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Dean R. Lillard
Joseph J. Sabia
Zihao Sheng
Casey Warman

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The Effect of School Disruptions on Parental Labor Supply: Evidence from Canadian Panel Data

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ABSTRACT

This study explores the effects of COVID-19 school closures on labor market outcomes of Canadian parents of school-aged children. Using newly collected data on grade-level-specific school closures across 141 Canadian cities along with individual-level panel data from the Labour Force Survey, difference-in-differences estimates provide robust evidence that restrictions on in-person schooling reduced employment among partnered mothers of school-aged children by approximately 2 percentage points. For fathers, labor supply adjustments are much weaker and concentrated on single fathers. Finally, auxiliary analyses using administrative tax records provide suggestive evidence that school closures increased public benefit receipt among single parents.

Dean R. Lillard
The Ohio State University
and NBER
lillard.13@osu.edu

Joseph J. Sabia
San Diego State University
Department of Economics,
and Center for Health Economics &
Policy Studies
and IZA & ESSPRI
jsabia@sdsu.edu

Zihao Sheng
Dalhousie University
Department of Economics
zihao.sheng@dal.ca

Casey Warman
Dalhousie University
Department of Economics
and NBER
warmanc@dal.ca

1 Introduction

The onset of the COVID-19 pandemic generated challenges to governments worldwide in how to best choose non-pharmaceutical interventions (NPIs) to mitigate the adverse public health effects of COVID-19 while minimizing the health, human capital, and labor market costs associated with NPIs. We examine the unintended consequences of one of the many policies Canadian governments introduced to mitigate the spread of the COVID-19 virus: elementary and secondary school closures. We use individual-level longitudinal data on Canadian parents' labor supply and newly collected, city-level, grade-specific (primary vs. secondary) school closure data to provide new estimates of the effects of in-person schooling disruption on employment and work hours of parents of school-aged children.

Evaluating the unintended effects of school closures on children and parents has attracted substantial global attention from scholars and policymakers. There is a growing body of work examining how COVID-19-induced school closures affected children's learning and the development of their human capital (Agostinelli et al., 2022; Jack and Oster, 2023). A related literature documents broader consequences of school disruptions, including effects on children's safety and well-being (Baron et al., 2020; Hansen et al., 2024c,a; Björkegren et al., 2024). A third literature explores the unintended consequences of school closures on parental labor supply, mostly focusing on the U.S. (Amuedo-Dorantes et al., 2023; Garcia and Cowan, 2024; Hansen et al., 2024b). This study contributes to this third literature, with a focus on Canada, a nation that enacted a wide set of NPIs (e.g., non-essential business closures, stay-at-home orders, bans on in-person gatherings) designed to stop the spread of COVID-19, but may also have had unintended consequences that affected the economic wellbeing of households. In particular, when Canada's federal, provincial, and city governments restricted in-person schooling, they may have inadvertently affected labor supply decisions of parents of school-aged children.

Parents jointly determine labor supply within the household, deciding who works and how much each person works given their constraints. Governments potentially upset those equilibrium choices by ordering schools to close partially or fully. COVID-19 school closures generated an abrupt and largely unanticipated disruption to in-person schooling that affected

household work and caregiving arrangements. We study whether parents change the time they spend in the labor market, with potential implications for time spent at home caring for and perhaps educating children, when governments constrained how schools operate and limited the availability of childcare.[1]

In Canada, the timing, incidence, and duration of school closures varied substantially across cities and during four distinct COVID-19 infection waves. In the first infection wave, governments suspended in-person instruction for students in all grades and schools nationwide, with variation arising from differences in reopening timing. We leverage local policy variation from subsequent COVID-19 waves, when governments alternately reopened, restricted, and closed schools. We exploit the variation in provincial and municipal school closures in 141 Canadian cities.

Our study contributes to, complements, and expands the body of evidence produced by several early studies that examined the effects of school closures on parental employment and work hours in the U.S. (Amuedo-Dorantes et al., 2023; Garcia and Cowan, 2024; Hansen et al., 2024b). Studying Canada provides a useful comparison because Canadian and U.S. local labor markets share many similarities, including closely integrated economies, similar local labor market structures, and decentralized authority over many policy decisions. At the same time, prior work comparing Canadian and U.S. local labor markets shows that responses to economic shocks are not always identical.[2] These similarities and differences make Canada an important setting for assessing whether the labor supply responses documented in the U.S. also arise in a related but institutionally distinct context. In contrast to the market-based shocks emphasized in prior work, COVID-19 school closures impose direct constraints on households, so differences in policy, institutions, and norms may play a more central role in shaping responses. We also compile detailed, geographically disaggregated data on COVID-19 school-closure policies to exploit this variation.

We are aware of only one study that investigates labor supply responses to school closures

[1] A broader set of school disruptions includes natural disasters (Holmes, 2002; Miller and Hui, 2022; Andrabi et al., 2023), severe weather (Marcotte and Hansen, 2010), and teacher strikes (Belot and Webbink, 2010; Jaume and Willén, 2019), which differ in scope.

[2] Albouy et al. (2019) find that local labor demand shifts raise nominal wages similarly in both countries, but that Chinese import competition had weaker negative effects on manufacturing employment in Canada, consistent with differences in immigration, transfer systems, and labor market adjustment.

in Canada. Beauregard et al. (2022) use data from the (Canadian) Labour Force Survey spanning the period January 2017-January 2021 to study the impact of primary school reopenings (following an initial nationwide shutdown) in Spring and Fall 2020 on labor market outcomes of parents of primary school-aged children. Using a difference-in-difference-in-differences (DDD) approach where parents of high school students serve as a within-region control group, they find that when schools reopened, single mothers were 18.1 percentage points more likely to work and worked, on average, 7.6 additional hours per week. For partnered parents of school-aged children, they find no evidence of school-reopening-induced increases in employment or work hours unless they restrict their identifying variation to only the Spring 2020 period. Thus, they conclude that single mothers were much more affected by in-person schooling disruptions relative to partnered parents.

Our study extends and builds upon Beauregard et al. (2022) in several ways. First, we collect granular, grade-specific (primary vs. secondary) school closure policy data over the full pandemic period, which extended into 2022. These new data allow us, for the first time, to (1) estimate the effects of in-person schooling disruptions on parents of secondary school-aged children (e.g., high school students), whose labor market outcomes have been found to be affected by school reopenings in the U.S. (Hansen et al., 2024b), and (2) exploit more policy variation than was available to Beauregard et al. (2022), both temporally and geographically. Second, because our new data on primary and secondary school closures show that grade-specific school closures are highly correlated, we are able to rely on a within-city control group (e.g., childless partnered adults) who are less likely to be affected by in-person schooling disruptions. Third, given concerns in the recent literature about potential bias that may be introduced in two-way fixed effects estimates (TWFE) due to heterogeneous and dynamic treatment effects, we also estimate stacked difference-in-differences models that avoid "forbidden comparisons" of treatment and control cities. Finally, we supplement our analysis of longitudinal survey data (in which we can control for individual fixed effects) with administrative tax records to broaden the set of outcomes of interest to include receipt of labor-market-related government benefits.

We document three key findings from our study. First, using data from the Labour Force

Survey, we find consistent evidence that school closures reduced employment among partnered Canadian mothers of school-aged children by approximately 2 percentage points. This finding is present for mothers with only primary school-aged children, only secondary school-aged children, or children in both age groups. We also find strong evidence of statistically significant reductions in unconditional work hours. In sharp contrast, estimated employment and hours effects of school closures for partnered fathers are much weaker, consistent with the hypothesis that school closures induce married mothers to devote more time to childcare. Results suggest that, when governments close schools, single mothers adjust labor supply less than partnered mothers and both single fathers and single mothers work fewer hours. A causal interpretation of our findings is supported by falsification tests on partnered childless adults as well as event-study analysis using stacked DD estimates, which ameliorate bias due to heterogeneous and dynamic treatment effects.

We proceed as follows. Section 2 provides background on the Canadian COVID-19 economic context and income support programs and discusses the prominent U.S. school closure literature. Section 3 describes the data on outcomes, COVID-19–related school closures, and other COVID-19 policies. Section 4 outlines the empirical approach, Section 5 presents the results, including an assessment of the identifying assumptions. Section 6 concludes.

2 Background

2.1 Canada During COVID-19: Economy and Income Support Policies

When federal and provincial governments locked down economic and social activities in March 2020, the Canadian economy entered a deep recession. From February to April 2020, the employment rate fell from 75.6 to 65.2 percent, and the unemployment rate rose from 6.1 to 13.2 percent.[3] These trends occurred concurrently with (1) the introduction of a variety of NPIs by federal, provincial, and local governments in Canada (e.g., non-essential business

[3]Author's calculations based on the Public Use File of the Labour Force Survey.

closures, stay-at-home orders, bans on large in-person gatherings, and travel restrictions) to mitigate the spread of COVID-19, which likely contributed to reduced employment, and (2) changes in voluntary private behavior that resulted in greater social distancing from non-household members.

After the initial economic decline, applications to Canada's Employment Insurance program (EI) surged nearly 20-fold (Burman, 2020). To offset earnings losses and business downturns associated with the federal and provincial governments' mitigation policies, they adapted their income-support programs. Appendix Section A.1 discusses the details of the Canadian programs that pay COVID-19 benefits and identifies the ones we measure. As governments subsequently relaxed lockdown policies, the economy began to recover. By September 2020, the employment rate rebounded to 75.3 percent, fell sharply during the next COVID-19 wave, and then rose steadily to around 80% by the fall of 2021 (Jones et al., 2023).[4]

2.2 Features of Prior Prominent U.S. Studies and Our Contribution

Localized, reliable administrative school closure data have been very difficult for researchers to obtain. For instance, in the U.S. many sources of administrative data on COVID-19 were crowdsourced, sporadically available at irregular intervals, and incomplete (see Hansen et al. (2024b) for a discussion). Thus, recent studies have exploited anonymized cell-phone foot traffic data (e.g., intensity of smartphone pings on elementary and secondary school campuses) to proxy for the physical presence of persons on school property for in-person schooling, using deviations from pre-pandemic traffic to infer full closures, reopenings and hybrid or partial schooling (Parolin and Lee, 2021; Garcia and Cowan, 2024; Hansen et al., 2024b). While this approach provides a more continuous and consistent measure of in-person K-12 attendance, this proxy for in-person schooling cannot differentiate grade-specific school closures, thus introducing measurement error that could attenuate estimated

[4]Also see Baylis et al. (2022) for evidence on the uneven distribution of employment risk across Canada during the COVID-19 pandemic.

treatment effects.

Despite data limitations in prior studies that constrained the ability to link school closure and reopening policies to the specific schools children attended, studies relying on foot traffic data provide important insights. For instance, three prior studies find that maternal labor supply is positively related to in-person schooling in the U.S. (Heggeness, 2020; Amuedo-Dorantes et al., 2023; Garcia and Cowan, 2024). Hansen et al. (2024b) show that married mothers who experience a school reopening are more likely to be employed (by about 3.3 percentage points), work more hours each week (0.76 on average), and are less likely to work remotely. They estimate no effect of school closures on labor market outcomes of married fathers with school-age children, consistent with broad evidence that married mothers spend more time in childcare and home production in general than fathers. Evidence for single parents is also mixed. Hansen et al. (2024b) find that both single mothers and single childless women did not change their labor supply when schools reopened, while Garcia and Cowan (2024) document larger effects for single mothers than for married mothers when schools closed.

To address concerns about measurement error surrounding proxies for school closures, we compile comprehensive, grade-specific city-level data on school closures and reopenings. We gather data from provincial and municipal government websites and local news archives on closures and reopenings that occurred from March 2020 through May 2022 in 141 cities in Canada and directly track school learning modes. Although we do not use foot traffic data to characterize school closures, we do use them to capture cross-jurisdictional differences over time in broader COVID-19-induced economic disruptions that plausibly affect labor supply decisions (see Section 3.4).

Many studies also emphasize the importance of heterogeneous treatment effects based on job characteristics and within-household support, but available data often constrain how these channels can be measured. Studies typically find smaller parental labor supply responses to school closures if they initially worked in jobs governments judged to be essential and whose employers allowed them to work remotely (Garcia and Cowan, 2024; Hansen et al., 2024b). Other studies find that parents supply more labor when they have someone to

help care for young children at home (Amuedo-Dorantes et al., 2023; Barkowski et al., 2025; Imre et al., 2026). Because of detailed information on a variety of pre-pandemic parental characteristics, our analyses will allow us to conduct a wider set of descriptive heterogeneity analyses than many prior studies have been able to conduct.

Relatedly, studies of the United States have uniformly used repeated cross-sectional data for their analysis (Amuedo-Dorantes et al., 2023; Garcia and Cowan, 2024; Hansen et al., 2024b). While such analyses can produce compelling causal evidence, there are important advantages of panel data. As noted above, longitudinal data allow us to measure pre-treatment parental characteristics for heterogeneity analysis. In addition, because the data track individuals over time, we can control for individual fixed effects. Relative to studies that only control for jurisdiction fixed effects (e.g., county), our use of individual fixed effects controls for time-invariant and unobserved heterogeneity.

We add to the above literature by using restricted-access longitudinal Labour Force Survey (LFS) data and administrative tax records that report rich measures of job characteristics, annual earnings, and COVID-19–related transfers. We use the LFS data to estimate whether and how school closures caused parents to immediately change their labor supply in the week schools closed. We use the Longitudinal Administrative Databank (LAD) to analyse how annual income and receipt of labor market-related COVID-19 benefits varied with annual days of school closure each year from 2019 to 2022.[5] These data allow us to capture a wider set of outcomes than have been explored in the above U.S.-based literature.

3 Data

3.1 Labour Force Survey (LFS)

We use panel data from the confidential version of the monthly LFS for January 2019 to December 2022. The government draws its official labor-market statistics from the LFS. The

[5]Evidence using UK panel data from the initial months of the pandemic finds substantial heterogeneity among UK residents whose earnings and income were impacted by COVID-19 mitigation policies (Crossley et al., 2021).

LFS consists of six samples in groups that rotate every six months. Each month, the LFS rotates out one-sixth of the sample and adds another sample of similar size. With sample weights, these data represent the adult Canadian population.

Each month, the LFS collects labor market information for a monthly reference week (Sunday through Saturday) that typically includes the 15^{th} day of the month, except in November and December (when interviews are conducted earlier). We use a restricted version of the LFS accessed through Canada's Research Data Centres that includes, for household respondents, data on demographics, labor-market outcomes, the number and ages of household children, geographical indicators that identify each household's location, and the characteristics and labor-market activity of all co-resident family members. The restricted version links respondents across six months, allowing us to follow individuals over the LFS's six-month rotation.

Our analysis focuses on parents whose labor supply may be affected by K–12 schooling policies. We consider three samples of mothers and fathers (separately). The groups include parents who: only have primary school–aged children (5–11); only have secondary school–aged children (12–17); and who have at least one child in each age group. The latter group is of particular interest because older children may help care for younger siblings. Parents may not alter labor supply because the older children care for their younger siblings.[6] We estimate separate specifications by marital status (partnered vs. single parents), as childcare constraints and intra-household adjustments may differ across these groups.

We restrict the sample to respondents aged 20-64. We exclude those with co-resident children aged 0–4 or 18 and older and respondents employed in K–12 education.[7] For partnered households, we restrict the sample to opposite-sex couples. To avoid complications arising from endogenous residential mobility, we exclude respondents who move across cities.[8]

We construct and model four binary dependent variables that indicate respectively whether

[6]Because parents in the latter group must have at least two children, we also estimate models with samples of the other two groups restricted to parents with two or more children. Those estimates are consistent and somewhat larger for households with two or more children.

[7]In our sample, 3.5% of parents with children aged 5–11 and 35.2% of parents with children aged 12–17 are between 55 and 64 years old. Appendix Table A11 shows that the estimates are not sensitive to excluding individuals aged 55–64.

[8]This rule excludes only 1% of LFS respondents.

or not a respondent was 1) employed and at work (not simply employed)[9], 2) working 30 or more hours per week ("full-time" work), 3) unemployed, and 4) out of the labor force. We also measure the hours a respondent worked at all jobs during the reference week. When respondents report that they did not work, we set weekly hours to zero.

