Melbourne Institute of
Applied Economic and Social Research

Working Paper Series

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Working Paper No. 03/25
June 2025

THE UNIVERSITY OF MELBOURNE
MELBOURNE INSTITUTE

The impact of school disruptions during the COVID-19 pandemic on parental labor supply and earnings in Australia

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June 2025

Abstract

We use quasi-experimental variation in suspension of in-person teaching at schools to estimate the causal impact of school disruptions on parents' labor supply. School disruptions have a large negative effect on labor force participation, especially for women and for people with weaker labor force attachment. Conditional on remaining employed, school disruptions have no impact hours worked or on wages. Exploring potential mechanisms, we find that school disruptions sharply increase working from home, which can help explain our null effects, and point to even more negative effects on labor force participation in the absence of this margin of adjustment.

Keywords: School disruptions; labor force participation; hours worked; wages; intertemporal treatment effect estimates; COVID-19 pandemic.
JEL Codes: J22, I28, I18

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[*] Corresponding author: n.salamanca@unimelb.edu.au. This paper uses unit-record data from the Household, Income and Labour Dynamics in Australia (HILDA) Survey. The HILDA Survey Project was initiated and is funded by the Australian Government Department of Social Services (DSS) and is managed by the Melbourne Institute of Applied Economic and Social Research (Melbourne Institute). The findings and views reported in this paper, however, are those of the authors and should not be attributed to either DSS or the Melbourne Institute. These data are available free of charge to researchers through the National Centre for Longitudinal Data Dataverse at the Australian Data Archive (https://dataverse.ada.edu.au/dataverse/ncld). Access is subject to approval by the DSS and is conditional on signing a license specifying terms of use. This research was supported by a US National Institutes of Health Grant (#R01AG071649: PI Lillard, with a sub-award to the University of Melbourne: PI Salamanca).

I. Introduction

In response to the COVID-19 outbreak in early 2020, governments in many countries implemented policies intended to contain the spread of the virus and reduce its impact on the health services system. In Australia, governments at both the federal and state levels imposed laws and orders that restricted population movement and reduced inter-personal contact. Among the suite of restrictions implemented were business closures, stay-at-home orders, and the suspension of in-person teaching at schools. It is the latter that is the focus of this study.

In the absence of alternative child-care arrangements, together with increased home-schooling demands, school and child-care disruptions may affect labor supply decisions, especially that of primary carers. More specifically, it is expected that some parents may have been forced to reduce their work hours or withdraw from the labor force entirely because of such disruptions. Some of this negative supply response, however, will have been offset by government advice directing employers to allow employees to work from home wherever possible, and by increased support via the welfare system.

The impact of school disruptions on parental labor supply in Australia has, however, not been the subject of rigorous research, possibly because of the difficulty in identifying causal effects. In contrast, studies have been conducted in the US that have made use of inter-state differences in the incidence of school closures or in teaching mode. While empirical evidence is somewhat mixed, with studies focused on the first weeks of the pandemic finding little evidence of marked effects on hours worked (e.g., Heggeness 2020), subsequent research shows school disruptions lead to non-negligible reductions in parental labor supply (e.g., Amuedo-Dorantes et al. 2023; Collins et al. 2021; García and Cowan 2022). Furthermore, these negative effects on labor supply have usually been found to be largest for mothers (Amuedo-Dorantes et al. 2023; Collins et al. 2021). Further supportive evidence is provided by studies that leveraged differences across states in the timing of school re-openings in both the US (Hansen et al. 2024) and Canada (Beauregard et al. 2023). Very differently, Meekes et al. (2023) focused on the difference in labor supply responses of persons employed in sectors defined by the Dutch Government as essential or non-essential, with essential workers permitted to send children to school and child care services during periods of lockdown while non-essential workers were not. This thus permitted a difference-in-differences design.

Our study adds to this literature by providing the first study of the impact of school disruptions (or more strictly, of the impact of the suspension of in-person teaching) on parental labor force engagement in Australia. We leverage longitudinal data from the Household, Income and Labour Dynamics in Australia (HILDA) Survey covering the period 2017 to 2021. This period thus includes the COVID-19 pandemic during which residents of some of Australia's largest states – Victoria, New South Wales, and the Australian Capital Territory – were subject to highly restrictive lockdowns involving curfews and compulsory stay-at-home orders for all but essential workers. Our quasi-experimental design exploits rich variation in households excepted from disruptions in in-person teaching, the timing and geolocation of school disruptions, and the age of children. Essential workers, who were able to continue sending their children to school, experienced minimal disruptions from the school closures effectively creating a group that was never effectively disrupted. In addition, Australia's federal government system and the variability in COVID-related policies among states led to significant differences both between and within states, particularly concerning school disruptions. As a result, parents with children in day care, primary, or secondary schools faced varying degrees of closure (including none) depending on their location and household structure. Interestingly, Australia's proactive implementation of strict international and interstate travel bans early in the pandemic pitches this identifying variation in a setting with remarkably lower COVID-19 case numbers in 2020 and 2021 compared to other countries, resulting in less widespread fear among the population regarding COVID-19 and its associated health risks. As a result, we are uniquely able to decouple the effect of school disruptions from the corelated confounding effect of actual or perceived fear and concerns regarding COVID-19 infections.

II. The Australian Response to the COVID-19 Pandemic

A. The Public Health Response

In Australia, the pandemic was initially met with an aggressive suppression strategy involving test, trace, and isolate methods to contain outbreaks (Heriot and Jamrozik 2021). This was supported by two major types of public health responses: (i) border closures; and (ii) social isolation and distancing measures.

Following the arrival of the first cases of COVID-19 infection in Australia in late January 2020, the national government began imposing travel bans that were soon extended to all non-residents and non-citizens. In addition, returning Australian residents were required to enter a 14-day quarantine at a designated facility (mostly hotels). With the notable exception of a period of quarantine-free travel between Australia and New Zealand (from 19 April to 23 July 2021), these restrictions would remain in place until the end of October 2021. International borders, however, would not fully re-open until 21 February 2022.

These international border closures were accompanied by internal border closures, with governments of the less populous states seeking to protect their populations from the growing number of infections in the two most populous states, Victoria and New South Wales. Such restrictions did not mean interstate travel was not feasible, but persons living in border zones required permits and negotiating hard border checkpoints, while others had to enter mandatory periods of quarantine (typically of 2 weeks duration) upon arrival at their destination.

Within states, the activity and movement of Australian residents would also become much restricted. A newly formed 'national cabinet' comprising the Prime Minister and all state and territory leaders agreed, on 15 March 2020, to a ban on non-essential organized public gatherings of more than 500 people. This was followed by ever tighter restrictions culminating on March 24 by the decision taken to close or heavily restrict the operation of all non-essential businesses. Australia was now effectively in a state of partial lockdown, with residents being advised to: (i) stay at home unless shopping for essentials, exercising, seeking medical care, or travelling to work or education; (ii) work from home wherever possible; (iii) keep visitors to the home to a minimum; and (iv) not congregate in groups when outdoors.

By early May this approach seemed to be working, with new COVID-19 cases numbering fewer than 20 per day. Consequently, the national cabinet announced a plan to ease restrictions, though the rate with which restrictions were eased varied across states (Stobart and Duckett 2021). In late June 2020, however, Melbourne, the largest city in Victoria, became the epicenter of an outbreak that saw a marked increase in the number of COVID-19 cases. This prompted the Victorian state government to re-impose a range of lockdown measures, including business closures, stay-at-home orders, remote schooling, bans on all public gatherings, restricted access to family members living in aged-care facilities, and making the wearing of face masks mandatory whenever outside the home. These restrictions were not mere advice but legally enforceable requirements subject to financial penalties. In Greater Melbourne, these restrictions commenced on 9 July and stay-at-home orders would remain in place until 27 October. In regional Victoria, stay-at-home orders commenced on 4 August and were lifted on 17 September. This, however, was to be only the first in a series of lockdowns for Victorian residents. Short snap lockdowns commenced on 13 February 2021 (for 5 days), on 28 May (for 7 days) and on 16 July (5 days). Then, on 5 August 2021, Victoria entered another prolonged lockdown that for Melbourne residents would not be lifted until 21 October.

