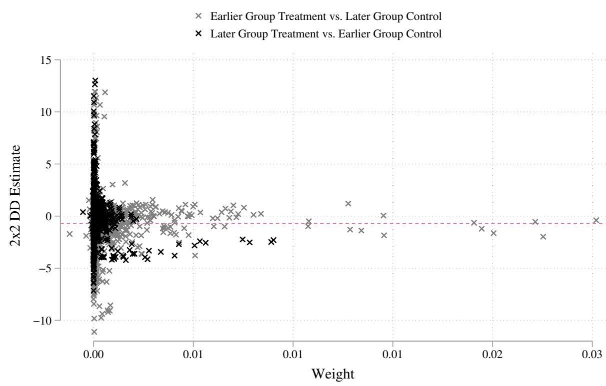
(f) School Opening: Rape

Figure A11: Tests of Differences between Re-opening and Closing in Demographic and Socio-economic Heterogeneity Analyses

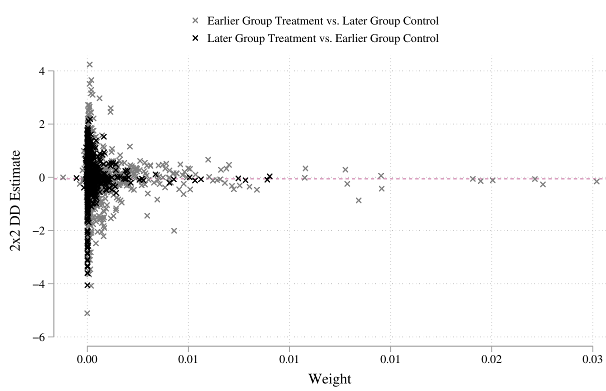
Notes to Fig. A11: Results replicate those of Figure 4 of main analysis, however now rather than comparing each of closure and re-opening to baseline periods, the effect of re-opening is compared to the closure period. Left-hand panels present the impact of school closure as compared to pre-closure periods (replicating black diamonds and CIs from Figure 4), while right-hand panels present the impact of school re-opening, compared to closure periods. Thus, if estimates in the right hand panel are above zero, this implies a significant increase in reporting in this group in the post-reopening period, compared with changes observed in the right-hand panel. All other details follow those in Figure 4.



(a) Intra-family Violence



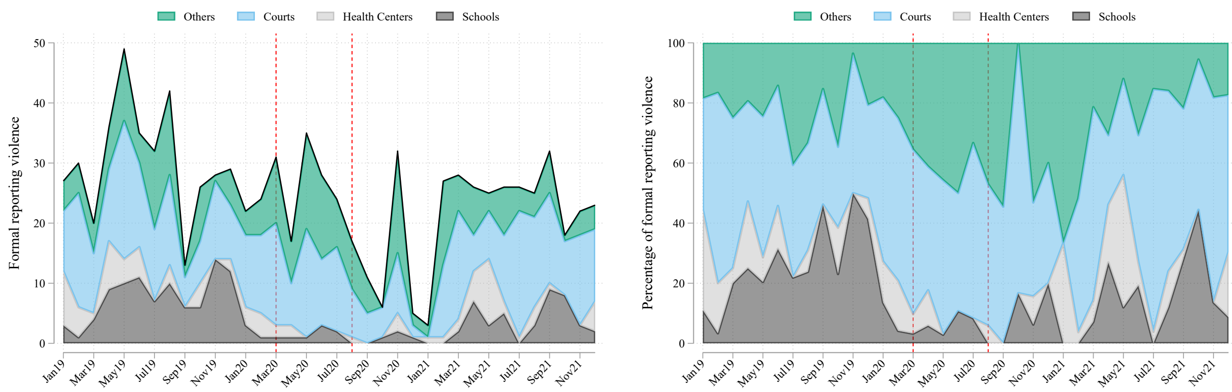
(b) Sexual Abuse



(c) Rape

Figure A12: Weights and 2×2 Double-Difference Estimates in Two-way Fixed Effect Models

Notes to Fig. A12: Plots document the double-difference decomposition laid out by (Goodman-Bacon, 2021) to decompose single coefficient estimates on School Reopening displayed in Table 2 of the paper. Here, each cross displays the proportional weight of each municipal×week switching estimate (horizontal axis), as well as the DD estimate for each switching pair (vertical axis), compared with the single-coefficient estimate (dotted horizontal line). Grey crosses represent individual estimates based on the (desired) comparison of treated to not yet treated units, while black crosses represent comparisons of later switchers to earlier switchers.

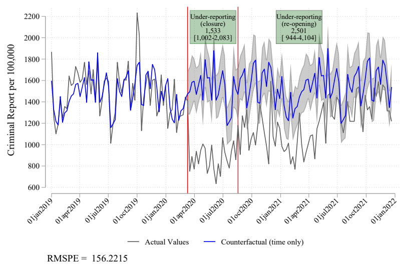


(a) Violence Reporting by Reporting Channel (b) Percentage of Violence Reporting by Reporting Channel

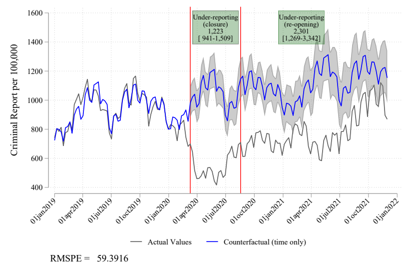
Figure A13: Actual Reporting Channels Reported by a Single Child Protection Office in a Large Municipality

Notes to Fig. A13: All reports of violence received by the child protection office of a large municipality in Santiago are displayed. These are broken down by reporting channels as from schools, from municipal health care centres, from courts and from other sources. Total numbers of cases by month in this municipality are displayed in the left-hand panel, and absolute proportions are displayed in the right-hand panel. Vertical dotted lines represent dates of school closure and school reopening in the municipality.