We code an individual as *employed and at work* if he/she was employed and currently at a job; as *unemployed* if he/she had no job in the reference week, was available to work, and either actively sought work in the past month, was temporarily laid off, or scheduled to start a job within four weeks; as *working full-time* if he/she worked 30 or more hours at his/her main job; and as *out of the labor force* if he/she was not employed in the reference week, did not actively seek work in the past month, and was not expecting to start a job in the next month. Finally, hours measures the total number of hours worked across all jobs during the reference week.[10]

We draw LFS data on each respondent's age, education, and characteristics of their job, household, and spouse (if partnered). We use information on each respondent's city of residence to merge in city-by-month school closure policies (See Section 3.3).

Appendix Tables A4-A5 report summary statistics on labor market outcomes and demographics for partnered parents of school-aged children, by gender and age of children, in the pre-pandemic (2019) and post-pandemic (2020–2022) periods. Appendix Table A6 presents corresponding statistics for single parents.[11] Following Warman and Worswick (2015), Imai et al. (2019) and Chernoff and Warman (2023), we construct, for each employed LFS respondent, measures of quantitative skills, physical strength, and occupation-specific exposure risk to the COVID-19 virus based on the job they held in the month before school closures. For

[9]Employed and at work is defined as the subset of the employed individuals who actually worked positive hours in the reference week, therefore excluding those who are employed but absent from work for reasons such as illness, vacation, family responsibilities, or other temporary absences. Jones et al. (2020, 2023) document that, especially in the first COVID-19 infection wave, the demographic characteristics of the employed and employed and at work samples distinctly differed.

[10]We do not use LFS earnings as a dependent variable because the LFS collects employees' earnings only in the first interview and updates them in subsequent months only when respondents report specific changes to their employment information. For respondents who remain in the same job, the LFS may not fully capture earnings variation arising from temporary work absences, changes in hours worked, or changes in pay. As an imperfect substitute, we use annual earnings data from the LAD (see Section 3.2).

[11]We do not report separate LFS summary statistics by year, as some annual cells do not meet Statistics Canada disclosure requirements.

respondents in cities without school closures, we use the job held in the same calendar month as the corresponding pre-closure period in treated cities. We use this coding to conduct heterogeneity analysis by pre-pandemic characteristics.

3.2 Tax File Analysis: Longitudinal Administrative Databank (LAD)

While the LFS is our main data source, we also conduct auxiliary analysis using annual data from the LAD. The LAD is a 20% random sample of Canada's annual family tax files comprised of data from individual income tax returns and other administrative sources. The LAD offers a large, nationally representative sample. Once selected, an individual remains in the LAD sample in every year they appear on a tax return. We draw a sample from the 2019 LAD data and follow these respondents annually through 2022. We analyze an individual's inflation-adjusted annual earnings and labor-market–related government benefits, including Employment Insurance (equivalent to Unemployment Insurance in the U.S.) and COVID-19 income-support programs with a labor market attachment component (see Appendix Section A.1). All values are expressed in constant 2022 Canadian dollars. We refer to the latter as "COVID-19 benefits."

The data include each person's age, marital status, and number of co-resident household adults. When partnered, we can also measure partner's age. We assign individuals to cities using postal codes.

In Appendix Tables A7-A8 we report, for parents of school-aged children and childless adults, the summary statistics of annual earnings and COVID-19–related benefits received in pre-pandemic (2019) and pandemic (2020, 2021, 2022) years for the LAD sample. We stratify samples by gender, partnership status, and age of children.

3.3 School Closures

We collect data on COVID-19 school closures across all four waves of the pandemic in Canada: the initial wave in spring 2020, the second wave in fall 2020 and winter 2020-2021, the third wave in spring 2021, and the fourth wave in winter 2021-2022. During the first

wave of the pandemic, provinces implemented province-wide school closures. In later waves, however, sub-province jurisdictions were permitted to adopt school modality policies. To obtain detailed, localized school closure data, we scraped general news sources and municipal government web pages. We collected the dates and duration of government-mandated school closures, reopenings, and instructional modes.

We collect school-by-month data in 141 cities from March 2020 to May 2022. The cities include 41 Census Metropolitan Areas (CMAs) with 100,000 or more residents and 100 Census Agglomerations (CAs) with 10,000 to fewer than 100,000 residents.[12]

We characterize closures, in-person, and remote instruction modes in terms of how they may change a parent's pre-pandemic childcare choices. When schools close or teach remotely, children must remain home, increasing parents' demand for childcare. To reflect this shift, we define a dichotomous closure measure that equals 1 when a city's schools are all closed (and/or are teaching remotely). If schools are in "hybrid mode" (e.g., some days remote and some days in-person), we code our school closure measure as equal to zero.[13] Our main treatment variable, therefore, focuses on the most intensive city-wide school closures, where we would most expect parental labor supply adjustment. On the other hand, by coding cities with some (but not all) schools closed (or in hybrid mode) as "untreated," the control group is contaminated to the extent that parental labor supply responds to partially treated cities. Our estimates may, therefore, represent a lower bound of the true effect of school closures on labor supply.[14]

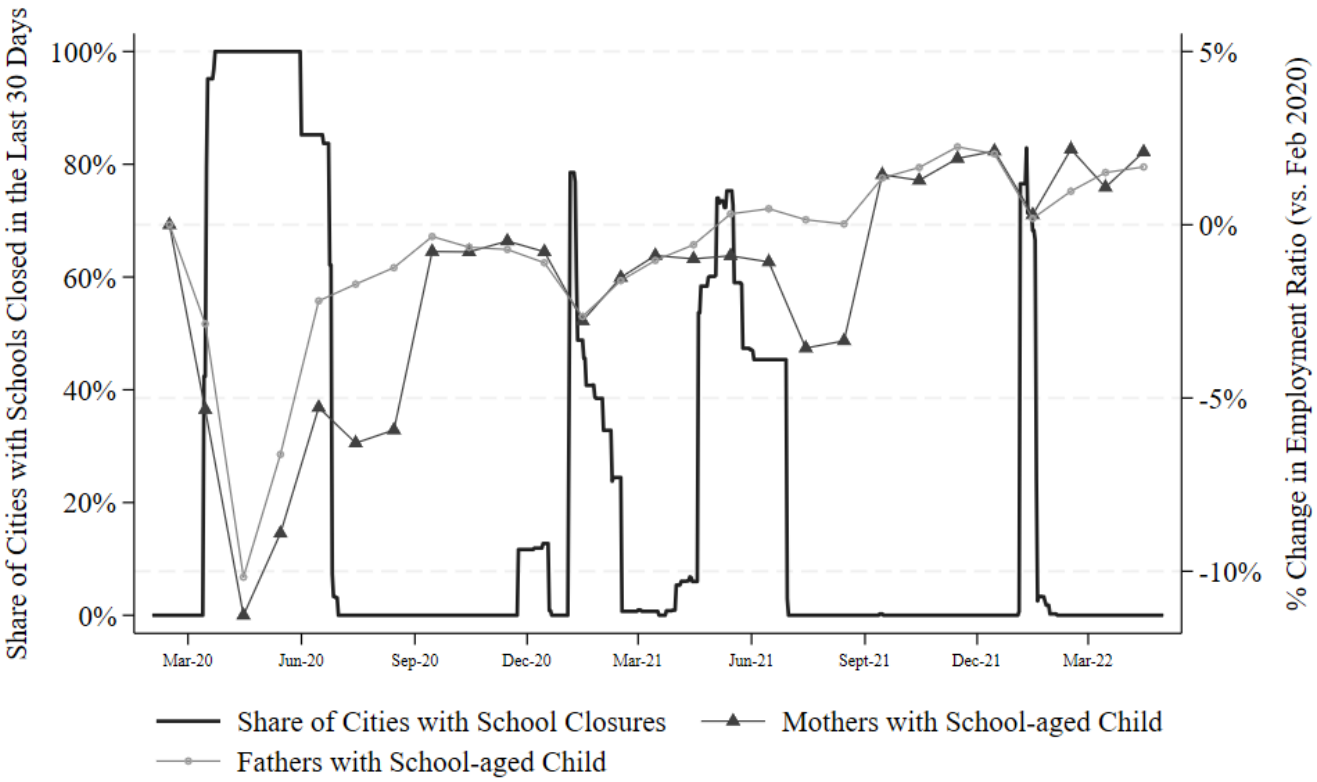
Figure 1 plots, for each day from March 2020 to May 2022, the proportion of 141 CMAs/CAs with uniformly closed K-12 schools, and the percent change in the employ-

[12]We drop 10 CAs because we lack foot traffic data and exclude cities with fewer than 10,000 residents because the LFS masks those cities' geographic location.

[13]Hybrid learning was relatively uncommon in our sample and was concentrated mainly in British Columbia, Quebec, and New Brunswick during the 2020–2021 school year, primarily affecting secondary schools. In our main analysis, we lump city schools operating in hybrid mode as "not closed". We do so partly because schools implemented hybrid teaching modes quite differently and often announced those modalities far in advance. In sensitivity analyses, we find that creating a third category of cities ("partially treated cities") does not substantially alter the qualitative pattern of findings we show.

[14]For example, in Ontario during the second COVID-19 wave, when there were no city-wide school-closure policies in place, at most 83 of 4,828 schools experienced an individual closure on any given day, suggesting that any impact of this pooling is likely to be small (Ontario Ministry of Education, https://data.ontario.ca/dataset/summary-of-cases-in-schools).

Figure 1: City-Level School Closures and Employment of Parents of School-Aged Children, March 1, 2020 to May 1, 2022



Notes—School closure policies were collected by the authors. Employment ratios are calculated for mothers and fathers aged 20–64 with school-aged children using the Public Use Microdata Files of the Canadian Labour Force Survey and are weighted using the sample weights provided by the survey.

ment rates of mothers and fathers aged 20–64 with school-aged children relative to the pre-pandemic level in February 2020.

Figure 1 shows that mothers' employment recovered more slowly than fathers after the dramatic 11.3% drop in employment in April 2020 relative to February 2020 and remained below the pre-pandemic level. Fathers' employment ratio returned to its pre-pandemic level by May 2021, whereas mothers did not fully recover until September 2021. The plots also show that, when schools close, mothers' employment appears to decline more dramatically than fathers' employment, foreshadowing the possibility of a gendered parental response to school closures.

Appendix Figure A1 plots the daily operating mode for schools in each of the 141 cities. We differentiate between COVID-19 school closures, scheduled school holidays, winter

breaks, and summer breaks.

We cross-validated our school closure measures across news reports, municipal postings, the Statistics Canada COVID-19 Restrictions Index (Clarke et al., 2022), and the Oxford COVID-19 Government Response Tracker (OxCGRT) (Hale et al., 2021).

Many early studies have treated school closures as relatively homogeneous and, due to data limitations, focused primarily on early pandemic closures. Our more detailed measures of school closures in Canada over each wave of the pandemic may generate more generalizable estimates to the extent that school closure policies and behavioral responses to those policies evolved over successive COVID-19 waves. For instance, as governments learned more about COVID-19, they may have adjusted whether, when, and how schools were closed; parents may also respond differently with respect to labor supply in successive school closures and reopenings. Thus, accounting for heterogeneous and dynamic treatment effects may be important.[15]

3.4 Proxies for Other Non-School Closure COVID-19 Shocks

With Advan Research foot traffic data, we construct a variable that measures foot traffic at particular locations (Advan Research, 2025). Advan Research gathers GPS data from over 3 million anonymized Canadian devices whose owners consented to provide their data. These data count daily visits to more than 1.6 million geo-specific points of interest (POIs). The POIs represent over 90% of locations accessible to the public. We use the foot traffic data at local businesses to proxy for variation in local economic conditions and other COVID-19 restrictions (e.g., lockdowns, business restrictions, capacity, and gathering limits) that likely

[15]We also note that over time, governments became less likely to regulate schools, businesses, and social interactions simultaneously, thus providing additional identifying variation to the researcher in a difference-in-differences framework. Appendix Table A2 supports this point. It shows the pairwise correlation between our daily city school closure measure and the Statistics Canada COVID-19 Restrictions Index for the province in which each city is located. Below each correlation for Waves 2–4, the table reports the change relative to Wave 1. Over time, school closures were progressively less correlated with other provincial regulations. For example, the correlation between school closures and workplace closures drops by 0.127 in Wave 3 and by 0.441 in Wave 4 relative to Wave 1. These patterns indicate that variation in school closures in later waves is less intertwined with other COVID-19 policies. The lower Wave 2 correlation arises mechanically because of Statistics Canada's coding rules for its index (see Appendix section A.3).

affected labor demand.[16]

We start with daily visit counts at POIs of retail establishments, restaurants, and personal service firms. We aggregate the counts in each city and reference week. For the LFS analyses we include the city-week counts as an index of local economic activity. For the LAD analyses, we aggregate foot traffic over each calendar year.[17]

We use daily active COVID-19 case counts from the Timeline of COVID-19 in Canada data (Berry et al., 2021). They compile the data from government reports, accredited news media, and social media accounts. The Timeline verifies the daily data using a multi-step data validation process. From these data, we use the case-count time-series at the "health region" level because it is the finest spatial resolution they could establish to be consistently collected over the sample period. The time-series measures case counts of more than 99% of the regions in every month of the data collection period.[18] The Public Health Agency of Canada republishes the Timeline.[19]

4 Empirical Approach

To estimate our main regression models, we use ordinary least squares (OLS) in the following two-way fixed effects (TWFE) difference-in-differences specification:[20]

$$Y_{ict} = \gamma_i + \theta_t + \beta \cdot \text{Closure}_{ct} + \Gamma X_{ct} + \epsilon_{ict} \tag{1}$$

[16]We did not use school foot traffic data to infer school closures, as prior studies have done (Hansen et al., 2024b; Parolin and Lee, 2021), because foot traffic information for school locations was removed from the academic version of the data following a change in ownership and was available only through a separate purchase that was beyond our available budget.

[17]We regress non-essential business foot traffic on our school closure measure with city and year-by-month fixed effects as in our main specification. A full week of school closure is associated with approximately 4% lower non-essential business foot traffic. School closures explain only 2.8% of the within-city, within-month variation in non-essential business visits, indicating substantial independent variation between these measures.

[18]Provincial health ministries collectively define health regions (105 in 2022) that generally group municipalities together. There are 105 health regions in 2022 providing complete coverage of Canada.

[19]https://health-infobase.canada.ca/covid-19/current-situation.html.

[20]We also estimate annual earnings and labor-market–related government benefits using Poisson pseudo–maximum likelihood (PPML), retaining observations with zero earnings or benefit receipt, and report these results in the appendix. We mention these results in a footnote in Section 5.5.

where Y_{ict} denotes a labor supply outcome of individual i who lives in city c in year-month t. γ_i are individual fixed effects and θ_t are year-by-month fixed effects. The former set of fixed effects absorbs time-invariant, person-specific characteristics that affect labor supply, while the latter absorbs national common shocks across cities. Our key treatment variable $Closure_{ct}$ is set equal to 1 if the city's schools are closed in the survey reference week; it is set equal to 0 otherwise (we note that estimates of β are robust to the inclusion of week-specific time effects). Note that because our sample is restricted to non-movers (as moving may be endogenous to school closures), the inclusion of individual fixed effects, γ_i, ensures that identification comes from within-individual changes over time in school-closure exposure in each individual's city. The vector X_{ct} includes city-level foot traffic in non-essential businesses to capture local macroeconomic shocks and COVID-19–related policies (e.g., non-essential business closures), as well as COVID-19 cases in the previous month. Standard errors are clustered at the city level to allow for arbitrary serial correlation in shocks common to residents of the same city. We also apply survey weights to obtain population-representative estimates.

We estimate equation 1 separately for mothers and fathers within four different samples: (1) parents of only primary school-aged children, (2) parents of only secondary school-aged children, (3) parents of both primary and secondary school-aged children, and (4) partnered childless adults. The first three samples constitute those who may be affected by school closures, while the fourth acts as a "placebo sample" as they should be less affected or unaffected by school closures (see Hansen et al. (2024b) for a discussion). For parents who may be affected, we explore heterogeneity by partnership status.[21]

Note that because we collect primary- and secondary-specific school closure data, our measure of $Closure_{ct}$ will vary by the analysis sample of interest. For instance, when we examine parents of only primary (secondary) school-aged children, $Closure_{ct}$ will measure city-by-month variation in primary (secondary) school closures. For parents with children across these school levels, we define exposure according to whether either school level was

[21]One concern is that school closures and reopenings may have occurred just prior to an individual's six-month panel, potentially affecting behavior at the start of the panel. We experimented with dropping individuals whose panels begin shortly after a reopening. In some cases, the coefficients are larger, but the overall conclusions remain unchanged.

closed in the reference week in the LFS and according to the longer annual cumulative closure period in the LAD and assess robustness to alternative treatment definitions based on primary or secondary school closures only. Because a city's primary and secondary schools were typically closed at the same time, there is limited independent variation between the two measures, resulting in very similar results across specifications.[22]

To produce unbiased estimates of β, the parallel trends assumption must hold. That is, we must assume that, in the absence of a school closure, parental labor supply in untreated cities is an unbiased reflection of how parents in treated cities would have supplied labor had those cities not closed their schools. To minimize the likelihood that this assumption is violated, we (1) control for COVID-19 foot traffic to capture any unmeasured COVID-19–related variables that could be correlated with school closures in Canada and parental labor supply, and (2) conduct falsification tests on partnered childless adults. Moreover, we estimate DDD models to formally test whether the estimates of β differ for parents of school-aged children and childless adults and present the p-value on those tests.