Elsewhere in Australia, life seemed to be slowly returning to some degree of normality. While many restrictions remained in place — notably capacity limits on public venues, mask-wearing mandates in certain settings, and of course restrictions on inter-state travel — stay-at-home orders were confined to a few short and often localized episodes.[1]

The notable exceptions here were New South Wales and the Australian Capital Territory (ACT), which from the middle of 2021 would, like Victoria, also be the subject of a prolonged lockdown. Following an upsurge in the number of cases of the relatively new Delta variant, mask mandates and a range of restrictions intended to reduce social interactions were imposed on residents and businesses in Greater Sydney and major adjacent regions commencing on 23 June. Three days later, stay-at-home orders were announced. On 14 August this lockdown went state-wide (and also included the ACT), and from 23 August would involve a 9 pm evening curfew for many Sydney residents. The state-wide lockdown would last until 11 September, after which date stay-at-home orders were relaxed for residents living in local government areas without any new cases during the preceding 14 days. It was not until 11 October that lockdowns in New South Wales came to an end, and even then, only for the double vaccinated. The unvaccinated would remain under stay-at-home orders until 15 December.

B. The Economic Policy Response

During the pandemic, Australian governments significantly boosted public spending to ease financial strain on the working-age population. According to Breunig and Sainsbury (2023), approximately 6.5 million Australians, representing 42% of the working-age population, received some form of financial assistance. The median recipient had 46% of their pre-COVID-19 wages replaced by income support measures. Most notable here was the JobKeeper program — a $88 billion wage subsidy initiative aimed at helping struggling businesses retain their workforce and keep employees employed. The Australian government also provided early access to private pension (superannuation) accounts, injecting an additional $38 billion into

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[1] See Online Appendix Table A1 for a list and summary of lockdowns involving stay-at-home orders that were put in place in Australia at some point during 2020 or 2021.

households, and allocated $52 billion in supplementary income payments to support various groups, including the unemployed, students, and parents (Breunig and Sainsbury 2023).

C. School Closures and Disruptions

School closures were, at least initially, controversial. The Australian Government insisted that it was safe for children to continue to physically attend school. This reflected the advice in its 2008 Australian Health Management Plan for Pandemic Influenza (Department of Health 2019, 145). Additionally, there were concerns about how essential workers, and especially those working in health and aged care settings, would be able to cope should their children be unable to attend school (and the associated before-and-after-school care services). Schools were thus exempt from the ban on public gatherings announced in March 2020.

School education, however, is a mainly state government responsibility and the states took a different view and moved rapidly away from the traditional in-person learning model to reliance on online learning. New South Wales, Victoria, Queensland and the ACT all mandated school closures in late March (but with exceptions for children of essential workers) and / or brought forward and extended the approaching school break between mid and late April. The other states opted for a hybrid approach where it was left to parents to decide whether they wished their child to physically attend school or to learn from home. These restrictions, however, did not last long. In South Australia and the Northern Territory, students were back in the classroom in late April. In other states the transition was slower, typically involving a period of transition and only commencing once other community restrictions were eased in early May. In Victoria, the return to the classroom was both the slowest and short-lived. On 13 July, schools in Melbourne were again required to cease in-person teaching as part of the wider lockdown imposed that month, a requirement that would in August be extended to all Victorian schools. The main exceptions here were Year 11 and Year 12 students, but even that exemption was removed in August. The other exception was children of essential workers, who were permitted to physically attend school but with learning still delivered online. In effect, schools were required to provide a childminding service for essential workers. This approach would form the model followed in later lockdowns, both in Victoria and elsewhere.

A major issue of contention was the determination of who was an essential worker. In the early months of the pandemic this was largely left to schools and parents, and thus was a source of confusion. Victoria, however, would eventually introduce a permit system (in August 2020) that required employers who were eligible to continue to keep trading during the lockdown to issue permits to their employees, which would then enable those employees to access child care and provide them with the right to take their children to school. This was supported by an official list of essential service providers and industries.

D. Child Care Services

Some parents also had to deal with reduced access to child care services. The treatment of child care by governments, however, was different to the approach taken to the operation of schools, with the emphasis on keeping formal child care services (which includes center-based day care, outside school hours care, and family day care) open as much as possible. In the early months of the pandemic, parental health concerns together with the rise in the number of parents at home saw a large drop in child care enrolments, threatening the financial viability of many operators in the sector (Baxter 2021). In response, the federal government introduced a financial assistance package to support child care service providers. Thus, for a three-month period commencing early April 2020, existing child care subsidy arrangements were suspended and in its place service providers were entitled to weekly payments that were tied to the number of enrolments during the preceding February (i.e., pre-pandemic). This assistance was conditional on providers remaining open and services being provided to families free of charge.

This is not to say that child care services were not affected by lockdowns, but the incidence of such disruptions was less common (mainly restricted to Victoria, the ACT, and some of the short snap lockdowns that occurred elsewhere). Furthermore, in almost all cases, centers were not actually closed: Families of essential workers and vulnerable children were always exempt from recommendations or requirements to not use child care services.

III. Data and Methods

A. Data Collection and Sample

As noted earlier, the primary data source is individual-level observations from the HILDA Survey, a household panel survey that commenced in 2001 with, after weighting for initial non-response, a nationally representative sample of Australian households residing in private dwellings (Watson and Wooden 2021). The first wave comprised 13,969 participants from 7,682 households. Interviews were then sought on an annual basis with all members of those initial households aged 15 years or older, along with any persons who subsequently joined a household in which an original household member resided. A top-up sample providing another 2,153 households was added in 2011. Rates of response to the HILDA Survey are relatively high, especially the annual re-interview rate, which rose from 87% in wave 2 to over 94% by wave 5 and, for the main sample at least, has remained at levels above that in every wave since (Summerfield et al. 2022, Table 8.3). As a result, it might be expected that sample characteristics will closely mirror the characteristics of the wider population. Unfortunately, the absence of any automatic mechanism for augmenting the sample each year with recent immigrant arrivals means that this group will be under-represented. This problem was partly redressed by the addition of a broad top-up sample in 2011.

Data are collected via a combination of interviews and self-administered questionnaires (though this analysis only draws on data collected by interview), and each year is concentrated in the months of August to November. Data collection in 2020 and 2021 thus overlapped closely with the timing of prolonged lockdowns in both Victoria and New South Wales. We then merged into these data a series of variables identifying the presence of a range of COVID-related mitigation and containment policies, the dates on which these restrictions were imposed and lifted, and the local government areas (LGAs; similar to US counties or municipalities in terms of scale and function) to which they were applied. Included here are the presence of stay-at-home orders and mandates imposing restrictions on the operation of schools (and especially the requirement to deliver teaching online).[2]

For our analyses, we restrict our sample to people aged 18 to 75 years who responded at least once in survey waves 17 to 21 (roughly the 5-year period 2017 to 2021) and who lived in a household where at least one of its members reported being in the labor force at the time of at least one of any of the five survey waves. This results in an initial sample of 75,573 observations from 18,729 unique respondents. For our main analysis, we further restrict our sample to an unbalanced panel among parents of day-care, primary and secondar school-aged children (0-17 years) residing in the same household as their parents. Our final estimation sample includes 24,571 person-year observations from 5,219 unique parents.

B. Identifying Variation

To identify the effects of school disruptions on parents' labor supply and earnings, we leverage variation in disruptions to schooling and day care services across geography and over time given that different states implemented different school disruption policies at different times and applied these policies to different areas within states. We exploit differences in the date of implementation of school disruption policies measured at the LGA level. We further augment this with variation in the place of residence and interview date of HILDA Survey respondents, noting that even within the same LGA and survey wave, individuals interviewed later in the pandemic would have been more likely to be exposed to more school disruptions.

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[2] This information was extracted from media releases, reports, and official guidelines issued by the Federal, State, and Territory Governments between January 2020 and December 2021.

