Schools as Safety Nets: Break-downs and Recovery in Reporting of Violence Against Children*

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Abstract

Schools are a key channel for formally reporting violence against children, but this channel broke down with the onset of the COVID-19 pandemic. We study how widespread such reporting declines were and to what extent they were recovered once schools reopened. Examining the universe of all criminal reports of violence against children in Chile, we observe sharp declines in reports of all types of violence (psychological, physical, and sexual), and find that full recovery in reporting had not occurred even nearly 2 years following initial school closures. Extending beyond the unexpected and long school closures during the pandemic, we find evidence of clear declines in violence reporting during regular school vacations and in a period of student strikes at the secondary level, suggesting a broader relevance of these results.

JEL codes: D10; I28; I18; K42.
Keywords: Violence against children; reporting; school closure; school attendance; Latin America.

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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 and earnings (Currie and Widom, 2010), and antisocial behavior (Thornberry et al., 2001). 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 costs 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 that may be more effective in altering abusive behavior (Benson et al., 2022). 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 (Benson et al., 2022; Puls et al., 2021). This crucial bridging role played by teachers and educational professionals was disrupted by the global pandemic caused by the SARS-CoV-2 virus (hereafter COVID-19) and the resulting public policy and public health responses. Among a host of other policy interventions, school closures were implemented to curb the spread of infection (Hale et al., 2021), with research documenting significant declines in reporting of child maltreatment following these closures (Baron et al., 2020).

Declines in reporting of violence against children mimic declines observed for almost all types of crimes at the beginning of the pandemic (Nivette et al., 2021; Trajtenberg et al., 2024; Solar and Ricart, 2022). However, unlike other classes of crime where policy responses reduced potential criminal exposure and opportunity (Stickle and Felson, 2020), children experience *higher* risk of victimization at home, with their most likely perpetrators being parents and other household members (Sandner et al., 2024; Devries et al., 2018). Thus, 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, with likely long-term consequences for the victims should these cases remain undetected over time.

In this paper, we study how formal educational systems lost and regained their specific function of identifying and reporting violence against children in response to the decline and recovery of face-

to-face learning. We extend the relatively nascent literature on the negative consequences of school closures in several ways. We *i)* analyze how school closures 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)* determine whether this decline was reversed once schools reopened, and, if so, over what time-frame and under what conditions. By analyzing the recovery of reporting, we also shed light on the importance of in-person interactions between children and educational professionals for the identification of maltreatment. We recognize that the responses adopted in the global pandemic caused by COVID-19 were exceptional. However, schools close annually due to planned summer and winter breaks (i.e., Benson et al., 2022) and sometimes also unexpectedly due to human or natural disasters (i.e., Shidiqi et al., 2023; Angrist et al., 2022)). While COVID presents an extraordinary opportunity to study the role of schools in the reporting of violence, we *iii)* test the external validity of our results using two additional identification strategies that are unaffected by the broader disruptions introduced by the pandemic. By doing so, we also account for a number of identifications challenges presented in this literature given the experience of multiple co-occurring changes that could have affected reporting beyond school closures, and provide evidence of the broader relevance of the findings driven by shocks from the global pandemic.

We study these questions in Chile, a germane context for several reasons. Similar to other Latin American economies, Chile was exposed to long school closures compared with other regions of the world (UNICEF, 2021).[1] And, while it is already known that the reporting of violence against children decreased during school closures (e.g., 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)), it is unclear whether this trend persisted over time as schools remained closed, whether and how reporting recovered when schools reopened, and the broader implications of this to non-COVID schooling shocks. To study this phenomenon, we generated rich micro-data covering all reported crimes against children, school closures, attendance, and other relevant factors over an extended period of time, which allows us to analyze the long-term cost and the potential recovery of the policies adopted during the pandemic. Our data cover the pre-closure, closure, and reopening periods, allowing us to provide a more comprehensive evaluation of

[1]The length of closures correlates strongly within world regions and with country income level. The fact that the prevalence of violence against children is also higher in developing countries (Hillis et al., 2016) suggests that these policy responses likely widened baseline disparities in child well-being.

the impact of school closure and reopenings on rates of violence reporting. Secondly, these data and the context enable us to consider results at a national scale using comparable reporting measures, including a wider range of types of violence.[2] Finally, but importantly, the nature of the pandemic response in Chile allows us to rule out a number of competing explanations for changes in reporting, given considerable geographic and temporal variation in COVID infection, and iin formal lockdown and similar public health policies across the country.

Our results suggest, first, that school closures generated sharp declines in reporting not only in rates of domestic 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. Importantly, we find evidence that rates of reported violence were slow to recover, not reaching baseline levels even nearly two years following original school closures. Overall, our findings indicate that prolonged school closures and delays in restoring reporting channels extended the damage originally attributed to the abrupt closures. We observe that reporting recoveries occurred more slowly in areas with low attendance, even conditional on measures of development and family income, pointing to attendance as a potential mediator. Leveraging information from before the COVID-19 shock, we find evidence suggesting that these results are informative both for other schooling shocks, such as the large student strikes that occurred in the country in 2011, and also for *expected* interruptions like regular school vacations. All in all, our results highlight the importance of face-to-face interactions between students and school staff and the relevance of these findings for a range of school interruptions.

In what remains of this paper, we provide background on the role of schools in safeguarding children's well-being, along with an overview of the Chilean child welfare system and Chile's specific policy response to COVID (Section 2). Section 3 describes the range of micro-data generated for this study. In Section 4, we lay out methods and identifying assumption. Section 5 documents all results, including the two additional analyses to account for the broader implications of school closures. Finally, Section 6 concludes.

[2]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 studies by Benson et al. (2022) and Puls et al. (2021), both of which use national-level data from the US. An alternative (pre-COVID) setting is the study by Sandner et al. (2024), which uses national-level data to examine a *pre-school expansion* policy in Germany, finding evidence that greater availability of pre-school spots reduced incidence of violence against children.

2 Background

2.1 Child Well-being and Schools

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% (Font and Kennedy, 2022). In contrast, self-report studies suggest prevalence rates of 7.6% and 18% for sexual abuse among boys and girls, respectively, as well as 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 considered, they also highlight the fact that official data only reveal the "tip of the iceberg" in cases of maltreatment (Currie and Tekin, 2012).

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 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).

Given the nature of the interactions between educational professionals and children, increasing time at school can lead to an increase in the reporting of violence against children. Puls et al. (2021) show a 16% decrease in reporting when schools were closed, which was not matched with the increases observed during the first two weeks following the closure period. Similarly, Benson et al. (2022) show that additional time in schools leads to an increase in reports of child maltreatment, using two different identification strategies (children's eligibility for Kindergarten and school calendars). Both studies confirm the important role that schools play in identifying and reporting child maltreatment, as well as closing the gap between prevalence and response to maltreatment.

However, one of the earliest and most widespread responses to the COVID-19 pandemic worldwide

was the closure of schools (see Figure A1 in the Online Appendix), with unintended consequences in terms of learning loss (Engzell et al., 2021), early child development (Abufhele et al., 2024), female labor market participation (Hansen et al., 2022), increased inequalities (Agostinelli et al., 2022), access to school meals (Bitler et al., 2020), and the mental health of children and adolescents (Viner et al., 2022). Among the relatively less-explored consequences is the impact that school closures may have had on the identification and reporting of maltreatment. Using a counterfactual design, Baron et al. (2020) observed an almost 30% decrease in the number of allegations in the first two months following school closure in Florida, US. Similar results are reported by Prettyman (2021b) for Colorado, Bullinger et al. (2020) for Georgia, and Padilla-Romo and Cabrera-Hernández (2020) for Mexico City. These studies confirm the broken link between schools and reporting during the first months of the pandemic when most educational settings were mandatorily closed at all levels.

By March 2021, nearly 60% of Latin American school-age children were still affected by school closures (UNICEF, 2021), having lost more days of school than any other region in the world (World Bank, 2021).[3] This extended closure interrupted three decades of educational progress in the region, potentially increasing existing inequalities (The Economist, 2021). Similarly, in Chile, schools closed nationwide in March 2020 and began reopening—although very gradually—in August of 2020. By March 2021, fewer than a third of all schools had had some kind of in-person education, rising to 98% by December 2021, the end of the school year. Student attendance, however, remained under 50% of the total student population. In the following two sections, we discuss the specific context of this study in detail.

2.2 The Child Protection System in Chile

The National Child Service (SENAME) is the national institution responsible for protecting children's rights in Chile. Created in 1979 under the Ministry of Justice, it combines services for young offenders and minors who were victims of abuse and neglect.[4]

[3] While schools began to resume in-person activities during Fall 2020 in most countries of the global north, they remained closed for significantly longer periods in less developed areas of the world. Schools were completely closed on average for around 20 weeks in Africa, 25 weeks in Asia, and 30 weeks in Latin America, compared to less than 12 weeks in higher income regions such as North America and Europe. Such a pattern leads to noteworthy correlations between country income levels and the duration of school closures (Figure A2).

[4] Following international recommendations (Gale, 2016; Muñoz-Guzman et al., 2023), in October 2021 the service was split in two: the new National Service for Specialized Protection of Childhood and Adolescence, which is responsible for protecting children's rights and is dependent on the Ministry of Social Development (MIDESO), and SENAME, which remains under the Ministry of Justice and now has an exclusive focus on juvenile justice and reintegration. Given

SENAME provides diagnosis and response programs to children and adolescents who have been reported as abused, maltreated, or neglected, and offers alternative care for those children who must be separated from their caregivers. There are two characteristics to highlight in the Chilean child protection system. The first is the high level of centralization. SENAME is a governmental agency that makes decisions and develops guidelines at the national level (Irarrázaval, 2016), while programs are implemented locally, primarily through private non-profit institutions that act as collaborating agencies of SENAME (de Iruarrizaga, 2016). The second is the high degree of judicialization in the protection of children's rights (Muñoz-Guzman et al., 2023; Gale, 2016), with Family Courts responsible for verifying rights violations when no criminal act is involved, determining protection measures, and referring children to specialized programs (Muñoz-Guzman et al., 2023).[5]

Unlike in other contexts (e.g., Benson et al., 2022; Sandner et al., 2024), in Chile there are many ways through which a suspected case of child maltreatment can be reported to the child welfare and/or judicial system (see Figure A3). A potential case of abuse or neglect can be reported directly at the Local Offices of Rights Protection (OPD for its acronym in Spanish), the prosecutor's office (*Fiscalía*), the police, or family courts. Highly complex cases—regardless of where they are reported—are derived to the Family Court, the institution in charge of reviewing the case and determining precautionary measures when needed. Based on their assessment, the judge can refer a child to SENAME for a diagnosis and/or enrollment in an ambulatory program, and—in more extreme situations—for residential or foster care (see Figure A3). In low complexity cases, OPDs can provide outpatient care and psychological and legal support for children and adolescents and their families (Irarrázaval, 2016). If the case has a possible criminal element (e.g., injuries are reported), it is referred to the District Attorney's Office, which is responsible for investigating and bringing criminal charges when appropriate. Thus, Family Courts and the Prosecutor's Office play complementary roles, often in the same case although not always in a coordinated manner.

While any person can report a case, by law, individuals who work in the public, health, or educational sector are mandatory reporters of any act that may constitute a crime against children (Procedural Penal Code, articles 175-178). Schools are required to refer cases of suspected maltreatment to

the time frame of this paper, we explain how the system worked until 2021.

[5]This over-reliance on Family Courts is expected to change with the creation of Local Children Offices (Law 21.430, 2022), which are currently in the process of being implemented.

OPDs, and to the judicial system—either police, family courts, or prosecutorial office—when sexual abuse is suspected. The Ministry of Education provides a range of documents highlighting this law and the duties of educational professionals to act in cases of suspected child abuse (Ministerio de Educación, 2017; Superintendencia de Educación, 2018). School closures may have disrupted this channel.

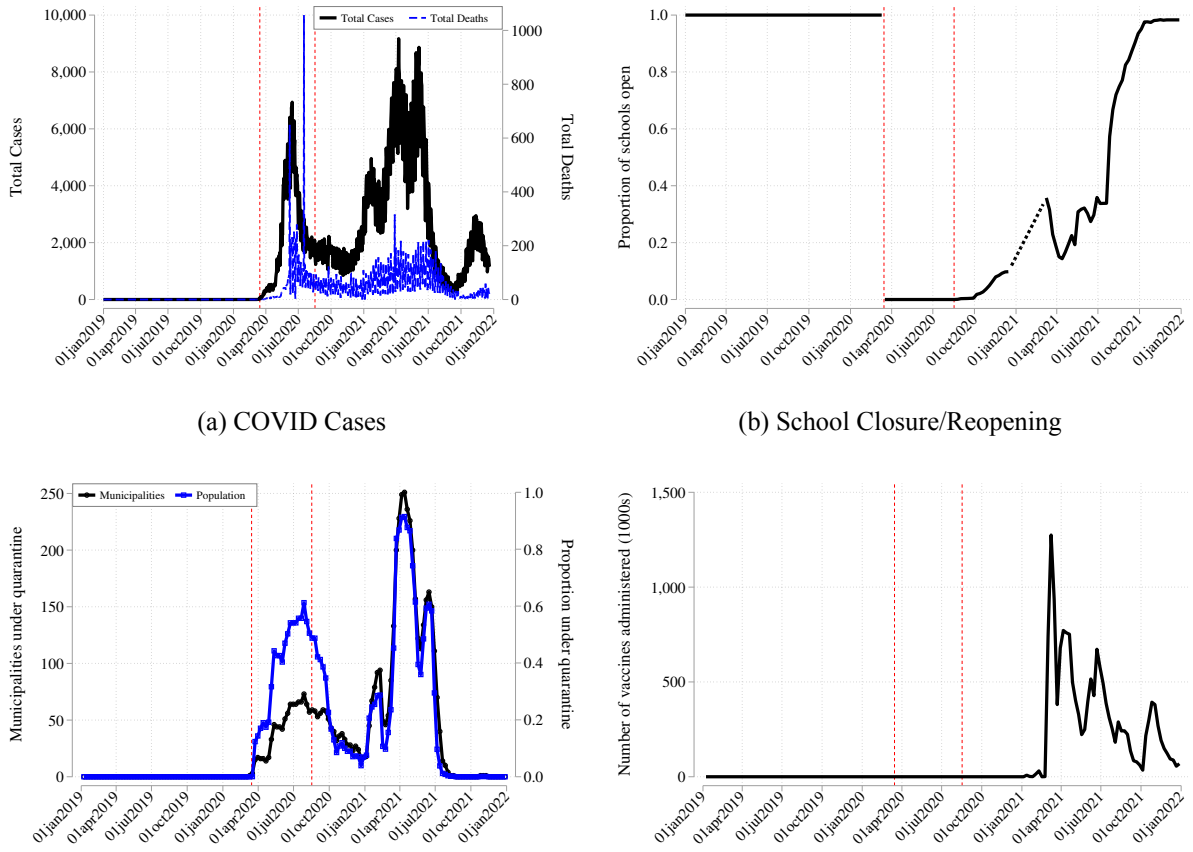
2.3 Policy Responses to COVID-19

The first COVID-19 case was identified in Chile on March 3, 2020, just one day before the start of the school year (see Figure A4). Cases quickly expanded through local transmission, reaching around 7,000-8,000 cases per day by mid-June 2020, at the peak of the first wave of the pandemic in Chile (see Figure 1, panel (a)). Following the arrival of the first cases, the Chilean government implemented several policies to promote social distance, such as early bans on large gatherings, mandatory face masks in public, and the enforcement of formal lockdowns (Tariq et al., 2021).[6] As one of the first responses to COVID-19, the government announced on Sunday March 15, 2020 that all K-12 schools and pre-schools nationwide were mandated to close, starting the next day (Figure 1, panel (b)).[7] Universities joined this decision, with all educational institutions transitioning to online education before there was a significant rise in COVID cases and before the first lockdowns were implemented in the country (see Figure 1).

The first mandatory lockdown was put in place on March 26, 2020. The particularity of Chile is that lock-downs were defined at the national level by the Ministry of Health (MoH), though implemented at the municipal level (Figure 1, panel (c)). 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 a 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 (Lee et al., 2021). Moreover, formal lockdowns were strictly

[6] Additional discussion of Chile's pandemic response can be found in Mena et al. (2021) and references therein.

[7] The Chilean school system includes private schools, public schools (administered by municipalities until 2017, when some public schools have been transferred to a new governmental organization (Kuzmanic et al., 2023)), and private state-subsidized schools (Bellei and Munoz, 2023). The latter represents over 50% of the total number of students, while another 35% are enrolled in public schools (Cuesta et al., 2020). School attendance in Chile is mandatory from the age of 6 years and up to the age of 18 years. However, a large proportion of children below the age of 6 years also attend educational institutions formally recognized by the Ministry of Education (see Figure A5).



(a) COVID Cases

(b) School Closure/Reopening

(c) Lockdowns

(d) Vaccines

Figure 1: Contextual Details – Epidemiological Measures and School Closure

Notes to Figure 1: All trends are 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 (c) 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 when schools were ordered to close by the central government, while the second vertical dashed line indicates the day when schools were officially allowed to reopen.

enforced, with police personnel conducting spot checks and citizens—with the exception of essential workers—allowed to leave their houses 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., 2024), beyond the decline already observed after schools were closed (Bennett, 2021). Similarly, the decision to lift lockdowns was determined by the MoH.

These decisions were made possible by a nationwide public reporting system known as EPIVIGILA, which recorded all COVID-19 cases. Since EPIVIGILA was already in place before the pandemic, data on case measurements was available from the earliest days of COVID-19. Chile, with its rel-

atively effective universal health coverage (Lozano et al., 2020), was one of the countries with the highest testing rates in Latin America, and its vaccination rollout was both early and rapid (Figure 1, panel (d)). Mandatory reporting of all PCR tests for suspected COVID-19 cases, along with automated reporting of positive tests (which included the individual's municipality of residence), ensured precise local measurements of testing, test positivity, and diagnosed cases.

In July 2020, the government implemented a "Step by Step" strategy (in Spanish, the *Paso a Paso* policy), which outlined five phases of gradual reopening for each municipality, ranging from full lockdown to no restrictions. Under this scheme, schools in municipalities not under full lockdown ("Phase 1") were allowed—though not mandated—to resume in-person education starting in August 2020. To reopen, schools had to adhere to protocols set by the Ministry of Education, which included mandatory mask-wearing, ensuring physical distancing, and providing adequate ventilation. In municipalities outside of Phase 1, the decision on how and when to resume in-person activities was left to individual schools. For instance, some schools implemented a gradual return with shifts alternating between in-person and remote learning or opened only on certain days of the week. Attendance remained voluntary until March 2022. By December 2020, only 10% of schools had held some of in-person activities, and these were mostly part time and with low attendance (Claro et al., 2021).

During the summer of 2021, the Ministry of Education required all schools to prepare a plan for a safe return to in-person learning. Early vaccination of teachers and educational personnel was prioritized to support this goal (Jara et al., 2023). In March 2021, as the new school year began, 32% of schools resumed some in-person activities. However, as COVID-19 cases increased and full lockdowns were reinstated, most schools remained or reverted to remote education (see Figure 1, panels (b) and (c)). In-person activities gradually increased again with the start of the spring semester in July 2021. That same month, the MoH allowed schools in municipalities under Phase 1 to reopen, coinciding with the beginning of the vaccination process among school children (Jara et al., 2023). By the end of the 2021 academic year, 98% of schools had resumed some in-person activities (Claro et al., 2021). Most of the schools, however, operated with shortened school days or alternating days/weeks among the students in order to comply with social distancing requirements.

Low-income schools were less likely to reopen and, on average, reopened for fewer days (Figure A6). However, the most significant predictor of reopening earlier in 2021 was the type of school

administration, which accounted for all the observed socioeconomic differences in reopening probabilities (Kuzmanic et al., 2023). Specifically, schools administered by municipalities were the least likely to reopen. By September 2021, the reopening rates across different school types had converged, but significant differences remained between private schools and other schools (public and state-subsidized private schools) in terms of the percentage of days open and attendance rates. For instance, according to Claro et al. (2021), in November 2021, around 70% of students in private schools attended in-person classes at least once a week, compared to only 40% of students in public and state-subsidized schools. By the end of the academic year, this disparity in in-person instruction was primarily due to students not returning to schools rather than differences in the reopening status of schools (Valenzuela et al., 2024).

A key aspect of school reopening decisions, relevant to the analysis in this paper, is that these decisions appear highly unlikely to be related to changes in the rates of intra-family or sexual violence against children. As outlined further in Section 4, an identifying concern would occur if schools that chose to reopen were located in municipalities experiencing systematically different trends in reported rates of intra-family or sexual violence against children. 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, however, the precise moment of school reopening for each school appeared to depend more on the school's capacity and willingness to meet the required criteria indicated by the Ministry of Education (Valenzuela et al., 2024; Kuzmanic et al., 2023).

3 Data

We collect administrative data from a number of national Ministries in Chile. These data—generally available at the individual or municipal by day level—are aggregated consistently to the level of municipality by week, covering each of Chile's 346 municipalities over the period of (at least) January 2019 to December 2021. In Section 5.2 we discuss analysis over a longer time frame. 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. This includes formal complaints made to the police, as well as crimes caught in flagrante. A single observation is provided for each victim, along with de-

mographic characteristics of the victim 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. We have 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. We generated weekly crime rates for each class of violence (intra-family violence, sexual abuse, and rape), overall, and in sub-groups by age and sex. Rates are consistently generated using detailed population data from Chile's National Statistics Agency (INE).

We complement the main results with data on all cases admitted to the Child Protection System, provided by the new National Service for Specialized Protection of Children and Adolescents. These data include all admissions to the various programs offered by the service, organized by month and municipality.[8] These additional results are reported in the Appendix and discussed in Section 5.1.4.

Educational Information: We obtained administrative records from the Ministry of Education on 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 each 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. For each school, we can observe its type (private or public) as well as measures of the socioeconomic characteristics of the students, namely whether or not students are classified as priority students by the Ministry of Education (a measure of socioeconomic vulnerability). Thus, along with a measure of whether schools are open in each municipality, we can observe the characteristics of schools open over time (see Appendix Figure A6).

[8]The unit of measurement in this data is each program entry, not each child, meaning that a single child might be enrolled in multiple programs within the same year, resulting in multiple entries. Importantly, this data doesn't provide information about victims' characteristics—e.g., age—or type of violence experienced, and is only available for 266 of the 346 municipalities.