An important concern with our TWFE approach is that estimates of β may be biased due to heterogeneous and dynamic treatment effects. To reduce bias of this form, we estimate dynamic stacked DD models following Cengiz et al. (2019) and restrict the set of counterfactuals to non-adopters from four months before to three months after a school closure. Event-study analysis also allows us to assess the parallel trends assumption descriptively by examining conditional employment differences between treatment and control cities prior to a school closure. We discuss this approach in more detail below.

5 Results

Our main findings are shown in Tables 1-3 and Figures 2-3. Supplemental analyses appear in the online appendix.

[22]The correlation between primary and secondary school closures is 0.86.

5.1 Weekly Labor Market Outcomes - LFS Sample

In Table 1, we present estimates of β for partnered mothers (columns 1-3), single mothers (columns 4-6) and partnered childless adults (column 7). For partnered mothers with only primary school-aged children (5–11), primary school closures reduce the probability of being employed (and at work) by 2.5 percentage points, representing a 3.3% reduction relative to the pre-pandemic employment rate (77%). We also find that primary school closures lower partnered mothers' unconditional hours worked by 1.2 hours per week, and decrease full-time employment by 3.4 percentage points. These estimates are statistically significant and economically sizable, aligning with U.S.-based studies showing that school closures generate sizable maternal labor supply reductions, potentially at both the extensive and intensive margins for partnered mothers with school-aged children (Garcia and Cowan, 2024; Hansen et al., 2024b). These results support the hypothesis that in coupled households, mothers remain the primary providers of childcare and adjust their labor supply when school-based childcare is disrupted.

For partnered mothers with only secondary school-aged children (12–17), estimated treatment effects for secondary school closures are similar in sign and magnitude but are somewhat less precisely estimated. Secondary school closures reduce the probability that mothers are employed and at work by 2.4 percentage points. Moreover, work hours are reduced by about 1.1 hours per week, and the probability of working full-time falls by approximately 2 percentage points, though these estimates are not statistically distinguishable from zero at conventional levels. These results suggest that school closures negatively affect maternal labor supply even when children are older, but the effects may be smaller to the extent that some of these teenagers may be able to care for themselves and do not require parental supervision during periods of online instruction.

Among partnered mothers with at least one primary (5–11) and one secondary (12–17) school-aged child, school closures reduce the probability of maternal employment by 1.65 percentage points and hours worked by 0.7 hours per week. We note that these estimated effects are somewhat smaller than those for partnered mothers with only younger children, suggesting that, perhaps, older siblings are an alternate source of childcare. Partnered

mothers with both younger and older children are also 1.2 percentage points more likely to be unemployed and 1.9 percentage points more likely to leave the labor force. This pattern indicates that partnered mothers with children in both primary school and secondary school respond to school closures by reducing labor market attachment through quitting jobs or exiting the labor force, rather than by reducing their hours of work through temporary leave. The result suggests that, although older children may provide some childcare for younger siblings, they do not fully offset mothers' perceived additional childcare demand generated by school closures.

Importantly, falsification tests on childless partnered adults show that school closures are essentially unrelated to their labor market outcomes. Because these results are obtained in the same local labor markets and time periods, they provide evidence that the maternal labor supply responses for similarly aged mothers with school-aged children are not driven by unmeasured local COVID-19 shocks (including COVID-19–related policy shocks) that may incidentally coincide with school closures. We also estimate fully interacted triple-difference models, formally comparing the estimated effects of school closures for mothers with school-aged children in our treated group to those of childless women. We present the p-values on the interaction terms. They confirm that the differences across groups are statistically significant.

For single mothers (columns 4-6), employment and hours responses to school closures are weaker than for partnered mothers. We find no evidence that school closures cause changes in the labor supply of single mothers with only younger or only older school-aged children. However, school closures reduce unconditional hours worked among single mothers of both primary and secondary school-aged children by 1.7 hours per week and lower their full-time employment by 4.2 percentage points. This pattern of findings—larger labor supply responses for partnered as compared to single mothers of school-aged children—is consistent with what is observed in the U.S. (Hansen et al., 2024b).

In sharp contrast to partnered mothers, partnered fathers' labor supply is largely unresponsive to school closures. Uniformly, estimates of β are much smaller than for partnered mothers and are statistically indistinguishable from zero at conventional levels. This pattern

of null effects for partnered fathers is consistent with results in the extant literature.

For single fathers, we find stronger labor supply responses to school closures than for partnered fathers, but the effects are still smaller than for mothers. Single fathers with only younger children work about 4.2 hours less per week in response to school closures. Single fathers with both primary and secondary school-aged children also reduce their labor supply, although these estimates are imprecise.

Taken together, the results suggest a gendered response to school closures among partnered adults. However, our findings also indicate that in the absence of a partner, single fathers do work less when schools close.

5.2 Stacked Event-Study Design

Recent advances in the difference-in-differences literature highlight potential biases in two-way fixed effects (TWFE) estimators under staggered treatment adoption, especially when treatment effects are heterogeneous over time (Goodman-Bacon, 2021; Callaway and Sant'Anna, 2021; de Chaisemartin and D'Haultfœuille, 2020, 2024). The bias arises when comparisons in which already-treated units serve as controls for later-treated units. When treatment effects vary over time, these "forbidden comparisons" can receive negative weights and lead to biased estimates (Borusyak et al., 2024).

To account for possible bias from "forbidden comparisons" under staggered adoption, we implement a stacked event-study and difference-in-differences approach (Cengiz et al., 2019; Deshpande and Li, 2019; Butters et al., 2022). For each school closure cohort e in a given month, we construct a separate dataset comprising treated cities that close schools in that month, as well as cities that experience no school closures or have school closures in the future as comparisons. We define cohort months s relative to the month of the first school closure. We include in the resulting datasets observations from four months before to three months after the first school closure. By defining treatment based on the timing of the first closure that treated units face, this setting is equivalent to a design in which all school closures happen at once with no staggered adoption. We then stack these cohort-specific

Table 1: Two-Way Fixed Effects (TWFE) Estimates of the Effects of COVID-19 School Closures on Labor Supply of Women, by Age of Children

	Partnered Mothers			Single Mothers			Placebo Group
Outcomes	Child Ages 5–11 only (1)	Child Ages 12–17 only (2)	Child Both 5–11 and 12–17 (3)	Child Ages 5–11 only (4)	Child Ages 12–17 only (5)	Child Both 5–11 and 12–17 (6)	Childless Women (7)
Employed, at work	−0.0254***	−0.0241*	−0.0165*	−0.0142	0.0119	−0.0137	−0.0003
	(0.0091)	(0.0122)	(0.0092)	(0.0205)	(0.0139)	(0.0208)	(0.0045)
p-value (DD treated vs. DD placebo)	0.0028	0.0346	0.1452	0.5180	0.3810	0.5520	
Pre-Pandemic Mean of Dep. Var.	0.768	0.814	0.771	0.745	0.771	0.776	0.668
Hours per week (unconditional)	−1.1615***	−1.0811	−0.7042**	−0.7506	−0.4156	−1.6924**	−0.1036
	(0.2706)	(0.6827)	(0.3559)	(0.9878)	(1.1171)	(0.8192)	(0.1530)
p-value (DD treated vs. DD placebo)	0.0002	0.1394	0.1379	0.5010	0.7820	0.0600	
Pre-Pandemic Mean of Dep. Var.	26.0	28.9	26.1	25.1	27.2	25.9	23.5
Full-Time Employment	−0.0344***	−0.0198	−0.0163	−0.0181	−0.0051	−0.0415**	−0.0056
	(0.0103)	(0.0215)	(0.0134)	(0.0302)	(0.0258)	(0.0200)	(0.0048)
p-value (DD treated vs. DD placebo)	0.0046	0.4864	0.4185	0.6830	0.9850	0.0692	
Pre-Pandemic Mean of Dep. Var.	0.556	0.619	0.551	0.523	0.581	0.508	0.515
Out of Labor Force	−0.0003	0.0126	0.0188***	−0.0102	−0.0195*	−0.0409	−0.0007
	(0.0055)	(0.0090)	(0.0067)	(0.0102)	(0.0116)	(0.0315)	(0.0024)
p-value (DD treated vs. DD placebo)	0.9348	0.1133	0.0042	0.3470	0.1020	0.1930	
Pre-Pandemic Mean of Dep. Var.	0.153	0.113	0.159	0.162	0.127	0.147	0.206
Unemployed	0.0070	−0.0019	0.0124*	0.0157	0.0022	0.0301*	−0.0028
	(0.0081)	(0.0043)	(0.0066)	(0.0156)	(0.0105)	(0.0154)	(0.0033)
p-value (DD treated vs. DD placebo)	0.1906	0.8542	0.0447	0.2570	0.6530	0.0368	
Pre-Pandemic Mean of Dep. Var.	0.038	0.031	0.032	0.056	0.053	0.041	0.062
Observations	33,010	24,995	23,080	7,501	7,441	4,040	154,105

NOTES—Estimates are obtained using data from the 2019–2022 Canadian Labour Force Survey and two-way fixed effects (TWFE) difference-in-differences regressions (using ordinary least squares). Our analysis samples are composed of mothers aged 20–64 with elementary and secondary school-aged children who had not moved during the analysis sample period. The placebo group contains partnered women aged 20–64 who are not cohabiting with children. Column headings for columns (1)–(3) and (4)–(7) denote partnership of women and ages of the resident children in the household. Column (7) shows results for childless women. All regression models control for year-by-month fixed effects and individual fixed effects, local foot traffic in non-essential businesses, and local COVID-19 cases in the previous month. City-clustered standard errors are in parentheses and regressions are weighted using survey-provided sample weights. *, **, and *** denote coefficients that statistically differ from zero with p-value $\leq$ 0.10, 0.05, and 0.01, respectively.

Table 2: Two-Way Fixed Effects (TWFE) Estimates of the Effects of COVID-19 School Closures on Labor Supply of Men, by Age of Children

	Partnered Fathers			Single Fathers			Placebo Group
Outcomes	Child Ages 5–11 only (1)	Child Ages 12–17 only (2)	Child Both 5–11 and 12–17 (3)	Child Ages 5–11 only (4)	Child Ages 12–17 only (5)	Child Both 5–11 and 12–17 (6)	Childless Men (7)
Employed, at work	0.0048	−0.0009	−0.0063	−0.0708*	0.0093	−0.0326	0.0055
	(0.0085)	(0.0098)	(0.0111)	(0.0407)	(0.0543)	(0.0609)	(0.0075)
p-value (DD treated vs. DD placebo)	0.9437	0.6391	0.2858	0.0466	0.9460	0.5550	
Pre-Pandemic Mean of Dep. Var.	0.899	0.857	0.883	0.832	0.820	0.821	0.741
Hours per week (unconditional)	0.2441	−0.0265	−0.6170	−4.1700**	−0.2767	−4.0079*	0.2678
	(0.4538)	(0.5414)	(0.7048)	(1.8291)	(2.5261)	(2.1996)	(0.2814)
p-value (DD treated vs. DD placebo)	0.9570	0.6339	0.1936	0.0099	0.830	0.0635	
Pre-Pandemic Mean of Dep. Var.	36.5	35.3	36.6	33.7	32.8	31.3	29.1
Full-Time Employment	−0.0023	0.0064	−0.0261*	−0.0915*	0.0415	−0.1115	0.0052
	(0.0117)	(0.0140)	(0.0156)	(0.0464)	(0.0557)	(0.0672)	(0.0075)
p-value (DD treated vs. DD placebo)	0.4424	0.9625	0.1683	0.0323	0.5260	0.0951	
Pre-Pandemic Mean of Dep. Var.	0.811	0.778	0.802	0.760	0.716	0.702	0.644
Out of Labor Force	−0.0010	0.0021	−0.0080	−0.0178	0.0124	0.0215	0.0025
	(0.0041)	(0.0074)	(0.0070)	(0.0280)	(0.0192)	(0.0273)	(0.0022)
p-value (DD treated vs. DD placebo)	0.4716	0.9442	0.0308	0.4700	0.6040	0.4750	
Pre-Pandemic Mean of Dep. Var.	0.035	0.062	0.052	0.056	0.086	0.104	0.143
Unemployed	−0.0024	−0.0054	−0.0020	0.0290	0.0031	−0.0753*	−0.0006
	(0.0064)	(0.0046)	(0.0054)	(0.0329)	(0.0297)	(0.0450)	(0.0038)
p-value (DD treated vs. DD placebo)	0.8147	0.3795	0.8440	0.3680	0.9030	0.1020	
Pre-Pandemic Mean of Dep. Var.	0.033	0.036	0.024	0.064	0.027	N/A	0.060
Observations	33,010	24,995	23,080	2,358	2,571	1,039	154,105

NOTES—Estimates are obtained using data from the 2019–2022 Canadian Labour Force Survey and two-way fixed effects (TWFE) difference-in-differences regressions (using ordinary least squares). Our analysis samples are composed of fathers aged 20–64 with elementary and secondary school-aged children who had not moved during the analysis sample period. The placebo group contains partnered men aged 20–64 who are not cohabiting with children. Column headings for columns (1)–(3) and (4)–(7) denote partnership of men and ages of the resident children in the household. Column (7) shows results for childless men. All regression models control for year-by-month fixed effects and individual fixed effects, local foot traffic in non-essential businesses, and local COVID-19 cases in the previous month. City-clustered standard errors are in parentheses and regressions are weighted using survey-provided sample weights. *, **, and *** denote coefficients that statistically differ from zero with p-value $\leq$ 0.10, 0.05, and 0.01, respectively.

datasets into a single dataset and estimate the following event-study specification:

$$Y_{icte} = \gamma_{ie} + \theta_{te} + \sum_{s \neq -1} \beta_s \times \mathbb{1}[s_{te} = s] \times D_{ce} + \Gamma X_{cte} + \epsilon_{icte} \tag{2}$$

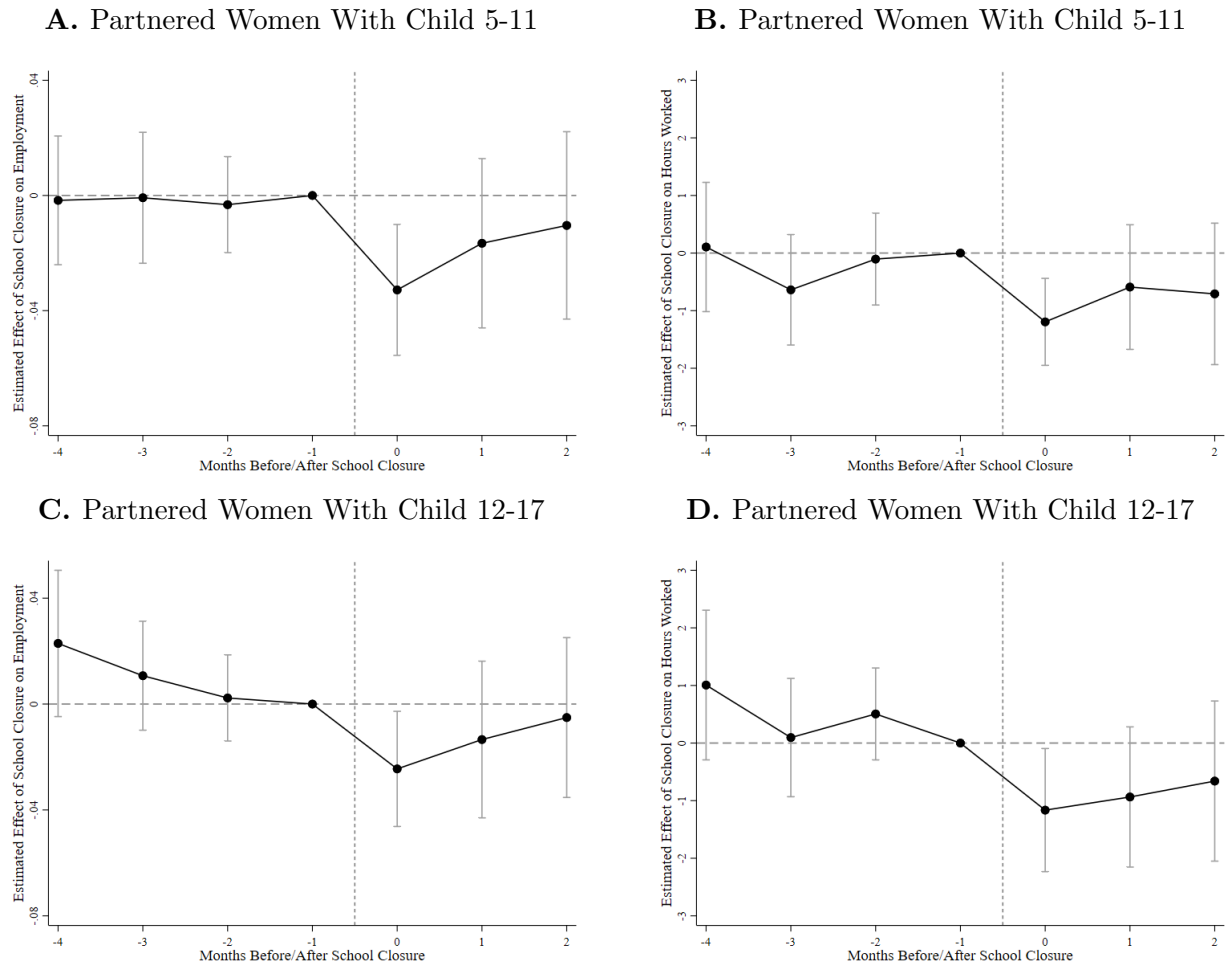
where Y_{icte} denotes a labor supply outcome of individual i who lives in city c in period t and cohort e. γ_{ie} is cohort-by-individual fixed effects, and θ_{te} is cohort-by-year-month fixed effects. s_{te} denotes event time, defined as calendar month t relative to the first COVID-19–related school closure in cohort e, with the reference period, $s = -1$, as the omitted category. D_{ce} is an indicator equal to one if city c was treated in cohort e. We cluster standard errors at the city level and weight regressions using the corrective stacked DiD sample weights of Wing et al. (2024).