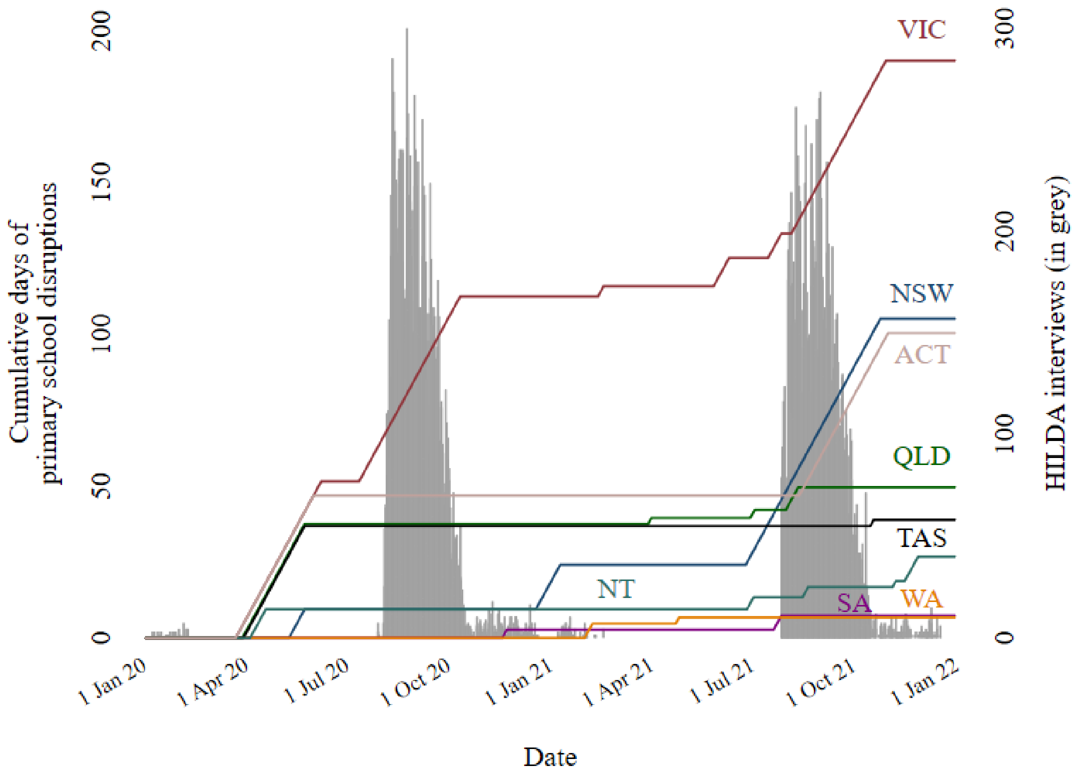

Fig. 1. Across-state variation in cumulative days of primary school disruption, with HILDA Survey interview dates, 2020 and 2021

In this figure, states are classified as experiencing primary school disruptions if at least one LGA within the state is experiencing them at any given date. HILDA Survey interview dates represented by the grey columns. ACT = Australian Capital Territory; NSW = New South Wales; NT = Northern Territory; QLD = Queensland; SA = South Australia; TAS = Tasmania; VIC = Victoria; WA = Western Australia.

Figure 1 illustrates the geographic and time variation in school disruption policies, and how it overlaps with HILDA Survey interview dates. This figure reports the cumulative number of days of exposure to primary school disruption (i.e., any government-imposed limitation on primary school attendance) and shows considerable variation across states in the timing of school disruptions. Most of the variation, however, comes from the more disrupted states of Victoria and New South Wales, as well as the ACT. Other states provide much less geographic variation, which simply reflects the fact that following the initial phase of the pandemic (i.e., March to May 2020) lockdowns were rarely used in these other states. The figure also plots a histogram of the HILDA Survey interviews in 2020 and 2021 by date (the grey columns). Most of the within-state variation in school disruptions in 2020 comes from Victoria, whereas in 2021 New South Wales and the Australian Capital Territory also contribute to this variation.[3]

Finally, we also exploit variation in the presence or absence of essential workers and school-age children in the household during the pandemic.[4] Essential workers were allowed to take their children to school in most states during spells of school disruptions. We assume that households with at least one essential worker were unaffected by school disruptions, providing one plausible comparison group. Moreover, parents of children of different ages were exposed to different school disruptions depending on whether their children were meant to attend day care, primary or secondary school (with further but limited variation on restrictions across some school grades at different points in time). In Online Appendix Table 2 we report summary statistics for the key variables and subgroups.

C. Identification and Estimation

In our main specification, we focus on estimating the causal effect of being exposed to school disruptions in the previous 30 days relative to the HILDA Survey interview date on parental labor supply. To estimate these effects, we use the de Chaisemartin and D'Haultfoeuille's

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[3] Online Appendix Figures A1 and A2 illustrate similar variation in school disruptions in day care and in secondary schools. These figures understates the identifying variation since they ignore substantial within-state variation in disruptions. In practice, within-state variation in school disruptions is relatively small compared to between-state variation (e.g., for primary school disruption the between-state variation is around five times larger than the within-state variation).

[4] Essential worker lists were released early in the pandemic by State governments. Each list loosely described a set of industries and occupations considered essential and therefore allowed exceptions from stay-at-home orders and described exceptions and other special conditions for each of these. For assigning essential worker status in our data, we constructed a concordance between these lists and 4-digit industry codes. Based on their 4-digit industry code, each worker was classified as:

1. *Essential:* If employed in an industry explicitly exempted from stay-at-home and school restrictions. These industries were listed as essential to continue day-to-day activities including agriculture, manufacturing, and other industries.
2. *Essential with restrictions:* If employed in an essential industry but where density quotas applied (e.g., only five people were allowed to work on a residential construction site at a time).
3. *Partially Essential:* If employed in an industry allowed to operate in their usual location, but on a skeleton staff (e.g., only click and collect were allowed in retail stores).
4. *Not essential:* If employed in an industry that was restricted to close their workplaces and operate from home.

For our main results, we treat categories 1 and 2 above as "essential" and the rest as non-essential.

(2023) difference-in-differences estimator of intertemporal treatment effects, which is particularly well suited to our design since: (i) everyone begins unexposed to pandemic-related school disruptions in wave 19; (ii) groups of people become exposed to school disruptions at different times in waves 20 and 21; (iii) some exposed people in wave 20 could become unexposed in wave 21; (iv) some groups are never exposed; and (v) we could expect school disruptions to affect outcomes over several periods (and we are interested in those dynamics). This estimator is unbiased for intertemporal dynamic treatment effects, relies on a weaker-than-usual version of parallel trends, is additionally robust to treatment heterogeneity across treatment groups, can seamlessly estimate alternative versions of school disruption treatments, and is easily implementable using the provided *did_multiplegt_dyn* command in *Stata*.

To implement this estimator, we define 932 time-invariant groups at the household level that capture the way school disruptions affected households. We construct these groups as all possible combinations of whether, during the pandemic and in a given household, there are: a) any children of day care age (0-4 years); b) any children of primary school age (5-11 years); c) any children of secondary school age (12-17 years); d) any essential worker; and e) the combination of LGAs of residence in waves 20 and 21. Of those groups, 269 are ever-treated, in the sense that they become exposed to disruptions at some point in waves 20 or 21 given the household characteristics above and the timing and geographical distribution of school disruptions in Australia. The remaining 663 groups are never-treated because they were never subject to imposed school disruptions relevant to their children's school-age bracket, were never interviewed within 30 days of a relevant disruption, or always had an essential worker residing in the household during the pandemic (which allowed them to send their children to day care and schools regardless of disruptions). Our estimates use outcomes in wave 19, the last wave before the pandemic started, as reference, and compares the outcome evolution of groups experiencing disruptions in waves 20 and 21 to groups that have not (yet) experienced disruptions at that time.[5] The de Chaisemartin and D'Haultfoeuille (2023) estimator is unbiased for these intertemporal treatment effects under a (weaker than usual) parallel-trends assumptions for treatment and comparison groups, which we test by comparing outcomes for these groups in waves 18 and 17.