We observe data on attendance which is available at the level of student by day. For each student, we observe their school and grade, as well as whether they were reported to attend each day of the academic year.[9] We aggregate these data to a level of municipality by week. In principal models discussed in Section 4 we consider baseline (pre-pandemic) measures of education as at 2019[10], though in additional models discussed in Section 5.2 we use educational measures over a longer period of time. Finally, as a baseline measures of school capacity we observe school-level records of the total number of educational assistants, which includes roles such as psychologists/educational psychologists and social assistants.[11] From these records we can calculate both baseline ratios of educational support staff to students, as well as measures of their experience and tenure in schools.

Additional Measures and Controls: We collected a number of other measures to partially capture potential confounders. This includes hand-collected records of the date municipalities entered any lockdowns during the pandemic period,[12] as well as data on COVID-19 infections, all PCR tests, PCR test positivity, and vaccination rates from open repositories maintained by the Chilean Ministry of Science. These 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 nationwide, and, in principal analyses, we aggregate these to municipal by week records. We additionally measure municipal-level employment rates, available from a 20% sample of all formal sector workers provided by the pension superintendency, allowing us to generate formal employment rates by gender by month.

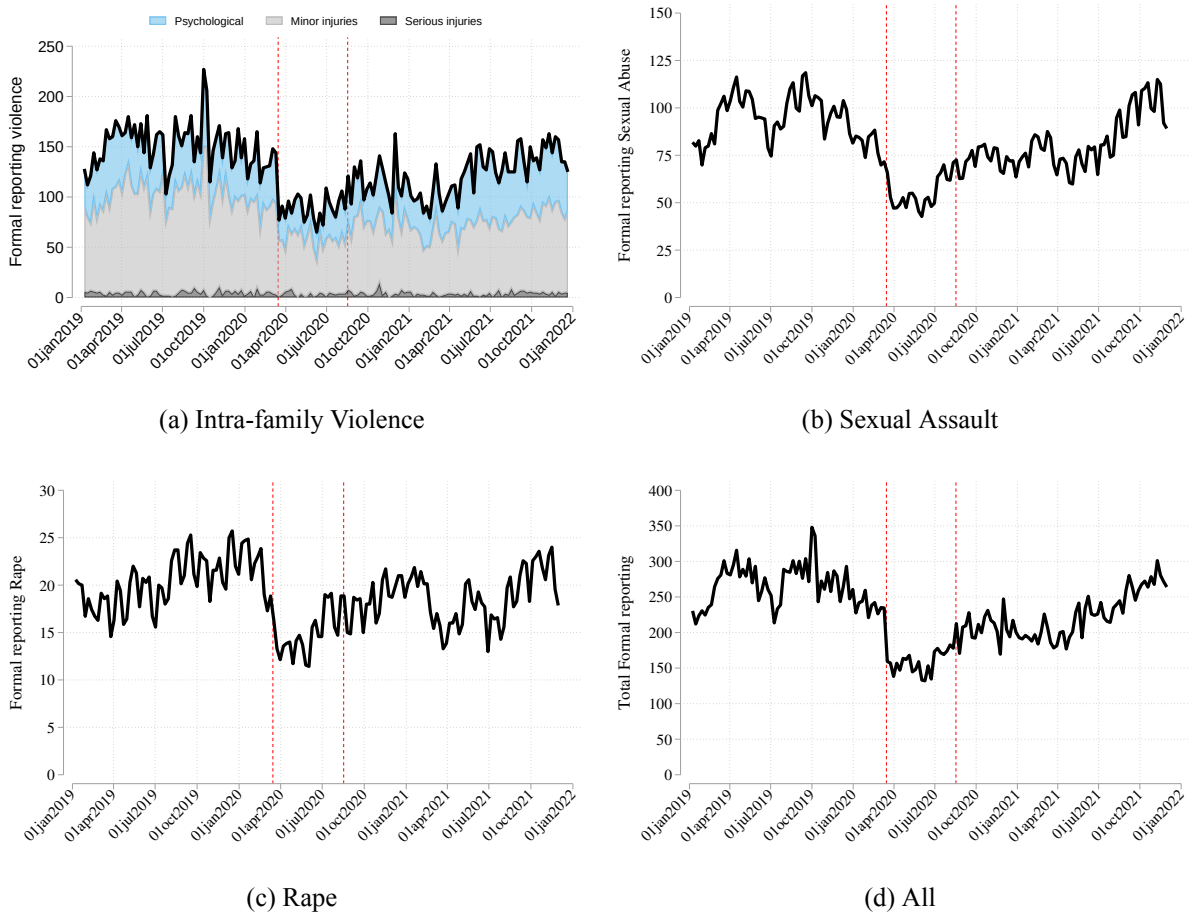
Descriptive statistics of all dependent variables, independent variables, and covariates are presented

[9] As public funding is a function of number of registered students and average monthly attendance (Cuesta et al., 2020), we formally test the consequences of these incentives on attendance mis-reporting. We did so by comparing official attendance reports on days of annual standardized tests, when we observe both administrative attendance records, as well as tests handed in by students. In Figure A7, we document a very close correspondence between these quantities. For example, in the particular case examined, on test day official attendance records suggest 215,002 students attended, while 204,956 tests were handed in (note that a small number of students with special educational needs are not required to complete SIMCE tests). What's more, we confirmed that administrative reporting does not change sharply on test days, instead observing similar reports on non-test days, suggesting that administrative data are trustworthy. Broader discussion is provided in Cuesta et al. (2020). It is important to note student vouchers were paid regardless of attendance between 2020 and 2021.

[10] In Appendix Figures, we note that pre-pandemic attendance correlates with attendance during the pandemic recovery, though attendance measures are only available by school, rather than at the individual level.

[11] *Psicólogos*, *psicopedagogos* and *asistentes sociales* by their names in Spanish.

[12] These were coded by hand by the authors based on daily televised reports made by the MoH, and open repositories of the Ministry of Science of Chile.



(a) Intra-family Violence

(b) Sexual Assault

(c) Rape

(d) All

Figure 2: Temporal Trends – Crimes Reported Against Children

Notes to Figure 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. The first vertical dashed line indicates the day when schools were ordered to close by the central government, while the second vertical dashed line indicates the day when schools were officially allowed to reopen.

in Table A1 in the Online Appendix. These are displayed over the full period of study, from January 1, 2019–December 31, 2021 unless indicated as baseline measures. 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 in Table A1 document variation in measures of school closure, reopening and attendance, and other controls for each municipality by week cell.

The geographic variation in school reopening is displayed in Appendix Figure A8-A9 for all of Chile and the Metropolitan Region of Santiago respectively. We observe considerable variation in initial school reopening dates, also across municipalities *within* the capital of Santiago, despite the fact that contagion rates were generally quite similar in the later case.

Finally, Figure 2 documents trends in reporting of intra-family violence, sexual assault, and rape against children.[13,14] 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 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. While rates of reporting are observed to gradually recover following school reopening, it is not clear if and when rates of reporting recover their prior trajectory. We take these questions forward in this paper.

4 Methods

4.1 Two-way Fixed Effect Models

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:

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

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. $\text{School Closure}_{mt}$ moves from 0 to 1 sharply at the week schools are closed in the country (week of March 16, 2020), and

[13] Longer trends in each of these variables are documented in Appendix Figure A10.

[14] 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 A11, 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.

then remains at 1 until schools reopen in each municipality m, at which point it moves back to 0. Schools Reopen$_{mt}$ is set at zero for the entire pre-closure, and closure period, and switches to 1 the moment that a school first reopens in municipality m. Because schools do not all necessarily reopen in a municipality at the same time, we consider an alternative specification where Schools Reopen$_{mt}$ measures the proportion of students in a municipality m and week t whose school is reopened.

In equation 1, 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. Regressions are consistently weighted by the population of individuals under the age of 18 in the municipality, and standard errors are clustered by municipality. The vector of time-varying controls $\boldsymbol{X}_{mt}$ is discussed in Section 4.3 below, where we discuss identifying assumptions and threats to identification at more length.

As school reopenings occurred in a time-varying fashion, estimates of the effects of school reopening 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 the Online Appendix, we provide the decomposition proposed by Goodman-Bacon (2021), allowing us to document the relatively low concern of this bias in this particular setting (Figure A22).

4.2 Event Study Estimates

As a supporting test of assumptions, and given the importance of considering dynamics in this setting (Goodman-Bacon and Marcus, 2020), we estimate event studies that consider rates of reporting in the lead up to, and following changes in, school closure or school reopening policies. This consists of estimating:

$$\text{Reporting}_{mt} = \sum_{\substack{\ell=-L \\ \ell \neq -1}}^{K} \tau_\ell \cdot \mathbb{1}\{t - E_m = \ell\} + \mu_{WoY} + \phi_m + \boldsymbol{X}'_{mt}\boldsymbol{\xi} + \eta_{mt}, \tag{2}$$

where $\mathbb{1}$ refers to the indicator function, and $E_m = \min\{t : \text{School Closure}_{mt} = 1\}$ for the case of school closures, or $E_m = \min\{t : \text{School Reopen}_{mt} = 1\}$ for the case of re-opening (i.e., the

municipality school closure and reopening dates). Event studies for school closure and school reopening are estimated separately, where in each case τ_ℓ captures pre-event leads and post-event lags. All other details 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 occurrence of school closure or reopening.

When considering school closure, we allow for L=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 reopenings, we consider J=20 leads (20 weeks prior to reopening), 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. Final leads and lags accumulate for all periods greater than this time, so in the case of the 20th lead in school reopening event studies, this will include pre-school closure periods, and similarly, the 20th lag in school closure event studies will include post-school reopening periods. Estimates are presented graphically along with 95% confidence intervals based on standard errors clustered by municipality, and are weighted by municipal population of minors. Because school closure occurs at a single moment in time we label lags and leads based on calendar time; however, in the case of reopenings, which are staggered over time, we label lags and leads simply based on their relative time.

4.3 Threats to Identification

Unobserved Confounders: The estimated effects of school closure and reopening in both event study and fixed effect models are conditioned on the week of year and municipality fixed-effects. Thus, estimates can be interpreted as changes in outcomes when comparing rates of violence reporting under periods of school closures or reopening with rates *in the same municipality and week of year* in years in which such measures had not been adopted. The key identifying concern is that, in a counterfactual scenario where no school closures and reopenings occurred, the COVID-19 pandemic still resulted in an upheaval of many other social, institutional, and policy changes. If these factors are not properly controlled for, we may capture those broader social changes rather than the effects of school closures and reopenings alone. While this concern is likely minimized for the initial effects of closures,[15] it is particularly relevant for municipal-level reopenings.

[15] School closures occurred before sharp increases in infection rates and before the announcement of formal lockdown policies (see Figure A4). We also test whether results vary by municipality lockdown status (Figure A21).

Research on violence reporting highlights relevant confounders, as any time- and municipal-varying factors related to both violence reporting *and* school closures/reopenings will bias estimates. One channel involves changes in mobility patterns, affecting exposure to educational personnel as well as other potential reporters like health providers, social workers, and friends—who, being more emotionally distant from the offender, are also more likely to report (Xie and Baumer, 2019; Felson et al., 2002). This is critical, as reports of child maltreatment and abuse cases are often mediated by other adults (Finkelhor et al., 2001). Another channel relates to perceived costs and benefits of reporting. Victims and witnesses may be less willing to report if they perceive the costs outweigh the benefits (Xie and Baumer, 2019). Fear of contracting the virus could explain the initial decline in reporting, as individuals avoided public spaces due to exposure risks. Conversely, the partial increase in reporting with school reopenings could be linked to declining fear as vaccines became available. On the benefit side, the willingness to report may decline if individuals perceive institutional responses as unlikely, given the reallocation of resources toward enforcing social distancing (Nivette et al., 2021; Kelley et al., 2022). Prior research also shows that socioeconomic disadvantage reduces neighborhood-level reporting, partially mediated by social cohesion (Goudriaan et al., 2006). The pandemic's economic impact, such as job losses and increased financial uncertainty (see e.g., Chetty et al. (2023) and Bhalotra et al. (2024) in Chile), may be related to decreases in reporting. Finally, while school closures occur simultaneously, we may be concerned that variation in the characteristics of reopened schools over time may cause compositional changes in estimated parameters.

For this reason, time-varying controls $\boldsymbol{X}_{mt}$ are included in equations 1-2. When presenting results, we document the impact of the progressive incorporation of controls starting with measures that capture the state of the pandemic and the intensity of other policy measures. Namely, we begin by incorporating 'lockdown and epidemiological controls' which consist of measures 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. These measures are collected in a standardized way nationwide. The logic of such controls is that they allow us to proxy time-varying pandemic intensity, which is correlated with both school closures and reopenings and potentially violence incidence and reporting via increased stress, declines in contact with authorities, declines in contact with other individuals, economic effects of the pandemic, and so forth. However, even conditional on measures of pandemic intensity, relevant factors are likely to be omitted. For example, even conditional on

facing similar rates of infection or lockdowns, changes in employment may vary substantially based on job profiles in a municipality. We thus include an additional sequence of controls, which may capture other relevant confounders. Specifically, we control for formal employment rates by gender, COVID-19 vaccination rates—as a proxy to potential changes in fear and anxiety that may explain changes in reporting, and for the composition of schools that have reopened. As discussed, these are factors theoretically relate to changes in reporting.

While we believe that this rich set of controls likely allows us to capture many of the potential confounders, we recognize that we cannot observe all relevant factors nor measure them entirely without error.[16] Therefore, we consider an alternative test that allows us to purge the effect of these confounders in a different way, which is to conduct a triple-difference analysis comparing rates of reporting among primary school-aged children (5–10 years old) compared with rates among children who have not yet reached school age (0–4 year olds). Specifically, we estimate:

$$\left(\text{Reporting}_{mt}^{5-10} - \text{Reporting}_{mt}^{0-4}\right) = \sum_{\substack{\ell=-L \\ \ell \neq -1}}^{K} \tau_\ell \cdot \mathbb{1}\{t - E_i = \ell\} + \mu_{WoY} + \phi_m + \boldsymbol{X}'_{mt}\boldsymbol{\xi} + \upsilon_{mt}, \quad (3)$$

where other details follow equation (2); however, now considering whether changes in rates of reporting among school-aged children respond in excess of changes in rates of reporting among non-school-aged children. Given the decrease in power when differencing across groups, estimates are reported at the level of the month rather than the level of the week. The logic of this specification is that if the effects of school closure and reopening are in fact driven by other time-varying unobservables, these will likely affect school-aged and non-school-aged children in a similar way. Any differential effects on school-aged children are thus likely to reflect changes in schools rather than other co-occurring changes.

We view the effects from equation (3) as a lower bound, given the fact that some proportion of non-school-aged children do indeed attend formal education (Figure A5), and because there are likely within-family spillovers in reporting whereby reports at school of violence against older children can also result in the detection of violence against younger children. Nevertheless, when considering estimates from equations (2) and (3) together, we argue that we can credibly bound the effects of

[16] For example, we do not have municipal-level data on the institutional capacity of the police, courts, or health system to identify and register cases of violence.

school closures and reopenings on rates of reporting of violence against children. In Section 5.2 we document alternative strategies to quantify these effects in other settings.

Reporting Rates: One of the main challenges with papers analyzing changes in crime due to COVID is the difficulty of distinguishing between changes in actual offenses from changes in reporting, as both could have changed as a result of the policies adopted (Stickle and Felson, 2020; Bourgault et al., 2021).[17] Therefore, a final concern related to the estimated results is that violence levels may have sharply changed, meaning the estimates of reported violence may merely be capturing changes in the incidence of maltreatment. While a decrease in violence is a possibility, we believe it is unlikely (see Benson et al. (2022); Sandner et al. (2024) for a similar argument). First, parents and other family members are the most likely perpetrators of child maltreatment, and children experience a higher risk of violence at home (Valenzuela et al., 2022; Benson et al., 2022; Devries et al., 2018).[18] From a rational choice perspective, violence may increase when the chances of being caught decrease (Berger, 2005). 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 that could increase the chances of detection (Sandner et al., 2024). 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), all of which increased during the COVID pandemic making an increase in violence against children a most likely hypothesis (Figure A13). 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 (see Cappa and Jijon (2021) for review). In Appendix A3 we consider a number of counterfactual scenarios, including alternative projections that provide bounds to missing reports based on alternative assumptions about levels of violence. The results are discussed in Section 5.1.4.

[17] Bhalotra et al. (2024) provide an example analyzing three different indicators of intimate partner violence (IPV) in Chile. They report an increase in calls and shelter use, but a decline in reporting, which, they argue, suggest a raise on barriers to reporting given the lockdowns. Indeed, studies that use survey data are more likely to report increases in domestic violence (Bourgault et al., 2021).

[18] 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 A12). And domestic violence happens, by definition, inside the family environment, with over 80% of the cases taking place at home.

4.4 Gradients in Reporting Recoveries

As reflected in Figure 2, at least descriptively, we do not observe an abrupt recovery in reports of violence against children when schools reopen. We test for gradients in this reporting recovery using a number of (pre-determined) measures that may mediate the role schools play in reporting suspected violence, as follows:

$$\begin{aligned} \text{Reporting}_{mt} \quad = \quad & \alpha + \beta \text{ School Closure}_{mt} + \gamma \text{ Schools Reopen}_{mt} \\ & + \delta(\text{Schools Reopen}_{mt} \times \text{Baseline Measure}_{m}) + \mu_{WoY} + \phi_m + \boldsymbol{X}'_{mt}\boldsymbol{\Gamma} + \upsilon_{mt}. \end{aligned} \tag{4}$$

All elements of this model follow those in equation 1; however, here we additionally include an interaction between school reopening and a series of baseline measures that capture relevant interactions between children and school. Namely, we consider municipal-level averages of attendance rates, and the ratio of specialist school support staff to students, consistently measured as Z-scores based on baseline coverage.[19] Estimated parameters $\widehat{\delta}$ capture gradients in reporting recovery, and, in the most demanding specifications, we additionally include similar interactions between reopening and baseline levels of development and employment rates to consider the relevance of attendance and school support staff conditional on municipal development and proxies of family incomes. This parameter is viewed as descriptive, allowing us to test whether reporting recoveries occur more quickly in areas with a greater latent capacity of capturing violence within schools prior to the shock.

5 Results

5.1 Impacts of School Closure and Reopening on Violence Reporting

5.1.1 Fixed Effect and Event Study Estimates

Table 1 reports estimates from equation 1. Columns (1), (4), and (7) present baseline fixed effect models for each type of violence, while columns (2)-(3), (5)-(6), and (8)-(9) incorporate time-varying controls discussed in Section 4.3. 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 of 5 cases. This is in line with the sharp declines

[19] This allows us to avoid clear endogeneity which may occur if attendance is limited by exposure to violence or if support staff are provided as a response to increasing violence. We note, however, that there is a strong correlation between the baseline and post-reopening measures of each indicator (refer to Appendix Figure A14).

Table 1: Estimated Impacts of School Closure and Reopening on Reporting of Violence Against Children

	Intra-family Violence			Sexual Abuse			Rape		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Binary Reopening Measure									
School Close	-1.593***	-1.164***	-1.168***	-0.955***	-0.910***	-0.909***	-0.099***	-0.072*	-0.073*
	(0.128)	(0.150)	(0.150)	(0.082)	(0.104)	(0.103)	(0.026)	(0.040)	(0.040)
Schools Reopening	-0.852***	-0.877***	-0.853***	-0.360***	-0.580***	-0.555***	-0.051*	-0.074*	-0.080*
	(0.130)	(0.176)	(0.180)	(0.072)	(0.109)	(0.115)	(0.028)	(0.044)	(0.044)
Test of $\beta = \gamma$ (p-value)	0.000	0.059	0.048	0.000	0.000	0.000	0.078	0.965	0.847
Observations	54,250	54,210	54,124	53,215	53,175	53,089	53,215	53,175	53,089
Baseline Mean	5.028	5.028	5.028	2.509	2.509	2.509	0.610	0.610	0.610
Panel B: Continuous Reopening Measure									
School Close	-1.429***	-0.752***	-0.833***	-0.854***	-0.579***	-0.627***	-0.088***	-0.034	-0.032
	(0.121)	(0.124)	(0.129)	(0.077)	(0.081)	(0.082)	(0.024)	(0.031)	(0.033)
Schools Reopening	-0.731***	-0.326*	-0.440**	-0.176*	-0.000	-0.070	-0.039	-0.014	-0.010
	(0.174)	(0.195)	(0.213)	(0.097)	(0.119)	(0.118)	(0.039)	(0.049)	(0.051)
Test of $\beta = \gamma$ (p-value)	0.000	0.043	0.070	0.000	0.000	0.000	0.206	0.693	0.662
Observations	54,250	54,210	54,124	53,215	53,175	53,089	53,215	53,175	53,089
Baseline Mean	5.028	5.028	5.028	2.509	2.509	2.509	0.610	0.610	0.610
Municipal & WoY FEs	Y	Y	Y	Y	Y	Y	Y	Y	Y
Epidemiological & policy controls		Y	Y		Y	Y		Y	Y
School composition controls			Y			Y			Y

Notes to Table 1: Each column presents coefficients and standard errors from separate weighted linear regression models with alternative sets of covariates indicated in table footers. 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 reopening as an indicator of at least one school in a municipality being open, while panel B measures reopening as the proportion of students in the municipality whose school has reopened. 'Test of $\beta = \gamma$' refers to coefficients in equation 1, i.e., the equality of estimates on School Closure and Schools Reopening. Standard errors clustered by municipality are presented in parentheses. *** p< 0.01; ** p< 0.05; * p< 0.10.

observed graphically (Figure 1). Once schools reopen, cases are observed to decline by 'only' 0.85 cases per 100,000 children. This suggests that cases sharply decline upon school closure and increase upon school reopening,[20] but that this increase upon reopening is not sufficient to recover baseline rates of violence reporting. Similar patterns, albeit 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 0.96 fewer cases per 100,000, while post-opening declines are more moderate, at 0.36 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 (reopening). In each case, these values are substantial when compared with baseline rates.