The coefficient β_s captures how parents dynamically adjust labor supply when schools close. To assess the parallel trend assumption, we test whether the pre-closure coefficients $(\beta_s | s < -1)$ individually and jointly differ from zero. Post-closure coefficients $(\beta_s | s \geq 0)$ capture how labor supply changes in the period when schools close and thereafter. If our identification assumption is valid, we expect β_s to be zero when $s < 0$, indicating no differential pre-trends.

Figure 2 plots the stacked event-study estimates for two outcomes for partnered mothers with school-aged children. The pre-closure coefficients are both individually and jointly statistically indistinguishable from zero in the periods before schools closed, and we cannot reject the null hypothesis of no pre-closure trends. However, note that in the event study that examines mothers of secondary school-aged children, the coefficients on estimated leads decline in the period four months prior to two months prior to a school closure before stabilizing in the immediate period prior to a closure. When schools close (period 0), partnered mothers with children aged 5–11 are approximately 4 percentage points less likely to be employed and at work, and they work one fewer hour each week. The post-treatment pattern indicates that maternal labor supply declines immediately when schools close and quickly bounces back to the pre-closure levels in the following months. The event study results are consistent with our TWFE difference-in-differences results, suggesting that bias in TWFE

estimates from staggered adoption is not driving our main findings.[23,24]

Figure 2: Event-Study Analysis of COVID-19 School Closures and Labor Supply of Parents of School-Aged Children



Notes— Estimates are obtained using data from the 2019-2022 Canadian Labour Force Survey and stacked difference-in-differences event-study regressions. For details on the stacked DiD specification, see Table A9. The outcome for Panels A and C is employed, at work; the outcome for Panels B and D is unconditional hours worked. The event-study regressions here replace the single treatment indicator with mutually exclusive leads and lags of school closures with period (s=-1) omitted as the reference period. All regressions control for cohort-by-year-by-month fixed effects and cohort-by-individual fixed effects, local foot traffic in non-essential businesses, and local COVID-19 cases in the previous month. Standard errors are clustered at the city level and regressions are weighted using the corrective stacked DiD sample weights of Wing et al. (2024).

[23]Appendix Figures A2 and A3 plot the event-study estimates for other employment outcomes analyzed in Table 1 for partnered mothers and childless women.

[24]Table A9 shows overall stacked DD estimates with a pattern of findings similar to those obtained using TWFE estimates in Table 1 and Table 2. Although some parents may experience more than one school closure within the six-month LFS panel, the consistency of results across estimators suggests that the bias from staggered adoption is not driving our main findings.

5.3 Sensitivity Analyses

Appendix Table A10 shows that our baseline estimates are robust to progressively richer sets of control variables. All specifications include individual and year-by-month fixed effects. We then add: (1) local foot traffic in non-essential businesses, (2) the number of local COVID-19 cases in the previous month at the health-region level, and (3) the local unemployment rate at the city level, calculated excluding workers in K–12 education (NAICS 6111). Across these specifications, the estimated effects of school closures on parents' employment, hours, and full-time work remain stable and consistent with our main results in magnitude and significance. We also re-estimate our main specification excluding parents aged 55-64. The results in column (2) of Table A11 are similar to our baseline estimates, indicating that the inclusion of older parents does not drive our findings.[25]

Finally, we estimate a robustness specification that restricts the sample to the pandemic period by excluding all pre-COVID-19 observations and any late-2022 months when some provinces stopped reporting COVID-19 cases. The estimates are reported in Table A11. The estimated effects of school closures on parents' labor supply outcomes remain essentially unchanged in this restricted sample.

5.4 Heterogeneity

We next investigate heterogeneity in labor supply responses to school closures among partnered mothers of school-aged children by estimating Equation 1 separately across subgroups defined by their pre-school-closure job characteristics.[26] We note that because pre-treatment job characteristics could proxy for other unobserved traits of mothers, this heterogeneity analysis should be treated as descriptive in nature. Figure 3 reports the estimated effects of school closures on four labor supply outcomes for partnered mothers with only children aged 5–11. The baseline estimates from Table 1 are shown with triangles for reference. Each

[25] We also estimate our main specification on parents with only children under age 5 and find that K–12 school closures did not have statistically significant effects on their labor supply outcomes.

[26] See Appendix Section A.4 and Appendix Table A3 for definitions and details of job characteristics, skill requirements, and demographics.

panel corresponds to a different outcome and plots subgroup-specific estimates with 95% confidence intervals, vertically ordered by the magnitude of the point estimate.

Across panels, mothers in more flexible or higher-skill positions—those who work in the public sector or unionized jobs, or have at least a bachelor's degree—exhibit larger reductions in hours worked and full-time employment, but are relatively less likely to leave the labor force. These patterns indicate that better-educated mothers and those in flexible jobs primarily respond to school closures by adjusting their work schedules rather than leaving employment. In contrast, even when mothers are the main earners in their households, the estimated effects on all labor supply outcomes remain sizable, suggesting that higher-earning mothers continue to shoulder a substantial childcare burden. A similar pattern for single mothers appears in Appendix Figure A4, where the effects on labor supply are modest in magnitude but underscore important disparities in their capacity to accommodate sudden increases in caregiving demands.

5.5 Annual Earnings and COVID-19 Benefits - LAD Sample

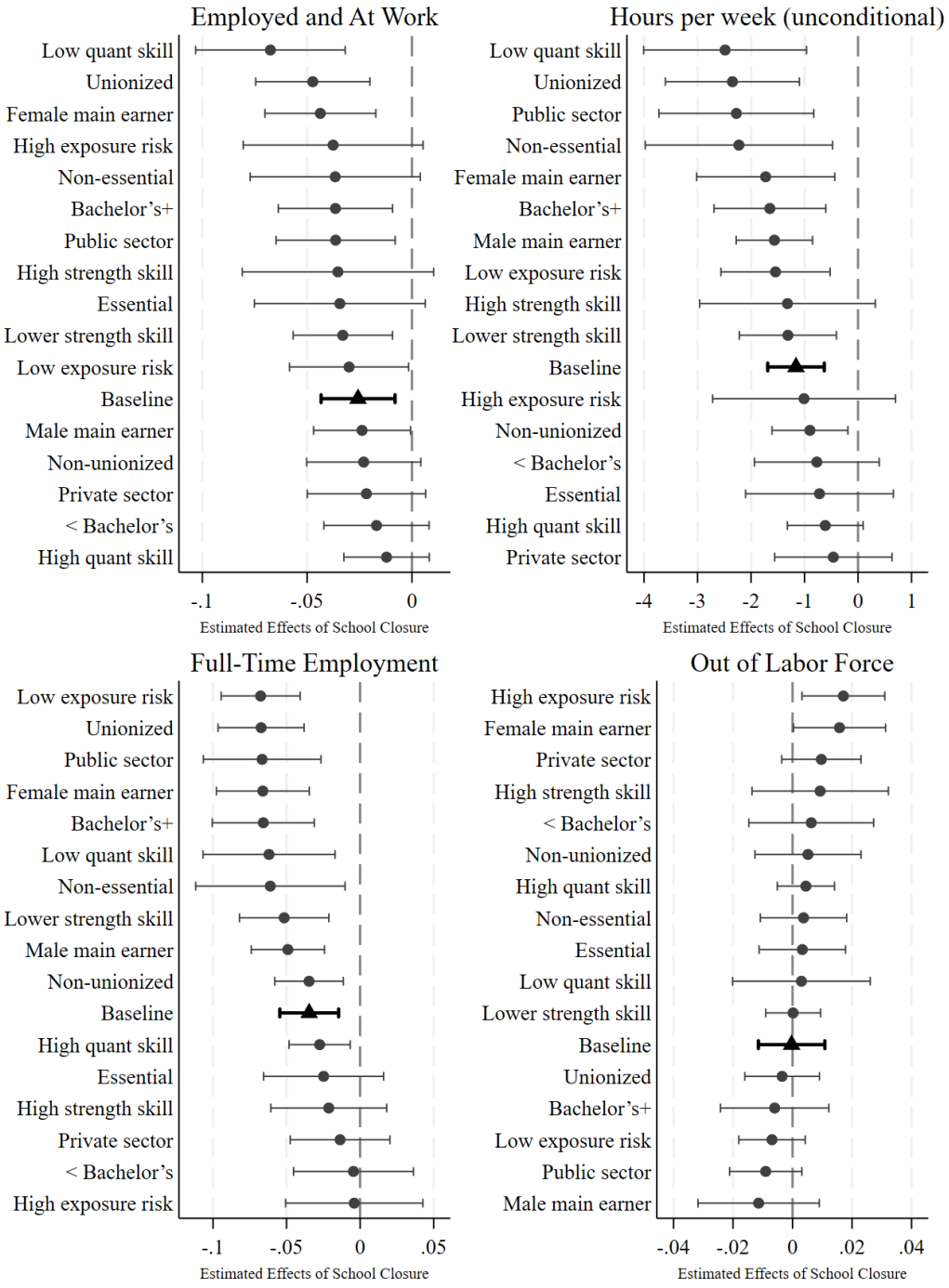
We next turn to auxiliary analyses using the LAD. One key limitation of the LAD is that its annual structure does not allow us to fully align school closures with the outcomes of interest, which matters when other shocks during the same year may also affect these outcomes. While this limitation cannot be resolved within the annual data, we address city-level shocks correlated with school closures by presenting a triple-differences specification using childless individuals as an additional comparison group to account for city-level shocks that affect annual outcomes, such as broader COVID-related disruptions to local labor markets.[27] In an additional specification, we include city-by-year fixed effects to control for common shocks to parents of school-aged children and childless adults.[28]

Table 3 presents the estimated effect of school closures (measured in units of 20 days) on an indicator for having positive earnings (extensive-margin, > 0), log positive earnings,

[27]Triple-differences models have been a standard approach used in the literature to study the labor market effects of the pandemic (Heggeness and Suri, 2021; Hansen et al., 2024b).

[28]In Appendix Table A12, we present results including city $\times$ year fixed effects, so identification relies only on within–city-year differences between parents and childless individuals. The estimated triple-difference coefficients are nearly identical to those from the baseline specification, reinforcing our main findings.

Figure 3: Labor Supply Response Heterogeneity, by Outcome and Pre-School-Closure Job Characteristics, Partnered Mothers with Child Aged 5–11



Notes—Estimates are obtained using data from the 2019–2022 Canadian Labour Force Survey and two-way fixed effects (TWFE) difference-in-differences regressions (using ordinary least squares). For details on the TWFE DiD specification, see Table 1. Each panel plots the coefficient and 95% confidence interval of the estimated effect of school closures for the main sample (labeled triangle) from Table 1 and for the sub-sample with the listed pre-school-closure job characteristics (defined in Appendix Section A.4). Estimates are ordered from top to bottom by the magnitude of coefficients. Outcomes are shown above each panel. Standard errors are clustered at the city level. See sample means and proportions in Appendix Table A4.

and an indicator for receiving labor-market–related government benefits (extensive margin), separately by gender, partnership, and age of children.[29] In the top row of Panel A in Table 3, we find that an additional 20 days of school closures leads to a small 0.17-0.18 percentage point decline in the probability of having any earnings for partnered mothers, with effects close to zero for partnered fathers.[30] Among single parents (Panel B), the effects are larger in absolute magnitude but remain small in economic terms. For single fathers, the estimates are statistically significant for those with younger children and those with children in both age groups.

In the middle row of Panels A and B, we examine log earnings effects of school closures. We find only modest effects on earnings. For instance, for every 20 days of school closures, the annual earnings of partnered mothers with younger children aged 5–11 fall by about 1.2 percent. The estimated effects are similar for partnered mothers with older school-aged children, while they are much smaller for partnered fathers. For single parents, the effects are larger and are also statistically significant for single fathers. These patterns suggest that school closures led to modest reductions in parents' annual earnings, with the effects concentrated among single parents and partnered mothers with younger children.

Finally, in the bottom row of Panels A and B, we report estimates for receipt of annual labor-market-related public benefits. Our DDD estimates show that partnered mothers with younger children aged 5–11 (with or without older siblings) are about 0.6 percentage points more likely to receive employment insurance or COVID-19 benefits for each additional 20 days of school closures. Partnered fathers exhibit small but precisely estimated declines in benefit receipt. Consistent with results for other LAD outcomes, single parents are more likely to receive benefits as the number of school closure days increases, with effects of about

[29] In Appendix Table A13, we present estimates using Poisson pseudo–maximum likelihood (PPML) models for earnings including zeros to address the scale-dependence and percentage-interpretation problems associated with "log-like" transformations highlighted by Chen and Roth (2023). As they emphasize, PPML coefficients reflect an average change in earnings scaled by the mean of the outcome, rather than a true average percentage effect, particularly when treatment effects are heterogeneous. We also present PPML estimates of labor-market–related government benefits including zeros.

[30] Effects on the extensive margin of annual earnings are mechanically limited because school closures often occur after individuals have already accumulated earnings earlier in the year, so many individuals will still record positive annual earnings even if they stop working afterward. This timing may also attenuate estimated effects on annual earnings more generally.

2–3 percentage points per additional 20 days of closures.[31] One explanation for why single parents had larger benefit receipt responses to school closures may be the higher baseline rates of receipt. In percentage terms, benefit receipt differences were not as stark between partnered and single parents.[32]

6 Conclusion

This paper examines whether and how Canadian parents adjusted their labor supply when in-person schooling was disrupted during the COVID-19 pandemic. We exploit granular city-level data on primary and secondary school closures across 141 Canadian cities and combine these data with two complementary, nationally representative longitudinal datasets to provide new difference-in-differences estimates of the effect of school closures on parental labor supply.