Practically, our key regressor, $Disruption_{igt}$, is a measure of whether person i belonging to a household in group g has been affected by school disruptions at time t. We consider affected people as those exposed to restrictions on teaching mode within schools or to the presence of government recommendations not to use formal child care (i.e., day care centers), which we collectively refer to as school disruptions. In our main specification, $Disruption_{igt}$ is coded as a binary variable that takes the value of one if, at the time of the interview, the respondent had been affected by school disruptions in the past 30 days, and zero if the respondent was unaffected by school disruptions. Thus, a respondent with a 7-year-old child will be coded as treated by school disruptions if, at the time of survey and in their LGA of residence, there have been disruptions to primary schools. Furthermore, respondents living in affected LGAs will only be coded to the treatment group if the restrictions in place apply to the type of school or child care provider that any of their children could potentially be attending.

Our intertemporal estimates should be interpreted as the effects of being exposed to a weakly higher doze of school disruptions (i.e., at least one more disruption than the comparison group) in the last 30 days for t periods. This interpretation might seem difficult, so it is worthwhile noting that in our main design virtually all treated groups are exposed to school disruptions in both waves 20 and 21 (i.e., they share an absorbing treatment status). This implies our estimates can be interpreted as the effect of being exposed to school disruptions for t

[5] In addition, treatment and comparison groups have to share the same treatment status in the beginning of the panel. This is trivially true for all groups in our design since the HILDA predates the pandemic and no one was exposed to pandemic-related school disruptions before the pandemic started. This also implies that our estimator is equivalent to a modified version of the unconditional event study estimator in Callaway and Sant'Anna (2021).

periods, and thus take on an interpretation closer to "treatment dosage". We also discuss the implied average total effect of school closures, which averages intertemporal effects over the number of time periods over which these effects are accumulated. These average total effects should be interpreted as the average effect of having imposed school disruptions as they were compared to the status quo of not having had school disruptions.

IV. Results

A. Effects of School Closures on Labor Supply and Earnings

Figure 2 presents the results of event-study difference-in-difference estimators of experiencing a school disruption in the last 30 days on our three main outcome variables (Online Appendix Table A3 shows the estimates underlying this figure).

Parents with day care and school-aged children reduced their labor force participation based on the availability of undisrupted day care and schools. Specifically, school disruptions cause sharp declines in labor force participation of 4.2 percentage points in the wave in which disruptions occur, and 5.1 percentage points after two waves. These effects aggregate to an average total effect of 4.9 percentage points (s.e. = 1.8 percentage points). This effect seems relatively small compared to the average labor force participation rate of observations used as controls, which is 86 percent — meaning the effect of school disruption is about 6 percent. However, it represents a quarter of the gap between labor force participation of women and men in our estimation sample (18.9 percentage points). The effect size also represents about 86 percent of the increase in labor force participation of parents between 2011 and 2021, which was steadily rising over this period. Overall, we see this as a sizable effect.

It needs to be borne in mind that, perhaps unlike many other countries, the macroeconomic decline in labor force participation and working hours precipitated by the pandemic in Australia was relatively short-lived. The official Labor Force Survey reports the aggregate labor force

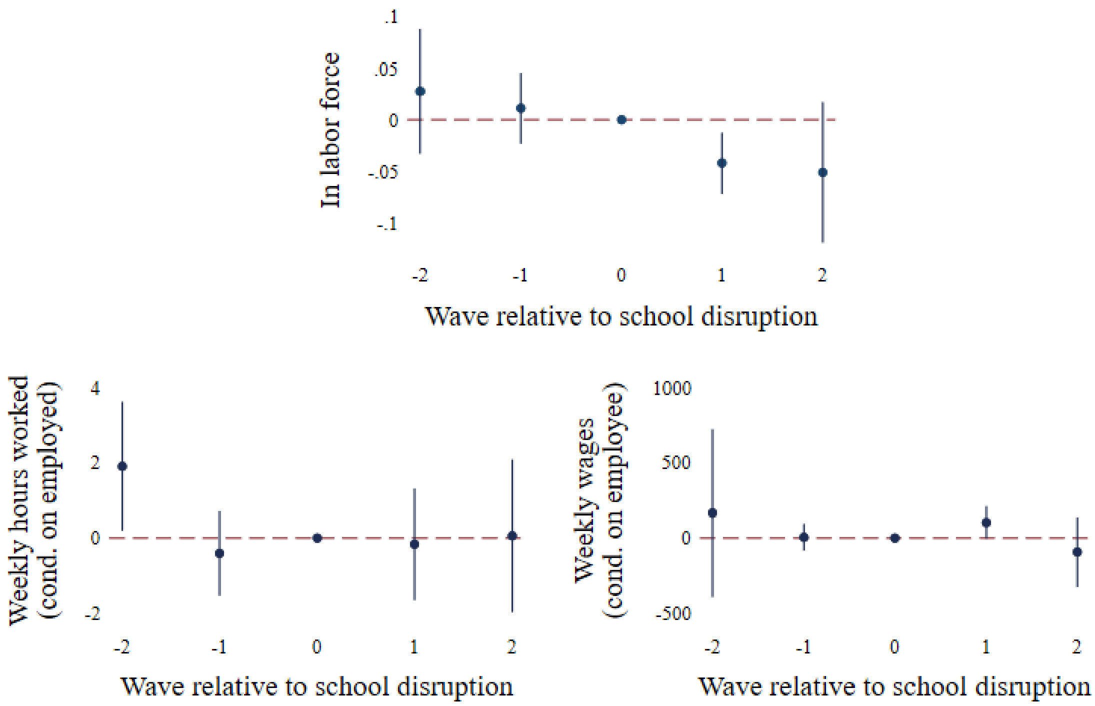

Fig. 2. Event study of the impact of school and day care disruptions on parental labor supply and earnings

Difference-in-Differences intertemporal treatment effect estimates of experiencing a school disruption in the last 30 days relative to outcomes in the last non-disrupted HILDA wave (t=0). Estimates are produced using the Stata command did_multiplegt_dyn *from de Chaisemartin and D'Haultfeuille (2024), as described in Section III.C. Underlying estimates are also reported in Online Appendix Table A3. Spikes show 95 percent confidence intervals based on cluster-robust standard errors at the group level.*

participation rate falling from 65.8% in February 2020 to just 62.5% in May 2020. By August, however, when wave 20 of the HILDA Survey went into field, this rate had already returned to 65.0%.[6] Similarly, the average hours actually worked by all employed persons in Australia in August 2020 was only about 0.8 of an hour less than in the comparable month a year earlier.

[6] https://www.abs.gov.au/statistics/labour/employment-and-unemployment/labour-force-australia

On the other hand, we find no significant changes in weekly (conditional) work hours or gross wages for parents with day care and school-aged children, suggesting that school disruption primarily affected parents' extensive rather than their intensive margins.[7]

B. Heterogeneous Effects

We expected that effects of school disruptions would vary across a range of parental and household characteristics. In Online Appendix Table A4 we show evidence that the effect of school closures on women's labor force participation is around twice as large in magnitude as the effect on men. These differences are driven by gendered longer-term effects of school closures. For both men and women, the short-term effects of school closures are a similar drop in labor force participation, yet in the longer-run these effects fade out for men while they double for women. We also find evidence of positive effects on wages for men and no effects for women, though the parallel trends assumption for men might not hold, casting doubt on this result. Online Appendix Table A5 further shows suggestive evidence that effects on labor force participation are twice as large for singles than for partnered respondents, though we have low statistical power when estimating effects on singles. Online Appendix Table A6 shows much larger negative effects on labor force participation or respondents with weak labor force attachments (as defined by having been unemployed or out of the labor force in the three years before the pandemic started). Finally, Online Appendix Table A7 shows that the negative effects on labor force participation are driven by larger negative effects for households with children in primary and secondary school ages and with children across school levels, and these effects are much smaller (but also less precise) for households with children in day care ages.

[7] The average total effect on hours worked is -0.121 (s.e. = 0.784). One caveat is the lack of parallel trends — we reject the hypothesis of nullity of placebo effects with a p-value of 0.025. The sign and size of these imbalances suggest the possibility of a small negative effect on hours worked. The average total effect on weekly wages is \$55 (s.e. = \$62).

There is also evidence of larger negative effects for this latter group on working hours, whereas hours increased in households with secondary school aged children.