When incorporating the full set of controls, results are ameliorated particularly in the case of school closures, emphasizing the correlated nature of shocks in the COVID-19 period. For example, in the case of intra-family violence, results with full controls suggest declines of 1.2 fewer cases per 100,000 children, compared to 1.6 fewer cases per 100,000 in baseline fixed effect models. Nevertheless, the substantive patterns are similar, 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 and hence least powered outcome, while we still observe reductions in rates of criminal reporting both during school closure and reopening, we can no longer conclude that reporting rates *increase* compared to closure periods when moving from closure to reopening.

Results documented in Panel A of Table 1 are based on binary measures of first school reopening. However this may underplay the importance of reopening, particularly in municipalities with more schools where reopening occurred only gradually. Panel B re-estimates the models with a measure of the continuous proportion of students whose school was reopened, 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 the first reopening.[21] Here, in the case of intra-family violence, we observe that, even if school reopening does indeed become complete, rates of reporting

[20] We formally test the difference between coefficients on closure and reopening, 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).

[21] 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%.

are still estimated to be below baseline rates. Estimates fully conditioning on time-varying controls in column (3) suggest complaints would still be 0.44 per 100,000 children lower than in baseline periods, though p-values of tests of differences are only marginally significant ($p = 0.07$) in column (3).[22] As in panel A, we consistently observe sharp reporting declines with initial school closures.

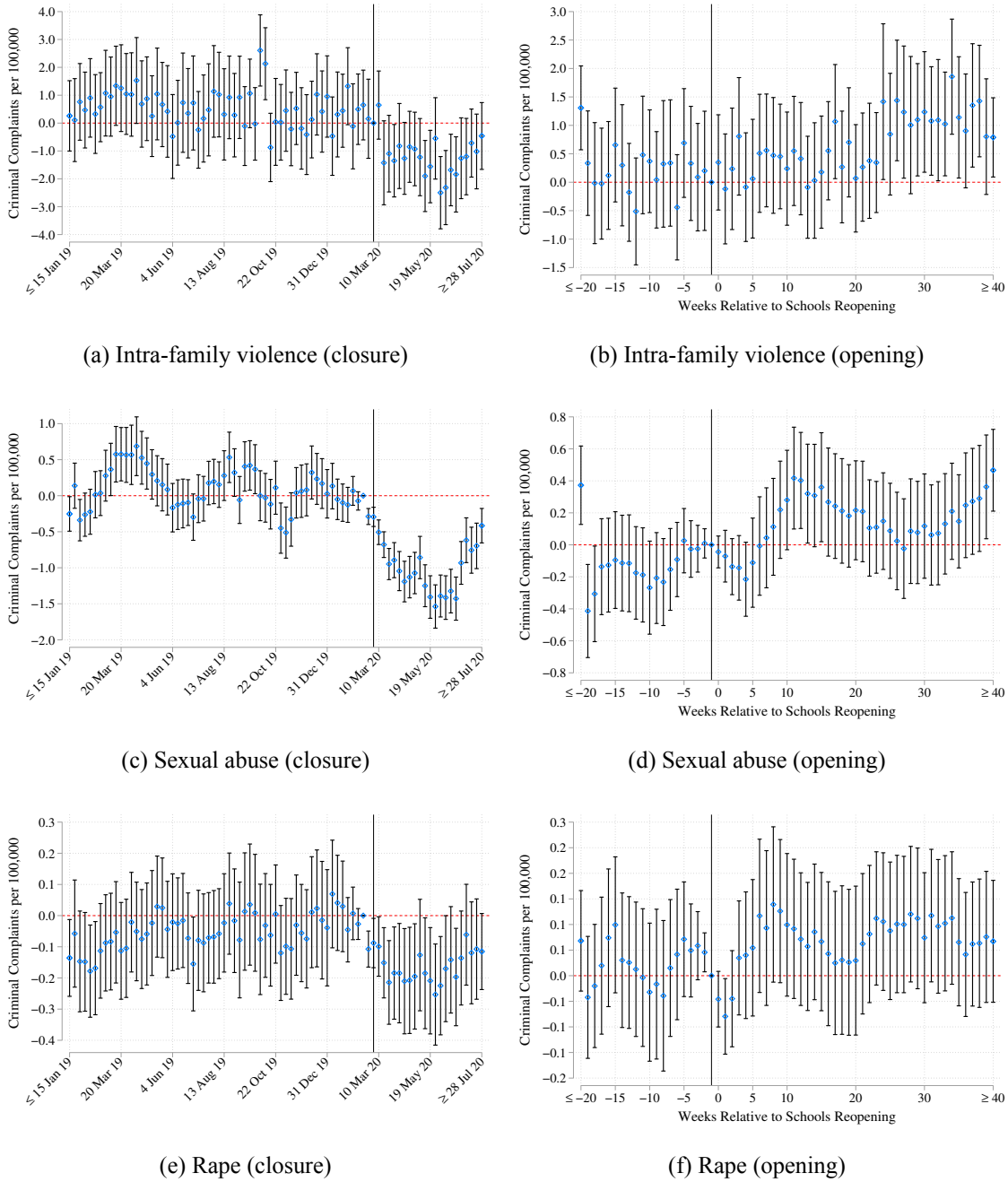
In the case of sexual abuse and rape, we once again observe sharp declines in reporting upon school closure, and evidence to suggest that reopening may provide recovery in rates of complaints, at least compared to baseline levels (see Section A3), once reopening reaches 100%. In columns (6) to (9), we observe that when schools fully reopened, we cannot reject that rates of reporting would have been the same as in pre-closure periods, with all point estimates in Panel B being slightly negative, but again not statistically distinguishable from zero.

Event study models are displayed in Figure 3,[23] for each of the three main outcomes and for school closure and 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 and 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, in line with the lags in time between schools being allowed to open and the children returning to school and to interact with educational professionals. Given that over-reported data from the first day of each month is smoothed throughout the month in the case of sexual abuse and rape, in these cases we use period -3 as baseline, as this ensures that the baseline period is in the calendar month prior to school closure, and will not be reflected in small pre-closure declines. 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 sexual abuse reporting observed clearly in Figure 2 (in line with lower rates of reports in winter and summer school vacation periods, even in the pre-school closure period).[24] We note, additionally,

[22] 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 reopened, 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 reopening.

[23] Versions of the models with graduated controls are included as Appendix Figures A15-A16, and are observed to be largely unchanged. However, Figure A17 shows the co-evolution of reporting and COVID-19 infection rates, making clear the importance of considering these controls.

[24] While week of year fixed effects capture such generic seasonality, rates of sexual abuse reports are increasing over time, leading to larger fluctuations around trend, picked up in this figure.



(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 Reopening on Reporting of Violence

Notes to Figure 3: Event studies are documented as described in equation 2. 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 2.

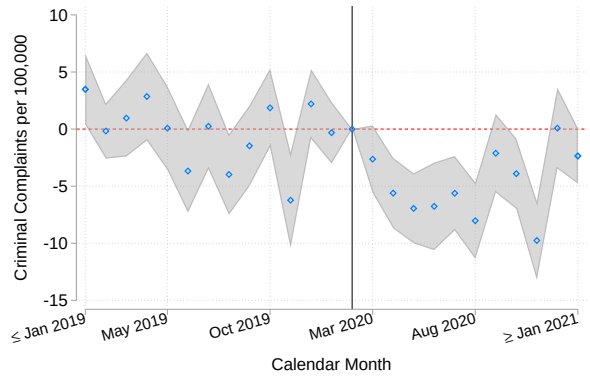
that even despite this variation, rates of sexual abuse reporting are observed to be at their lowest immediately following school closure.

5.1.2 Triple Difference Estimates

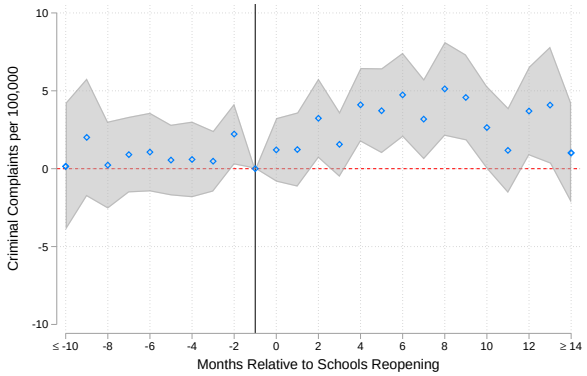
Figure 4 presents the triple difference estimates, as well as the underlying double-difference estimates for older (exposed) and younger (less exposed) individuals. Left-hand panels consider school closures, while right-hand panels consider reopening. Top panels (a) and (b) present event study estimates for school-aged individuals, while middle panels (c) and (d) present estimates for children below school age. Finally, panels (e) and (f) present formal tests of the triple-difference estimate. Given that these estimates are based on sub-samples and are somewhat more noisily estimated, we aggregate effects by month (refer to Appendix Figure A18 for monthly event studies for the full sample).

Consistent with our expectations, the effects are larger among school-aged children. Declines in reporting are more pronounced in panel (a) than panel (c), with magnitudes generally at least 2 times larger among school-aged children than younger children. In the case of reopenings, we observe statistically significant increases in reporting from around 4 months after first reopening among school-aged children (panel (b)), while no significant effect among younger children (panel (d)). We present triple differences estimates which purge the effect of any generalized differences across ages in panels (e) and (f). We see that the effects discussed above are significantly larger among school-aged children both in closure and reopening, suggesting that the aforementioned results are not simply capturing effects outside of school. As already discussed, we view these results as conservative (i.e., attenuated) for two reasons. The first is that some proportion of younger children actually *do* attend formal educational institutions, as documented in Figure A5. The second is that even though younger children who are outside of school may not be picked up by contact with school staff, violence against them may also be detected if they have older siblings for whom violence is reported.

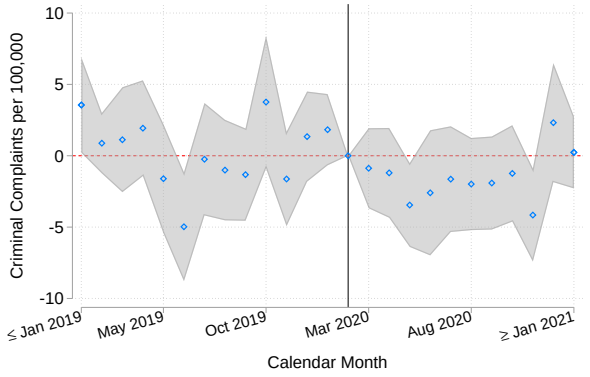
These results are illustrative in allowing us to assess the results previously documented based on the inclusion of covariates. In order to compare these estimates, in Appendix Table A2 we present single-coefficient age-specific double difference estimates following equation (1) as well as the corresponding triple difference estimator. If comparing the triple difference effects to the effects among



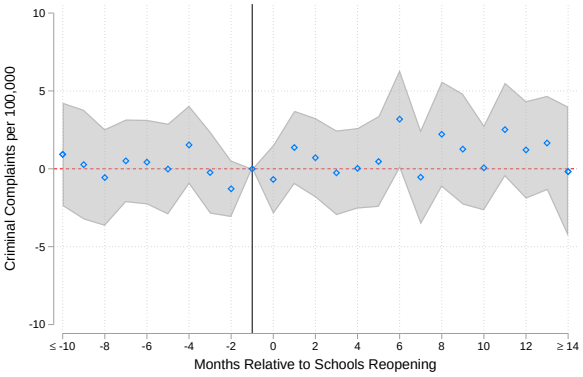
(a) Closure "Difference 1" (5-10 years)



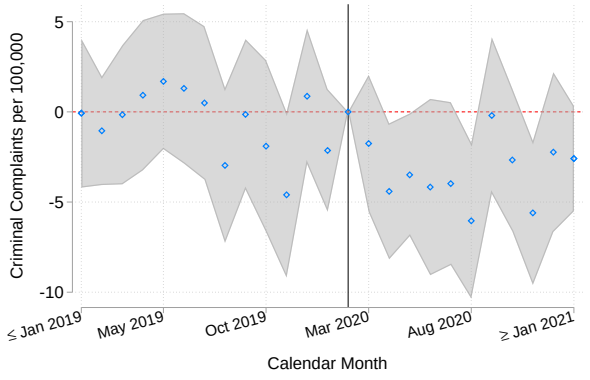
(b) Opening "Difference 1" (5-10 years)



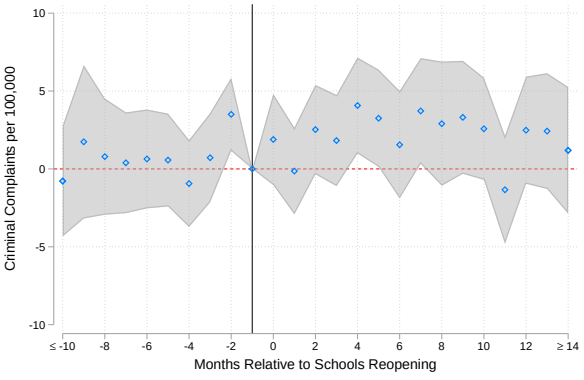
(c) Closure "Difference 2" (0-4 years)



(d) Opening "Difference 2" (0-4 years)



(e) Closure Triple Difference



(f) Opening Triple Difference

Figure 4: Triple Differences and Exposure to School Closure

Notes to Figure 4: Event studies following (2) are presented for primary aged (5–10; panels (a) and (b)) and pre-primary aged (0–4; panels (c) and (d)) year old children. A triple difference specification is estimated in panels (e) and (f) following (3). Event studies are estimated based on month by municipality cells. Outcomes are consistently reported as per 100,000 individuals of each age group. Point estimates are presented as blue diamonds, and 95% confidence intervals based on standard errors clustered by municipality are presented as grey shaded areas.

just school-aged children, the triple difference effects are 65.6% as large in the case of school closure and 86.5% as large in the case of school reopenings. In the case of models with controls analogous to those in Table 1, estimates among school-aged children are 86.3% as large in the case of school closure and effectively unchanged in the case of school reopening. If we view triple difference estimates as likely over-controlling for unobservables and difference-in-differences estimates with controls as under-controlling, combining these estimates suggests that the degree of uncaptured unobservables in double difference estimates is relatively minor, bounded between 65.6 and 86.3% of the unconditional effect in the case of closure, and above 86.5% of the unconditional effect in the case of reopening. This provides support for our difference-in-differences effects with controls reported previously.

5.1.3 Gradients in Reporting Recoveries

We test for any gradients in reporting recoveries as per equation (4), with results presented in Table 2. In columns (1)-(2), we consider whether reporting recoveries occur more quickly with school reopenings in areas with higher baseline attendance, while columns (3)-(4) consider whether gradients are observed based on baseline availability of support staff for students in school. Finally, column (5) includes all measures together, permitting a conditional interpretation.

We observe that reporting recoveries occur significantly more rapidly in areas with higher baseline attendance. Grouping across all violence classes and time periods, we observe that areas with a 1 standard deviation higher rate of attendance had 0.25 more reports per capita once schools re-opened, or around 10% of the magnitude of the initial decline in reporting with school closures. In column (2), we observe that this value does not simply capture municipal-level development or proxies of family income, with gradients still being substantial and statistically significant. A similar, though noisier, pattern is observed in unconditional models when considering rates of availability of educational assistants in schools (column (3)). These results do not hold, however, once conditioning on other municipal-level factors in column (4). While such gradients are only indicative of heterogeneity of responses and could indeed be capturing correlates of attendance, the results in column (5) appear to point to the relevance of attendance in mediating reporting. Here, even conditional on all other measures considered, we see that areas with higher baseline attendance recovered reporting significantly faster, in a way not observed with other potential mediators.

Table 2: Gradients in Reporting Recoveries in Municipal Characteristics

	Attendance		Educational Assistants		All
	(1)	(2)	(3)	(4)	(5)
School Closure	-2.551***	-2.530***	-2.464***	-2.475***	-2.519***
	(0.193)	(0.193)	(0.190)	(0.188)	(0.190)
School Reopening	-1.742***	-1.484***	-1.524***	-1.384***	-1.444***
	(0.243)	(0.279)	(0.250)	(0.263)	(0.267)
School Reopening× Baseline Attendance (Z)	0.250***	0.216***			0.211***
	(0.071)	(0.071)			(0.070)
School Reopening× Baseline Support (Z)			0.318*	0.140	0.078
			(0.183)	(0.211)	(0.204)
School Reopening× Baseline Development (Z)		-0.119		-0.160	-0.098
		(0.168)		(0.185)	(0.185)
School Reopening× Baseline Employment (Z)		-0.234		-0.197	-0.233
		(0.209)		(0.208)	(0.208)
Baseline Mean	8.14	8.14	8.14	8.14	8.14
Observations	54,250	54,164	54,164	54,164	54,164

Notes: Each column reports results of equation (4). Columns 1-4 incorporate baseline municipal-level measures in a sequential fashion, while column 5 incorporates all measures in a single model. Epidemiological and policy controls included in Table 1 are consistently included. All specifications are weighted by the municipalities population of children. Standard errors clustered by municipality are presented in parentheses below coefficient estimates. *** p< 0.01; ** p< 0.05; * p< 0.10.

While we focus on baseline municipal characteristics to avoid capturing the effect of the COVID shock itself, we note that such historical measures strongly correlate with contemporaneous measures (Figure A14). These results suggest shocks may be ameliorated in areas in which attendance is higher and that attendance in particular seems to be a key channel. We return to examine how reporting and attendance line up in another setting when considering the timing of school vacations in Section 5.2.

5.1.4 Additional Results and Identification Checks

A number of additional results are displayed in the Online Appendix to this paper. We summarize here some key points laid out in these alternative models and consistency checks.

Child Welfare: We replicate these results using data from all cases admitted to the Child Protection System in Chile. This measure provides a complementary view of the total number of cases addressed by the protection system, regardless of how they are identified (Figure A3). Results are reported in Appendix Figure A19, and are largely consistent with what we observe with police data: a significant decline in entries when schools were closed–although larger in magnitude when com-

pared with police data (see Figure A18), and a slow–but sightly earlier–recovery with reopening.[25]

Heterogeneous Effects: Figure A21 documents variations in estimates from Table 1 within different sectors of the population. In line with effects being driven by school-aged children, 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 is consistent with schools acting to channel complaints most frequently for children above 6 who are most connected to school systems, and slightly reduced when children are older (16-17) and potentially more able to make their own complaints. Effects are larger among girls than among boys, and much larger in the case of sexual abuse and rape, in line with the much higher incidence rates among girls and boys. 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 reopening, we generally observe that declines in reporting are substantially reversed, regardless of a municipality's lockdown status.

Additional Specification Checks: Results hold when eliminating months of summer vacations when schools are closed (Table A4) 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 (Table A5). When we disaggregate intra-family violence by specific classifications (Table A6), 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.

[25] The limitations of these data are discussed in Section 3. These results are complemented with a descriptive analysis of the changes in the different institutional channels that refer children to the system, based on detailed data provided by the OPD of a large municipality in the capital city of Santiago. Figure A20 and Table A3 in the Appendix show a sharp decrease in the number of referrals coming from schools in March 2020, and a slow recovery, but not to pre-pandemic levels, during the second semester of 2021. We also observed a decrease in cases reported by the health system, particularly during the first months of the pandemic.

In the case of estimated impacts of school reopening, staggered adoption of reopening is potentially affected by the forbidden comparison problem (Goodman-Bacon, 2021; de Chaisemartin and D'Haultfœuille, 2020). In Appendix Figure A22 and Appendix Table A7 we consider whether this is likely to substantively affect global estimates documented in Table 1. In Figure A22 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 Table A7, 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 the 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.

Counterfactual Projections of Reporting: Finally, to understand the impact of school closure, as well as the dynamics of recovery, we conducted a number of counterfactual projections (Appendix A3). Figure A26, panel A, documents how complaints of violence against children would have performed if projecting the cyclical (week of the year) and temporal trends of the pre-pandemic period. In panel B, we consider an alternative projection 'turning off' the school reporting channel to estimate its contribution. Finally, in panel C of Figure A26, we consider alternative projections in which we assume, following the prior literature, that violence may actually have increased (see Section A3 for a more extensive discussion). As we do not know the magnitude of the increase, we provide bounds estimates based on different levels. This bounding exercise aims to reflect on the even larger

number of cases missed had the violence, in fact, increased. As a lower bound, these projections suggest that there were approximately 1,500 'missing' reports of violence against children during periods of school closure versus around 2,500 missing cases once schools were permitted to reopen.

5.2 Broader Implications

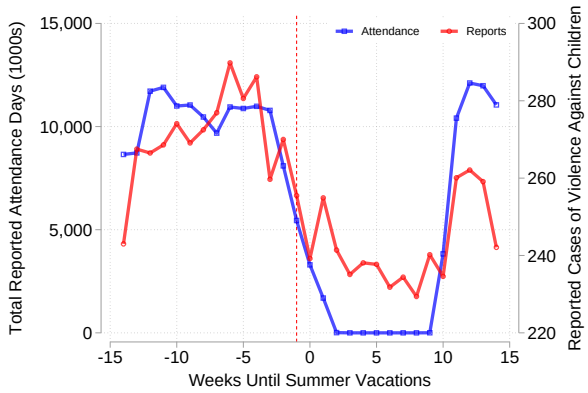
What can we learn from these results? Concerns about school closures and child well-being may be muted if these lessons are limited to extreme circumstances such as the COVID-19 pandemic and are unlikely to be informative of the costs of school closures or lost school days. To consider whether these results may be indicative of the costs of school closure more generally, we consider two additional designs, each in the same setting but with quite different drivers of school closures. A first design considers whether regular (predictable) school closures—namely school vacations—echo into violence reporting. A second design considers whether a particular sharp shock to school attendance in certain areas, grades, and times is reflected in violence reporting. This latter case considers school closures driven by large student strikes in Chile in 2011 which have been documented to have large impacts on attendance (González, 2020; González and Prem, 2022).