Consistent with prior U.S. studies, we find evidence that COVID-19 school closures in Canada led to a gendered employment response among partnered parents of school-aged children. Specifically, school closures led to a 2–3 percentage point decline in employment among partnered mothers but had no effect on the labor supply of partnered fathers. This finding is consistent with (1) the hypothesis that mothers substitute childcare at home for labor market time in response to in-person schooling disruptions, and (2) evidence that mothers disproportionately care for children. However, for single fathers, we also find evidence that school closures reduce labor supply on the intensive margin, suggesting that the presence of a partner plays an important role in labor supply adjustment. A causal interpretation of our estimates is supported by falsification tests on partnered childless adults as

[31] These patterns should be interpreted with some caution given the sensitivity of benefit receipt to specification. While the baseline estimates suggest larger increases in the likelihood of receiving benefits for single parents, Appendix Table A13 indicates that these patterns are not uniform across specifications. One explanation is that single parents have higher baseline rates of benefit receipt, as shown in Tables A7 (partnered) and A8 (single), leaving less scope for increases along the extensive margin, whereas partnered parents may be more likely to enter receipt when school closures increase. In contrast, the PPML estimates incorporate both the likelihood of receipt and the amount received, capturing adjustments along the intensive margin as well. Overall, the results point to modest changes in benefit receipt, with some evidence of increased reliance on income-support programs, though the distribution of these effects across household types depends on whether one focuses on the extensive or combined margins.

[32] Descriptive statistics on benefit receipt and amounts received are reported in Tables A7 and A8.

Table 3: DDD Estimates of the Effects of Days of COVID-19 School Closures on Annual Income and Benefits Received, by Age of Children

	Females			Males		
Outcomes	Child Ages 5–11 only (1)	Child Ages 12–17 only (2)	Child Both 5–11 and 12–17 (3)	Child Ages 5–11 only (4)	Child Ages 12–17 only (5)	Child Both 5–11 and 12–17 (6)
Panel A: Partnered						
Had Positive Earnings	-0.0017***	-0.0017***	-0.0018***	-0.0005**	-0.0005	-0.0002
	(0.0005)	(0.0004)	(0.0004)	(0.0002)	(0.0004)	(0.0003)
Observations	807,700	761,000	735,700	877,300	822,200	800,000
Log Positive Earnings	-0.0116***	-0.0016	-0.0090***	-0.0020*	-0.0026**	-0.0022*
	(0.0024)	(0.0024)	(0.0020)	(0.0011)	(0.0011)	(0.0011)
Observations	675,400	636,400	610,100	792,000	736,900	717,300
Received Benefits/EI	0.0055***	-0.0006	0.0056***	-0.0022**	-0.0022***	-0.0004
	(0.0013)	(0.0008)	(0.0011)	(0.0009)	(0.0007)	(0.0007)
Observations	807,700	761,000	735,700	877,300	822,200	800,000
Panel B: Single						
Had Positive Earnings	-0.0069***	-0.0037***	-0.0053***	-0.0071***	-0.0022	-0.0054***
	(0.0006)	(0.0006)	(0.0009)	(0.0015)	(0.0016)	(0.0017)
Observations	623,400	632,400	588,100	606,500	608,600	600,000
Log Positive Earnings	-0.0293***	-0.0159***	-0.0277***	-0.0237***	-0.0100***	-0.0092
	(0.0038)	(0.0030)	(0.0028)	(0.0038)	(0.0035)	(0.0072)
Observations	509,900	517,100	483,200	535,400	537,200	530,400
Received Benefits/EI	0.0270***	0.0119***	0.0302***	0.0302***	0.0174***	0.0207***
	(0.0034)	(0.0021)	(0.0033)	(0.0045)	(0.0026)	(0.0043)
Observations	623,400	632,400	588,100	606,500	608,600	600,000

Notes— Estimates are obtained using data from the 2019–2022 Canadian Longitudinal Administrative Databank and triple-differences regressions (using ordinary least squares). Our analysis samples are composed of parents aged 20–64 with school-aged children and childless adults who had not moved during the analysis sample period. The estimated effects compare changes in outcomes for parents of school-aged children relative to those of childless adults per 20 additional school-closure days. Column headings for columns (1)–(3) and (4)–(6) denote the gender of parents and the ages of the resident children in the household. All regression models control for individual and calendar-year fixed effects. City-clustered standard errors in parentheses. *, **, and *** denote coefficients that statistically differ from zero with p-value $\leq$ 0.10, 0.05, and 0.01, respectively.

well as event-study analysis using stacked DD estimates, which ameliorate bias in TWFE estimates caused by heterogeneous treatment effects over time. Finally, we find evidence that COVID-19 school closures increased take-up of employment-related public benefits, with larger estimated treatment effects for single parents.

Together, the findings from this study are important for our understanding of the full costs of pandemic-era school closures and, more broadly, the role of in-person schooling in parental time spent at home versus the labor market. Schools provide instruction, but they also provide a predictable childcare infrastructure that supports parental employment, especially for partnered mothers and single fathers. When in-person schooling was disrupted, the labor-market consequences were borne disproportionately by mothers and single parents, groups with fewer margins of adjustment. The results suggest that school closures affected not only children's learning and human-capital development, but also parents' ability to maintain work and earnings.

Several limitations qualify the interpretation of these findings. First, our school-closure measure cannot identify closures that affected only a subset of schools when no city-wide school-closure policies were in place. This measurement error likely attenuates the estimates toward zero. Second, the annual tax data cannot align earnings and benefit receipt precisely with the timing of school closures. Finally, future research that jointly models both parents' labor supply within households would help clarify how families allocate paid work and caregiving when in-person schooling becomes unavailable.

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A Online Appendix

A.1 COVID-19 Income Support Programs

We use tax file data on individual COVID-19 benefits paid by six federal and one provincial program (see Section 3.2). The COVID-19 benefits from these programs require labor market participation. The federal programs included Employment Insurance, Canada Emergency Student Benefit, Canada Recovery Benefit, Canada Recovery Caregiving Benefit, Canada Recovery Sickness Benefit, and Canada Worker Lockdown Benefit. The provincial program is the British Columbia Emergency Benefit for Workers. We exclude programs that paid COVID-19 benefits but do not require labor-market participation. These programs included the Canada Child Benefit, Guaranteed Income Supplement (and GIS Allowance), Old Age Security, Goods and Services Tax credit, Climate Action Tax Credit, and disability-related cash benefits. Appendix Table A1 lists program specific information.

Table A1: COVID-19 Income Support Programs

Program	Description	Used in analysis
Panel A: Existing Programs available in the LAD		
Employment Insurance (EI)	Canada's primary pre-existing income-support program for unemployed workers; eligibility rules and benefit generosity were temporarily modified during the COVID-19 pandemic. **Dates:** *Pre-existing; COVID-related temporary EI sickness measures began March 15, 2020, with broader temporary access and benefit changes beginning September 27, 2020.*	✓
Panel B: Programs available in the LAD		
Canada Emergency Response Benefit (CERB)	Temporary income replacement for workers who stopped working or experienced income loss due to COVID-19. **Dates:** *March 15, 2020 – September 26, 2020.*	✓
Canada Emergency Student Benefit (CESB)	Income support for post-secondary students and recent graduates unable to find employment due to COVID-19. **Dates:** *May 10, 2020 – August 29, 2020.*	✓
Canada Recovery Benefit (CRB)	Income support for workers not eligible for Employment Insurance following the end of CERB. **Dates:** *September 27, 2020 – October 23, 2021.*	✓

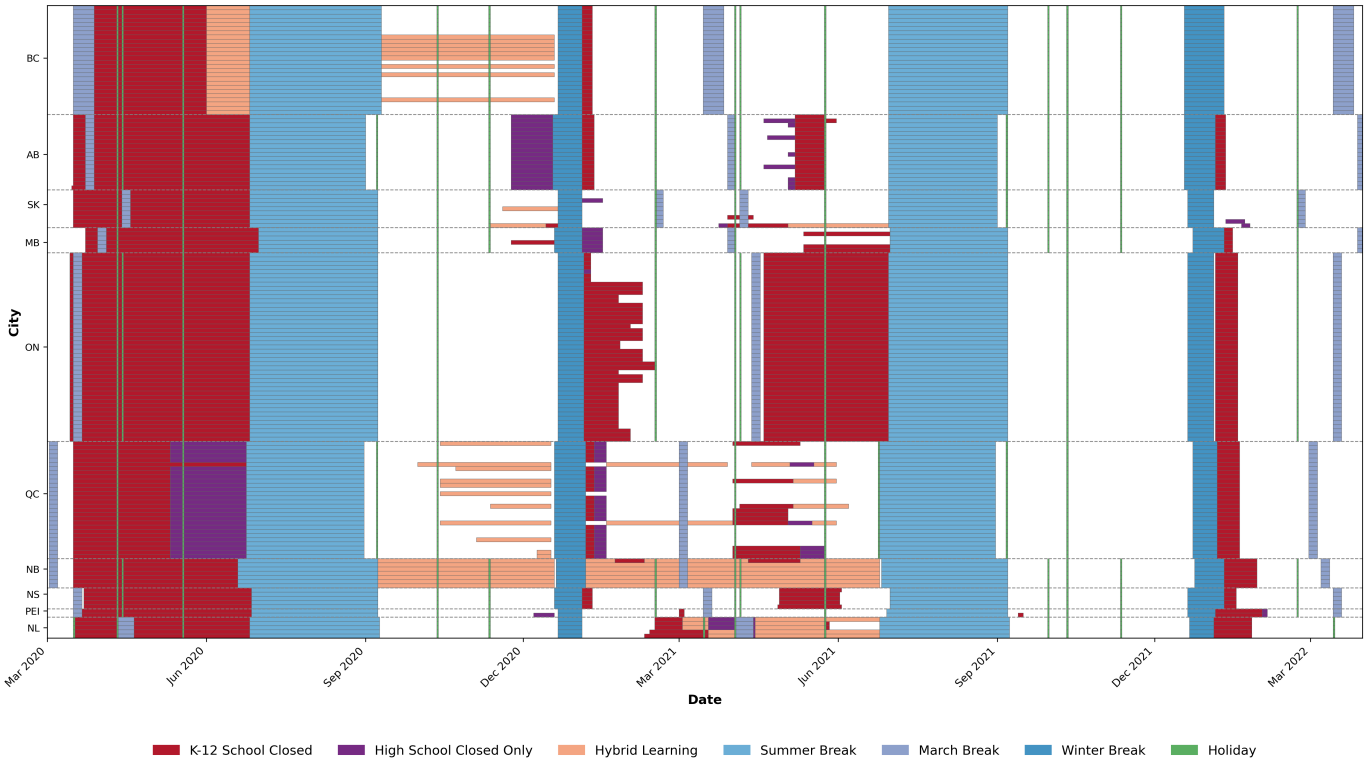
Canada Recovery Sickness Benefit (CRSB)	Income support for workers unable to work because they were sick or required to self-isolate due to COVID-19. **Dates:** *September 27, 2020 – May 7, 2022.*	✓
Canada Recovery Caregiving Benefit (CRCB)	Income support for workers unable to work because of caregiving responsibilities arising from school, daycare, or care-facility closures. **Dates:** *September 27, 2020 – May 7, 2022.*	✓
Canada Worker Lockdown Benefit (CWLB)	Income support for workers in designated regions subject to government-mandated lockdowns. **Dates:** *October 24, 2021 – May 7, 2022.*	
GST/HST Credit COVID-19 top-up	One-time additional GST/HST credit payment provided during the COVID-19 pandemic. **Dates:** *2020.*	
Guaranteed Income Supplement (GIS) / Allowance COVID-19 top-up	One-time supplement to GIS and Allowance recipients during the COVID-19 pandemic. **Dates:** *2020.*	
Old Age Security (OAS) COVID-19 supplement	COVID-related one-time payments to Old Age Security recipients during the COVID-19 pandemic. **Dates:** *2020–2021.*	
Canada Child Benefit (CCB) COVID-19 top-up	Temporary additional payments to Canada Child Benefit recipients during the COVID-19 pandemic. **Dates:** *2020–2021.*	
Disability-related COVID-19 supplement	payment to recipients of federal disability-related benefits during the COVID-19 pandemic. **Dates:** *2020–2021.*	
Provincial and territorial COVID-19 emergency payments	One-time or recurring income-support payments provided by provincial and territorial governments in response to COVID-19. **Dates:** *2020–2022 (varies by province).*	
Panel C: Some programs not available in the LAD		
Canada Emergency Wage Subsidy (CEWS)	Employer wage subsidy paid directly to eligible employers to support employee retention. **Dates:** *March 2020 – October 2021.*	
Canada Emergency Rent Subsidy (CERS)	Subsidy covering commercial rent and property expenses for eligible businesses during COVID-19. **Dates:** *September 2020 – October 2021.*	
Lockdown Support (CERS top-up)	Supplementary rent subsidy for eligible businesses subject to government-mandated lockdowns. **Dates:** *September 2020 – October 2021.*	
Local Lockdown Program	Wage and rent support for businesses subject to qualifying local public-health restrictions. **Dates:** *October 2021 – May 2022.*	
Canada Emergency Business Account (CEBA)	Partially forgivable loans provided to small businesses during COVID-19. **Dates:** *April 2020 – June 2021 (applications).*	
Highly Affected Sectors Credit Availability Program	Loan guarantee program supporting businesses in severely affected sectors. **Dates:** *February 2021– March 2022.*	

Provincial business grants and sector-specific relief programs	Business-focused COVID-19 grants and relief measures delivered by provincial governments. **Dates:** *2020–2022 (varies by province).*

A.2 Detailed City-level school closures

Appendix Figure A1 plots, for 141 cities, whether, when, and how schools were closed on each day of the pandemic. To read the figure, note that cities in western provinces appear at the top and cities in eastern provinces appear at the bottom of the figure. When governments closed schools to mitigate the spread of COVID-19 infections, we shade days in red.

Figure A1: City-level school closures, March 1, 2020 to May 1, 2022



Note.—School closure policies were collected by authors. Cities are grouped by province on the vertical axis and ordered from top to bottom within each province bin. We categorize schools as closed if they are physically closed or teach remotely. Province codes from top to bottom: BC-British Columbia, AB-Alberta, SK-Saskatchewan, MB-Manitoba, ON-Ontario, QC-Quebec, NB-New Brunswick, NS-Nova Scotia, PEI-Prince Edward Island, NL-Newfoundland and Labrador.

For each city, we code primary and secondary schools as being closed if at least one grade served was closed. Note that we assign the closure status to all schools in the city even

though it was possible that an individual school might have closed when most others were open. Evidence from Ontario—the largest school system in Canada—suggests that, in later COVID-19 waves, individual school closures were relatively rare. During the 2020-21 school year, fewer than 1% of Ontario schools were closed even when more than 20% of schools reported active COVID-19 cases.[33]

A.3 StatCan Provincial COVID-19 Restrictions Index

In Appendix Table A2 we list the correlation between our city school closures and provincial-level regulations measured by the Statistics Canada COVID-19 indices. The possibility of lower correlation between our city school closure measure and the Statistics Canada COVID-19 Restrictions Index arises mostly because the StatCan index takes on positive values when provinces impose COVID-19 mask mandates and distancing requirements, and restrict university learning modes.

In addition, the StatCan index uses the policy of the strictest school in the province even when most provincial schools were open. This coding strategy means that the StatCan school closure index was positive in Fall 2020 when all K–12 schools were teaching in-person but required students to wear masks and maintain social distancing and universities taught asynchronous classes. More generally, the StatCan coding rules could mechanically increase the index in periods when K–12 schools taught in-person classes.

A.4 Job-Characteristic Subgroups Used in Heterogeneity Analysis

We classify parents into subgroups based on pre-school-closure job characteristics, skill requirements, and household demographics. These subgroup indicators are used in the heterogeneity analysis reported in Figure 3. Job-characteristic measures draw on external occupation-level indices of teleworkability, exposure risk, and task content, as well as

[33] "Schools COVID-19 data", Ontario Ministry of Education, https://data.ontario.ca/dataset/summary-of-cases-in-schools.

Table A2: Pairwise Correlation Between City School Closure Measure and Provincial COVID-19 Restrictions Indices, by and across Pandemic Waves

	Pandemic Wave			
	(1)	(2)	(3)	(4)
Index	Mar–Aug 2020	Sep 2020–Feb 2021	April–Nov 2021	Dec 2021–May 2022
School closures	0.851	0.326	0.849	0.833
		(-0.027)	(-0.002)	(-0.018)
Workplace closures	0.770	0.156	0.643	0.329
		(-0.614)	(-0.127)	(-0.441)
Stay-at-home requirements	0.531	0.185	0.360	0.225
		(-0.346)	(-0.171)	(-0.306)
Restaurant restrictions	0.700	0.148	0.583	0.524
		(-0.552)	(-0.117)	(-0.176)
Non-essential retail closures	0.505	0.169	0.390	0.312
		(-0.336)	(-0.115)	(-0.193)
Observations	1,820	1,820	1,820	1,820

Notes— Pairwise correlations are calculated with the school closure measures collected by authors and the provincial COVID-19 index over four pandemic waves. Values in parentheses indicate changes in correlation in each subsequent wave relative to Wave 1. Provincial COVID-19 index data from Statistics Canada COVID-19 Restrictions Index (https://www150.statcan.gc.ca/n1/pub/36-28-0001/2022008/article/00002-eng.htm). To match our treatment variable as closely as possible, we compute the school closure index as a simple average of the physical school closure and remote learning mode indices for each day. It excludes the provincial hybrid learning index.

self-reported information from the Canadian Labour Force Survey.[34] Appendix Table A3 summarizes the definition of each subgroup used in the heterogeneity figures.