C. Sensitivity Analyses

In our Online Appendix, we show the robustness of our results to various alternative specifications and modelling choices. First, Online Appendix Table A8 shows that traditional two-way fixed effect models yield slightly more negative effects on labor force participation and produce null effects for hours worked and wages. Online Appendix Table A9 then shows that our estimates remain essentially unchanged once we control for the potentially confounding exposure to stay-at-home orders in the last 30 days, and for gender, marital status, Indigenous status, highest level of education, one-month-lagged local state unemployment rate, average COVID-19 cases reported in the state in the last 30 days, and respondent age. Online Appendix Table A10 shows that estimates on labor force participation and wages remain unchanged if we use individual respondents as treatment groups. This alternative estimator produces more negative and marginally significant effects on hours worked and shows better performance in terms of parallel trends for this outcome. Online Appendix Table A11 shows marginally smaller effects on labor force participation when we allow our comparison groups to include non-parents of school-aged children (because they did not have school-aged children in the household during the pandemic, or they never had children). Conversely, Online Appendix Table A12 illustrates the importance of having households with essential workers during the pandemic as part of the comparison groups; when we exclude these, our estimates on labor force participation halve in size and become statistically insignificant, whereas all other estimates remain small and insignificant too. Online Appendix Table A13 shows that our estimates remain unchanged if we exclude households with children in secondary school only.

Finally, Online Appendix Table A14 shows that the estimated negative average total effects on labor supply remain similar in magnitude regardless of whether we estimate the effect of school disruptions in the past 60 days, in the past 90 days, or since the start of the pandemic. We do estimate small positive effects on hours worked of experiencing school disruptions since the start of the pandemic, and in these specifications we also find stronger evidence in favor of parallel trends.

D. Mechanisms

We also explored a range of potential mechanisms driving the negative effects of school disruptions on parents' labor force participation, or explaining the null effects on hours worked and wages. One such mechanism could be the extent to which parents work from home. That is, many employers mandated parents to work from home during the school closures. This, in turn, could have enabled parents to better manage both child-care and professional responsibilities from home, leading to fewer labor supply adjustments. Another possibility is that parents sought assistance from grandparents who lived in the same household, allowing them to continue working despite school closures. On the other hand, long-term health conditions, high child-care costs, and the need to care for older household members could be associated with more substantial adjustments in parents' labor supply. Either way, effects on these mechanisms could explain our findings or could conversely point to larger counterfactual effects without these margins of adjustment.

In Figure 3 we report results of the effects of school disruptions in the last 30 days on measures of these various mechanisms. The main finding is that school closures increase the probability of working mostly from home by 19 percentage points, more than doubling the average working-from-home rate of 13 percent in the control groups. The size of this effect increases over time. We also find some weak evidence that school disruptions decrease respondent reports of long-term health conditions limiting their work, though for this outcome there are doubts regarding parallel trends. We found no effect of any of the other mechanisms examined.

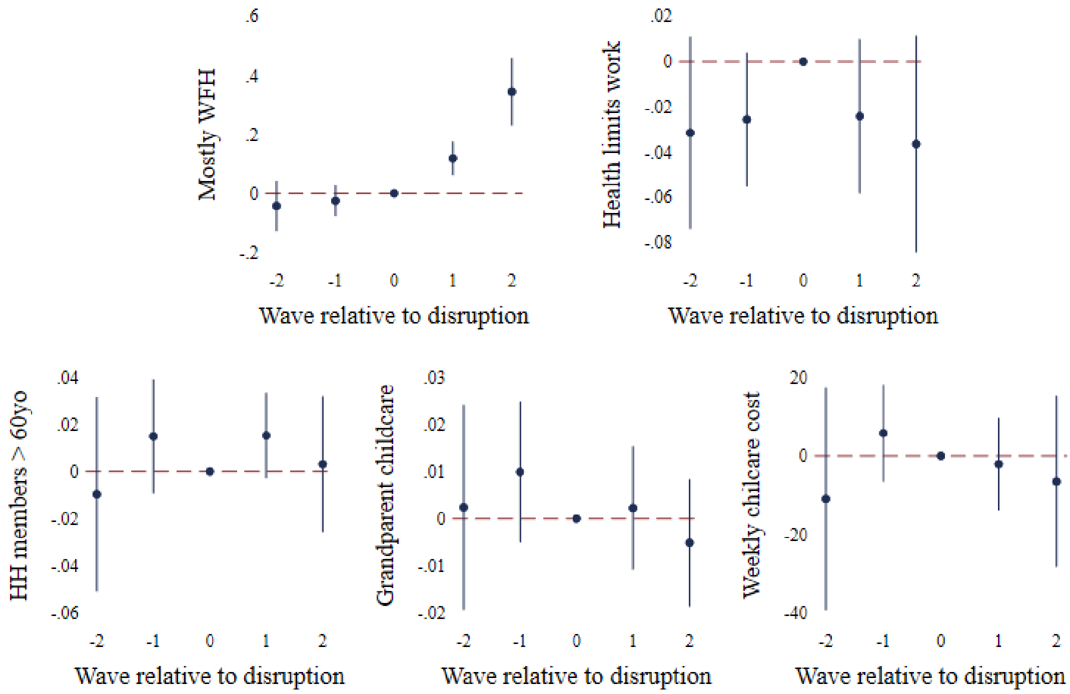

Fig. 3. Event study of the impact of school and day care disruptions on parental labor supply and earnings

Difference-in-Differences intertemporal treatment effect estimates of experiencing a school disruption in the last 30 days relative to outcomes in the last non-disrupted HILDA wave (t=0). Estimates are produced using the Stata command did_multiplegt_dyn *from de Chaisemartin and D'Haultfeuille (2024), as described in Section III.C. Underlying estimates are also reported in Online Appendix Table A15. Spikes show 95 percent confidence intervals based on cluster-robust standard errors at the group level.*

V. Discussion

Our findings paint a consistent picture. We find clear and robust evidence that government-imposed school disruptions had a marked negative impact on parents' labor force participation. These effects are much larger for women and for people more loosely attached to the labor force. We find little evidence of effects on hours worked or earnings. We also show clear evidence that affected parents sharply increased their propensity to mostly work from home. This suggests that (i) school disruptions could have been even more devastating for labor force participation without this margin of adjustment, and (ii) the flexibility to work from home could have countered potential negative effects of disruptions on working hours and wages. Some of these findings might then be interpreted as evidence that policies introduced by the Australian Government with a view to countering the potential adverse effects of the pandemic on labor supply, such as its signature JobKeeper wage subsidy scheme (Cassells and Duncan 2020) and the quick and decisive mass encouragements to work from home, were highly effective.

Despite the many strengths of the data used, there are some limitations that likely impact our analysis. Most obvious is the hours worked variable and the fact that it is based on hours worked in a 'usual' week. It is unclear how respondents whose hours were temporarily cut during the pandemic responded to such a question. Did they report the hours worked in a usual week pre-pandemic or the hours typically being worked at the time of interview? We thus expect that, at least in wave 20 (i.e., in 2020), the number of work hours reported by some respondents will exceed the number of weekly hours actually being worked around the time of the interview, biasing our estimates on hours worked towards zero. Another unfortunate data limitation is the under-representation of recent immigrants, a group often argued to have been more affected by the COVID-19 pandemic restrictions than other groups (e.g., van Barneveld et al. 2020).

We also acknowledge two potential problems with the distinction between essential workers and non-essential workers, which is an important part of our identification strategy. First, since essential worker lists were vaguely worded when released and did not correspond one-to-one with standard industry or occupation classifications, our classification of essential workers will be subject to errors. If these errors are unsystematic, misclassification likely made our treatment and comparison groups more similar to one another than they should be, attenuating our effect estimates. Second, it is unlikely that school restrictions for non-essential workers were applied with full rigor. Ultimately, it is up to school and day care administrators and teachers to determine whether children will be allowed to physically attend. We suspect there was considerable variability across providers in the extent to which government rules were policed and enforced, and that enforcement was more lenient for students attending private schools and those with learning difficulties. This, again, would blur the difference between treatment and comparison groups, attenuating effects. Because of this, we view our estimates as lower bounds of the effects of school closures on labor outcomes.