5.2.1 School breaks

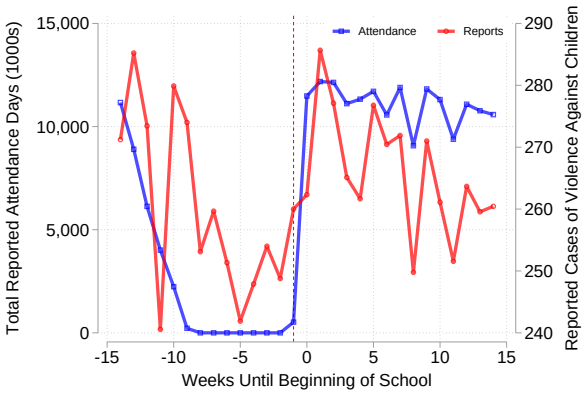
We examine reporting declines in a broader setting by analyzing school vacations during the period 2012–2017. This allows us to assess whether the findings from the previous analysis apply to more routine and expected interruptions, while also considering how school attendance and reporting patterns align in this alternative setting. Benson et al. (2022) have documented the relevance of school vacations in driving violence reporting in the United States, and descriptively, we observe results that support their findings. Even when considering simple cyclical variation in violence reporting in Figure A23, clear declines are observed in school closure months corresponding to summer vacations (December-February), as well as winter vacations (July). Such yearly troughs are recurrent and occur only in months when schools are closed. To examine this more formally, we collected data on all dates of school vacations in Chile occurring between 2011–2019, which are published by the government for both winter and summer vacations for each of the country's 16 regions (Table A8)[26]. Figure 5 plots the total reported violence against children as well as the total reported days of

[26]While we have data on vacation dates from 2011–2019, our analysis focuses on the period of 2012–2017 because we are interested in analyzing lags and leads of vacation periods, which requires sub-setting to years in which we observe vacations in prior and following years. We end our analysis in 2017 rather than 2018 to avoid vacations in 2019 which are close to the COVID period studied previously, though results are not sensitive to this choice.

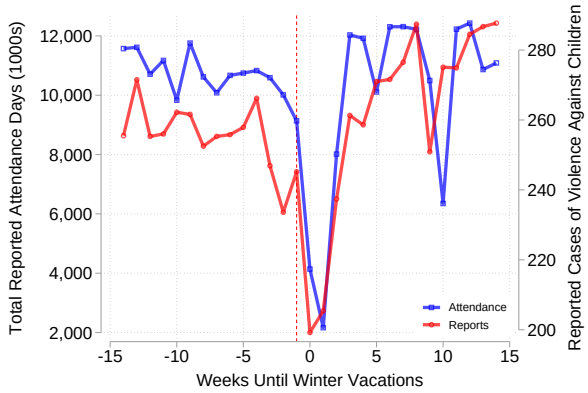
attendance by all students in the country in each week relative to the start and end of summer vacations (panels (a) and (b)), as well as surrounding winter vacations (panels (c) and (d)). These results make two points quite clear. First, declines in violence reporting exist with school closures with corresponding recoveries with school reopenings even with programmed school vacations. Second, these recoveries appear to scale in line with attendance, though perhaps with some evidence of muted recoveries at the beginning of the school year.



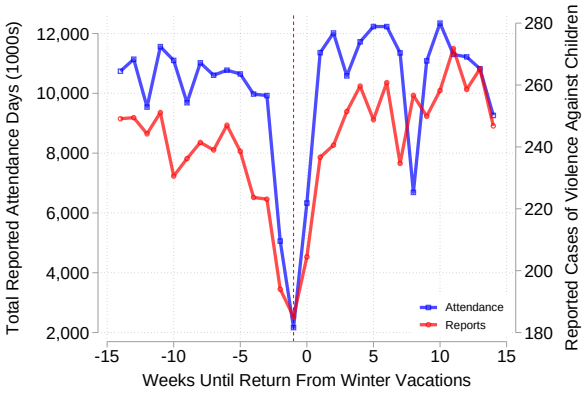
(a) Summer Vacations (Leaving)



(b) Summer Vacations (Returning)



(c) Winter Vacations (Leaving)



(d) Winter Vacations (Returning)

Figure 5: Reported Violence Against Children, School Attendance and School Vacations

Notes to Figure 5: Total numbers of cases of reported violence against children (red line, right-hand axis) and total numbers of days attended by school students nationwide (blue line, left-hand panel) are reported by weeks relative to school vacations. In each case, weeks relative to vacations are year and region-specific, as it depends on dates dictated by the Ministry of Education which vary at this level. Each quantity is aggregated from microdata and refers to all quantities in the week. In the case of attendance, this refers to the total number of days by week, so a week of 5 days will sum 5 for students that attended each day, thus accounting for shorter weeks or public holidays.

We quantify the magnitude of these effects by estimating the following equation:

$$\text{Reporting}_{mt} = \beta_0 + \beta_1 \text{Vacation Period}_{mt} + \beta_2 \text{Post Vacation Period}_{mt} + \mu_m + \mu_{WoY} + \varepsilon_{mt} \quad (5)$$

where rates of reporting (and in alternative specifications, rates of attendance) for municipalities m and weeks t are regressed on an indicator for schools being in a vacation period or being in a period just following vacations. Thus, β_1 will capture declines in reporting when passing from terms to vacation, and β_2 will capture rates of reporting in periods just following vacations compared with the rest of the term. Municipal fixed effects are included as μ_m. We classify the post-vacation period as the month following the return to school. Because violence reporting is not flat within the school year, we consider an alternative specification where we include an indicator for term time before the month immediately preceding vacations, as in this case, all effects will be cast as relative to the period just before vacations. We initially consider specifications without week of year fixed effects μ_{WoY} given that many vacation weeks are always in the same period each year, though the richest specifications do include week of year fixed effects. In this case, identification is drawn off of the weeks of vacation which vary across each year (generally winter vacations, and the first few weeks of summer vacations).

Table 3: Vacations, Attendance and Reporting of Violence Against Children

	Versus Term Times		Versus Pre-Vacation Times		Week of Year FEs	
	Attendance	Reporting	Attend.	Reports	Attend.	Reports
	(1)	(2)	(3)	(4)	(5)	(6)
Vacation Period	-0.710***	-0.812***	-0.463***	-0.430**	-0.103***	-0.882***
	(0.005)	(0.079)	(0.017)	(0.173)	(0.014)	(0.277)
Post-Vacation Period	-0.061***	-0.239	0.136***	0.067	0.065***	-0.419
	(0.009)	(0.148)	(0.013)	(0.195)	(0.010)	(0.271)
Term (Non Pre-Vacations)			0.261***	0.404**	0.089***	-0.813***
			(0.014)	(0.182)	(0.011)	(0.302)
Constant	0.795***	7.041***	0.544***	6.652***	0.574***	7.633***
	(0.001)	(0.021)	(0.014)	(0.172)	(0.007)	(0.255)
Observations	126,636	126,636	126,636	126,636	126,636	126,636
Dep. Var. Mean (Term Time)	0.796	7.043	0.796	7.043	0.796	7.043
Municipal FEs	Y	Y	Y	Y	Y	Y
Week of Year FEs					Y	Y

Notes to Table 3 Estimates presented correspond to equation (5). Observations consist of municipality by week cells between the period of 2012 to 2017. Summary statistics are provided in Table A9. Results are consistently weighted by the population of 5–18 years olds in a municipality, and standard errors are clustered by municipality. *** p< 0.01; ** p< 0.05; * p< 0.10.

Descriptively, results presented in columns 1-4 of Table 3 suggest that attendance declines by around 90% in vacations,[27] while reporting declines by around 11%. In columns (1) and (2) we observe that while attendance is slightly lower in the post-vacation period than in the rest of the year, reporting is not significantly lower (although effect sizes do point in that direction). In columns (3) and (4) we observe that if instead we compare post vacation periods with periods immediately preceding vacations, we observe slightly elevated rates of attendance after vacations, and correspondingly slightly higher (though not statistically distinguishable) increases in rates of violence reporting. Columns (5) and (6) additionally condition on week of year FEs. These models effectively compare attendance and reporting in similar weeks of the year between years where vacations occur and where vacations do not occur. These models thus derive identification from quite specific weeks in which vacations vary across years. For example, the main weeks of summer vacations where attendance is zero always occur in February, and hence will not contribute to estimates of vacations given multicolinearity with week of year FEs. Nevertheless, columns (5) and (6) point to significant declines in attendance and reporting with vacations, recoveries in attendance upon reopening, with weak evidence of slightly depressed rates of reporting, at least initially. These findings (echoing Benson et al. (2022)) appear to highlight the relevance of contact with schools in channeling violence reporting even outside of the COVID-19 period, and suggest that attendance and reporting line up well, though not perfectly.

5.2.2 Students protests

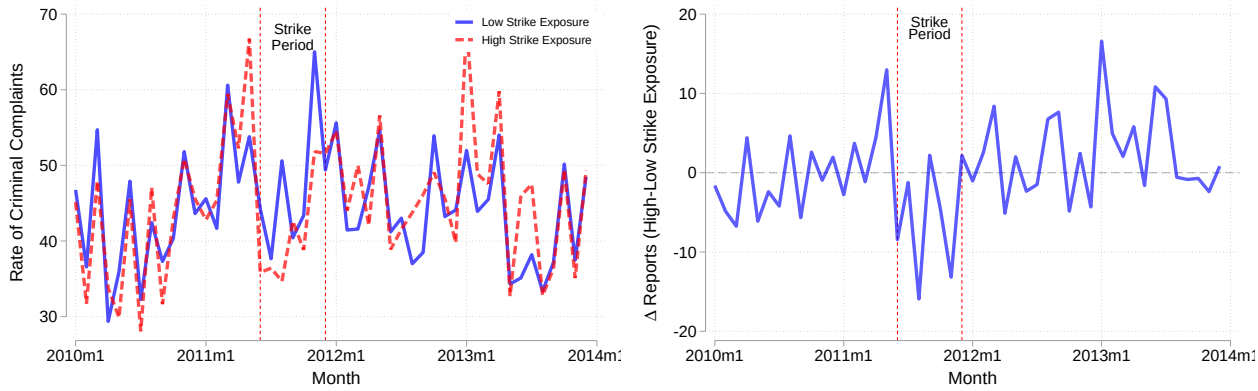
To further test for the relevance of schools in channeling violence reports, we consider unexpected and sharp changes in school attendance driven by student strikes in 2011 (the Chilean student movement of 2011). These strikes, previously studied by González (2020); González and Prem (2022); and Celhay et al. (2024), resulted in substantial loss of school days, with this loss varying considerably by municipality. Student strikes occurred overwhelmingly at the secondary level, and so in principal analyses we consider rates of reporting among secondary school students only. The increase in strike activity is notable following a strike day that occurred on June 1st, 2011, as discussed in (González, 2020), and evident in daily attendance figures plotted in Figure A24.

[27] We do not expect 100% declines because some minor temporal modifications can be made in vacation dates in certain schools, particularly during winter vacations, and because certain schools such as those with special educational programs remain in session during winter vacations.

As our principal estimation strategy, we follow a model previously proposed by Celhay et al. (2024) to study rates of teenage pregnancy, which consists of regressing an outcome (in our case, violence reporting) on time and municipal variation in exposure to school strikes. Specifically, for each municipality m and month t, considering the period 2010-2017, we estimate:

$$\text{Reporting}_{mt} = \alpha + \beta \text{Strike Intensity}_{mt} + \phi_m + \lambda_t + \varepsilon_{mt}, \tag{6}$$

where $\text{Strike Intensity}_{mt} = \text{Strike Period}_t \times \frac{\sum_{s=1}^{S_m} \text{Days Absence}_{smt}}{S_m \times \text{School Days}_t}$. The measure Strike Period_t takes 1 for months in which strikes occurred (June 2011-December 2011 inclusive). This is interacted with municipal-level averages of absences among all secondary students (S_m) in each municipality. We use (6) as our principal specification given that it has been previously proposed in the literature, but present in the Online Appendix results from a triple difference strategy differencing across primary and secondary students given that primary students were much less likely to strike (see Figure A25).



(a) High versus low strike exposure

(b) Difference

Figure 6: Complaints of Intra-family Violence Against High School Children

Notes to Figure 6: Monthly rates of total criminal complaints of violence against children (intra-family violence, sexual abuse, or rape) are plotted over time, separately for municipalities with above- and below-median exposure to student strikes. Panel (a) documents mean reporting rates in both municipalities, while panel (b) reports differences between high and low exposure areas.

In Figure 6, we present descriptive figures showing rates of reporting of violence against secondary school-aged children occurring in areas more and less exposed to strikes. We observe a relative decline in reporting in areas more exposed to strikes during the strike period than in other periods. In Table 4 we present results corresponding to equation (6). Here, we see a significant decline in

Table 4: Student Strikes in 2011 and Intra-Family Violence Against Children

	All Complaints (1)	Intra-family Violence (2)	Sexual Abuse (3)	Rape (4)
Strike Intensity × Strike	-29.47*	-21.03*	-5.466	-2.969
	(15.15)	(11.39)	(5.332)	(2.722)
Observations	31,356	31,356	31,356	31,356
R-squared	0.16	0.13	0.07	0.03
Scaled Effect	-2.75	-1.96	-0.51	-0.28
Mean Dep. Var.	55.75	40.93	11.12	3.70

Notes to Table 4: Estimates are presented corresponding to β in equation (6). Each observation is a municipality by month cell, and is weighted by the population of high school students. Summary statistics are provided in Table A9. All outcomes are presented as rates, with average rates indicated in the table footer. The Scaled Effect refers to a standard deviation increase in strike intensity (approximately 17% lower attendance). Standard errors clustered by municipality are presented in parentheses. *** p< 0.01; ** p< 0.05; * p< 0.10.

reporting in areas more affected by student strikes precisely when student strikes occurred. When considering all complaints of violence against children, we estimate that a one standard deviation exposure in strike intensity (corresponding to 17% lower attendance rates) results in 2.88 fewer complaints per 100,000 teenagers in a given month, or around a 5% decline in reporting compared to baseline rates. We observe that this decline is statistically significant when considering reporting of violence against children, and although also large and negative in the case of sexual abuse and rape, not statistically distinguishable from zero.

These results suggest that declines in reporting are observed even at the secondary level, and in the face of a quite different shock. Similar to the triple differences specification documented in Section 5.1.2, in Appendix Table A10 we note that these results effectively are driven by secondary students (exposed to the strike) rather than primary students, providing further support for this identification strategy. The fact that we observe declines in reporting in three different settings (pandemic, vacations, and strikes) with different identification strategies suggests a broader relevance of these findings beyond the extreme circumstances of the COVID-19 pandemic.

6 Conclusion

Early detection of child maltreatment is a fundamental step in preventing the continuity and escalation of violence. Educators have a privileged position in this regard, given their daily in-person interactions with children. That interaction, however, can be interrupted, with potential negative impacts on child well-being. Indeed, one of the most widespread policies to respond to the COVID-19 pandemic was the closure of schools, which, for some children, remained closed for almost two years.

We study the impact of school closure on reports of violence against children and whether resuming in-person interactions leads to a subsequent recovery in reporting. Our results suggest a significant and sharp decline in reports for different types of violence—intra-familiar, sexual, and rape—once schools were closed and a slow and incomplete recovery with reopening. While the results are observed across different contexts and sociodemographic groups, they are largest among school-age children, suggesting that the changes observed owe to changes in in-school interactions rather than to other (also impacted) non-school channels. We also discuss two potential school-level mediators that may explain slow reporting recoveries: a school's ability to identify cases of violence and in-person attendance at schools. We observe that higher levels of historical attendance are associated with a faster recovery in reports.

While the COVID-19 pandemic provides a unique opportunity to study the relevance of schools in identifying child maltreatment, its unprecedented nature raises questions about whether it is informative for other planned and unplanned interruptions in schooling. Using two additional identification strategies, we find that reporting rates consistently decline with school closures, both in cases of unplanned shocks (such as student strikes) and scheduled vacations. This suggests that findings from COVID-19 in this setting are broadly informative about the role of schools in identifying violence. Additionally, in contrast with the slow recovery observed after COVID, attendance and reporting move in concert during regular school years, with reporting rates after vacations being indistinguishable from average rates across the school year. These results further point to the importance of face-to-face contact in facilitating reporting.

The results of this study confirm that schools act as a social safety net for children, detecting and formalizing complaints for violence that otherwise may be left undetected. Their role in protecting

children is likely substantially interrupted when school days are lost due to holidays, weather, war or natural disasters, and when attendance is chronically low. Such costs need to be considered in any calculus of the effects of school closures and in the development of alternative child protection strategies when schools are not in session.

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

Pilar Larroulet, Daniel Pailañir, Daniela Quintana & Damian Clarke

A1 Supplementary Figures

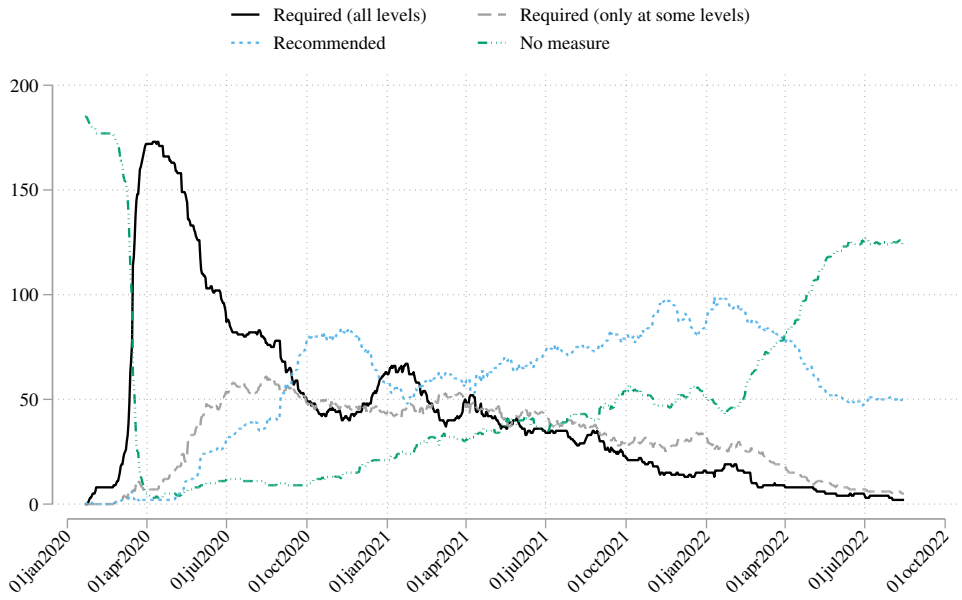


Figure A1: Country-Level School Closure Policies

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

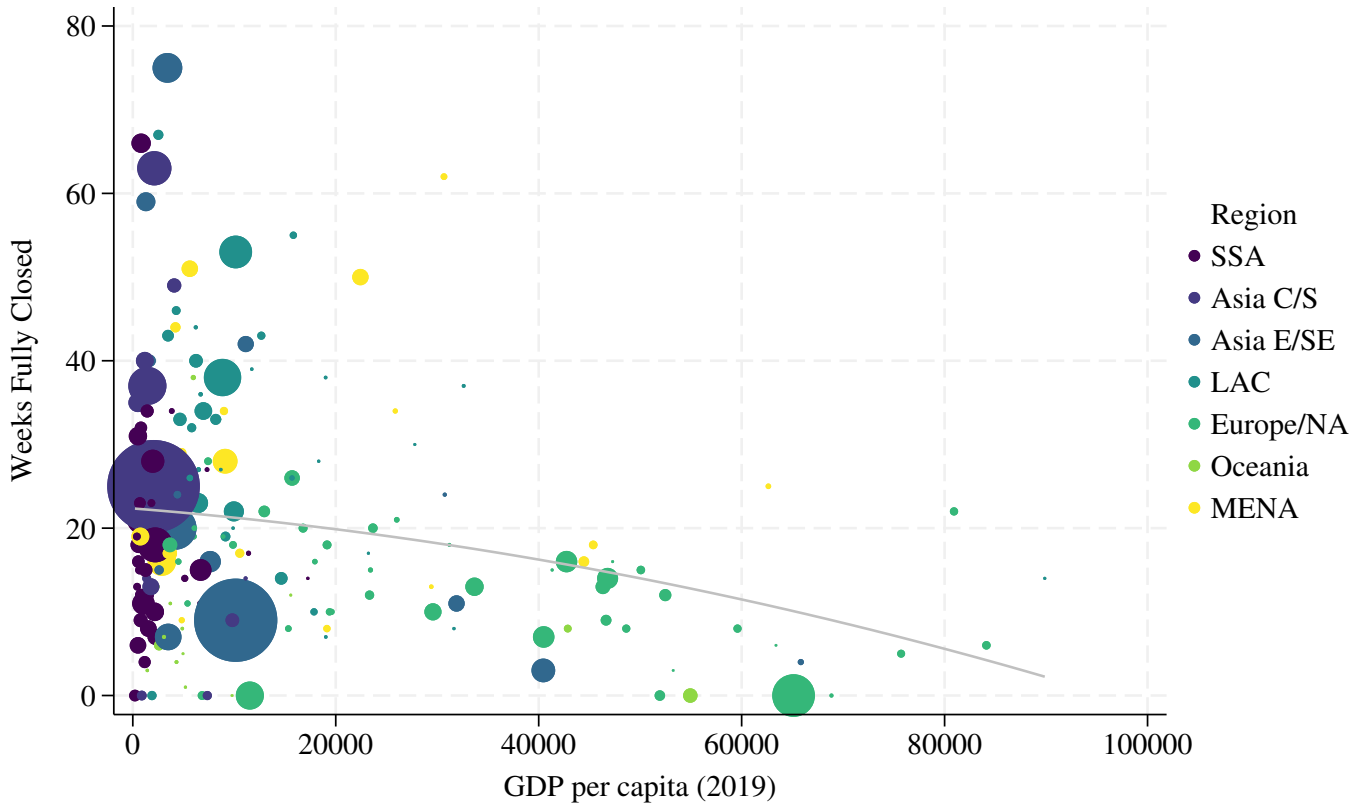


Figure A2: Duration of School Closures and Country Income Levels

Notes to Figure A2: Scatter plot displays the length of school closures as reported by UNESCO (2022), and GDP per capita in 2019 (pre-school closures). Each point represents a single country, and the size of each point indicates the total enrollment for this country. Points are colored by region. The gray line plots a quadratic fit. A very small number of countries with GDP above 100,000 USD per capita have been omitted for ease of vizualisation.