Table A3: Definition of Job-Characteristic Subgroups

Figure label	Description	Source / Construction
Panel A: Job characteristics		
Baseline	Main estimation sample of partnered mothers with children aged 5–11 used in the baseline TWFE DiD specification.	Main LFS analytic sample
Remote	Occupation classified as feasible to be performed entirely from home using Dingel and Neiman (2020)'s work-from-home index.	Dingel and Neiman (2020) index
Non-remote	Occupation classified as not feasible to be performed entirely from home using Dingel and Neiman (2020)'s work-from-home index.	Dingel and Neiman (2020) index
Public sector	Class of worker in main (or last) job is "Public employee" in the LFS class-of-worker question (code = 1), among those employed or who worked in the last 12 months.	Canadian Labour Force Survey
Private sector	Class of worker in main (or last) job is a private employee or self-employed (LFS class-of-worker codes = 2–7), among those employed or who worked in the last 12 months.	Canadian Labour Force Survey
Unionized	Currently employed employees who are union members or non-members covered by a collective agreement (union_ca = 1 or 2).	Canadian Labour Force Survey
Non-unionized	Currently employed employees who are not union members and not covered by a collective agreement (union_ca = 3).	Canadian Labour Force Survey
Essential	Occupation classified as essential according to essential-service lists issued by provincial and territorial governments at the beginning of the COVID-19 pandemic.	Prov./terr. essential lists

[34]Remote-workable occupations are defined using the O*NET-based work-from-home measure from Dingel and Neiman (2020). Exposure-risk measures draw on Chernoff and Warman (2023), while task-content indices follow Warman and Worswick (2015) and Imai et al. (2019). Public/private sector and union status are constructed from the LFS survey questions.

Non-essential	Occupation not included on provincial or territorial essential-service lists at the beginning of the COVID-19 pandemic.	Prov./terr. essential lists
Panel B: Skill requirements and exposure risk		
High strength skill	Occupation with above-median non-routine manual/physical-strength index constructed from O*NET task items (e.g., physical effort, handling objects).	Imai et al. (2019) index
Lower strength skill	Occupation with below-median non-routine manual/physical-strength index constructed from O*NET task items.	Imai et al. (2019) index
High quant skill	Occupation with above-median quantitative or analytical task intensity based on O*NET items (e.g., mathematics, data analysis, information processing).	Imai et al. (2019) index
Low quant skill	Occupation with below-median quantitative or analytical task intensity based on O*NET items.	Imai et al. (2019) index
High exposure risk	Occupation with above-median values of the viral transmission risk index, which averages O*NET measures of physical proximity, face-to-face discussions, exposure to diseases or infections, and indoor work.	Chernoff and Warman (2023) index
Low exposure risk	Occupation with below-median values of the viral transmission risk index based on O*NET measures of physical proximity, face-to-face discussions, exposure to diseases or infections, and indoor work.	Chernoff and Warman (2023) index
Panel C: Demographics and relative earnings		
Bachelor's+	Highest completed education is a bachelor's degree or higher.	Canadian Labour Force Survey
< Bachelor's	Highest completed education is less than a bachelor's degree.	Canadian Labour Force Survey
Female main earner	Partnered mother earned more than her spouse or partner in the reference period before school closures, based on pre-pandemic individual weekly earnings.	Canadian Labour Force Survey

Male main earner	Partnered mother whose spouse or partner earned more than the mother in the reference period before school closures, based on pre-pandemic weekly earnings.	Canadian Labour Force Survey

B Summary Statistics

The LAD summary statistics for partnered parents, reported in Table A7, show relatively stable annual earnings before and during the pandemic, with no clear evidence of large or persistent earnings declines across child-age groups. In contrast, labor-market-related benefit receipt increases sharply in 2020 for both mothers and fathers, with elevated receipt and amounts persisting into 2021 and partially receding by 2022. These patterns suggest that the primary adjustment to pandemic-era disruptions among partnered parents occurred through increased reliance on income support programs rather than sizable reductions in annual earnings.

Appendix Table A8 reports LAD summary statistics for single parents. Annual earnings for single mothers and fathers decline modestly in 2020 and recover unevenly thereafter, particularly among those with younger children. In contrast, labor-market-related benefit receipt rises sharply in 2020 across all child-age groups, with elevated receipt and amounts persisting into 2021 and remaining above pre-pandemic levels in 2022. Relative to partnered parents, these patterns suggest greater exposure of single parents to pandemic-era labor market disruptions and heavier reliance on income support programs.

Table A4: Pre- (2019) and Post-Pandemic Years (2020–2022) Summary Statistics for Partnered Mothers - Canadian Labour Force Survey (LFS)

	Partnered Mothers					
	Child Ages 5–11 only		Child Ages 12–17 only		Child Ages Both 5–11 and 12–17	
	Pre	Post	Pre	Post	Pre	Post
Labor Market Outcomes						
Employed, at work	0.768	0.744	0.814	0.775	0.771	0.734
Hours per week (unconditional)	26.0	25.8	28.9	27.3	26.1	25.0
Full-Time Employment	0.556	0.561	0.619	0.594	0.551	0.529
Out of Labor Force	0.153	0.144	0.113	0.124	0.159	0.161
Unemployed	0.038	0.055	0.031	0.041	0.032	0.051
Demographics						
Age	39.3	39.8	46.0	46.2	41.8	42.0
Below High School	0.025	0.026	0.036	0.026	0.030	0.026
High School Graduate	0.103	0.096	0.138	0.121	0.136	0.110
Some Post-Secondary	0.032	0.029	0.027	0.028	0.035	0.033
Post-Secondary Diploma	0.356	0.322	0.378	0.369	0.341	0.369
Bachelor's Degree	0.330	0.333	0.271	0.314	0.303	0.308
Above Bachelor's Degree	0.153	0.193	0.150	0.142	0.155	0.153
Public Sector	0.254	0.274	0.236	0.259	0.285	0.254
Unionized	0.358	0.350	0.287	0.346	0.313	0.334
Number of Children	1.675	1.666	1.544	1.544	2.509	2.470
Dual-earner Family	0.572	0.556	0.556	0.550	0.535	0.528
Primary Earner	0.294	0.300	0.326	0.314	0.303	0.284
Weighted N	3,748,100	7,936,900	2,547,200	5,644,400	2,493,400	5,431,000

NOTES— Summary Statistics are calculated using data from the 2019–2022 Canadian Labour Force Survey for partnered mothers aged 20–64 with elementary and secondary-school-aged children in the household who did not move during the analysis period. Column headings denote the ages of resident children in the household and pre-pandemic period (from January 2019 to February 2020) and post-pandemic period (from March 2020 to December 2022). Groups defined in text. All statistics are weighted using survey-provided sample weights.

Table A5: Pre- (2019) and Post-Pandemic Years (2020–2022) Summary Statistics for Partnered Fathers - Canadian Labour Force Survey (LFS)

	Partnered Fathers					
	Child Ages 5–11 only		Child Ages 12–17 only		Child Ages Both 5–11 and 12–17	
	Pre	Post	Pre	Post	Pre	Post
Labor Market Outcomes						
Employed, at work	0.899	0.853	0.857	0.847	0.883	0.864
Hours per week (unconditional)	36.5	34.4	35.3	34.0	36.6	35.1
Full-Time Employment	0.811	0.770	0.778	0.758	0.802	0.781
Out of Labor Force	0.035	0.047	0.062	0.066	0.052	0.046
Unemployed	0.033	0.046	0.036	0.035	0.024	0.035
Demographics						
Age	41.7	42.3	48.3	48.6	44.6	44.5
Below High School	0.046	0.040	0.056	0.041	0.044	0.049
High School Graduate	0.143	0.130	0.163	0.161	0.165	0.151
Some Post-Secondary	0.031	0.029	0.044	0.033	0.035	0.027
Post-Secondary Diploma	0.383	0.375	0.376	0.367	0.353	0.357
Bachelor's Degree	0.241	0.264	0.231	0.247	0.246	0.257
Above Bachelor's Degree	0.157	0.163	0.130	0.151	0.157	0.158
Public Sector	0.150	0.175	0.157	0.161	0.178	0.173
Unionized	0.306	0.305	0.281	0.296	0.344	0.339
Number of Children	1.675	1.666	1.544	1.544	2.509	2.470
Dual-earner Family	0.572	0.556	0.556	0.550	0.535	0.528
Primary Earner	0.593	0.586	0.533	0.555	0.553	0.593
Weighted N	3,748,100	7,936,900	2,547,200	5,644,400	2,493,400	5,431,000

NOTES— Summary Statistics are calculated using data from the 2019–2022 Canadian Labour Force Survey for partnered fathers aged 20–64 with elementary and secondary-school-aged children in the household who did not move during the analysis period. Column headings denote the ages of resident children in the household and pre-pandemic period (from January 2019 to February 2020) and post-pandemic period (from March 2020 to December 2022). Groups defined in text. All statistics are weighted using survey-provided sample weights.

Table A6: Pre- (2019) and Post-Pandemic Years (2020–2022) Summary Statistics for Single Parents - Canadian Labour Force Survey(LFS)

	Single Mothers						Single Fathers					
	Child Ages 5–11 only		Child Ages 12–17 only		Child Ages Both 5–11 and 12–17		Child Ages 5–11 only		Child Ages 12–17 only		Child Ages Both 5–11 and 12–17	
	Pre	Post	Pre	Post	Pre	Post	Pre	Post	Pre	Post	Pre	Post
Labor Market Outcomes												
Employed, at work	0.745	0.698	0.771	0.726	0.776	0.656	0.832	0.794	0.820	0.777	0.821	0.852
Hours per week (unconditional)	25.1	23.8	27.2	25.9	25.9	22.8	33.7	30.5	32.8	30.5	31.3	34.0
Full-Time Employement	0.523	0.512	0.581	0.567	0.508	0.494	0.760	0.655	0.716	0.663	0.702	0.756
Out of Labor Force	0.162	0.161	0.127	0.151	0.147	0.204	0.056	0.074	0.086	0.082	0.104	0.049
Unemployed	0.056	0.065	0.053	0.070	0.041	0.071	0.064	0.071	0.027	0.069	*N/A*	*N/A*
Demographics												
Age	37.5	38.1	45.0	45.7	40.7	40.5	41.4	41.8	47.9	48.1	43.5	44.2
Below High School	0.077	0.046	0.074	0.062	0.063	0.070	0.066	0.082	0.071	0.064	0.079	0.029
High School Graduate	0.151	0.157	0.142	0.147	0.159	0.191	0.096	0.194	0.152	0.128	0.109	0.136
Some Post-Secondary	0.062	0.048	0.048	0.044	0.047	0.061	0.072	0.046	0.054	0.076	0.097	0.037
Post-Secondary Diploma	0.456	0.462	0.434	0.430	0.451	0.415	0.421	0.435	0.463	0.454	0.405	0.467
Bachelor's Degree	0.186	0.197	0.226	0.199	0.188	0.188	0.256	0.164	0.178	0.173	0.214	0.189
Above Bachelor's Degree	0.068	0.090	0.076	0.118	0.092	0.075	0.089	0.078	0.082	0.105	0.096	0.142
Public Sector	0.189	0.227	0.210	0.211	0.228	0.232	0.150	0.186	0.161	0.139	0.241	0.180
Unionized	0.317	0.340	0.323	0.346	0.357	0.354	0.276	0.346	0.304	0.308	0.416	0.336
Number of Children	1.357	1.392	1.316	1.334	2.504	2.513	1.456	1.471	1.292	1.326	2.384	2.317
Weighted N	835,800	1,586,800	767,000	1,629,500	411,600	809,500	219,600	451,900	295,100	527,400	115,700	224,100

NOTES— Summary Statistics are calculated using data from the 2019–2022 Canadian Labour Force Survey for single parents aged 20–64 with elementary and secondary-school-aged children in the household who did not move during the analysis period. Column headings denote the ages of resident children in the household and pre-pandemic period (from January 2019 to February 2020) and post-pandemic period (from March 2020 to December 2022). Groups defined in text. All statistics are weighted using survey-provided sample weights.

Table A7: Pre- (2019) and Post-Pandemic Years (2020-2022) Summary Statistics for Partnered Parents on Annual Earnings and Benefits Received - Canadian Longitudinal Administrative Databank (LAD)

	Partnered Women															
	Child Ages 5–11 only				Child Ages 12–17 only				Child Ages Both 5–11 and 12–17				Childless Adults			
Outcomes	2019	2020	2021	2022	2019	2020	2021	2022	2019	2020	2021	2022	2019	2020	2021	2022
Annual earnings																
Had	85.6	85.0	84.4	85.6	86.0	85.5	84.8	86.0	83.3	82.5	82.5	83.8	84.7	83.3	82.1	81.7
Mean (with 0)	56,400	56,600	59,600	60,600	57,000	57,700	60,700	61,500	52,300	52,400	55,100	56,500	47,500	47,500	49,600	49,400
Mean (no 0)	66,000	66,600	70,600	70,800	66,200	67,500	71,700	71,600	62,800	63,600	66,800	67,400	56,100	57,000	60,400	60,500
Annual labor related benefits (COVID-19+EI)																
Received	4.3	32.9	19.0	10.6	5.0	30.8	17.2	9.2	4.6	33.1	18.8	10.4	5.7	31.0	17.4	9.5
Mean (with 0)	250	3,900	2,200	400	290	3,500	2,100	380	270	3,900	2,200	400	350	3,500	2,000	440
Mean (no 0)	5,900	11,800	11,400	3,700	5,900	11,500	11,900	4,100	5,800	11,800	11,600	3,900	6,200	11,100	11,600	4,700
	Partnered Men															
	Child Ages 5–11 only				Child Ages 12–17 only				Child Ages Both 5–11 and 12–17				Childless Adults			
Outcomes	2019	2020	2021	2022	2019	2020	2021	2022	2019	2020	2021	2022	2019	2020	2021	2022
Annual earnings																
Had	94.7	93.8	93.7	94.2	93.5	92.6	91.9	92.5	93.7	93.0	92.6	93.2	90.4	88.7	87.5	87.1
Mean (with 0)	95,300	95,700	103,600	99,600	99,100	97,200	101,800	99,600	98,500	96,600	101,300	100,700	70,700	69,200	71,900	70,800
Mean (no 0)	100,600	102,100	110,600	105,700	105,900	105,000	110,700	107,700	105,100	103,900	109,300	108,100	78,100	78,000	82,200	81,300
Annual labor related benefits (COVID-19+EI)																
Received	6.2	30.5	16.8	10.3	5.9	29.5	15.9	9.0	5.8	30.9	17.3	10.0	7.9	32.0	17.8	10.9
Mean (with 0)	430	3,200	1,800	510	420	3,100	1,800	460	410	3,300	1,900	490	560	3,300	2,000	640
Mean (no 0)	7,000	10,600	10,600	4,900	7,100	10,400	11,200	5,200	7,100	10,600	11,100	4,900	7,100	10,400	11,100	5,900

NOTES— Summary Statistics are calculated using data from the 2019–2022 Canadian Longitudinal Administrative Databank for partnered mothers and fathers aged 20–64 with school-aged children and childless women and men in the household who did not move during the analysis period. Column headings denote the ages of resident children in the household and the pre-pandemic year and post-pandemic years. Groups defined in text.