Finally, we point out that even if it is accepted that school disruptions had a sizable negative effect on the labor supply of affected parents (and especially mothers), the overall impact of school disruptions in Australia during the pandemic has to be small because so few persons were actually affected. Just 6% of persons in our sample were ever affected by school disruptions. Even among those affected, the average number of disrupted days was just 42, which pales compared to the global average of 5.5 months of full and localized schools disruptions (UNESCO 2023). This reflects the fact that many regions within Australia were not affected by school disruptions and, more importantly, that many households were able to avoid the negative effects because of the presence of someone who could claim to be an essential worker.

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Online Appendix A: Supplementary Tables and Figures

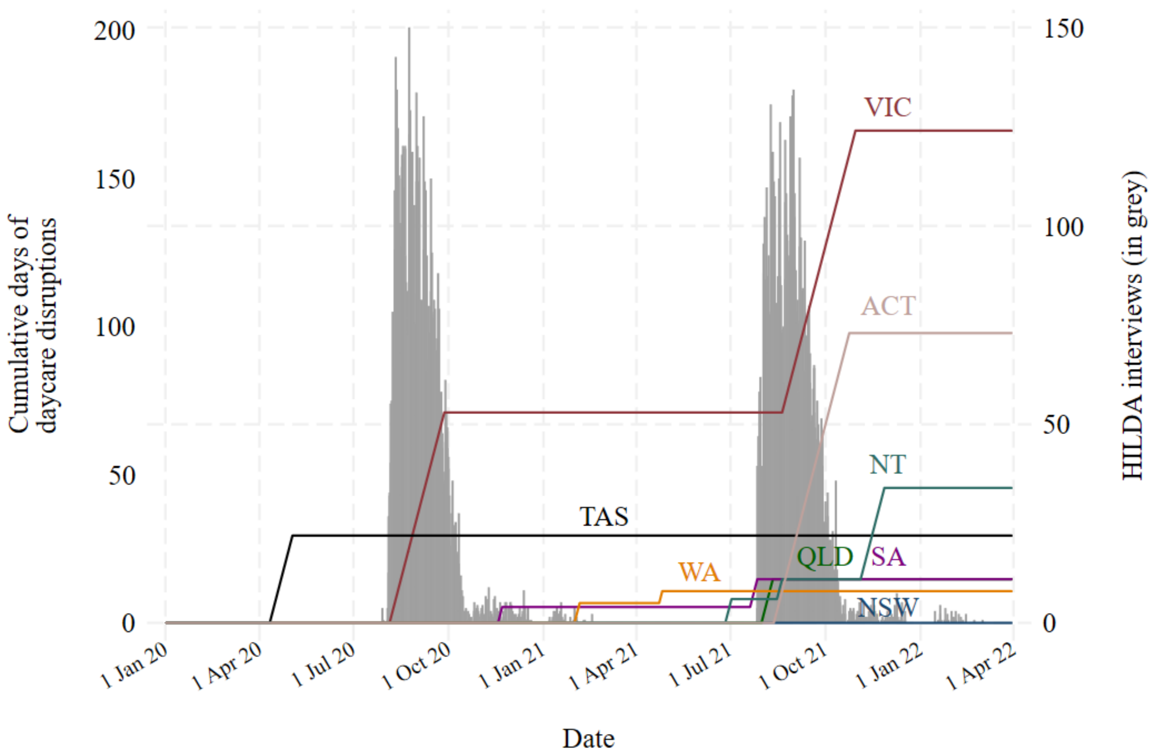

Fig. A1. Across-state variation in cumulative days of daycare disruption, with HILDA Survey interview dates, in HILDA waves 20 and 21

In this figure, states are classified as experiencing daycare disruptions if at least one LGA within the state is experiencing them at any given date. HILDA Survey interview dates represented by the grey columns. ACT = Australian Capital Territory; NSW = New South Wales; NT = Northern Territory; QLD = Queensland; SA = South Australia; TAS = Tasmania; VIC = Victoria; WA = Western Australia.

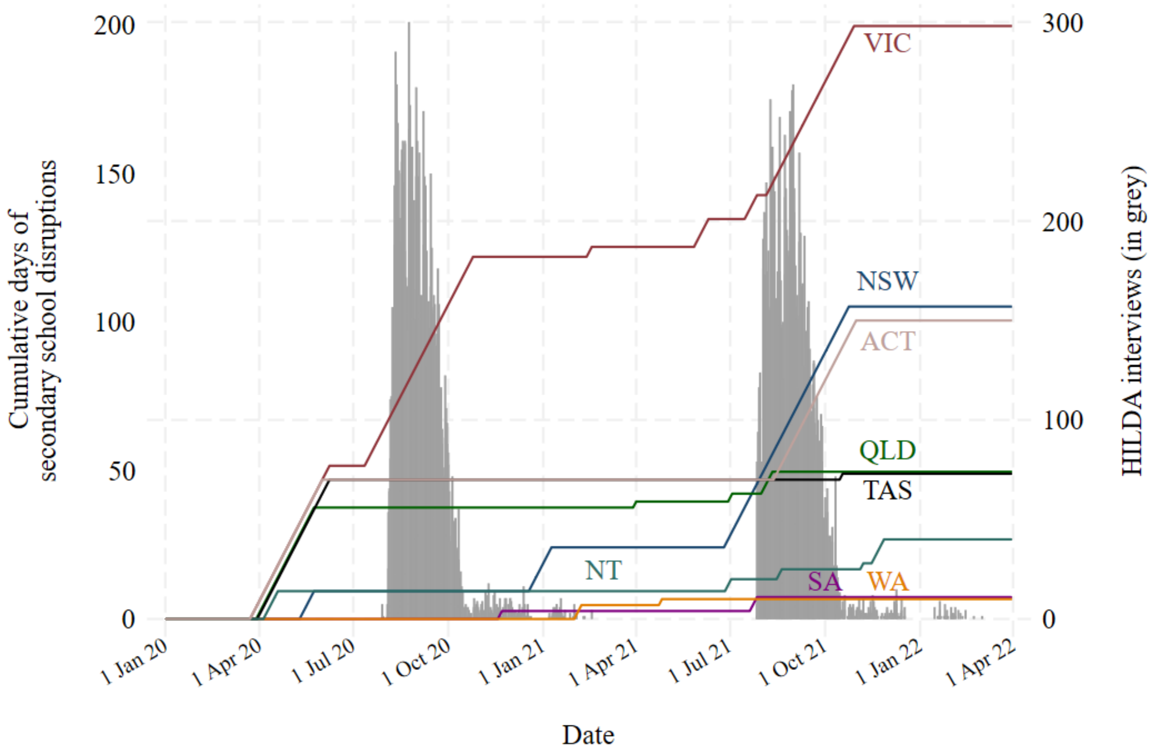

Fig. A2. Across-state variation in cumulative days of secondary school disruption, with HILDA Survey interview dates, in HILDA waves 20 and 21

In this figure, states are classified as experiencing secondary school disruptions if at least one LGA within the state is experiencing them at any given date. HILDA Survey interview dates represented by the grey columns. ACT = Australian Capital Territory; NSW = New South Wales; NT = Northern Territory; QLD = Queensland; SA = South Australia; TAS = Tasmania; VIC = Victoria; WA = Western Australia.