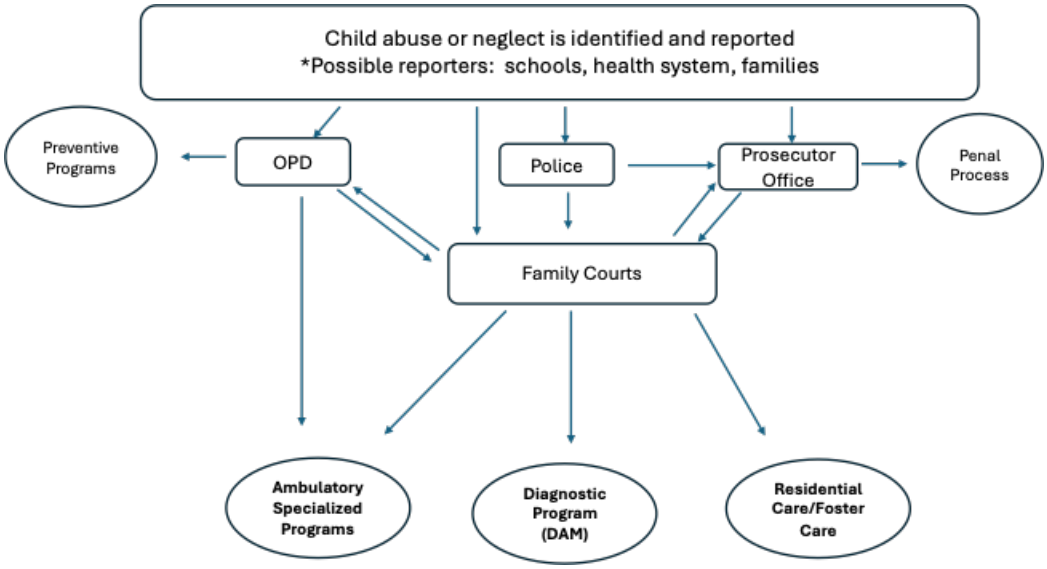
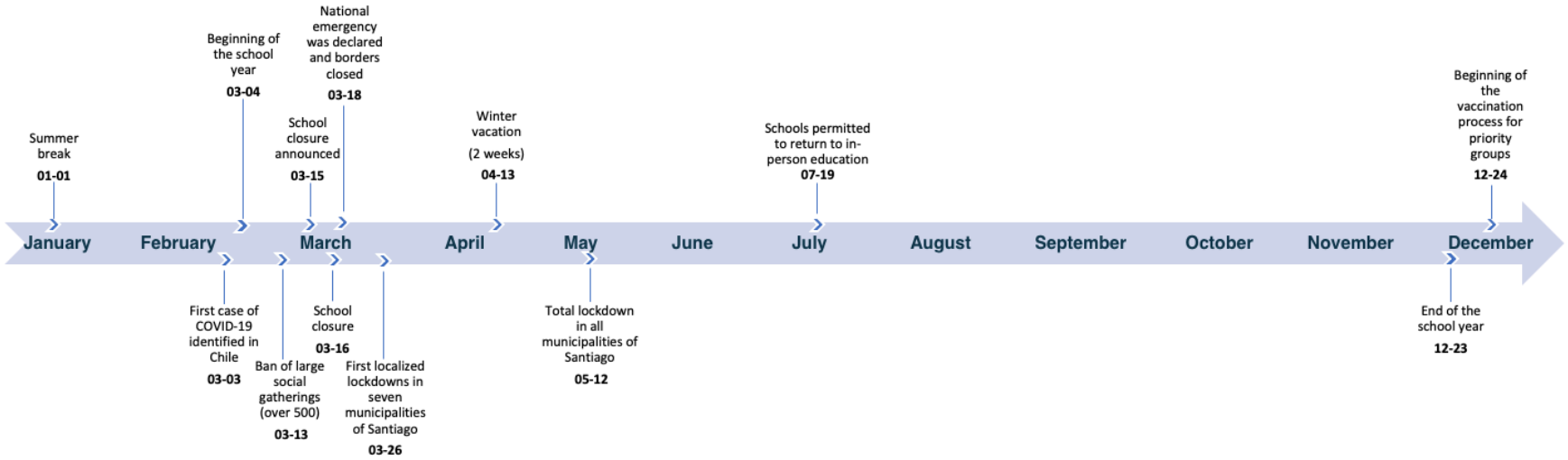
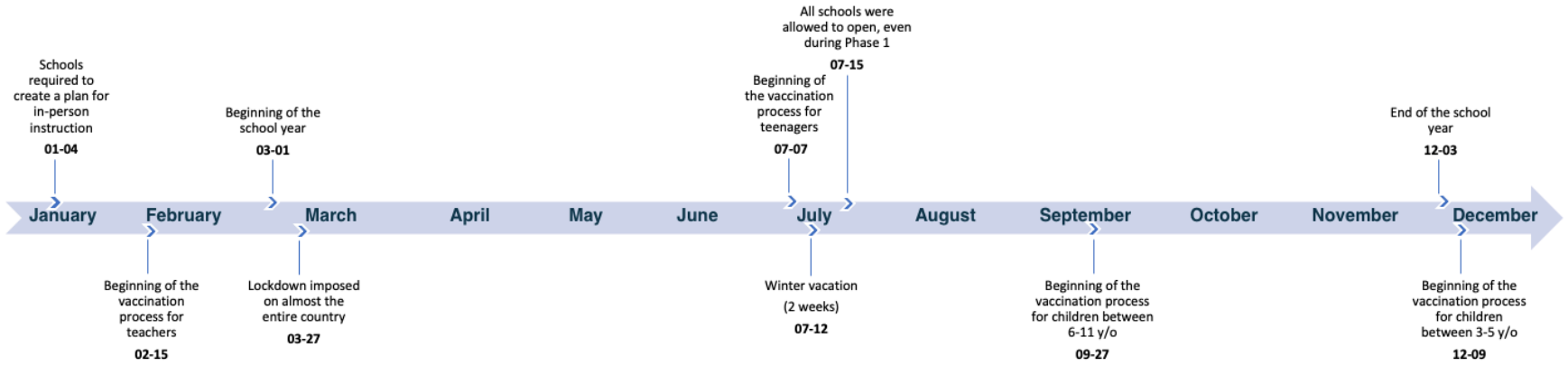


Figure A3: Child Protection Procedures in Chile

Notes to Figure A3: Figure based on information from de Iruarrizaga (2016); SENAME (2016); Stutzin Vallejos (2018). In **bold**, different types of programs under the supervision of SENAME/Mejor Niñez. OPDs are the local offices responsible for providing support and protection to children and adolescents who have experienced rights violations. Co-funded and overseen by SENAME, these offices have two main functions: delivering intervention programs for less complex cases and referring higher-risk cases to Family Courts and other programs; and coordinating inter-sectoral efforts to develop the local protection system. As part of their mission, OPDs are tasked with promoting the rights of children and adolescents and developing municipal policies for their protection. They are currently in the process of being replaced by the Local Children's Offices. Procedures reflect how the system worked until October 2021.



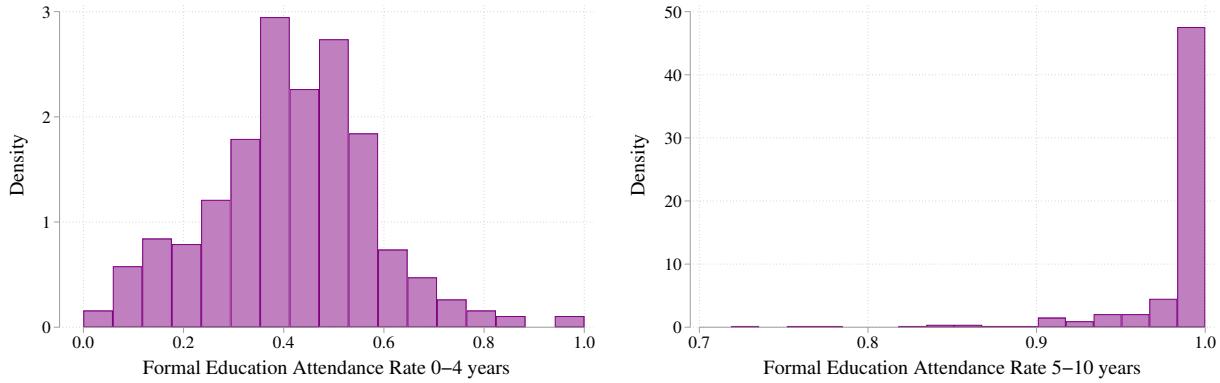
(a) Year 2020



(b) Year 2021

Figure A4: Key Events: 2020–2021

Notes to Figure A4: Key dates are plotted from January-December 2020 (top panel) and January-December 2021 (bottom panel). Precise dates (MM-DD) are noted below events in text.



(a) Ages 0–4

(b) Ages 5–10

Figure A5: Attendance Rate by Age

Notes to Figure A5: Each panel presents attendance rates estimated at the municipality level among children aged 0-4 (panel (a)) and 5-10 (panel (b)). Rates are estimated as the proportion of children attending in each municipality observed in a large nationally representative national survey (the CASEN) in the pre-pandemic period, with each observation referring to a single municipality.

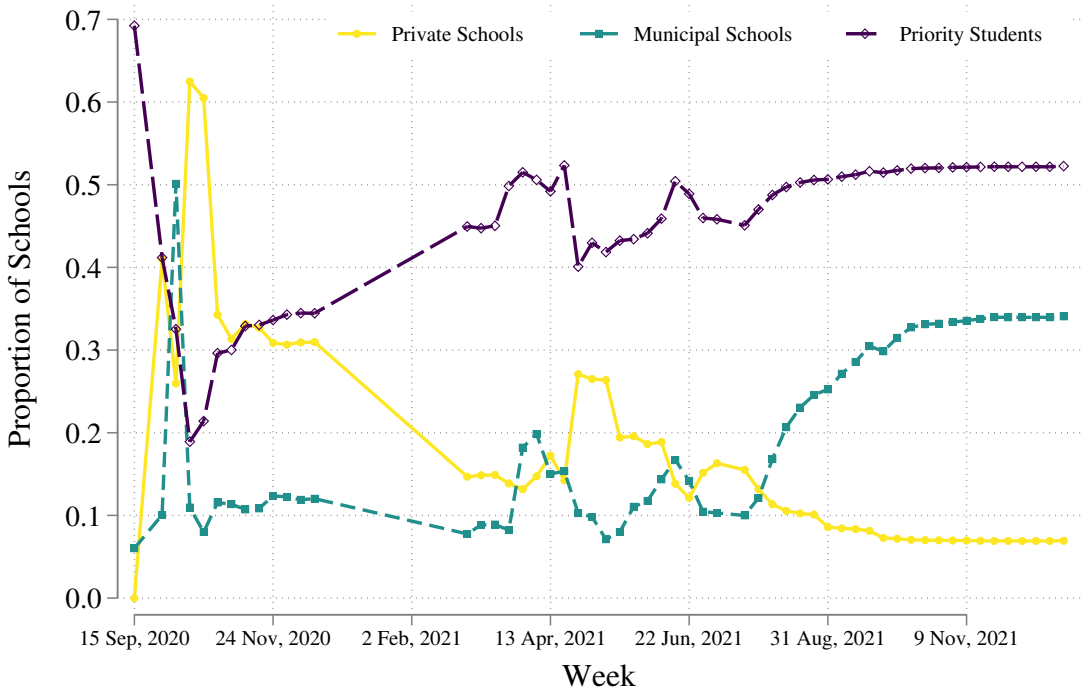
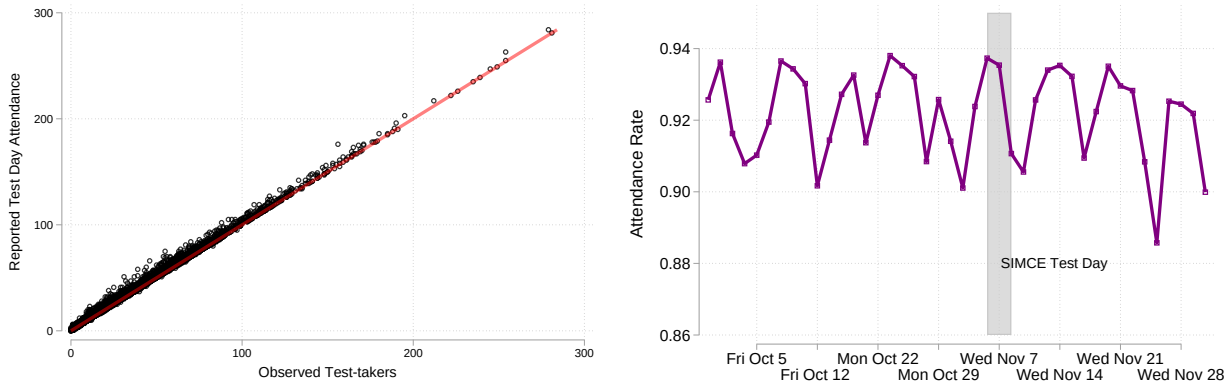


Figure A6: Characteristics of Open Schools

Notes to Figure A6: Each line plots the proportion of open schools which are private schools or municipal-run (public schools), as well as the proportion of students classified as priority among open schools. This, trends capture the change in compositions of open schools as school reopening occurs from September 2020 (first reopenings), up until November 2021 (all schools reopened).



(a) Test Day to Administrative Correspondence

(b) Attendance Surrounding Test Day

Figure A7: Administrate Attendance Records Consistency Check

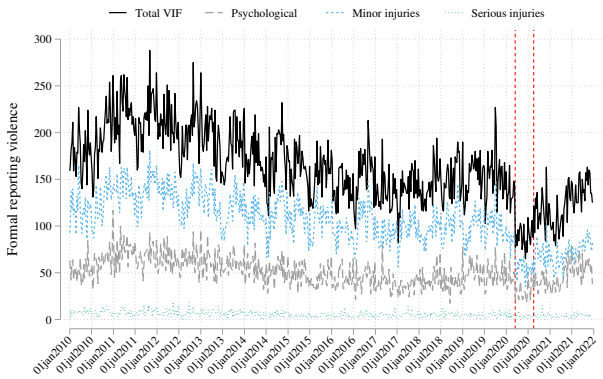
Notes to Figure A7: Left-hand panel documents reported attendance on the day of the fourth grade SIMCE mathematics test in 2018 (y-axis) versus actual observed number of students completing tests. Each point is a single school in the country. The 45 degree red line indicates a 1:1 correspondence between the quantities on each axis. Right-hand panel documents attendance rates among all fourth grade students on all school days around the SIMCE test, where the gray shaded area (November 7th) indicates the test day for mathematics. Note that in the right-hand plot, some weeks have fewer than 5 days given public holidays in early November and mid-October.

Figure A8: Administrative Records of School Reopening Across Chile

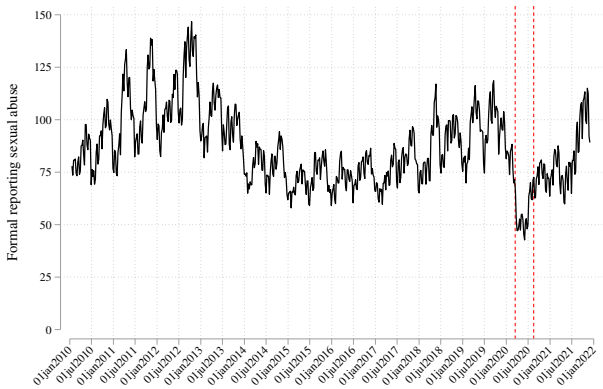
Notes to Figure A8: School opening is displayed across the entire of country of Chile for weeks beginning 5 August 2020 (the week of first reopening is 17 August 2020), up to 27 September 2021. After this date, schools were nearly entirely reopened (see Figure 1). Proportion of schools reopened are displayed 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 A9: Administrative Records of School Reopening Across Chile's Metropolitan Region

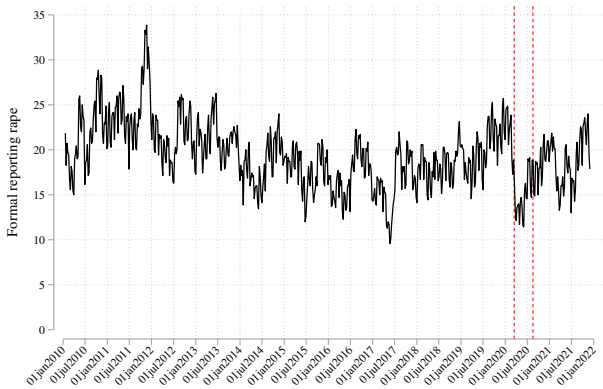
Notes to Figure A9: School opening is displayed across the Metropolitan Region of Santiago (the capital of Chile) for weeks beginning 5 August 2020 (the week of first reopening is 17 August 2020), and ending 27 September 2021. After this date, schools were nearly entirely reopened (see Figure 1). Proportion of schools reopened 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



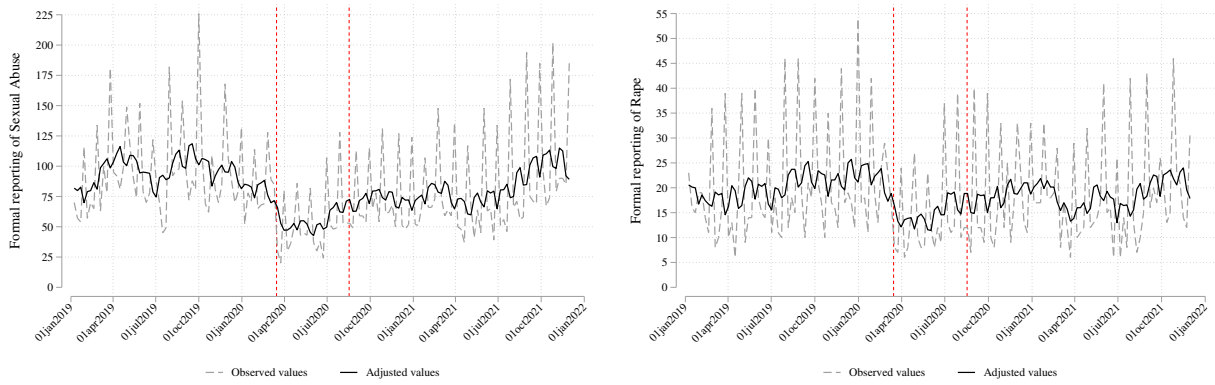
(b) Sexual Abuse



(c) Rape

Figure A10: Extended Trends: Intra-family violence, Sexual Abuse and Rape Against Minors

Notes to Figure A10: 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 re-opening. Panel (a) additionally breaks down total intra-family violence (black solid line), into complaints classified as psychological violence, minor injuries, or serious injuries.

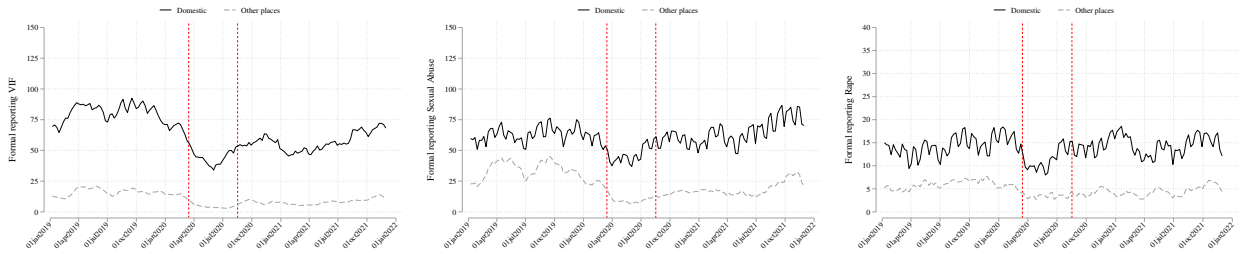


(a) Reporting of sexual assault against minors

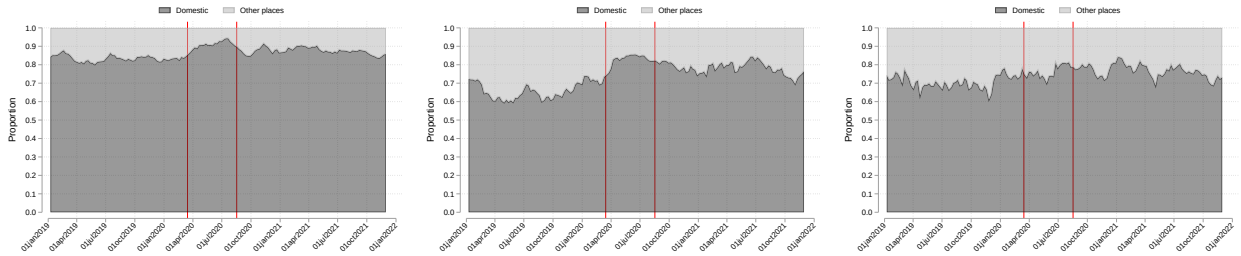
(b) Reporting of rape against minors

Figure A11: Temporal Trends – Sexual abuse and Rape against Minors, Smoothed and Unsmoothed Outcomes

Notes to Figure A11: 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.