Table A8: Pre- (2019) and Post-Pandemic Years (2020-2022) Summary Statistics for Single Parents on Annual Earnings and Benefits Received - Canadian Longitudinal Administrative Databank (LAD)

	Single Women															
	Child Ages 5–11 only				Child Ages 12–17 only				Child Ages Both 5–11 and 12–17				Childless Adults			
Outcomes	2019	2020	2021	2022	2019	2020	2021	2022	2019	2020	2021	2022	2019	2020	2021	2022
Annual earnings																
Had	75.2	72.1	72.1	74.3	75.6	73.4	73.0	74.5	73.0	70.1	69.3	71.4	81.9	80.1	79.3	79.9
Mean (with 0)	37,100	34,800	36,400	37,400	42,200	40,700	42,600	43,400	34,700	33,000	33,900	34,700	44,300	43,900	46,300	47,100
Mean (no 0)	49,300	48,300	50,500	50,300	55,800	55,400	58,400	58,200	47,400	47,000	49,000	48,600	54,100	54,800	58,500	58,900
Annual labor related benefits (COVID-19+EI)																
Received	7.6	46.7	32.5	19.4	6.7	38.8	24.7	14.1	6.9	47.1	32.6	19.0	6.2	34.7	20.7	11.4
Mean (with 0)	510	6,400	4,300	910	470	5,000	3,200	660	480	6,600	4,400	900	380	4,100	2,500	540
Mean (no 0)	6,800	13,600	13,300	4,700	7,000	12,900	13,000	4,700	6,900	13,900	13,500	4,800	6,300	11,700	12,100	4,800
	Single Men															
	Child Ages 5–11 only				Child Ages 12–17 only				Child Ages Both 5–11 and 12–17				Childless Adults			
Outcomes	2019	2020	2021	2022	2019	2020	2021	2022	2019	2020	2021	2022	2019	2020	2021	2022
Annual earnings																
Had	82.4	79.2	78.7	80.6	82.8	80.9	79.4	80.5	84.5	80.5	80.6	82.1	81.5	79.3	78.7	79.6
Mean (with 0)	54,800	51,900	53,100	52,400	61,900	58,100	59,000	59,200	64,000	60,700	61,400	62,100	48,200	47,700	50,900	52,400
Mean (no 0)	66,500	65,600	67,500	65,000	74,700	71,900	74,400	73,500	76,000	75,300	76,000	75,600	59,200	60,100	64,700	65,800
Annual labor related benefits (COVID-19+EI)																
Had	11.5	47.2	31.6	20.0	10.2	41.6	24.4	14.9	10.3	41.8	26.7	16.0	8.7	39.0	23.2	13.6
Mean (with 0)	830	5,800	3,800	1,200	790	4,900	3,100	860	760	5,000	3,400	940	640	4,500	2,800	790
Mean (no 0)	7,200	12,300	12,100	5,900	7,800	11,900	12,800	5,800	7,400	12,000	12,800	5,800	7,400	11,500	12,200	5,800

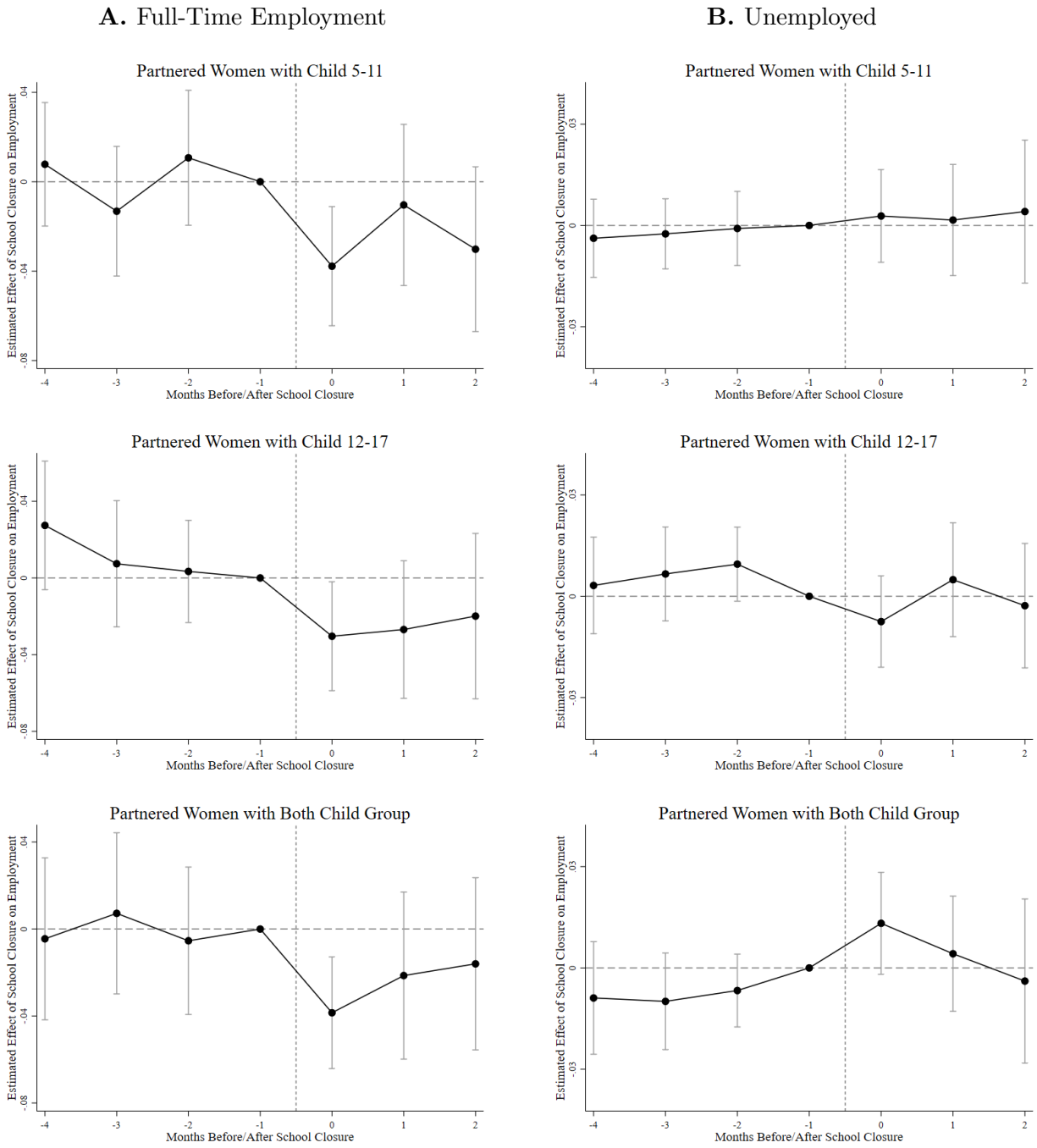
NOTES— Summary Statistics are calculated using data from the 2019–2022 Canadian Longitudinal Administrative Databank for single mothers and fathers aged 20–64 with school-aged children and childless women and men in the household who did not move during the analysis period. Column headings denote the ages of resident children in the household and the pre-pandemic year and post-pandemic years. Groups defined in text.

Table A9: Stacked Difference-in-Differences Estimates of the Effects of School Closures on Labor Supply of Parents of School-Aged Children

	Partnered Parents			Single Parents			Placebo Group
	Child Ages 5–11 only	Child Ages 12–17 only	Child Both 5–11 and 12–17	Child Ages 5–11 only	Child Ages 12–17 only	Child Both 5–11 and 12–17	Childless Adults
Panel A: Women							
Employed, at work	−0.0311***	−0.0349***	−0.0175	−0.0304	0.0240	−0.0415*	−0.0050
	(0.0107)	(0.0117)	(0.0123)	(0.0225)	(0.0182)	(0.0234)	(0.0043)
Hours per week (unconditional)	−1.1043***	−1.4601**	−1.1992***	−1.3727	0.7541	−3.4663***	−0.1289
	(0.3693)	(0.5802)	(0.4223)	(1.0972)	(0.8195)	(0.9354)	(0.1616)
Full-Time Employment	−0.0349***	−0.0329*	−0.0227*	−0.0018	0.0292	−0.0786***	−0.0041
	(0.0122)	(0.0170)	(0.0124)	(0.0251)	(0.0284)	(0.0283)	(0.0045)
Out of Labor Force	0.0026	0.0131**	0.0115**	0.0090	−0.0029	−0.0270	0.0049*
	(0.0050)	(0.0064)	(0.0053)	(0.0122)	(0.0120)	(0.0221)	(0.0027)
Unemployed	0.0083	−0.0044	0.0122*	0.0146	0.0150	0.0414**	0.0003
	(0.0062)	(0.0062)	(0.0065)	(0.0134)	(0.0134)	(0.0169)	(0.0024)
Observations	51,630	33,750	31,260	11,170	10,260	5,550	209,065
Panel B: Men							
Employed, at work	−0.0109	−0.0130	−0.0229*	−0.0647	−0.0047	0.0390	0.0010
	(0.0109)	(0.0112)	(0.0125)	(0.0427)	(0.0412)	(0.0609)	(0.0056)
Hours per week (unconditional)	−0.3246	−0.8973	−1.0928*	−4.5359**	−0.1145	−1.0724	−0.1087
	(0.5062)	(0.6038)	(0.6368)	(1.8019)	(2.3637)	(2.6565)	(0.2346)
Full-Time Employment	−0.0171	−0.0147	−0.0397**	−0.0948*	0.0241	−0.0645	0.0020
	(0.0134)	(0.0149)	(0.0156)	(0.0491)	(0.0608)	(0.0596)	(0.0056)
Out of Labor Force	0.0057	−0.0005	0.0003	−0.0196	0.0044	−0.0347	0.0035
	(0.0053)	(0.0055)	(0.0048)	(0.0209)	(0.0240)	(0.0277)	(0.0025)
Unemployed	0.0057	−0.0056	0.0009	0.0598*	−0.0289	−0.0744**	−0.0022
	(0.0053)	(0.0049)	(0.0061)	(0.0317)	(0.0219)	(0.0362)	(0.0033)
Observations	51,630	33,750	31,260	3,550	3,280	1,360	209,065

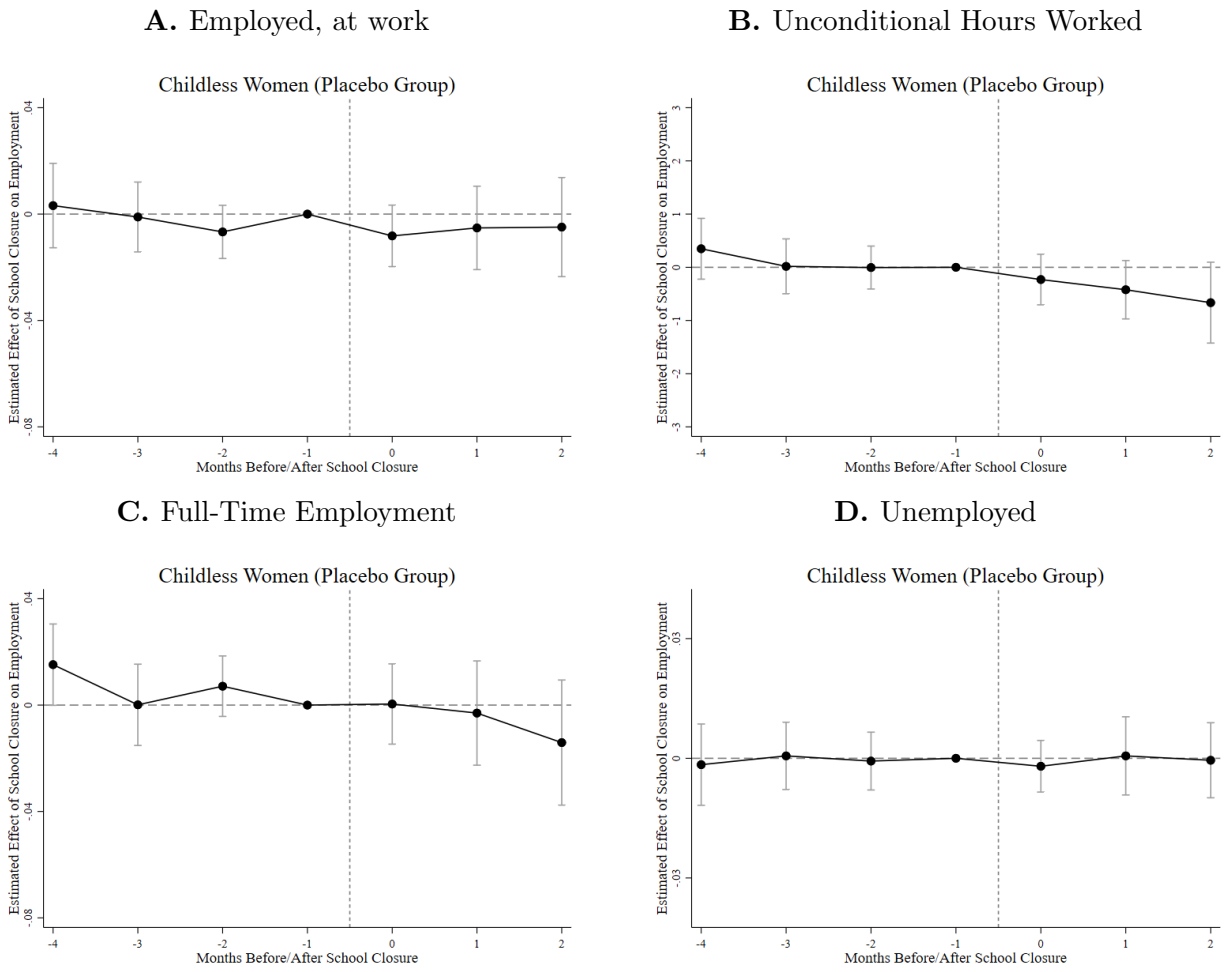
Notes: Estimates are obtained using data from the 2019–2022 Canadian Labour Force Survey and stacked difference-in-differences estimators. Our analysis samples are composed of partnered and single parents aged 20–64 of elementary and secondary school-aged children who had not moved during the analysis sample period. Column headings for columns (1)–(3) and (4)–(6) denote partnership of parents and ages of the resident children in the household. Column (7) shows results for childless adults. Here, a "cohort" is defined by the city and calendar month in which schools first close, and its control group consists of cities that have not yet closed schools in that month. All regression models control for cohort-by-year-by-month fixed effects and cohort-by-individual fixed effects, local foot traffic in non-essential businesses, and local COVID-19 cases in the previous month. City-clustered standard errors are in parentheses and regressions are weighted using the corrective stacked DiD sample weights of Wing et al. (2024). *, **, and *** denote coefficients that statistically differ from zero with p-value $\leq$ 0.10, 0.05, and 0.01, respectively.

Figure A2: Event-Study Analysis of COVID-19 School Closures and Labor Supply of Parents of School-Aged Children



Notes— Estimates are obtained using data from the 2019–2022 Canadian Labour Force Survey and stacked difference-in-differences event-study regressions. For details on the stacked DiD specification, see Table A9. The event-study regressions here replace the single treatment indicator with mutually exclusive leads and lags of school closures (omitting period s=-1 as the reference). All regressions control for cohort-by-year-by-month fixed effects and cohort-by-individual fixed effects, local foot traffic in non-essential businesses, and local COVID-19 cases in the previous month. Standard errors are clustered at the city level and regressions are weighted using the corrective stacked DiD sample weights of Wing et al. (2024).

Figure A3: Event-Study Analysis of COVID-19 School Closures and Labor Supply of Childless Women



Notes— Estimates are obtained using data from the 2019–2022 Canadian Labour Force Survey and stacked difference-in-differences event-study regressions. For details on the stacked DiD specification, see Table A9. The event-study regressions here replace the single treatment indicator with mutually exclusive leads and lags of school closures (omitting period s=-1 as the reference). All regressions control for cohort-by-year-by-month fixed effects and cohort-by-individual fixed effects, local foot traffic in non-essential businesses, and local COVID-19 cases in the previous month. Standard errors are clustered at the city level and regressions are weighted using the corrective stacked DiD sample weights of Wing et al. (2024).