Tab. A1. Summary of Pandemic-Related Stay-at-Home Orders in Australia in 2020 and 2021

State	Start date (D / M / Y)	End date (D / M / Y)	Duration (days)	Geographic coverage
New South Wales	31/03/20	14/05/20	45	Statewide
	19/12/20	09/01/21	22	Northern Sydney beaches
	26/06/21	12/09/21	79	Greater Sydney, Blue Mountains, Central Coast, Wollongong and Shellharbour
	14/08/21	10/09/21	28	Statewide
	11/09/21	11/10/21	31	Regional NSW[(a)]
Victoria	31/03/20	12/05/20	43	Statewide
	09/07/20	02/08/20	25	Metropolitan Melbourne and Mitchell Shire
	02/08/20	05/08/20	4	Metropolitan Melbourne
	06/08/20	16/09/20	42	Statewide
	17/09/20	27/10/20	41	Metropolitan Melbourne
	13/02/21	17/02/21	5	Statewide
	28/05/21	03/06/21	7	Statewide
	03/06/21	10/06/21	8	Metropolitan Melbourne
	15/07/21	27/07/21	13	Statewide
	05/08/21	09/08/21	5	Statewide
	10/08/21	20/08/21	11	Metropolitan Melbourne
	21/08/21	09/09/21	20	Statewide
	10/09/21	21/10/21	42	Metropolitan Melbourne
	10/09/21	17/09/21	8	Shepparton
	16/09/21	22/09/21	7	Ballarat
	20/09/21	26/09/21	7	Greater Geelong and Surf Coast
	20/09/21	13/10/21	24	Mitchell Shire
	29/09/21	05/10/21	7	City of Latrobe
	02/10/21	08/10/21	7	Shepparton and Moorabool Shire
	09/10/21	21/10/21	13	Mildura Rural City Council
Queensland	29/03/20	01/05/20	34	Statewide
	08/01/21	11/01/21	4	Greater Brisbane region
	29/03/21	01/04/21	4	Greater Brisbane region
	29/06/21	03/07/21	5	Greater Brisbane region
	29/06/21	02/07/21	4	SE Qld, Townsville and Palm Island
	31/07/21	11/08/21	12	Greater Brisbane region
	31/07/21	08/08/21	9	SE Queensland
	08/08/21	11/08/21	4	Cairns and Yarrabah Aboriginal Shire
South Australia	19/11/20	21/11/20	3	Statewide
	20/07/21	27/07/21	8	Statewide
Western Australia	31/01/21	05/02/21	6	Greater Perth region, Peel and Southwest
	24/04/21	26/04/21	3	Greater Perth region and Peel
	29/06/21	02/07/21	4	Greater Perth region and Peel
Tasmania	12/04/20	03/05/20	22	Northwest Coast

State	Start date (D / M / Y)	End date (D / M / Y)	Duration (days)	Geographic coverage
	15/10/21	18/10/21	4	Southern region
Northern Territory	27/06/21	02/07/21	6	Greater Darwin region
	30/06/21	02/07/21	3	Alice Spring
	16/08/21	19/08/21	4	Greater Darwin region
	16/08/21	20/08/21	5	Katherine
	05/11/21	07/11/21	3	Katherine
	15/11/21	27/11/21	13	Katherine
ACT	12/09/21	15/10/21	34	Statewide

(a) Not reported here is the considerable variability across LGAs in regional NSW in the timing of lockdowns over this period.

Tab. A2. Means of Key Variables Across Subgroups and Periods in our Estimation Sample

	Essential worker in household during pandemic			
	No essential workers in household		**Essential worker in household with restrictions**	
Pandemic:	**Pre**	**Post**	**Pre**	**Post**
Outcome variables				
In the labor force	0.65	0.59	0.88	0.88
Weekly hours in all jobs (cond. on employed)	36.8	36.8	37.8	36.8
Weekly current gross wages (cond. on employee)	$1,585	$1,742	$1,597	$1,636
Treatment variables				
Any daycare/school disruptions in last 30 days	n/a	0.36	n/a	n/a
# days of daycare/school disruptions in last 30 days (cond. on any)	n/a	13.7	n/a	n/a
Exposure to school disruptions (based on location and interview date)				
# days exposed to daycare restrictions in last 30 days	n/a	2.38	n/a	2.65
# days exposed to primary school restrictions in last 30 days	n/a	5.61	n/a	6.87
# days exposed to secondary school restrictions in last 30 days	n/a	5.62	n/a	6.88
Key covariates for treatment assignment				
Resident children:				
Day care age (0-4)	0.48	0.47	0.41	0.42
Primary school age (5-11)	0.51	0.58	0.48	0.49
Secondary school age (12-17)	0.18	0.30	0.28	0.38
Essential worker status:				
Not-essential	0.47	0.51	0.24	0.21
Essential	0.11	n/a	0.62	0.66
Other identification covariates				
Any stay-at-home restrictions in last 30 days	n/a	0.4	n/a	0.46
# days in stay-at-home restrictions in last 30 days	n/a	20.79	n/a	21.76
Potential mechanisms				
Working from home ≥ 50% main job	0.12	0.33	0.06	0.23
Childcare cost while parents work ($/week)	$18	$21	$19	$23
Long-term health condition that limits work	0.18	0.2	0.08	0.1
Adults in household (60+)	0.06	0.09	0.04	0.05
Grandparents help in providing childcare	0.02	0.01	0.02	0.01
Other covariates				
Female	0.61	0.62	0.54	0.55
Age	37.07	39.41	37.75	40.01

	Essential worker in household during pandemic			
	No essential workers in household		Essential worker in household with restrictions	
Pandemic:	**Pre**	**Post**	**Pre**	**Post**
Indigenous and/or Torres Strait Islander	0.09	0.1	0.04	0.04
Country of birth:				
Australia	0.83	0.83	0.80	0.80
Main English-speaking country	0.06	0.06	0.08	0.08
Another country	0.11	0.11	0.12	0.12
Highest level of education attained:				
Year 11 or below	0.21	0.20	0.11	0.10
Year 12	0.16	0.16	0.13	0.12
Certificate or diploma	0.37	0.38	0.38	0.38
University degree or higher	0.26	0.26	0.39	0.39
Members in the household:				
Adults in household (15+)	2.02	2.04	2.25	2.39
Health status in the household:				
Long-term health condition in household	0.02	0.02	0.02	0.02
Unemployment rate of previous month (based on location and interview date)	5.50	6.04	5.39	5.94
COVID-19 cases (30 days average)	n/a	8.76	n/a	5.14
Obs. (person-year)	2,760	1,869	11,879	8,063

(a) Conditional on employment.
(b) Conditional on wage and salary earner employment. Values are adjusted for price inflation (set to March quarter 2022 levels) using the Wage Price Index (ABS 2023).
(c) Canada, Ireland, New Zealand, South Africa, UK and the USA. (d) Conditional on value of government income support being non-zero.

Tab. A3. The Effects of Any School Disruptions in the last 30 Days on Labor Market Outcomes of Parents

Outcome:	Effects of any school disruption in last 30 days on: In labor force	Weekly hours worked (cond. on employed)	Weekly wages (cond. on employee)
Average total effect	-0.049***	-0.121	55.451
	(0.018)	(0.784)	(62.940)
Effect in t = 2	-0.051	0.062	-92.638
	(0.035)	(1.034)	(117.861)
Effect in t = 1	-0.042***	-0.163	103.232*
	(0.015)	(0.755)	(55.251)
Placebo effect in t = -1	0.011	-0.406	5.666
	(0.017)	(0.571)	(45.438)
Placebo effect in t = -2	0.028	1.906**	166.040
	(0.031)	(0.873)	(283.654)
Joint nullity of placebo (p-value)	0.351	0.025	0.603
Avg. in control (g,t) cells	0.86	37.4	$1,614
Obs. average total effect	9,503	7,589	6,212
Switchers avg. total effect	700	355	255

Difference-in-Differences intertemporal treatment effect estimates of experiencing a school disruption in the last 30 days relative to outcomes in the last non-disrupted HILDA wave (t=0). Estimates are produced using the Stata command `did_multiplegt_dyn` *from de Chaisemartin and D'Haultfeuille (2024), as described in Section III.C. Cluster-robust standard errors at the treatment group level in parentheses. ***, ** and * mark estimates statistically different from zero at the 90, 95 and 99 percent confidence level.*

Tab. A4. Heterogeneous Effects of School Disruptions Across Parent Sex

	Effects of any school disruption in last 30 days on		
Outcome:	**In labor force**	**Hours worked**	**Wages**
		for men:	
Average total effect	-0.034*	0.126	193.177**
	(0.021)	(1.089)	(93.129)
Effect in t = 2	0.008	1.595	193.490
	(0.040)	(1.567)	(150.260)
Effect in t = 1	-0.043**	-0.343	164.596*
	(0.019)	(1.064)	(89.827)
Joint nullity of placebo (p-value)	0.343	0.028	0.418
Avg. in control (g,t) cells	0.95	43.5	$2,005
Obs. average total effect	4,197	3,758	2,937
Switchers avg. total effect	258	183	129
		for women:	
Average total effect	-0.057**	-0.595	1.303
	(0.026)	(1.032)	(72.603)
Effect in t = 2	-0.088*	-1.403	-164.191
	(0.049)	(1.502)	(109.602)
Effect in t = 1	-0.040*	-0.267	56.912
	(0.021)	(1.030)	(67.387)
Joint nullity of placebo (p-value)	0.834	0.661	0.174
Avg. in control (g,t) cells	0.78	31.2	$1,253
Obs. average total effect	5,291	3,783	3,222
Switchers avg. total effect	428	164	119