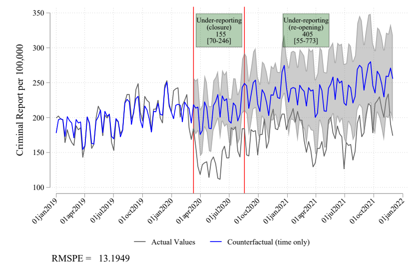
Panel A: Simple counterfactual (time only)



(a) Intra-family Violence

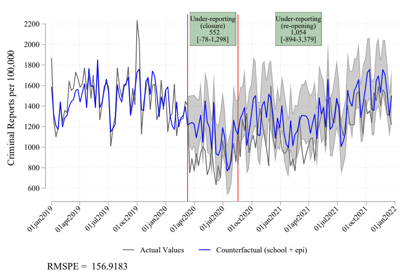


(b) Sexual Abuse

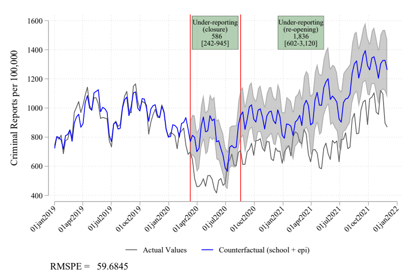


(c) Rape

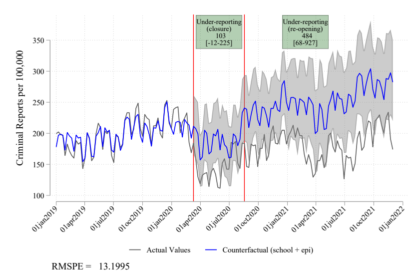
Panel B: Counterfactual (No school channel + Epidemiological controls)



(d) Intra-family Violence

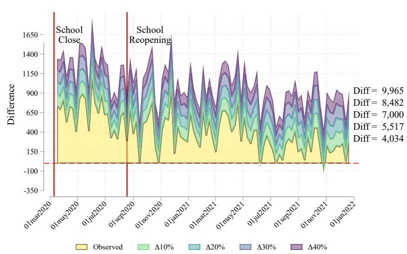


(e) Sexual Abuse

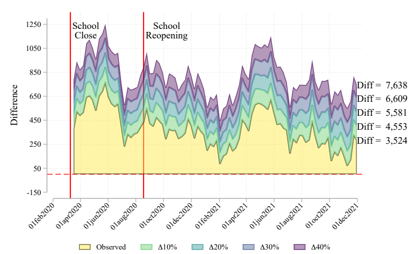


(f) Rape

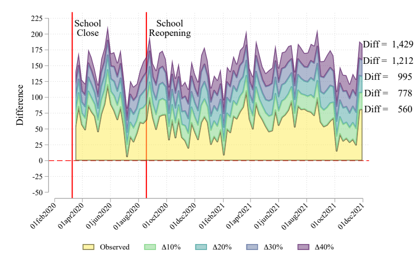
Panel C: Projected under-reporting



(g) Intra-family Violence



(h) Sexual Abuse

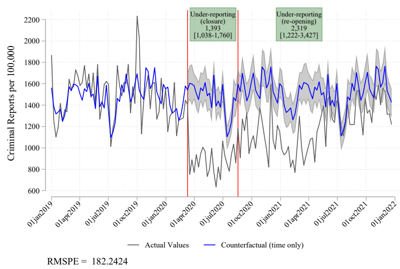


(i) Rape

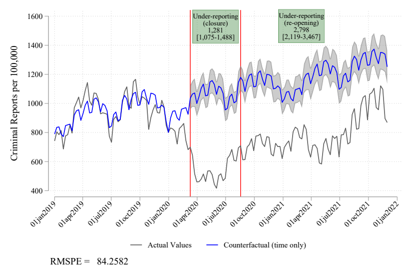
Figure A14: Alternative Counterfactual Models – Including Additional Controls

Notes to Fig. A14: Results replicate those of Figure 5 of main analysis, however here in Panel B additionally include epidemiological controls used in all main models, as well as controls for the schooling channel documented in Figure 5. Panels A and C are identical, and are displayed in the interest of comparison. All other details follow Figure 5.

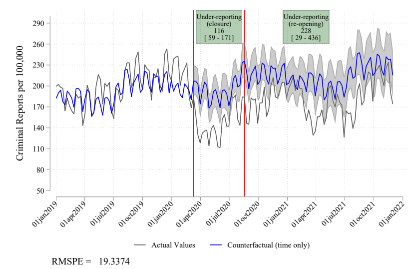
Panel A: Simple counterfactual (time only)



(a) Intra-family Violence

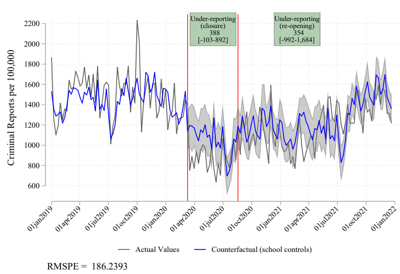


(b) Sexual Abuse

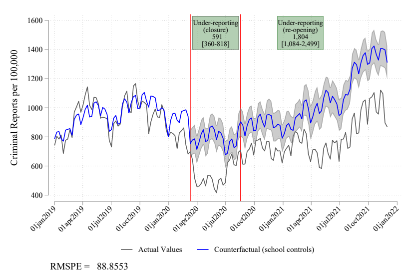


(c) Rape

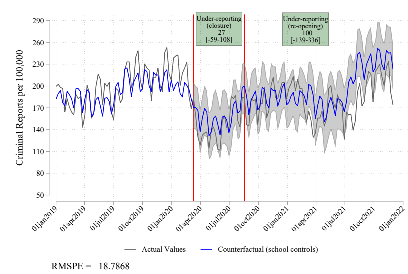
Panel B: Counterfactual (No school channel)