Table A10: Partnered Parents with Children Aged 5–11: Robustness to Controls

	Partnered mothers with child aged 5–11 only			
Outcome	(1)	(2)	(3)	(4)
Employed, at work	−0.0231**	−0.0257***	−0.0254***	−0.0241***
	(0.0091)	(0.0090)	(0.0091)	(0.0085)
Hours worked	−1.1567***	−1.1582***	−1.1615***	−1.1086***
	(0.2367)	(0.2709)	(0.2706)	(0.2588)
Work Full-time	−0.0408***	−0.0346***	−0.0344***	−0.0325***
	(0.0095)	(0.0102)	(0.0103)	(0.0097)
Out of Lab. Force	0.0007	−0.0003	−0.0003	0.0004
	(0.0057)	(0.0057)	(0.0055)	(0.0055)
Unemployed	0.0079	0.0068	0.0070	0.0060
	(0.0083)	(0.0082)	(0.0081)	(0.0080)
N	33,010	33,010	33,010	33,010
	Partnered fathers with child aged 5–11 only			
Employed, at work	0.0033	0.0054	0.0048	0.0085
	(0.0078)	(0.0086)	(0.0085)	(0.0072)
Hours worked	0.0789	0.2527	0.2441	0.4249
	(0.4030)	(0.4652)	(0.4538)	(0.3909)
Work Full-time	−0.0038	−0.0030	−0.0023	0.0019
	(0.0106)	(0.0117)	(0.0117)	(0.0103)
Out of Lab. Force	0.0015	−0.0005	−0.0010	−0.0007
	(0.0038)	(0.0041)	(0.0041)	(0.0039)
Unemployed	−0.0016	−0.0020	−0.0024	−0.0046
	(0.0059)	(0.0063)	(0.0064)	(0.0062)
N	33,010	33,010	33,010	33,010
Controls				
Foot Traffic		Yes	Yes	Yes
COVID Cases			Yes	Yes
UE Rates				Yes

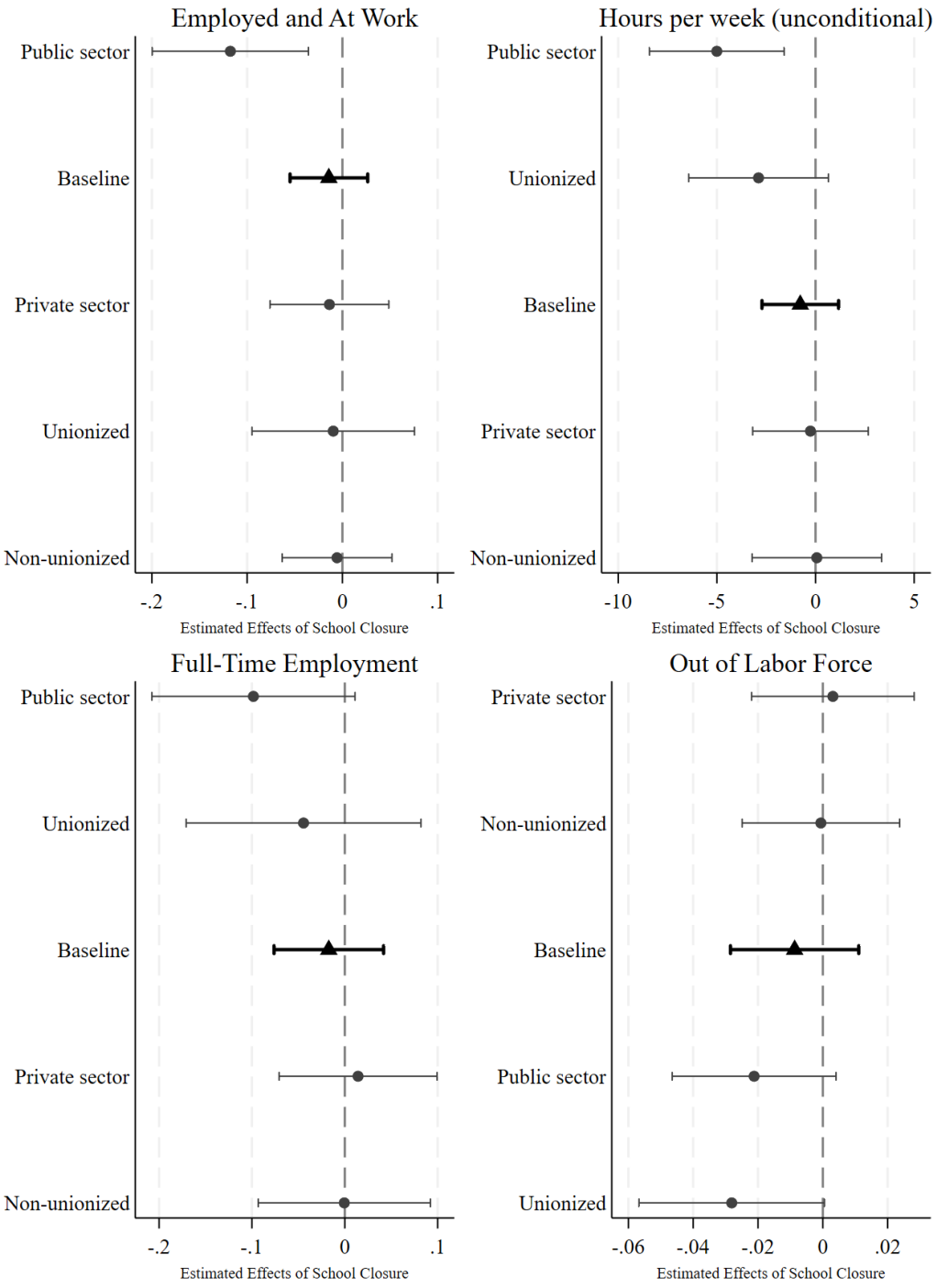
Notes: Estimates are obtained using data from the 2019–2022 Canadian Labour Force Survey and two-way fixed effects (TWFE) difference-in-differences regressions (using ordinary least squares). Our analysis samples are composed of mothers and fathers aged 20–64 of elementary and secondary school-aged children who had not moved during the analysis sample period. Column headings for columns (1)–(4) denote progressively added control variables. All regression models control for year-by-month fixed effects and individual fixed effects. City-clustered standard errors are in parentheses and regressions are weighted using survey-provided sample weights. *, **, and *** denote coefficients that statistically differ from zero with p-value $\leq$ 0.10, 0.05, and 0.01, respectively.

Table A11: Robustness of Estimated Effects to Sample Restrictions

	With child 5–11 only			With child 12–17 only			With child in both groups		
	Main	Aged 20–54	COVID Period Only	Main	Aged 20–54	COVID Period Only	Main	Aged 20–54	COVID Period Only
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Partnered mothers									
Employed, at work	−0.0254***	−0.0243**	−0.0281***	−0.0241*	−0.0338***	−0.0201*	−0.0165*	−0.0184**	−0.0183*
	(0.0091)	(0.0094)	(0.0095)	(0.0122)	(0.0127)	(0.0116)	(0.0092)	(0.0090)	(0.0100)
Hours worked	−1.1615***	−1.0507***	−1.1770***	−1.0811	−1.3946**	−0.8929	−0.7042**	−0.8269**	−0.7287**
	(0.2706)	(0.2981)	(0.3072)	(0.6827)	(0.6348)	(0.6384)	(0.3559)	(0.3579)	(0.3519)
Work Full-time	−0.0344***	−0.0314***	−0.0340***	−0.0198	−0.0263	−0.0182	−0.0163	−0.0205	−0.0165
	(0.0103)	(0.0107)	(0.0107)	(0.0215)	(0.0207)	(0.0213)	(0.0134)	(0.0138)	(0.0147)
Out of Lab. Force	−0.0003	−0.0002	−0.0009	0.0126	0.0152	0.0135	0.0188***	0.0184***	0.0214***
	(0.0055)	(0.0054)	(0.0059)	(0.0090)	(0.0099)	(0.0095)	(0.0067)	(0.0068)	(0.0066)
Unemployed	0.0070	0.0071	0.0075	−0.0019	−0.0003	−0.0017	0.0124*	0.0135**	0.0136**
	(0.0081)	(0.0086)	(0.0082)	(0.0043)	(0.0046)	(0.0041)	(0.0066)	(0.0066)	(0.0068)
N	33,010	31,850	20,615	31,715	20,560	15,645	23,080	21,950	14,125
Partnered fathers									
Employed, at work	0.0048	0.0091	0.0057	−0.0009	−0.0003	0.0008	−0.0063	−0.0108	−0.0041
	(0.0085)	(0.0092)	(0.0092)	(0.0098)	(0.0113)	(0.0099)	(0.0111)	(0.0110)	(0.0112)
Hours worked	0.2441	0.3835	0.3573	−0.0265	−0.4879	−0.0769	−0.6170	−0.6702	−0.5417
	(0.4538)	(0.5234)	(0.5047)	(0.5414)	(0.6407)	(0.5436)	(0.7048)	(0.7415)	(0.7223)
Work Full-time	−0.0023	−0.0008	−0.0003	0.0064	−0.0062	0.0050	−0.0261*	−0.0265	−0.0196
	(0.0117)	(0.0129)	(0.0123)	(0.0140)	(0.0177)	(0.0143)	(0.0156)	(0.0168)	(0.0151)
Out of Lab. Force	−0.0010	−0.0014	−0.0004	0.0021	0.0063	0.0029	−0.0080	−0.0082	−0.0091
	(0.0041)	(0.0041)	(0.0043)	(0.0074)	(0.0084)	(0.0081)	(0.0070)	(0.0075)	(0.0069)
Unemployed	−0.0024	−0.0042	−0.0023	−0.0054	−0.0087*	−0.0061	−0.0020	−0.0003	−0.0020
	(0.0064)	(0.0060)	(0.0066)	(0.0046)	(0.0050)	(0.0047)	(0.0054)	(0.0058)	(0.0054)
N	33,010	31,850	20,615	31,715	20,560	15,645	23,080	21,950	14,125

Notes: Estimates are obtained using data from the 2019–2022 Canadian Labour Force Survey and two-way fixed effects (TWFE) difference-in-differences regressions (using ordinary least squares). Our analysis samples are composed of mothers and fathers aged 20–64 of elementary and secondary school-aged children who had not moved during the analysis sample period. Column headings denote the following sample restrictions: baseline sample (Main), individuals aged 20–54 only (Aged 20–54), and observations in months where COVID case data are available (COVID Period Only), separately for households with only children aged 5–11, only children aged 12–17, and with children in both age ranges. All regression models control for year-by-month fixed effects and individual fixed effects, local foot traffic in non-essential businesses, and local COVID-19 cases in the previous month. City-clustered standard errors are in parentheses and regressions are weighted using survey-provided sample weights. *, **, and *** denote coefficients that statistically differ from zero with p-value $\leq$ 0.10, 0.05, and 0.01, respectively.

Figure A4: Labor Supply Response Heterogeneity, by Outcome and Pre-School-Closure Job Characteristics, Single Mothers with Child Aged 5–11



Notes—Estimates are obtained using data from the 2019–2022 Canadian Labour Force Survey and two-way fixed effects (TWFE) difference-in-differences regressions (using ordinary least squares). For details on the TWFE DiD specification, see Table 1. Each panel plots the coefficient and 95% confidence interval of the estimated effect of school closures for the main sample (labeled triangle) from Table 1 and for the sub-sample with the listed pre-school-closure job characteristics. Estimates are ordered from top to bottom by the magnitude of coefficients. Outcomes are shown above each panel. Standard errors are clustered at the city level. See sample means and proportions in Appendix Table A6.

Table A12: Estimated Effects of Days of COVID-19 School Closures on Annual Income and Benefits Received with City by Year Fixed Effects, by Age of Children

	Females			Males		
Outcomes	Child Ages 5–11 only (1)	Child Ages 12–17 only (2)	Child Both 5–11 and 12–17 (3)	Child Ages 5–11 only (4)	Child Ages 12–17 only (5)	Child Both 5–11 and 12–17 (6)
Panel A: Partnered						
Had Positive Earnings – Childless						
× Treated	-0.0018***	-0.0017***	-0.0019***	-0.0005**	-0.0004	-0.0001
	(0.0004)	(0.0004)	(0.0004)	(0.0002)	(0.0004)	(0.0004)
Observations	807,700	761,000	735,700	877,300	822,200	800,000
Log Positive Earnings – Childless						
× Treated	-0.0116***	-0.0013	-0.0089***	-0.0017*	-0.0022**	-0.0018*
	(0.0021)	(0.0021)	(0.0017)	(0.0010)	(0.0009)	(0.0010)
Observations	675,400	636,400	610,100	792,000	736,900	717,300
Received Benefits/EI – Childless						
× Treated	0.0055***	-0.0006	0.0054***	-0.0023***	-0.0019***	-0.0004
	(0.0012)	(0.0008)	(0.0010)	(0.0008)	(0.0006)	(0.0007)
Observations	807,700	761,000	735,700	877,300	822,200	800,000
Panel B: Single						
Had Positive Earnings – Childless						
× Treated	-0.0069***	-0.0037***	-0.0054***	-0.0070***	-0.0021	-0.0054***
	(0.0006)	(0.0006)	(0.0008)	(0.0015)	(0.0016)	(0.0018)
Observations	623,400	632,400	588,100	606,500	608,600	600,000
Log Positive Earnings – Childless						
× Treated	-0.0293***	-0.0156***	-0.0275***	-0.0242***	-0.0104***	-0.0100
	(0.0035)	(0.0028)	(0.0026)	(0.0039)	(0.0036)	(0.0074)
Observations	509,900	517,100	483,200	535,400	537,200	530,400
Received Benefits/EI – Childless						
× Treated	0.0271***	0.0118***	0.0303***	0.0290***	0.0162***	0.0187***
	(0.0034)	(0.0021)	(0.0033)	(0.0051)	(0.0031)	(0.0048)
Observations	623,400	632,400	588,100	606,500	608,600	600,000

Notes— Estimates are obtained using data from the 2019–2022 Canadian Longitudinal Administrative Databank and triple-differences regressions (using ordinary least squares). Our analysis samples are composed of parents aged 20–64 with school-aged children and childless adults who had not moved during the analysis sample period. The estimated effects compare changes in outcomes for parents of school-aged children in addition to those of childless adults. Column headings for columns (1)–(3) and (4)–(6) denote the gender of parents and the ages of the resident children in the household. All regression models control for individual, calendar-year fixed effects, and city-by-calendar-year fixed effects. City-clustered standard errors in parentheses. *, **, and *** denote coefficients that statistically differ from zero with p-value $\leq$ 0.10, 0.05, and 0.01, respectively.

Table A13: Estimated Effects of Days of COVID-19 School Closures on Annual Earnings and Benefits, a Triple-Differences Design by Age of Children using Pseudo Poisson Maximum Likelihood

	Females			Males		
Outcomes	Child Ages 5–11 only (1)	Child Ages 12–17 only (2)	Child Both 5–11 and 12–17 (3)	Child Ages 5–11 only (4)	Child Ages 12–17 only (5)	Child Both 5–11 and 12–17 (6)
Panel A: Partnered						
Earnings – Childless	0.0015	0.0008	0.0013	0.0066	0.0007	0.0001
	(0.0031)	(0.0028)	(0.0032)	(0.0045)	(0.0032)	(0.0037)
× Treated	-0.0052***	-0.0018*	-0.0038***	-0.0033**	-0.0015	-0.0021
	(0.0010)	(0.0009)	(0.0013)	(0.0014)	(0.0012)	(0.0017)
Observations	719,600	677,200	651,600	827,900	772,000	751,800
Benefits – Childless	0.0325***	0.0317***	0.0308***	0.0192**	0.0147	0.0131
	(0.0070)	(0.0075)	(0.0076)	(0.0087)	(0.0092)	(0.0095)
× Treated	0.0627***	0.0319***	0.0602***	0.0361***	0.0398***	0.0393***
	(0.0099)	(0.0035)	(0.0069)	(0.0084)	(0.0075)	(0.0095)
Observations	285,700	265,200	260,900	309,100	289,000	284,700
Panel B: Single						
Earnings – Childless	0.0015	0.0011	0.0010	0.0004	-0.0003	-0.0002
	(0.0035)	(0.0034)	(0.0035)	(0.0037)	(0.0039)	(0.0040)
× Treated	-0.0163***	-0.0081***	-0.0143***	-0.0090***	-0.0032*	-0.0046
	(0.0020)	(0.0016)	(0.0019)	(0.0023)	(0.0019)	(0.0037)
Observations	548,000	554,700	518,600	565,800	567,400	560,100
Benefits – Childless	0.0447***	0.0374***	0.0382***	0.0289***	0.0211**	0.0210**
	(0.0067)	(0.0069)	(0.0071)	(0.0081)	(0.0096)	(0.0096)
× Treated	-0.0142	-0.0070	0.0071	0.0275	0.0154	0.0249
	(0.0088)	(0.0077)	(0.0111)	(0.0212)	(0.0135)	(0.0169)
Observations	238,200	236,600	220,400	231,500	231,300	227,700

Notes— Estimates are obtained using data from the 2019–2022 Canadian Longitudinal Administrative Databank and triple-differences regressions. Our analysis samples are composed of parents aged 20–64 with school-aged children and childless adults who had not moved during the analysis sample period. The estimated effects compare changes in outcomes for parents of school-aged children in addition to those of childless adults. Column headings for columns (1)–(3) and (4)–(6) denote the gender of parents and the ages of the resident children in the household. All regression models control for individual and calendar-year fixed effects. City-clustered standard errors in parentheses. *, **, and *** denote coefficients that statistically differ from zero with p-value $\leq$ 0.10, 0.05, and 0.01, respectively.