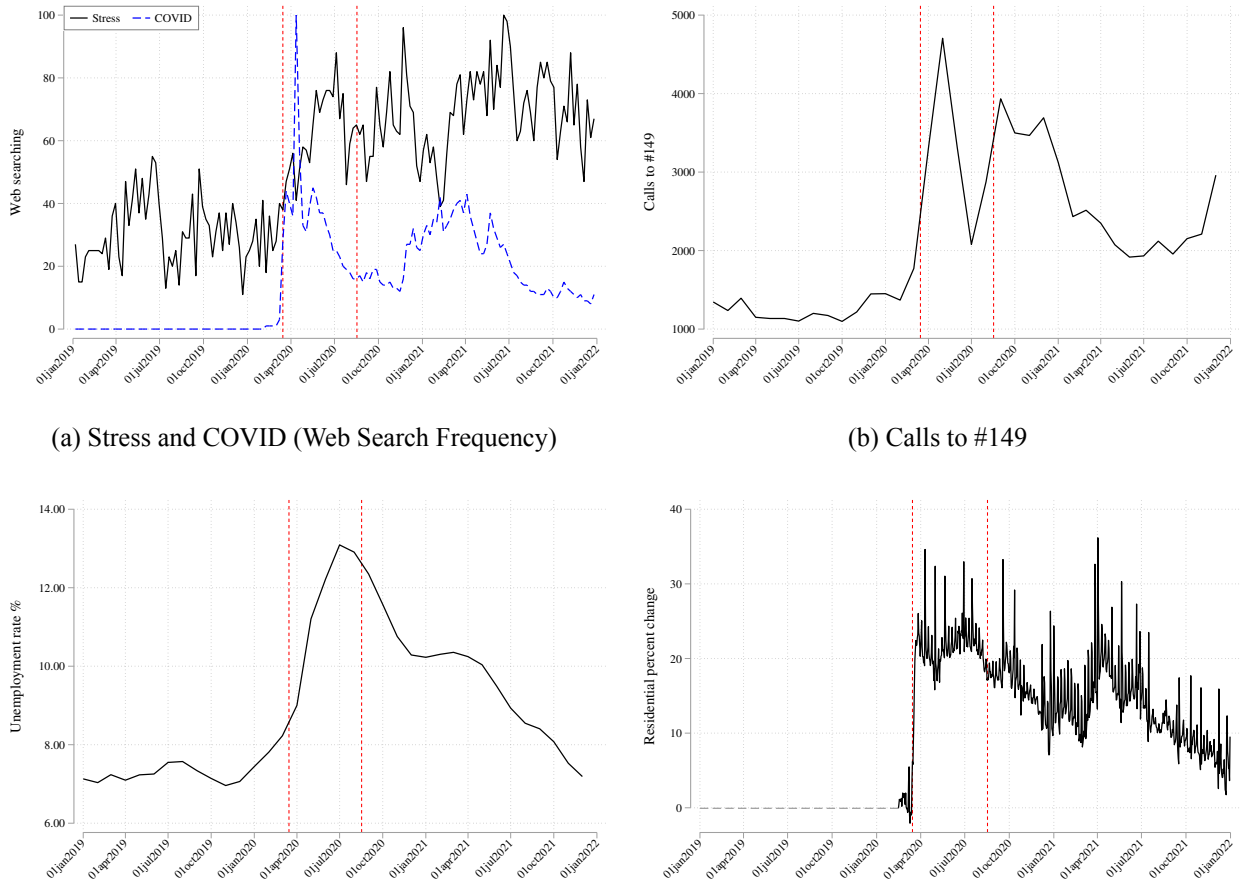
(a) Reporting of Intra-family Violence Against Minors (b) Reporing of Sexual Assault Against Minors (c) Reporting of Rape Against Minors



(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 A12: Temporal Trends – Crimes Reported Against Children by Place of Occurrence

Notes to Figure A12: 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 reopening.



(a) Stress and COVID (Web Search Frequency)

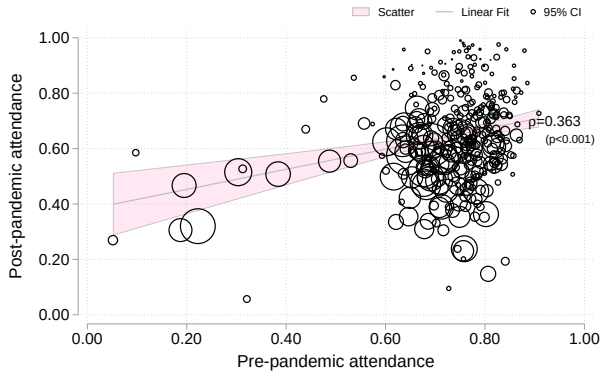
(b) Calls to #149

(c) Unemployment

(d) Home time

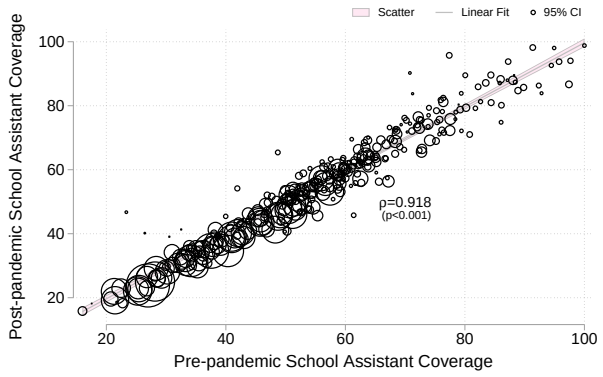
Figure A13: Trends in Other Relevant Factors

Notes to Figure A13: 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 reopening.

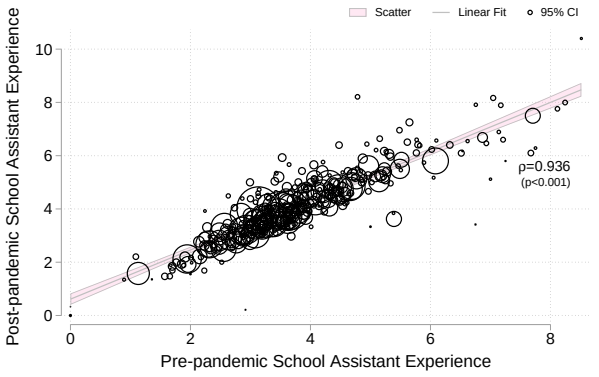


(a) Attendance

(b) Formal Employment



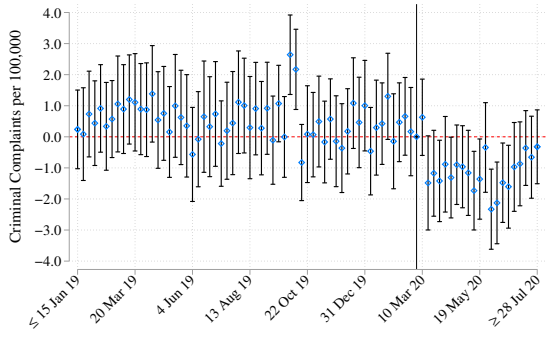
(c) School Assistants (rate)



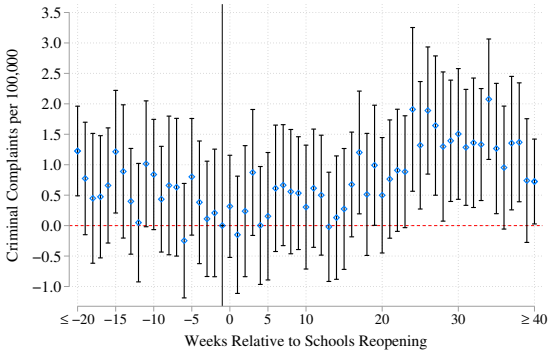
(d) School Assistants (experience)

Figure A14: Correlation Between Mediators: Baseline and Post-Pandemic

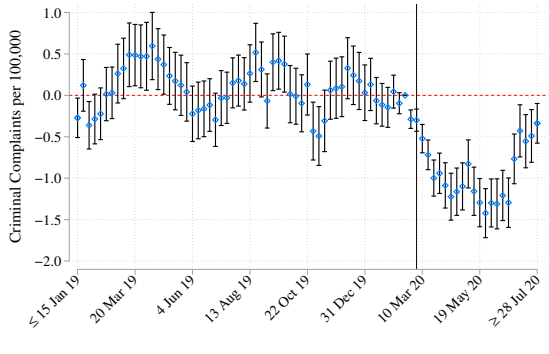
Notes to Figure A14: Scatter plot and linear fit documents the correlation between measures indicated in plot titles in the pre-pandemic period (baseline is 2019), and post-pandemic school reopening period (2021). For monthly measures (panels (a) and (b)) baseline and post-pandemic periods refer to the month of November. In panel (a), given data restrictions, measurement of attendance is slightly different – at baseline it refers to average daily attendance, while post-pandemic it refers to the proportion of individuals attending at least 10 days per month. Each point is a municipality, with sizes indicating the municipal population.



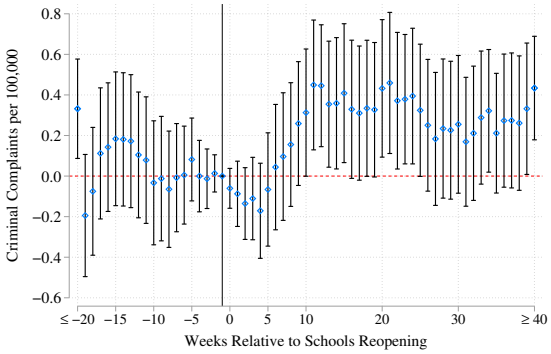
(a) Intra-family violence (closure)



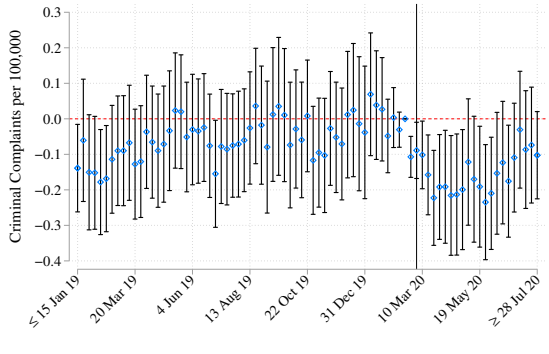
(b) Intra-family violence (opening)



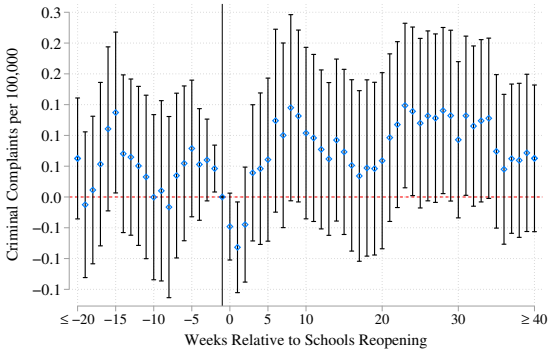
(c) Sexual abuse (closure)



(d) Sexual abuse (opening)



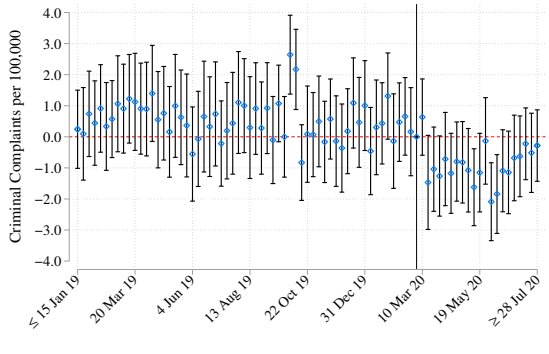
(e) Sexual abuse (closure)



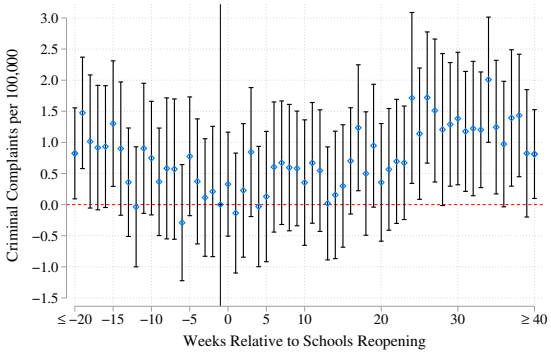
(f) Sexual abuse (opening)

Figure A15: Event Study Estimates of School Closure and Reopening on Reporting of Violence Against Children

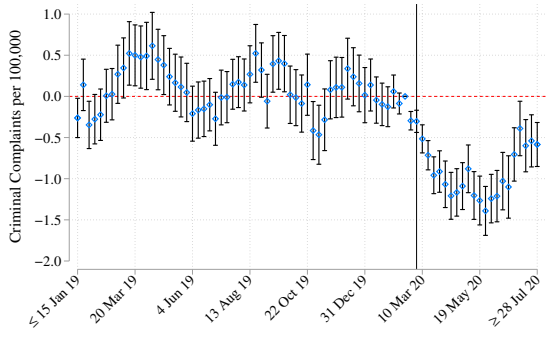
Notes to Figure A15: Event studies are documented as described in equation 2, and details follow those in Notes to Figure 3. Here, lock-down controls are consistently included. Implementations follow Clarke and Schythe (2020). All other details follow those described in equation 2.



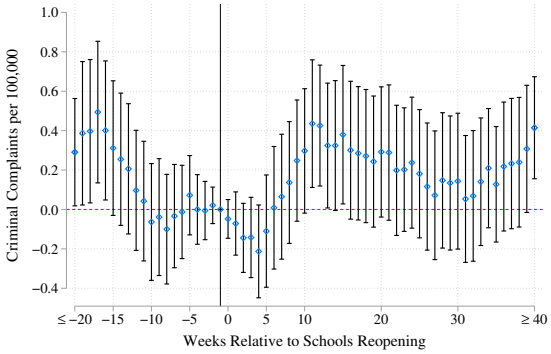
(a) Intra-family violence (closure)



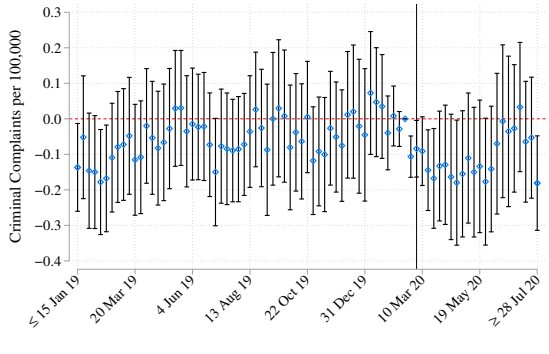
(b) Intra-family violence (opening)



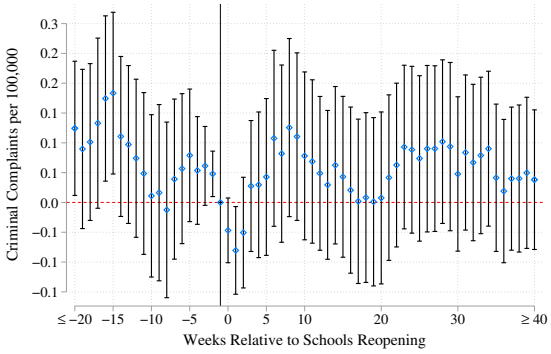
(c) Sexual abuse (closure)



(d) Sexual abuse (opening)



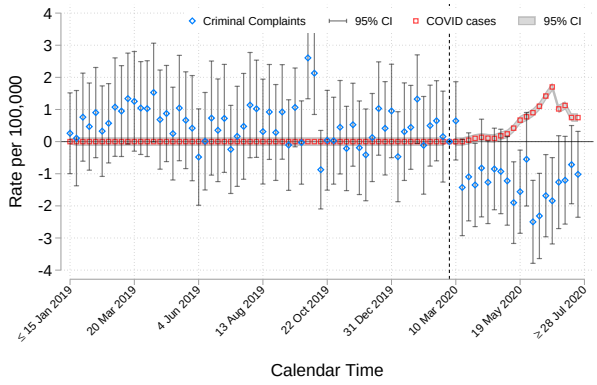
(e) Sexual abuse (closure)



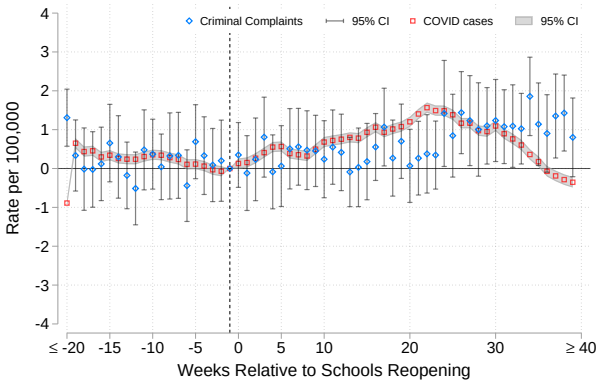
(f) Sexual abuse (opening)

Figure A16: Event Study Estimates of School Closure and Reopening on Reporting of Violence Against Children

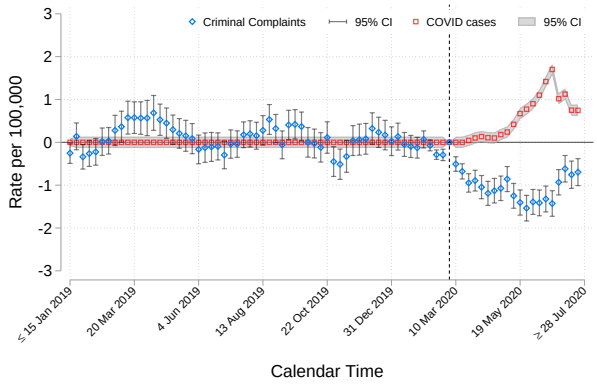
Notes to Figure A16: Event studies are documented as described in equation 2, and details follow those in Notes to Figure 3. Here, lock-down and COVID-intensity controls are consistently included. All other details follow those described in equation 2.



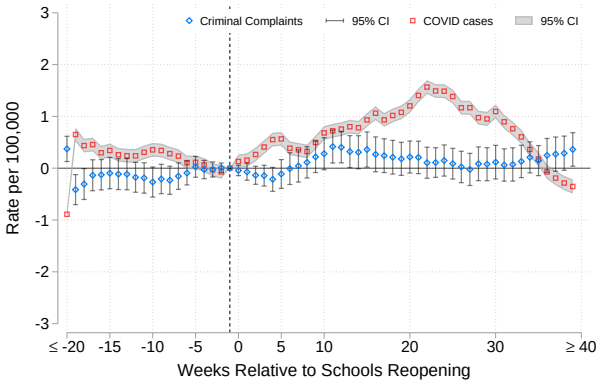
(a) Intra-family Violence (closure)



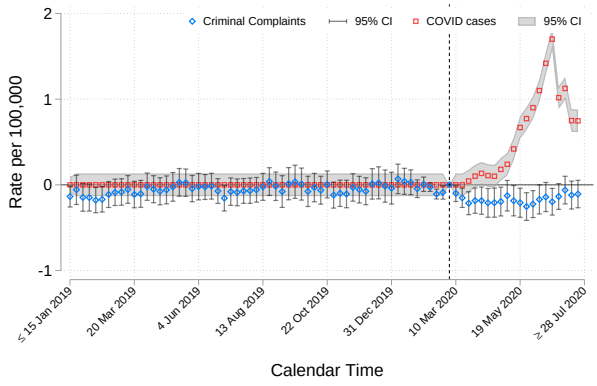
(b) Intra-family Violence (opening)



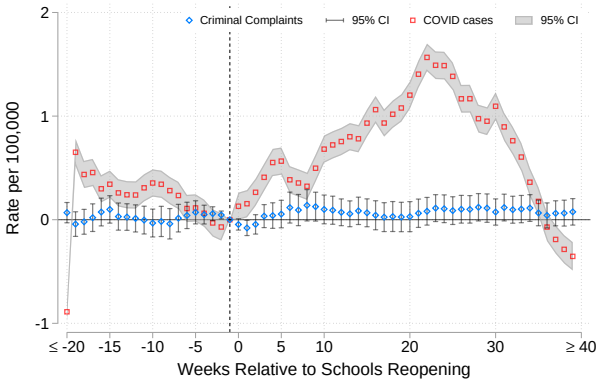
(c) Sexual abuse (closure)



(d) Sexual abuse (opening)



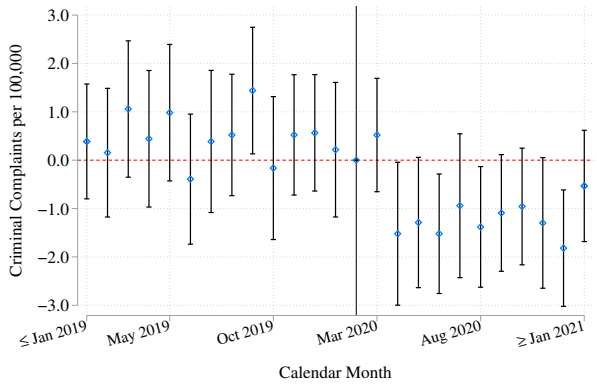
(e) Rape (closure)



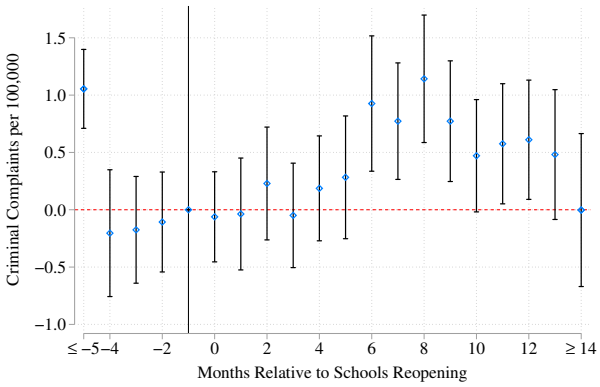
(f) Rape (opening)

Figure A17: Event Studies and Infection Rates

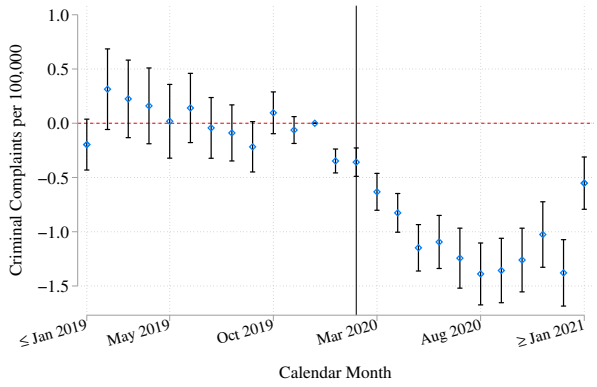
Notes to Figure A17: Event studies are estimated as in Figure 3, however now presenting lags and leads simultaneously: criminal complaints of violence against children (blue points and grey CIs), and recorded COVID-19 cases (red points and grey shaded CIs). Estimates for recorded COVID-19 cases are identical in all panels considering school closure ((a), (c) and (e)), and school all panels considering reopening ((b), (d) and (f)). In each case, identical scaling is used across the left and right plots (school closure and reopening), but scales for each type of violence are altered for ease of visualisation. All other details follow those described in Figure 3.