(d) Intra-family Violence

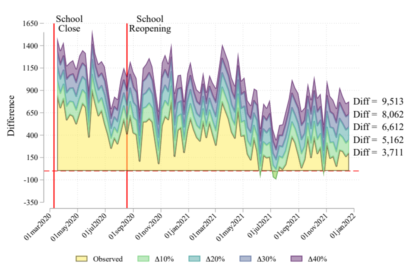


(e) Sexual Abuse

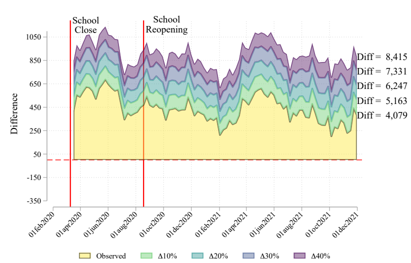


(f) Rape

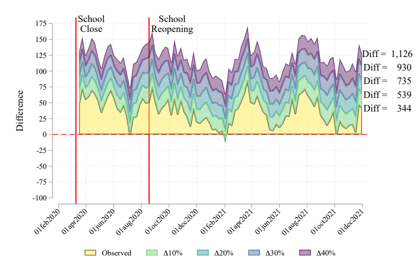
Panel C: Projected under-reporting



(g) Intra-family Violence



(h) Sexual Abuse

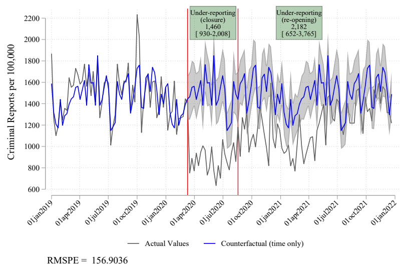


(i) Rape

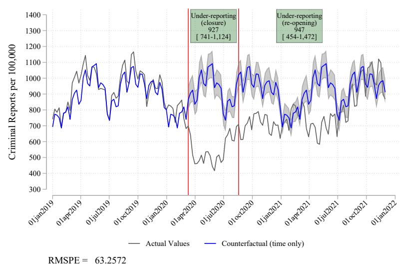
Figure A15: Alternative Counterfactual Models – Optimal Based on 2015 Onwards

Notes to Fig. A15: Alternative projections and reporting differentials are reported, where rather than basing projections off MSE optimal models choosing both the length of pre-COVID prediction years as well as secular trends, optimal secular trends are chosen, in each case using all years from 2015 onwards. Refer to notes to Figure 5 for further details.

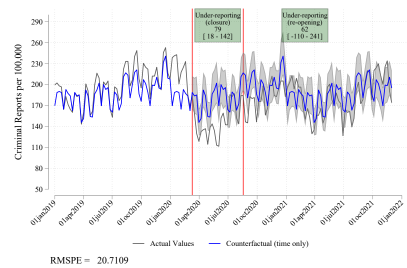
Panel A: Simple counterfactual (time only)



(a) Intra-family Violence

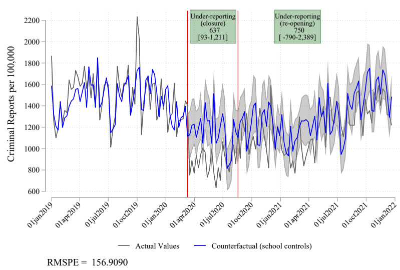


(b) Sexual Abuse

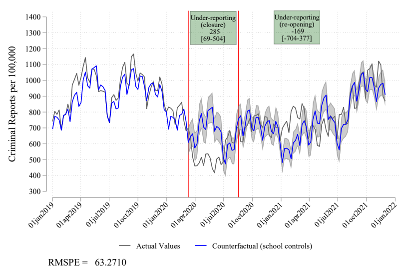


(c) Rape

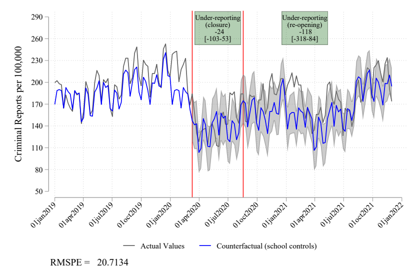
Panel B: Counterfactual (No school channel)



(d) Intra-family Violence

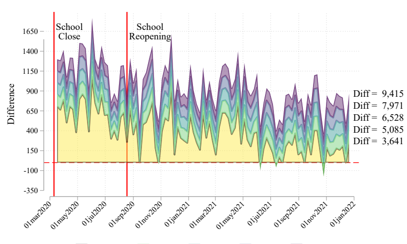


(e) Sexual Abuse

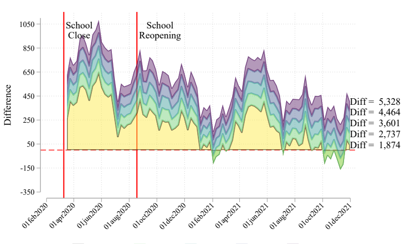


(f) Rape

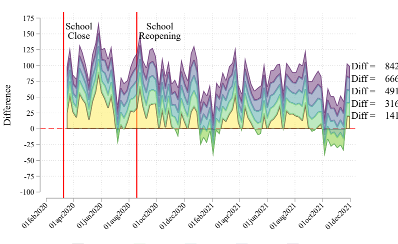
Panel C: Projected under-reporting



(g) Intra-family Violence



(h) Sexual Abuse

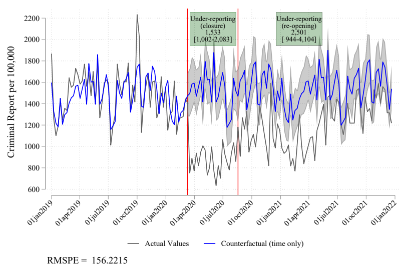


(i) Rape

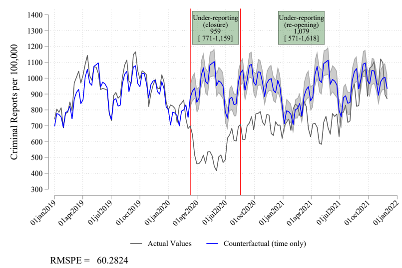
Figure A16: Alternative Counterfactual Models – No Trend (2018)

Notes to Fig. A16: Alternative projections and reporting differentials are reported, where rather than basing projections off MSE optimal models, projections are based on simple cyclical estimates (week of year fixed effects) based only off year 2018, rather than additionally incorporating a secular trend and choosing pre-periods optimally. Refer to notes to Figure 5 for further details.

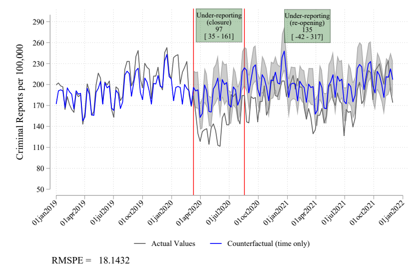
Panel A: Simple counterfactual (time only)



(a) Intra-family Violence

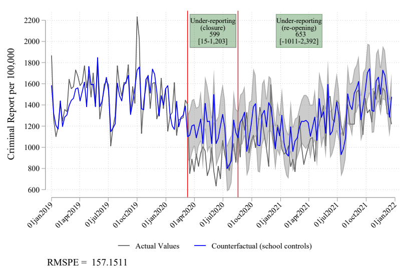


(b) Sexual Abuse

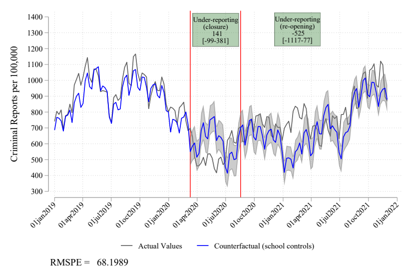


(c) Rape

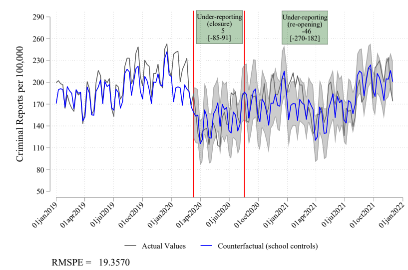
Panel B: Counterfactual (No school channel)



(d) Intra-family Violence

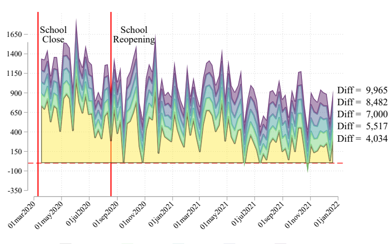


(e) Sexual Abuse

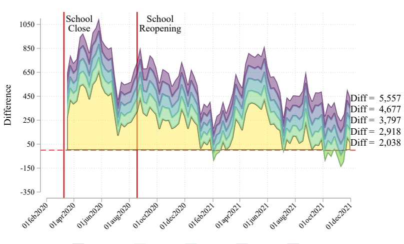


(f) Rape

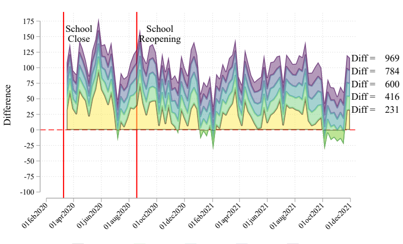
Panel C: Projected under-reporting



(g) Intra-family Violence



(h) Sexual Abuse

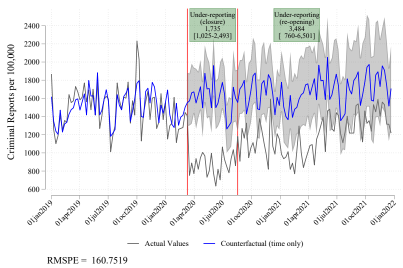


(i) Rape

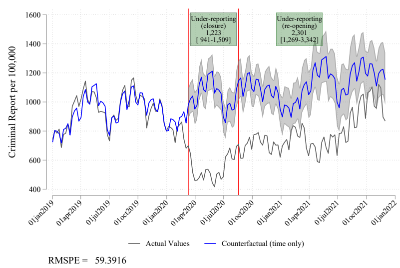
Figure A17: Alternative Counterfactual Models – Linear (2018)

Notes to Fig. A17: Alternative projections and reporting differentials are reported, where rather than basing projections off MSE optimal models, projections are based on simple cyclical estimates (week of year fixed effects) and a linear secular trend, based only off year 2018, rather than optimally choosing parametrization of the secular trend and choosing pre-periods optimally. Refer to notes to Figure 5 for further details.

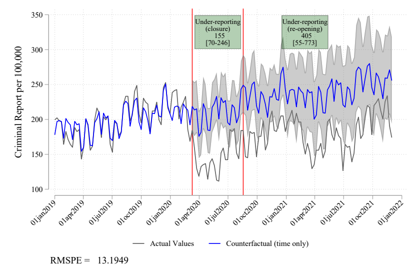
Panel A: Simple counterfactual (time only)