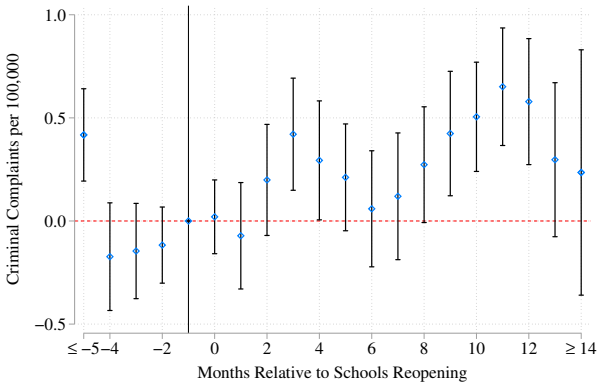
(a) Intra-family Violence (closure)



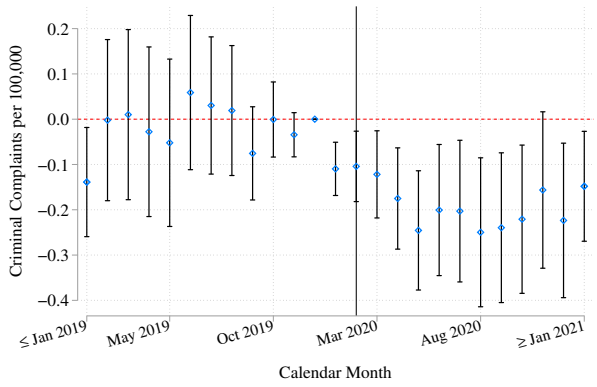
(b) Intra-family Violence (opening)



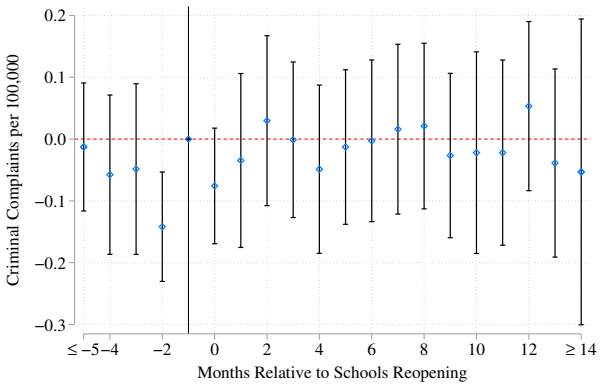
(c) Sexual abuse (closure)



(d) Sexual abuse (opening)



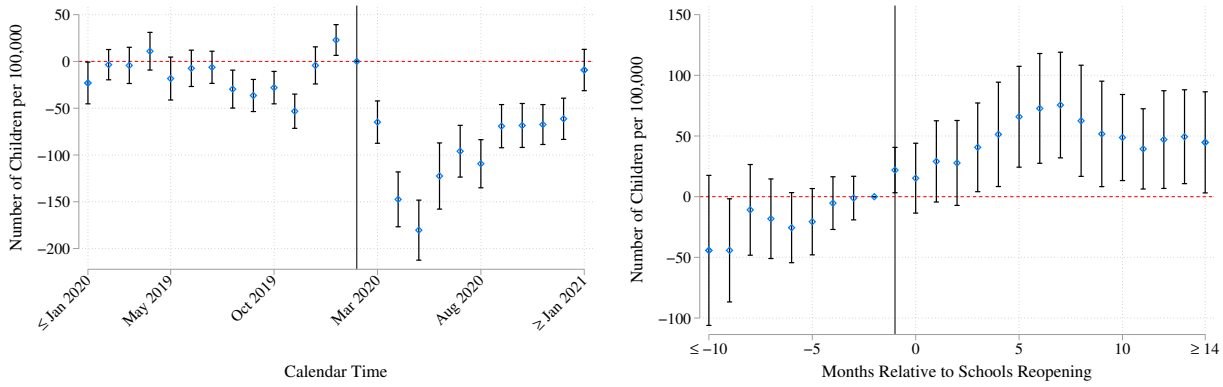
(e) Rape (closure)



(f) Rape (opening)

Figure A18: Event Studies by Month: Rates of Crime Reporting

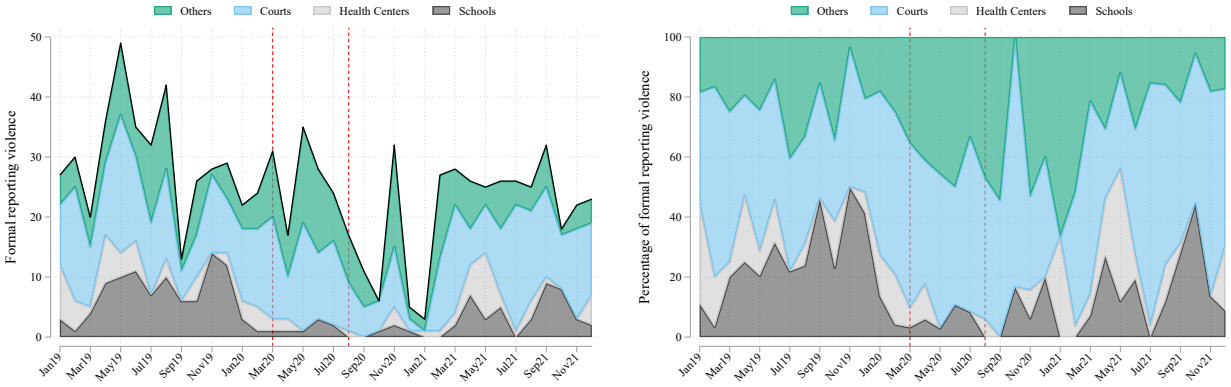
Notes to Figure A18: Event studies follow those in Figure 3, however grouping lags and leads by month (-1 refers to the month of February 2020, a period of school vacations). Point estimates refer to lags and leads in months, and 95% confidence intervals are based on standard errors clustered by municipality. All other details follow those laid out in Notes to Figure 3.



(a) School Closure (b) Reopening

Figure A19: Event Studies by Month: Program Entries to Child Protection System

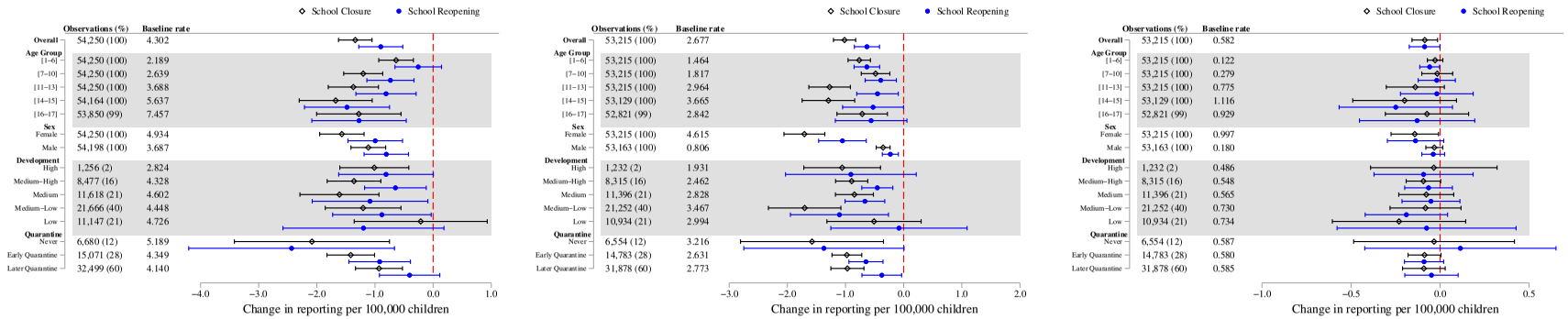
Notes to Figure A19: Monthly event studies are presented based on cases which are entered into the child protection system. These data are only available at the monthly level, and refer to the number of programs in which a child begins participating which is managed by the child protection system. Monthly lags and leads are reported as blue points, alongside 95% confidence intervals based on standard errors clustered by municipality. All municipalities are weighted by the population of 5-18 year-olds in each municipality.



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

Figure A20: Actual Reporting Channels Reported by a Single Child Protection Office in a Large Municipality

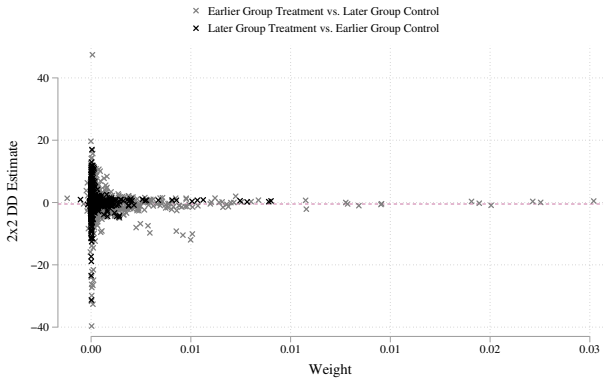
Notes to Figure A20: All reports of violence received by the child protection office of a large municipality in Santiago are displayed. These are broken down by reporting channels as from schools, from municipal health care centres, from courts and from other sources. Total numbers of cases by month 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. January and February correspond to summer break in Chile.



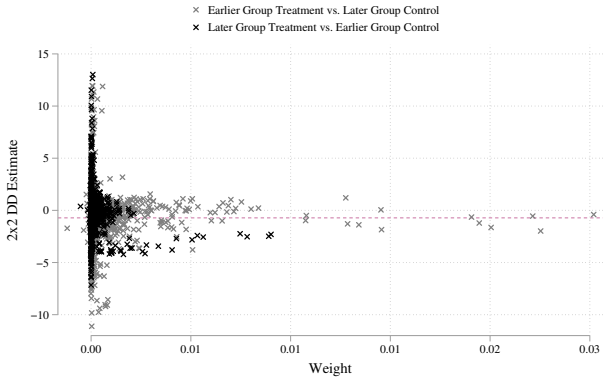
(a) Intra-family Violence (b) Sexual Abuse (c) Rape

Figure A21: Impacts of School Closures and Openings: Demographic and Socio-Economic Variation

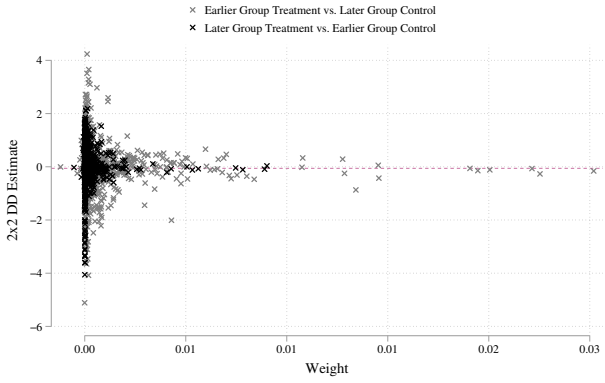
Notes to Figure A21: Estimates (diamonds and circles) and 95% confidence intervals are displayed for models corresponding to sub-groups indicated on the vertical axis. Estimates for each group correspond to coefficients on School Closure (diamonds), and Schools Reopening (circles), following equation 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".



(a) Intra-family Violence



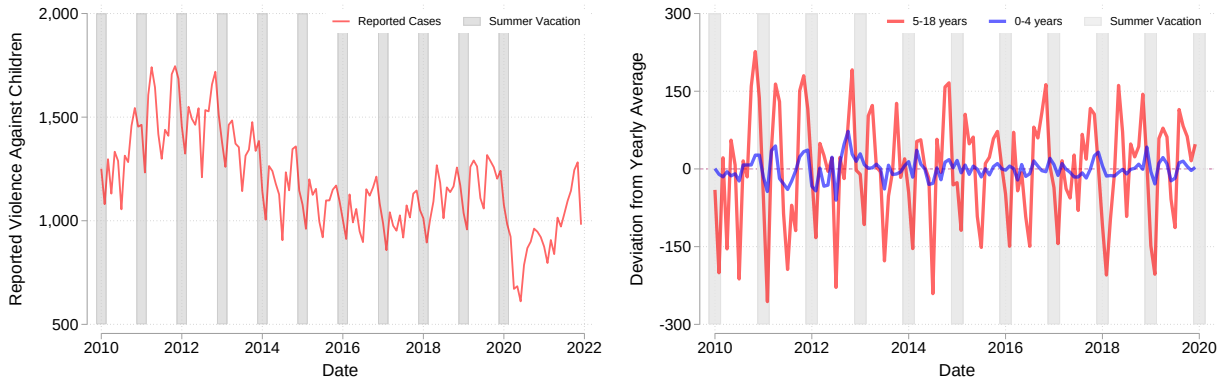
(b) Sexual Abuse



(c) Rape

Figure A22: Weights and 2×2 Double-Difference Estimates in Two-way Fixed Effect Models

Notes to Figure A22: Plots document the double-difference decomposition laid out by (Goodman-Bacon, 2021) to decompose single coefficient estimates on School Reopening displayed in Table 1 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.

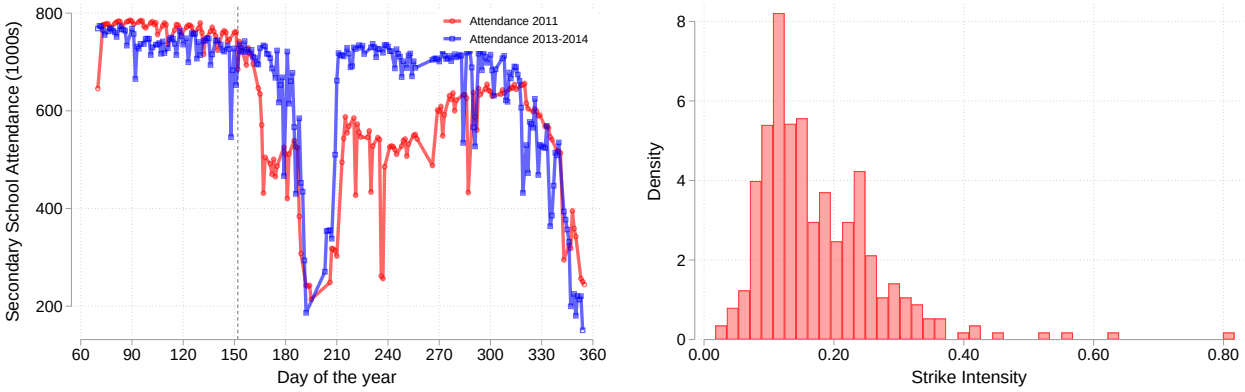


(a) All Cases

(b) Deviation from Yearly Average by Age

Figure A23: Reported Violence Against Children by Month

Notes to Figure A23: Total reported cases of violence against children (intra-family violence, sexual abuse and rape) are plotted over time in panel (a) with shaded areas corresponding to summer vacation months (December-February). Deviations from yearly averages are documented in panel (b) for school-aged (5-17 year olds) and under school-aged (0-4) children.



(a) Attendance Over Time

(b) Strike Intensity

Figure A24: Exposure to Student Strikes

Notes to Figure A24: In panel (a), dashed vertical line indicates 1 June 2011, date where student strikes ramp up (see González, 2020). Points refer to the number of students attending secondary school each day for the strike period (2011, red line) and post-strike period (2013-2014, blue line). 2012 given that monthly attendance was reported for a small number of months. Weekends, public holidays and summer holidays are omitted as these have close to 0 recorded attendance. Panel (b) documents strike intensity at the municipal level as the proportion of total days lost in this municipality between June and December 2011.

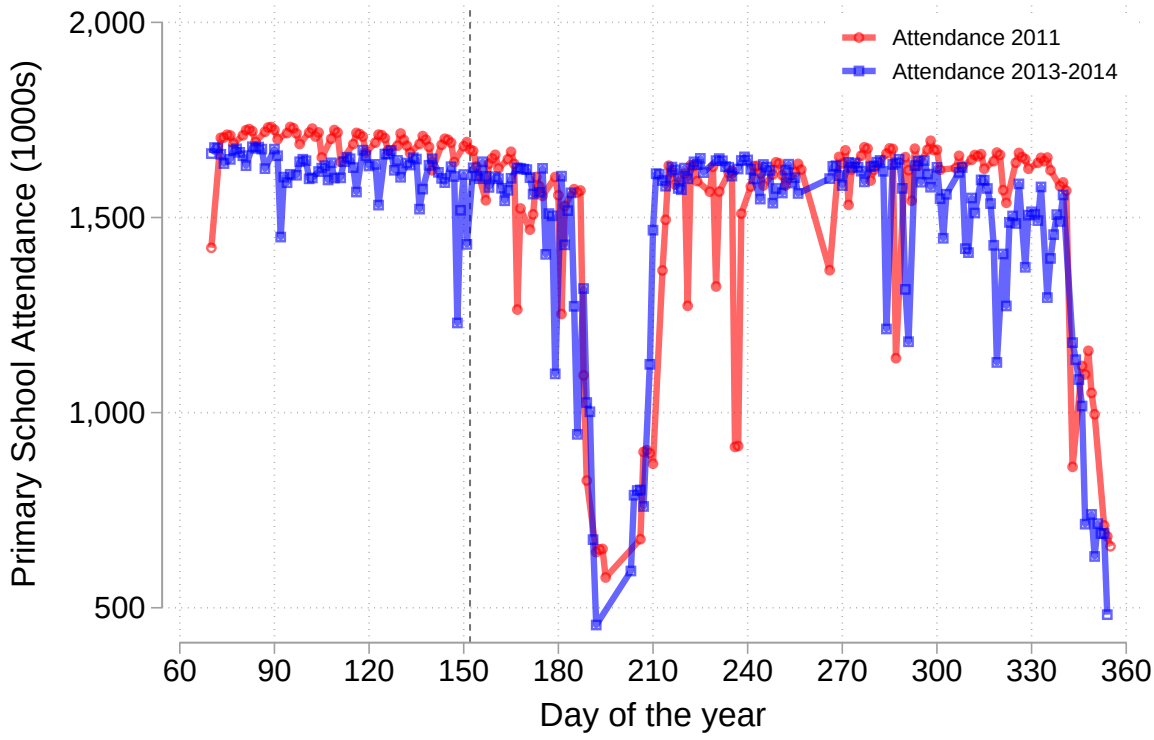


Figure A25: School Strikes and Primary Attendance

Notes to Figure A25: An identical plot to Figure A24 is presented, however here for primary-aged children attendance, instead of secondary-aged children.

A2 Supplementary Tables

Table A1: Summary Statistics of Principal Variables (2019–2021)

	Observations	Mean	Std. Dev.	Min.	Max.
Panel A: Violence Against Children					
Intrafamily 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	0.32	0.46	0.00	1.00
School Reopening	54250	0.29	0.45	0.00	1.00
Schools Reopening	54250	0.19	0.35	0.00	1.00
Baseline Attendance	54321	0.74	0.11	0.05	0.91
Baseline Educational Specialists	54164	57.62	22.53	15.07	204.11
Composition of Students (private)	54164	0.04	0.12	0.00	1.00
Composition of Students (municipal)	54164	0.54	0.34	0.00	1.00
Composition of Students (priority)	54164	0.66	0.18	0.00	1.00
Panel C: COVID/Other Measures					
Quarantine in place	54321	0.10	0.30	0.00	1.00
COVID cases per 1,000	54321	0.68	1.88	0.00	233.58
PCR tests per 1,000	54321	8.10	10.69	0.00	179.53
PCR test positivity	54321	5.27	9.66	0.00	100.00
COVID-19 vaccinations administered	54321	294.29	1234.20	0.00	34485.00
Proportion of adults in formal employment	54281	0.23	0.08	0.00	0.47
Population between 5 and 18 years	54321	10222.90	15866.86	9.00	127390.00

Notes to Table A1: 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 reopening, as well as baseline attendance rates and coverage of school support workers (students per worker). Panel C documents epidemiological, vaccination, and economic controls.

Table A2: Triple Difference versus Conditional Difference-in-differences

	Difference-in-Differences				Triple
	0-4 year	5-10 years	0-4 year	5-10 years	difference
	(1)	(2)	(3)	(4)	(5)
Panel A: 5-10 year olds versus 0-4 year olds					
School Close	-0.630***	-1.189***	-0.439***	-1.069***	-0.630***
	(0.127)	(0.131)	(0.144)	(0.146)	(0.162)
Schools Reopening	-0.085	-0.572***	-0.125	-0.652***	-0.527***
	(0.118)	(0.124)	(0.180)	(0.181)	(0.164)
Observations	54,250	54,250	54,250	54,250	54,250
Coefficient Ratio (Close)				0.899	0.589
Coefficient Ratio (Open)				1.141	0.808
	0-4 year	5-17 years	0-4 year	5-17 years	
Panel B: 5-17 year olds versus 0-4 year olds					
School Close	-0.630***	-1.479***	-0.439***	-1.276***	-0.837***
	(0.127)	(0.111)	(0.144)	(0.133)	(0.163)
Schools Reopening	-0.085	-0.880***	-0.125	-0.924***	-0.799***
	(0.118)	(0.110)	(0.180)	(0.170)	(0.188)
Observations	54,250	54,250	54,250	54,250	54,250
Coefficient Ratio (Close)				0.863	0.656
Coefficient Ratio (Open)				1.049	0.865
Controls			Y	Y	Y

Notes to Table A2: Columns (1) to (4) present difference-in-differences specifications of the impact of school closure and reopening on rates of criminal complaints against children of ages indicated in column headings. Each observation is a municipal by week cell. Columns (1) and (2) are conditional only on time and municipal fixed effects, while (3) and (4) incorporate time-varying controls. Column 5 presents the triple difference specifications comparing changes among older (more exposed) versus younger (less exposed) children. Coefficient ratio refers to the ratio of coefficients on closure or reopening comparing column 4 or column 5 to unconditional difference-in-differences estimates in column 2. Standard errors clustered by municipality are presented in parentheses. *** $p< 0.01$; ** $p< 0.05$; * $p< 0.10$.

Table A3: 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. A3: 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.