(a) Intra-family Violence

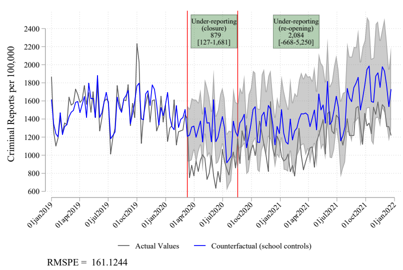


(b) Sexual Abuse

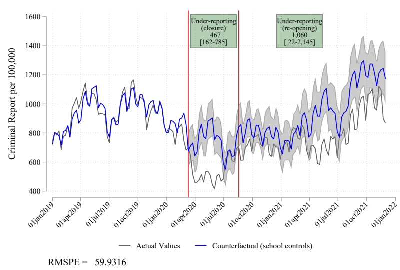


(c) Rape

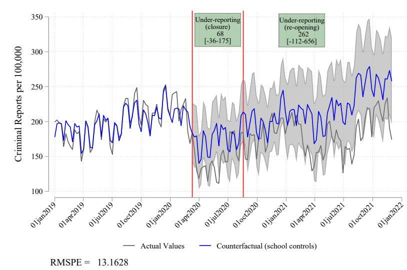
Panel B: Counterfactual (No school channel)



(d) Intra-family Violence

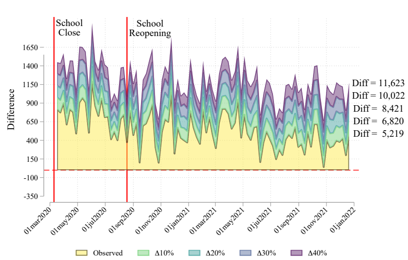


(e) Sexual Abuse

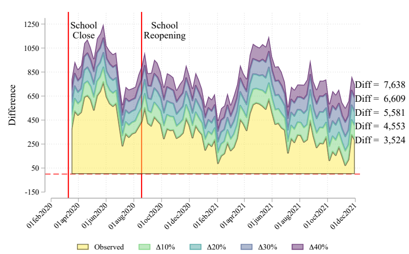


(f) Rape

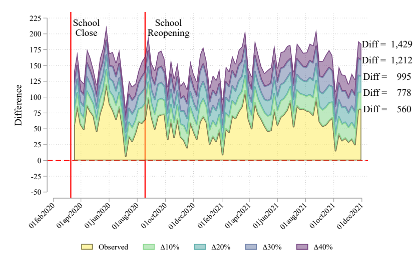
Panel C: Projected under-reporting



(g) Intra-family Violence



(h) Sexual Abuse

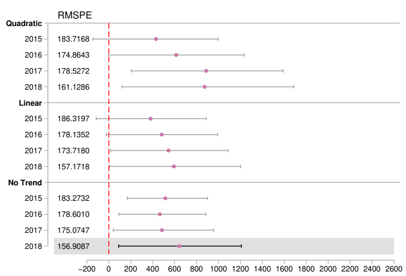


(i) Rape

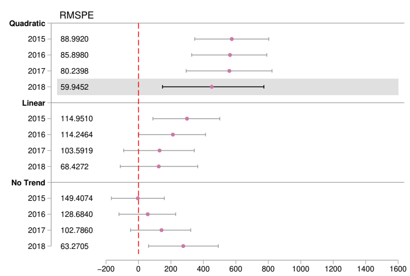
Figure A18: Alternative Counterfactual Models – Quadratic (2018)

Notes to Fig. A18: Alternative projections and reporting differentials are reported, where rather than basing projections off MSE optimal models, projections are based on cyclical estimates (week of year fixed effects) and a quadratic secular trend, based only off of year 2018, rather than optimally choosing parametrization of the secular trend and choosing pre-periods optimally. Refer to notes to Figure 5 for further details..

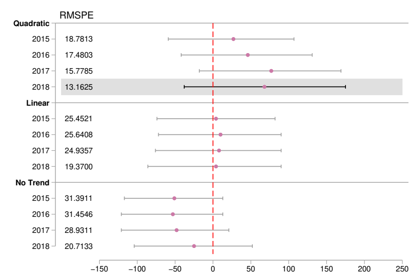
Panel A: Post School Closure and before School Reopening



(a) Intra-family violence (No school channel)

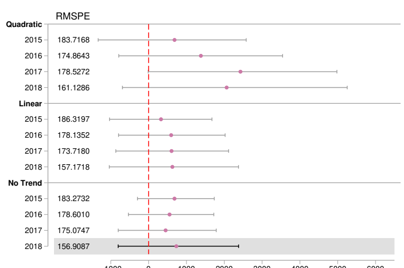


(b) Sexual Abuse (No school channel)

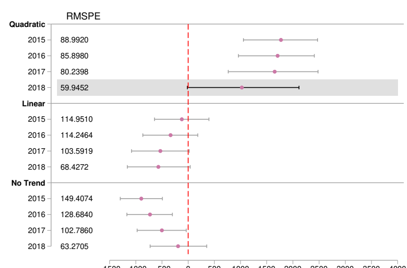


(c) Rape (No school channel)

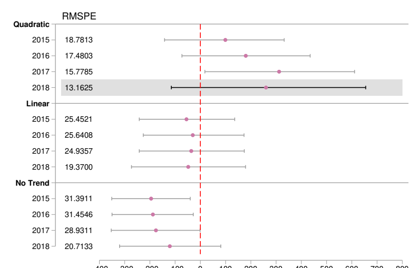
Panel B: Post School Reopening



(d) Intra-family violence (No school channel)



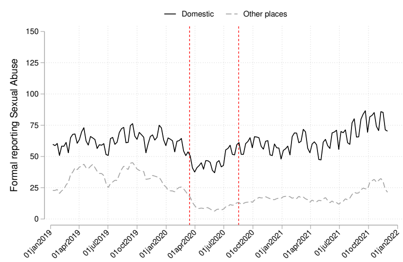
(e) Sexual Abuse (No school channel)

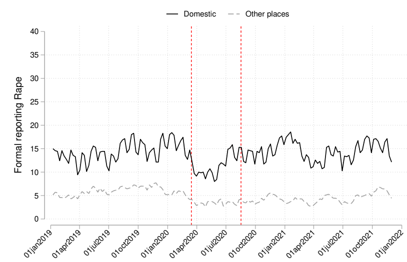


(f) Rape (No school channel)

Figure A19: Projected Under-reporting under Various Counterfactual Assumptions (Projection with no School Channel)

Notes to Fig. A19: Refer to Notes to Figure 6. Identical sensitivity analyses are reported, however now for under-reporting displayed in Panel B of Figure 5, where the school closure channel is conditioned out.

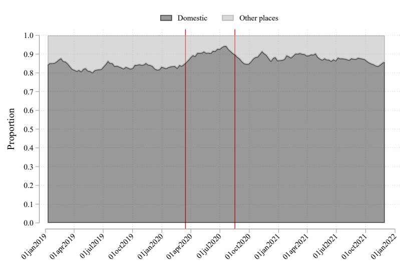


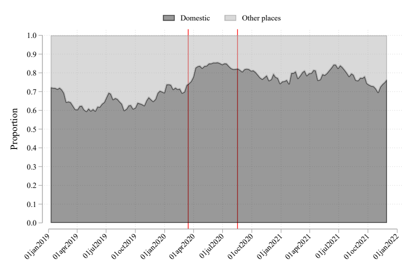


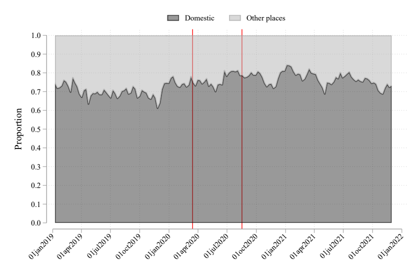
(a) Reporting of Intra-family Violence Against Minors

(b) Reporing of Sexual Assault Against Minors

(c) Reporting of Rape Against Minors







(d) Proportion of Reporting Violence Against Minors

(e) Proportion of Reporting of Sexual Assault Against Minors

(f) Proportion of Reporting of Rape Against Minors

Figure A20: Temporal Trends – Crimes Reported Against Children by Place of Occurrence

Notes to Fig. A20: Descriptive trends show the total number of crimes against minors (top panels) and proportion of crimes against minors (bottom panels) classified as occurring in a private home ("Domestic"), or in another place ("Other places"). The total number and proportion of cases are documented by week for the full period of analysis. Information on the location of occurrence is not available in data on crime victims, but rather an auxiliary database covering all crimes. As crimes can have more than 1 victim, these descriptive trends have slightly less crimes than victimization data documented in Figure 1 of the main analysis. In all cases, crimes are defined as in the main analysis (intra-family violence in panels (a) and (d), sexual assault in panels (b) and (e), and rape in panels (c) and (f)). Vertical red lines document the data of first school closure, and first re-opening.

A2 Supplementary Tables

Table A1: Attendance, School Closure and School Reopening

	Intra-family Violence					
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Intra-family Violence						
School Closure	-1.360***	-1.380***	-1.514***	-1.526***	-1.198***	-1.229***
	(0.102)	(0.103)	(0.130)	(0.132)	(0.139)	(0.143)
School Reopening	-0.331*	-1.942***	-0.655***	-1.211***	-0.540**	-1.209***
	(0.181)	(0.385)	(0.205)	(0.397)	(0.249)	(0.455)
School Reopening × Attendance		3.504***		1.228		1.365
		(0.828)		(0.829)		(0.856)
Reopening Effect at Percentile 25 of Attendance		-1.144		-0.931		-0.898
Reopening Effect at Percentile 50 of Attendance		-0.253		-0.619		-0.551
Reopening Effect at Percentile 75 of Attendance		0.412		-0.386		-0.292
Reopening Effect at Percentile 90 of Attendance		0.994		-0.182		-0.065
Baseline Mean	4.302	4.302	4.302	4.302	4.302	4.302
Observations	45,802	45,802	45,802	45,802	45,802	45,802
Panel B: Sexual Abuse						
School Closure	-0.803***	-0.824***	-0.952***	-0.968***	-0.923***	-0.965***
	(0.067)	(0.067)	(0.083)	(0.083)	(0.092)	(0.092)
School Reopening	0.093	-1.534***	-0.189*	-0.923***	-0.314**	-1.238***
	(0.091)	(0.226)	(0.106)	(0.218)	(0.139)	(0.252)
School Reopening × Attendance		3.540***		1.622***		1.888***
		(0.489)		(0.487)		(0.494)
Reopening Effect at Percentile 25 of Attendance		-0.729		-0.554		-0.809
Reopening Effect at Percentile 50 of Attendance		0.172		-0.141		-0.329
Reopening Effect at Percentile 75 of Attendance		0.843		0.167		0.029
Reopening Effect at Percentile 90 of Attendance		1.431		0.436		0.343
Baseline Mean	2.677	2.677	2.677	2.677	2.677	2.677
Observations	45,802	45,802	45,802	45,802	45,802	45,802
Panel C: Rape						
School Closure	-0.109***	-0.112***	-0.092***	-0.093***	-0.052	-0.055
	(0.023)	(0.023)	(0.026)	(0.026)	(0.036)	(0.037)
School Reopening	-0.011	-0.265***	-0.041	-0.070	-0.053	-0.109
	(0.041)	(0.091)	(0.046)	(0.095)	(0.059)	(0.110)
School Reopening × Attendance		0.551***		0.065		0.113
		(0.184)		(0.190)		(0.195)
Reopening Effect at Percentile 25 of Attendance		-0.139		-0.056		-0.083
Reopening Effect at Percentile 50 of Attendance		0.001		-0.039		-0.054
Reopening Effect at Percentile 75 of Attendance		0.105		-0.027		-0.033
Reopening Effect at Percentile 90 of Attendance		0.197		-0.016		-0.014
Baseline Mean	0.582	0.582	0.582	0.582	0.582	0.582
Observations	45,802	45,802	45,802	45,802	45,802	45,802
Municipal & WoY FEs			Y	Y	Y	Y
Lockdown & Epidemiological controls					Y	Y

Notes to Tab. A1: Results replicate those of Table 1 from the main text for the outcome of intra-family violence against children, however here additionally interacting School Reopening measures with the proportion of attendance in each municipality by week cell. Attendance data is not observed for all periods (refer to section 3, and as such, these models are only estimated for periods in which all data are available. Columns 1, 3 and 5 present baseline models for the period in which attendance data are available, while columns 2, 4 and 6 additionally include the School Reopening by Attendance interaction. In table footers, linear estimated effects on school re-opening are reported at various margins of attendance, namely percentiles 25, 50, 75 and 90 of attendance from observed data. These percentiles are p25=26.6% attendance, p50=49.9% attendance, p75=68.2% attendance, and p90=84.7% attendance. All other details follow those laid out in Table 1. *** $p< 0.01$; ** $p< 0.05$; * $p< 0.10$.