Table A4: Modelled Impacts of School Closure and Reopening 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 Reopening Measure									
School Close	-1.582***	-1.124***	-1.132***	-1.010***	-0.961***	-0.961***	-0.103***	-0.062	-0.062
	(0.133)	(0.162)	(0.161)	(0.085)	(0.110)	(0.110)	(0.026)	(0.041)	(0.041)
Schools Reopening	-0.769***	-0.820***	-0.811***	-0.419***	-0.670***	-0.632***	-0.061**	-0.075*	-0.079*
	(0.146)	(0.198)	(0.201)	(0.082)	(0.121)	(0.126)	(0.030)	(0.045)	(0.046)
Test of $\beta = \gamma$ (p-value)	0.000	0.074	0.068	0.000	0.000	0.000	0.144	0.736	0.672
Observations	45,608	45,568	45,499	44,573	44,533	44,464	44,573	44,533	44,464
Baseline Mean	4.979	4.979	4.979	2.540	2.540	2.540	0.611	0.611	0.611
Panel B: Continuous Reopening Measure									
School Close	-1.433***	-0.790***	-0.865***	-0.884***	-0.601***	-0.665***	-0.087***	-0.022	-0.022
	(0.122)	(0.135)	(0.140)	(0.077)	(0.083)	(0.085)	(0.024)	(0.033)	(0.034)
Schools Reopening	-0.656***	-0.405**	-0.524**	-0.178*	-0.000	-0.109	-0.035	-0.002	-0.002
	(0.177)	(0.201)	(0.223)	(0.098)	(0.117)	(0.120)	(0.041)	(0.050)	(0.054)
Test of $\beta = \gamma$ (p-value)	0.000	0.083	0.137	0.000	0.000	0.000	0.193	0.698	0.710
Observations	45,608	45,568	45,499	44,573	44,533	44,464	44,573	44,533	44,464
Baseline Mean	4.979	4.979	4.979	2.540	2.540	2.540	0.611	0.611	0.611
Municipal & WoY FEs	Y	Y	Y	Y	Y	Y	Y	Y	Y
Lockdown & Epidemiological controls		Y	Y		Y	Y		Y	Y
School composition controls			Y			Y			Y

Notes to Table A4: Refer to notes to Table 1 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 A5: Impacts of School Closure and Reopening on Sexual Violence (Unadjusted Variables)

	Sexual Abuse			Rape		
	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Binary Reopening Measure						
School Close	-0.972***	-0.945***	-0.946***	-0.098***	-0.086**	-0.087**
	(0.085)	(0.110)	(0.109)	(0.027)	(0.044)	(0.044)
Schools Reopening	-0.384***	-0.617***	-0.613***	-0.049*	-0.070	-0.080*
	(0.072)	(0.112)	(0.118)	(0.028)	(0.046)	(0.046)
Test of $\beta = \gamma$ (p-value)	0.000	0.000	0.000	0.098	0.672	0.860
Observations	54,250	54,210	54,124	54,250	54,210	54,124
Baseline Mean	2.506	2.506	2.506	0.611	0.611	0.611
Panel B: Continuous Reopening Measure						
School Close	-0.886***	-0.642***	-0.685***	-0.094***	-0.063*	-0.060
	(0.082)	(0.092)	(0.095)	(0.026)	(0.036)	(0.038)
Schools Reopening	-0.279***	-0.180	-0.245*	-0.065*	-0.062	-0.056
	(0.095)	(0.124)	(0.126)	(0.039)	(0.049)	(0.053)
Test of $\beta = \gamma$ (p-value)	0.000	0.000	0.000	0.461	0.984	0.936
Observations	54,250	54,210	54,124	54,250	54,210	54,124
Baseline Mean	2.506	2.506	2.506	0.611	0.611	0.611
Municipal & WoY FEs	Y	Y	Y	Y	Y	Y
Lockdown & epidemiological controls		Y	Y		Y	Y
School composition controls			Y			Y

Notes to Table A5: Refer to notes to Table 1 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 Table 1. Column numbers (4)-(9) are used here for sake of comparison with column numbers in Table 1. *** p< 0.01; ** p< 0.05; * p< 0.10.

Table A6: Impacts of School Closure and Reopening 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 Reopening Measure									
School Close	-0.030**	0.002	0.002	-1.148***	-0.796***	-0.796***	-0.415***	-0.370***	-0.374***
	(0.013)	(0.020)	(0.020)	(0.087)	(0.106)	(0.106)	(0.067)	(0.082)	(0.082)
Schools Reopening	-0.023	0.012	0.020	-0.803***	-0.736***	-0.722***	-0.026	-0.153	-0.151
	(0.014)	(0.018)	(0.019)	(0.076)	(0.111)	(0.115)	(0.083)	(0.103)	(0.106)
Test of $\beta = \gamma$ (p-value)	0.681	0.612	0.379	0.000	0.521	0.453	0.000	0.010	0.012
Observations	54,250	54,210	54,124	54,250	54,210	54,124	54,250	54,210	54,124
Baseline Mean	0.189	0.189	0.189	3.191	3.191	3.191	1.649	1.649	1.649
Panel B: Continuous Reopening Measure									
School Close	-0.025*	-0.000	-0.006	-1.019***	-0.488***	-0.561***	-0.386***	-0.264***	-0.266***
	(0.013)	(0.017)	(0.017)	(0.082)	(0.086)	(0.092)	(0.059)	(0.064)	(0.067)
Schools Reopening	-0.017	0.016	0.007	-0.790***	-0.418***	-0.535***	0.075	0.076	0.088
	(0.022)	(0.024)	(0.024)	(0.105)	(0.126)	(0.131)	(0.101)	(0.109)	(0.123)
Test of $\beta = \gamma$ (p-value)	0.733	0.522	0.612	0.029	0.598	0.841	0.000	0.004	0.004
Observations	54,250	54,210	54,124	54,250	54,210	54,124	54,250	54,210	54,124
Baseline Mean	0.189	0.189	0.189	3.191	3.191	3.191	1.649	1.649	1.649
Municipal & WoY FEs	Y	Y	Y	Y	Y	Y	Y	Y	Y
Lockdown & Epidemiological controls		Y	Y		Y	Y		Y	Y
School composition controls			Y			Y			Y

Notes to Table A6: Refer to notes to Table 1 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 Table 1), 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 Table 1 is classified as one (and only one) of the three classes displayed here. All other details follow those in Table 1. *** p< 0.01; ** p< 0.05; * p< 0.10.

Table A7: 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 A7: 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 A22. 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 A8: Published School Vacation Dates from Government Resolutions

Region	2011		2012		2013		2014		2015		2016		2017		2018		2019	
	Start	End	Start	End	Start	End	Start	End	Start	End	Start	End	Start	End	Start	End	Start	End
Region I																		
Summer	07/03	23/12	05/03	21/12	05/03	20/12	05/03	19/12	04/03	18/12	03/03	16/12	06/03	22/12	05/03	26/12	05/03	26/12
Winter	11/07	22/07	09/07	20/07	08/07	19/07	07/07	18/07	13/07	24/07	11/07	22/07	10/07	21/07	09/07	20/07	08/07	19/07
Region II																		
Summer			01/03	21/12	04/03	20/12	05/03	19/12	04/03	18/12	03/03	16/12	06/03	22/12	05/03	21/12	05/03	20/12
Winter			17/07	27/07	15/07	26/07	14/07	25/07	13/07	24/07	11/07	22/07	10/07	21/07	09/07	20/07	08/07	19/07
Region III																		
Summer	03/03	16/12	05/03	21/12	05/03	20/12	05/03	19/12	04/03	18/12	03/03	14/12	06/03	22/12	05/03	21/12	05/03	18/12
Winter	11/07	22/07	17/07	27/07	15/07	26/07	14/07	25/07	13/07	24/07	11/07	22/07	17/07	28/07	16/07	27/07	15/07	26/07
Region IV																		
Summer	03/03	16/12	05/03	21/12	04/03	20/12	03/03	19/12	04/03	18/12	03/03	21/12	06/03	22/12	05/03	19/12	05/03	20/12
Winter	18/07	29/07	16/07	27/07	15/07	26/07	14/07	25/07	13/07	26/07	11/07	24/07	10/07	23/07	09/07	22/07	13/07	27/07
Region V																		
Summer	03/03	21/12	05/03	21/12	05/03		05/03		04/03		03/03	16/12	05/03		05/03		05/03	20/12
Winter	11/07	22/07	09/07	20/07	08/07	19/07	07/07	18/07	13/07	24/07	11/07	22/07	10/07	21/07	09/07	20/07	15/07	26/07
Region VI																		
Summer	03/03	16/12	05/03	21/12	05/03	24/12	05/03	19/12	04/03	18/12	03/03	16/12	06/03	22/12	05/03	21/12	05/03	20/12
Winter	18/07	29/07	09/07	20/07	15/07	26/07	14/07	25/07	13/07	24/07	11/07	22/07	10/07	21/07	09/07	20/07	08/07	19/07
Region VII																		
Summer	03/03	16/12	05/03	21/12	05/03	20/12	05/03	19/12	04/03	18/12	03/03	16/12	06/03	15/12	05/03	21/12	05/03	20/12
Winter	18/07	29/07	16/07	27/07	15/07	26/07	14/07	25/07	13/07	24/07	11/07	22/07	10/07	21/07	09/07	20/07	08/07	19/07
Region VIII																		
Summer	03/03	21/12	05/03	19/12	05/03	19/12	06/03	18/12	04/03	17/12	03/03	12/12	06/03	08/12	05/03	21/12	05/03	19/12
Winter	18/07	30/07	17/07	28/07	15/07	26/07	14/07	25/07	13/07	24/07	11/07	22/07	10/07	21/07	17/07	27/07	15/07	26/07
Region IX																		
Summer	03/03	16/12	05/03	21/12	05/03	20/12	05/03	23/12	04/03	22/12	03/03	16/12	06/03	22/12	05/03	21/12	05/03	20/12
Winter	18/07	29/07	16/07	27/07	15/07	26/07	14/07	25/07	13/07	24/07	11/07	22/07	17/07	28/07	16/07	27/07	15/07	26/07
Region X																		
Summer	03/03	16/12	05/03	21/12	05/03	20/12	05/03	19/12	04/03	18/12	03/03	16/12	06/03	22/12	05/03	21/12	05/03	20/12
Winter	18/07	29/07	16/07	27/07	15/07	26/07	14/07	25/07	13/07	24/07	18/07	29/07	17/07	28/07	17/07	27/07	15/07	26/07
Region XI																		
Summer	03/03	23/12	05/03	28/12			05/03	26/12	04/03	24/12	03/03	23/12	06/03	29/12	05/03	28/12	05/03	31/12
Winter	11/07	29/07	09/07	27/07			14/07	01/08	13/07	31/07	11/07	29/07	10/07	28/07	09/07	27/07	08/07	26/07

Notes to Table A8: Dates are hand-coded from resolutions issued by the Ministry of Education which define vacation dates for each of the 16 regions of Chile. Region XVI (Ñuble) was created in 2018 and previously belonged to the Region VII (Biobío), and as such prior to 2018 simply takes the same dates as those indicated in Region VII. A small number of dates could not be determined from Ministerial documents.

Table A8 (Cont'd): Published School Vacation Dates from Government Resolutions

Region	2011		2012		2013		2014		2015		2016		2017		2018		2019	
	Start	End	Start	End	Start	End	Start	End	Start	End	Start	End	Start	End	Start	End	Start	End
Region XII																		
Summer	03/03	23/12	05/03	23/12	26/03	27/12	05/03	12/12	04/03	18/12	03/03	23/12	06/03	29/12	05/03	28/12	05/03	27/12
Winter	11/07	29/07	09/07	27/07	08/07	26/07	14/07	01/08	13/07	31/07	11/07	29/07	10/07	28/07	09/07	27/07	08/07	26/07
Region XIII																		
Summer	03/03	21/12	05/03	21/12	05/03	20/12	05/03	19/12	04/03	19/12	03/03	21/12	06/03	22/12	05/03	21/12	05/03	20/12
Winter	11/07	22/07	09/07	20/07	08/07	19/07	14/07	25/07	13/07	24/07	11/07	22/07	10/07	21/07	17/07	27/07	15/07	26/07
Region XIV																		
Summer	03/03	23/12	05/03	21/12	05/03	20/12	05/03	19/12	04/03	18/12	03/03	16/12	06/03	22/12	05/03	21/12	05/03	20/12
Winter	18/07	29/07	16/07	27/07	15/07	26/07	14/07	25/07	13/07	24/07	11/07	22/07	10/07	22/07	09/07	20/07	08/07	19/07
Region XV																		
Summer	03/03	16/12	05/03	21/12	05/03	24/12	05/03	19/12	04/03	18/12	03/03	16/12	06/03	22/12	05/03	21/12	05/03	20/12
Winter	11/07	22/07	09/07	20/07	08/07	29/07	14/07	25/07	13/07	24/07	11/07	22/07	10/07	21/07	09/07	20/07	08/07	19/07
Region XVI																		
Summer	03/03	21/12	05/03	19/12	05/03	19/12	06/03	18/12	04/03	17/12	03/03	12/12	06/03	08/12	05/03	21/12	05/03	19/12
Winter	18/07	30/07	17/07	28/07	15/07	26/07	14/07	25/07	13/07	24/07	11/07	22/07	10/07	21/07	17/07	27/07	15/07	26/07

See notes on previous page.

Table A9: Summary Statistics of Principal Variables (School Strikes and Vacation Analysis)

	Observations	Mean	Std. Dev.	Min.	Max.
Panel A: School Vacation Analysis					
All Violence Against Children	126636	1.03	3.85	0.00	571.43
Attendance	126636	0.61	0.37	0.00	1.00
Vacation Period	126636	0.26	0.44	0.00	1.00
Term Time (non Post-vacations)	126636	0.70	0.46	0.00	1.00
Post-vacation Period	126636	0.02	0.15	0.00	1.00
Population	126636	10192.29	15665.85	6.00	129878.00
Panel B: School Strikes Analysis					
All Violence	31356	55.75	102.40	0.00	5882.35
Intrafamily Violence	31356	40.93	89.10	0.00	5882.35
Sexual Abuse	31356	11.12	40.80	0.00	1587.30
Rape	31356	3.70	24.12	0.00	1538.46
Strike Period	31356	0.09	0.29	0.00	1.00
Strike Intensity	31356	0.15	0.10	0.00	0.82
Population of Secondary Students	31356	3261.07	4855.77	1.00	40619.00

Notes to Table A9: Summary statistics are displayed across all municipal by week cells for the period of January 2012–December 2017 in the case of vacation analysis (Panel A). Statistics are displayed across all municipal by month cells for the period of January 2010–December 2017 in the case of analysis of school strikes (Panel B).

Table A10: School Strikes in 2011 and Intra-Family Violence Against Children (Triple Difference)

	All Complaints (1)	Intra-family Violence (2)	Sexual Abuse (3)	Rape (4)
Strike Intensity × Strike	-43.39***	-25.11**	-5.828	-12.45***
	(15.35)	(10.87)	(5.536)	(4.252)
Observations	31,356	31,356	31,356	31,356
R-squared	0.08	0.09	0.06	0.06
Scaled Effect	-4.05	-2.34	-0.54	-1.16
Mean Dep. Var.	55.75	40.93	11.12	3.70

Notes to Table A10: Estimates are presented following details laid out in (6) and Notes to Table 4, however here considering as an outcome differences in reporting rates between secondary and primary children, implementing a triple difference specification, where the third difference is schooling level (secondary is exposed, primary is not). All other details follow those in Table 4. *** p< 0.01; ** p< 0.05; * p< 0.10.

A3 Considering Counterfactual Projections and Evolution of Violence Rates

In this Appendix we describe an activity which seeks to compared observed rates of violence with rates of violence based off of "counterfactual" projections assuming that recent trends are informative of what may have happened in the absence of the pandemic. Based off of 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.

This counterfactual activity, which we describe below, 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 (Bullinger et al., 2020; Bhalotra et al., 2024; Erten et al., 2022; Pereda and Díaz-Faes, 2020) suggests that violence may have in fact increased, given the change in risk factors associated with child maltreatment (i.e., time spent at home, and increase in economic strain, financial hardship, mental stress, and family conflict). If this was the case, the counterfactuals may actually under-estimate the true expected reporting if rates of reporting had been maintained constant. Thus, we also conduct additional tests, allowing for potential increases in rates of violence. Overall, we see these counterfactual projections as a bounding exercise of the range of reports that may have been missed due to school closures and slow recovery, under different assumptions of changes in true rates of violence.

A3.1 Methods

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

$$\widehat{\text{Reporting}}_{mt}^{post} = \widehat{\alpha}^{pre} + \widehat{\mu}_{WoY}^{pre} + \widehat{\phi}_{m}^{pre}, \tag{A1}$$

where projected reporting in the post school closure and reopening 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. In Clarke et al. (2022), we document an extended methodology which also allowed for temporal reporting trends. 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:

$$\text{Difference}_{mt}^{post} = \text{Reporting}_{mt}^{post} - \widehat{\text{Reporting}}_{mt}^{post} \tag{A2}$$

Note that here, we can calculate a difference *for each* municipality and week. In the following results, 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:

$$\text{Reporting Differential}_t = \sum_{m=1}^{346} \text{Difference}_{mt}^{post} \times \frac{Population_{mt}}{100,000}. \tag{A3}$$

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.

In a second step, we re-estimate counterfactual reporting now controlling additionally for the channel of school closures and reopenings. This is calculated as:

$$\widehat{\text{Reporting}}_{mt}^{post}\Big|_{SO} = \widehat{\alpha}^{pre} + \widehat{\mu}_{WoY}^{pre} + \widehat{\phi}_{m}^{pre} + \widehat{\delta}\text{School Opening}_{mt}, \tag{A4}$$

where all details follow equation A1, 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 reopen upon school reopening periods. We follow identical procedures in calculating reporting differentials from equation A3, however now conditional on School Opening channels, and document relative movements when calculating the unconditional counterfactual in equation A1 and the conditional counterfactual in equation A4 to estimate the proportional contribution of school closures and reopenings to reporting differentials.

Finally, to account for potential increase in violence, Reporting Differentials were calculated considering increases by fixed rates, for example by 10%, as below:

$$\text{Difference}_{mt}^{post,\Delta 10\%} = \text{Reporting}_{mt}^{post} - \left(\widehat{\text{Reporting}}_{mt}^{post}\right) \times 1.10. \tag{A5}$$

We display Reporting Differentials following equation A1 under these alternative sensitivity assumptions.

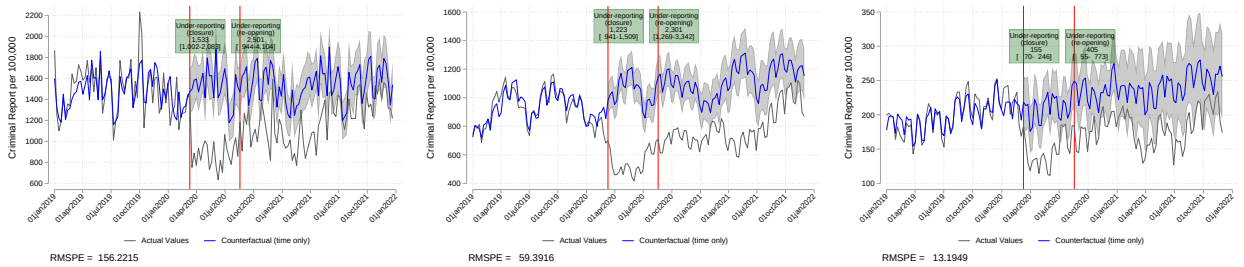
Finally, in conducting inference on these projections, we must take account of the fact that counterfactual outcomes are estimated based on observed data, and hence are subject to sampling uncertainty inherent in this estimation procedure. To conduct inference, we undertake a block bootstrap procedure,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

A3.2 Results

Figure A26 presents the counterfactual projections. In panel A we document simple projections: how would complaints of violence against children perform if simply projecting cyclical (week of year) and municipal-specific factors 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 reopened. 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.

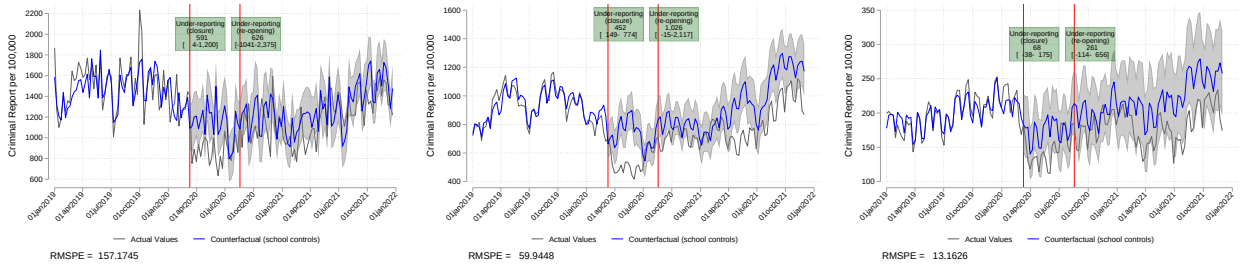
In Panel B, we consider alternative projections, now 'turning off' the school reporting channel. If school closure accounts for the full difference in reporting, we would expect that these counterfactual

Panel A: Simple Counterfactual (Time Only)



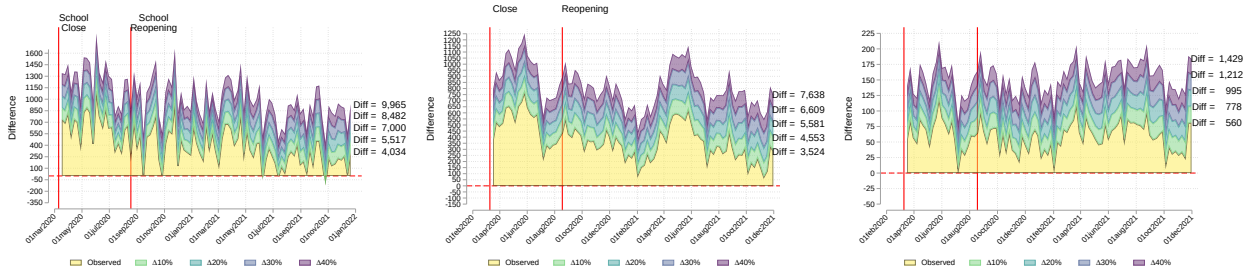
(a) Intra-family Violence (b) Sexual Abuse (c) Rape

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 A26: Reporting, Projected Reporting, and Under-reporting Under Various Counterfactual Assumptions

Notes to Figure A26: 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 A1. 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 A4. 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 A5, in which rates of projected violence are allowed to increase in the post-pandemic period.

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. These results are broadly similar if additionally controlling for COVID case rates, testing and positivity rates, as well as municipality lockdown status.

Finally, if, as prior literature suggest (see Section 4.3), violence increases during this time, the models presented in Panels A and B could be read as the lower bound of reports missing due to school closure. In panel C, 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 we do not know the magnitude of the increase,[28] we document the difference between counterfactuals and actual reporting under alternative assumptions of increases in underlying violence by {10, 20, 30, 40}%. The results suggest that, 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).

[28] 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., 2024), pointing to an increases in violence within the household based on objective measures. a wide range of values ranging from 0 to as much as a 40% increase.

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