Table A2: Percentage of Violence Reporting by Age Group and Channel in a Single Child Protection Office

	Reporting Channel			
Age group	Schools	Courts	Health Centers	Others
Panel A: 2019-2021				
$[1-6]$	0.12	0.41	0.13	0.34
$[7-10]$	0.29	0.42	0.08	0.21
$[11-13]$	0.21	0.49	0.08	0.22
$[14-15]$	0.11	0.57	0.08	0.24
$[16-17]$	0.14	0.58	0.08	0.20
Panel B: 2019 Only				
$[1-6]$	0.24	0.35	0.15	0.26
$[7-10]$	0.38	0.35	0.09	0.18
$[11-13]$	0.33	0.44	0.10	0.13
$[14-15]$	0.12	0.61	0.04	0.23
$[16-17]$	0.17	0.54	0.08	0.21

Notes to Tab. A2: Descriptive values are reported documenting official recorded channels of violence reporting received by a single child protection office in a large municipality in Santiago. Here channels are separated as entering via schools, courts, health centres, or other sources, and relative proportions of each type of reporting channel by age of the victim is documented. Panel A reports values over all years in which data was provided (2019–2021), while panel B reports values only in the period entirely preceding COVID (year 2019).

Table A3: Modelled Impacts of School Closure and Re-opening on Violence Against Children (no January and February)

	Intra-family Violence			Sexual Abuse			Rape		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Binary Re-opening Measure									
School Closure	-1.561***	-1.582***	-1.311***	-0.974***	-1.010***	-1.069***	-0.115***	-0.104***	-0.083**
	(0.120)	(0.131)	(0.149)	(0.082)	(0.085)	(0.099)	(0.025)	(0.026)	(0.036)
School Reopening	-0.782***	-0.767***	-0.818***	-0.407***	-0.413***	-0.694***	-0.052*	-0.059**	-0.083*
	(0.144)	(0.146)	(0.214)	(0.082)	(0.082)	(0.119)	(0.029)	(0.030)	(0.045)
Test of $\beta = \gamma$ (p-value)	0.000	0.000	0.002	0.000	0.000	0.000	0.014	0.117	0.995
Observations	45,572	45,572	45,572	44,537	44,537	44,537	44,537	44,537	44,537
Baseline Mean	4.472	4.472	4.472	2.825	2.825	2.825	0.581	0.581	0.581
Panel B: Continuous Re-opening Measure									
School Closure	-1.383***	-1.435***	-0.983***	-0.811***	-0.892***	-0.721***	-0.100***	-0.089***	-0.042
	(0.108)	(0.122)	(0.119)	(0.072)	(0.077)	(0.077)	(0.023)	(0.024)	(0.030)
School Reopening	-0.630***	-0.649***	-0.306	-0.085	-0.197**	-0.041	-0.029	-0.035	-0.008
	(0.186)	(0.175)	(0.208)	(0.099)	(0.096)	(0.117)	(0.039)	(0.040)	(0.048)
Test of $\beta = \gamma$ (p-value)	0.000	0.000	0.001	0.000	0.000	0.000	0.051	0.171	0.448
Observations	45,572	45,572	45,572	44,537	44,537	44,537	44,537	44,537	44,537
Baseline Mean	4.472	4.472	4.472	2.825	2.825	2.825	0.581	0.581	0.581
Municipal & WoY FEs		Y	Y		Y	Y		Y	Y
Lockdown & Epidemiological controls			Y			Y			Y

Notes to Tab. A3: Refer to notes to Tab. 2 of the main text. Identical specifications are estimated, however here consistently removing all observations in months January-February of all years, when schools are closed for summary vacations. *** $p< 0.01$; ** $p< 0.05$; * $p< 0.10$.

Table A4: Modelled Impacts of School Closure and Re-opening on Sexual Violence (Unadjusted Variables)

	Sexual Abuse			Rape		
	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Binary Re-opening Measure						
School Closure	-0.878***	-0.966***	-1.031***	-0.130***	-0.098***	-0.092**
	(0.069)	(0.082)	(0.099)	(0.025)	(0.028)	(0.040)
School Reopening	-0.381***	-0.377***	-0.655***	-0.065**	-0.047*	-0.078*
	(0.070)	(0.073)	(0.116)	(0.026)	(0.028)	(0.046)
Test of $\beta = \gamma$ (p-value)	0.000	0.000	0.000	0.012	0.096	0.706
Observations	54,214	54,214	54,214	54,214	54,214	54,214
Baseline Mean	2.705	2.705	2.705	0.588	0.588	0.588
Panel B: Continuous Re-opening Measure						
School Closure	-0.752***	-0.886***	-0.743***	-0.121***	-0.095***	-0.071**
	(0.064)	(0.079)	(0.086)	(0.023)	(0.026)	(0.034)
School Reopening	-0.156*	-0.288***	-0.213*	-0.072*	-0.062	-0.068
	(0.090)	(0.093)	(0.122)	(0.037)	(0.039)	(0.048)
Test of $\beta = \gamma$ (p-value)	0.000	0.000	0.000	0.173	0.412	0.945
Observations	54,214	54,214	54,214	54,214	54,214	54,214
Baseline Mean	2.705	2.705	2.705	0.588	0.588	0.588
Municipal & WoY FEs		Y	Y		Y	Y
Lockdown & Epidemiological controls			Y			Y

Notes to Tab. A4: Refer to notes to Tab. 2 of the main text. Identical specifications are estimated for models where the outcome is sexual abuse or rape, however using original un-smoothed data, where over-reporting occurs on the first day of each month, rather than smoothed data re-assigning excess reporting uniformly across the month. All other details follow those in Tab. 2. Column numbers (4)-(9) are used here for sake of comparison with column numbers in Tab. 2. *** $p< 0.01$; ** $p< 0.05$; * $p< 0.10$.

Table A5: Modelled Impacts of School Closure and Re-opening on Reporting of Violence Against Children by Type of Intra-family Violence

	Physical Violence (Serious)			Physical Violence (Moderate)			Psychological Violence		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Binary Re-opening Measure									
School Closure	-0.031**	-0.029**	-0.005	-1.016***	-1.149***	-0.947***	-0.345***	-0.410***	-0.390***
	(0.013)	(0.013)	(0.017)	(0.073)	(0.086)	(0.105)	(0.057)	(0.067)	(0.078)
School Reopening	-0.018	-0.024	0.008	-0.745***	-0.798***	-0.760***	-0.007	-0.022	-0.139
	(0.014)	(0.015)	(0.019)	(0.073)	(0.077)	(0.119)	(0.083)	(0.083)	(0.109)
Test of $\beta = \gamma$ (p-value)	0.423	0.754	0.514	0.000	0.000	0.038	0.000	0.000	0.003
Observations	54,214	54,214	54,214	54,214	54,214	54,214	54,214	54,214	54,214
Baseline Mean	0.136	0.136	0.136	2.783	2.783	2.783	1.383	1.383	1.383
Panel B: Continuous Re-opening Measure									
School Closure	-0.024**	-0.024*	-0.002	-0.868***	-1.026***	-0.652***	-0.310***	-0.382***	-0.286***
	(0.012)	(0.013)	(0.016)	(0.066)	(0.081)	(0.085)	(0.049)	(0.060)	(0.061)
School Reopening	-0.005	-0.016	0.023	-0.673***	-0.787***	-0.375***	0.122	0.077	0.091
	(0.020)	(0.021)	(0.023)	(0.101)	(0.104)	(0.126)	(0.104)	(0.099)	(0.110)
Test of $\beta = \gamma$ (p-value)	0.371	0.722	0.328	0.049	0.019	0.019	0.000	0.000	0.000
Observations	54,214	54,214	54,214	54,214	54,214	54,214	54,214	54,214	54,214
Baseline Mean	0.136	0.136	0.136	2.783	2.783	2.783	1.383	1.383	1.383
Municipal & WoY FEs		Y	Y		Y	Y		Y	Y
Lockdown & Epidemiological controls			Y			Y			Y

Notes to Tab. A5: Refer to notes to Tab. 2 of the main text. Identical specifications are estimated, however here rather than considering total reports of intra-family violence against children (as reported in columns (1)-(3) of Tab. 2), reports of intra-family violence against children for each sub-class of intra-family violence are reported. These classifications are generated from police reports, and each case of intra-family violence from Tab. 2 is classified as one (and only one) of the three classes displayed here. All other details follow those in Tab. 2. *** p< 0.01; ** p< 0.05; * p< 0.10.

Table A6: Weights in Time-Varying Adoption Two-Way Fixed Effect Estimate on School Reopening

Comparison Group	Weights	Average DD Estimate
Panel A: Intra-family violence		
Earlier Treatment vs. Later Control	0.788	-0.637
Later Treatment vs. Earlier Control	0.212	0.127
Panel B: Sexual abuse		
Earlier Treatment vs. Later Control	0.788	-0.544
Later Treatment vs. Earlier Control	0.212	-1.368
Panel C: Rape		
Earlier Treatment vs. Later Control	0.788	-0.074
Later Treatment vs. Earlier Control	0.212	-0.001

Notes to Tab A6: Aggregate values for two-way FE weights are reported (following (Goodman-Bacon, 2021; Goodman-Bacon et al., 2019)) for estimates of impacts of time-varying school closure on outcomes indicated in each panel. These are aggregate values across all points documented in Figure A12. Weights refer to the total proportion of estimates based on each comparison type (earlier treatment vs. later adopter, or later adopter vs. earlier treated), and "Average DD Estimate" refers to the average difference between these groups in a 2×2 DD setting. For all models, the proportion of negative weights following (de Chaisemartin and D'Haultfœuille, 2020) is 0.

Table A7: Actual Violence Reporting Channels Reported by a Single Child Protection Office – Temporal Differences

	Jan-Feb		Mar-Sep		Oct-Dec		Aug-Dec	
	2019	2020	2019	2020	2019	2020	2019	2021
Total Reporting	57	46	227	163	83	43	138	120
Total Reporting by Schools	4	4	57	8	32	4	48	25
Percentage of Reporting by Schools	0.07	0.09	0.25	0.05	0.39	0.09	0.35	0.21
Percentage of Schools Open	–	–	1.00	0.00	1.00	0.08	1.00	0.99

Notes to Tab. A7: Descriptive values document official recorded channels of violence reporting received by a single child protection office in a large municipality in Santiago. Here year by year comparisons are documented of reports received via schools and total reports received in various periods. Jan-Feb is vacations in both years. Mar-Sep covers periods where schools returned in 2019, but were closed in 2020. Oct-Dec covers periods where schools were completely open in 2019, and only very partially open in 2020. Aug-Dec covers periods in which schools sere completely open in 2019, and nearly entirely open in 2021.