Difference-in-Differences intertemporal treatment effect estimates of experiencing a school disruption in the last 30 days relative to outcomes in the last non-disrupted HILDA wave (t=0). Estimates are produced using the Stata command `did_multiplegt_dyn` *from de Chaisemartin and D'Haultfeuille (2024), as described in Section III.C. Cluster-robust standard errors at the treatment group level in parentheses. ***, ** and * mark estimates statistically different from zero at the 90, 95 and 99 percent confidence level.*

Tab. A5. Heterogeneous Effects of School Disruptions Across Marital Status

Outcome:	Effects of any school disruption in last 30 days on		
	In labor force	Hours worked	Weekly wages
		for singles:	
Average total effect	-0.076*	-1.123	28.281
	(0.043)	(2.363)	(117.056)
Effect in t = 2	-0.088	-3.025	-149.904
	(0.081)	(3.188)	(168.632)
Effect in t = 1	-0.065*	-0.129	115.017
	(0.037)	(2.768)	(130.046)
Joint nullity of placebo (p-value)	0.798	0.683	0.002
Avg. in control (g,t) cells	0.78	34.3	$1,281
Obs. average total effect	1,164	714	601
Switchers avg. total effect	189	50	36
		for partnered:	
Average total effect	-0.039*	-0.461	20.613
	(0.020)	(0.869)	(72.478)
Effect in t = 2	-0.041	0.471	-82.181
	(0.036)	(1.069)	(140.528)
Effect in t = 1	-0.034*	-0.690	54.390
	(0.017)	(0.821)	(61.459)
Joint nullity of placebo (p-value)	0.230	0.087	0.819
Avg. in control (g,t) cells	0.87	37.7	$1,651
Obs. average total effect	8,190	6,784	5,537
Switchers avg. total effect	469	293	213

Difference-in-Differences intertemporal treatment effect estimates of experiencing a school disruption in the last 30 days relative to outcomes in the last non-disrupted HILDA wave (t=0). Estimates are produced using the Stata command `did_multiplegt_dyn` *from de Chaisemartin and D'Haultfeuille (2024), as described in Section III.C. Cluster-robust standard errors at the treatment group level in parentheses. ***, ** and * mark estimates statistically different from zero at the 90, 95 and 99 percent confidence level.*

Tab. A6. Heterogeneous Effects of School Disruptions Across Labor Force Attachment

	Effects of any school disruption in last 30 days on:		
Outcome:	**In labor force**	**Hours worked**	**Weekly wages**
	for people with higher pre-pandemic labor force attachment:		
Average total effect	-0.049**	-0.187	57.566
	(0.020)	(0.788)	(67.463)
Effect in t = 2	-0.048	-0.120	-69.698
	(0.034)	(1.081)	(125.794)
Effect in t = 1	-0.045**	-0.189	100.413
	(0.019)	(0.798)	(62.767)
Joint nullity of placebo (p-value)	0.846	0.319	0.808
Avg. in control (g,t) cells	0.98	38.7	$1,695
Obs. average total effect	6,901	6,481	5,313
Switchers avg. total effect	347	303	218
	for people with lower pre-pandemic labor force attachment:		
Average total effect	-0.111***	-2.287	-112.139
	(0.032)	(2.262)	(152.643)
Effect in t = 2	-0.149**	-2.701	-268.313
	(0.062)	(4.241)	(293.377)
Effect in t = 1	-0.086***	-1.758	-56.715
	(0.026)	(1.797)	(112.813)
Joint nullity of placebo (p-value)	0.121	0.012	0.434
Avg. in control (g,t) cells	0.49	27.9	$1,021
Obs. average total effect	2,592	1,021	822
Switchers avg. total effect	339	41	31

Difference-in-Differences intertemporal treatment effect estimates of experiencing a school disruption in the last 30 days relative to outcomes in the last non-disrupted HILDA wave (t=0). Estimates are produced using the Stata command `did_multiplegt_dyn` *from de Chaisemartin and D'Haultfeuille (2024), as described in Section III.C. Cluster-robust standard errors at the treatment group level in parentheses. ***, ** and * mark estimates statistically different from zero at the 90, 95 and 99 percent confidence level.*

Tab. A7. Heterogeneous Effects of School Disruptions Across School Levels

Outcome:	Effects of any school disruption in the last 30 days on: In labor force	Hours worked	Weekly wages
Average total effect for disruptions:			
For HHs with daycare age children	-0.003	-2.934	-105.065
	(0.075)	(2.507)	(142.260)
Joint nullity of placebo (p-value)	0.127	0.443	0.140
For HHs with primary school age children	-0.033	-0.020	-53.148
	(0.033)	(1.133)	(96.047)
Joint nullity of placebo (p-value)	0.485	0.000	0.250
For HHs with secondary school age children	-0.051	5.052**	314.495
	(0.035)	(2.561)	(255.965)
Joint nullity of placebo (p-value)	0.132	0.669	0.191
For HHs with children across schooling ages	-0.075***	-0.952	111.283
	(0.026)	(1.073)	(100.409)
Joint nullity of placebo (p-value)	0.441	0.115	0.821

Difference-in-Differences intertemporal treatment effect estimates of experiencing a school disruption in the last 30 days relative to outcomes in the last non-disrupted HILDA wave (t=0). Estimates are produced using the Stata command `did_multiplegt_dyn` *from de Chaisemartin and D'Haultfeuille (2024), as described in Section III.C. Cluster-robust standard errors at the treatment group level in parentheses. ***, ** and * mark estimates statistically different from zero at the 90, 95 and 99 percent confidence level.*

Tab. A8. Effects of School Disruptions Using Two-Way Fixed Effects Models

Panel A: In labor force					
Any school disruption in last 30 days	-0.074***	-0.070***	-0.067***	-0.076***	-0.067***
	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)
Outcome mean	0.84	0.84	0.84	0.84	0.84
Obs.	23,320	23,320	23,304	23,244	23,225
Panel B: Weekly hours worked (cond. on employed)					
Any school disruption in last 30 days	-0.485	0.181	-0.617	0.274	-0.533
	(0.737)	(0.754)	(0.740)	(0.797)	(0.746)
Outcome mean	37.40	37.40	37.50	37.40	37.50
Obs.	18,753	18,753	18,577	18,689	18,507
Panel C: Weekly wages (cond. on employee)					
Any school disruption in last 30 days	40.706	15.120	-17.580	23.150	-13.201
	(56.573)	(56.133)	(50.297)	(57.988)	(52.005)
Outcome mean	\$1,626	\$1,626	\$1,636	\$1,627	\$1,637
Obs.	15,573	15,573	15,297	15,502	15,222
Treatment group FE	✓	✓		✓	
Wave FE	✓	✓	✓		
Controls		✓	✓	✓	✓
Person FE			✓		✓
Interview date FE				✓	✓

*Ordinary Least Squares regression estimates of experiencing a school disruption in the last 30 days. Cluster-robust standard errors at the treatment group level in parentheses. ***, ** and * mark estimates statistically different from zero at the 90, 95 and 99 percent confidence level.*

Tab. A9. Effects of School Disruptions Controlling for Covariates

	Effects of any school disruption in last 30 days on:		
Outcome:	**In labor force**	**Weekly hours worked (cond. on employed)**	**Weekly wages (cond. on employee)**
Average total effect	-0.046**	0.172	71.111
	(0.018)	(0.778)	(60.471)
Effect in t = 2	-0.047	0.484	-24.229
	(0.035)	(0.971)	(100.074)
Effect in t = 1	-0.040***	0.050	97.304*
	(0.015)	(0.779)	(56.589)
Placebo effect in t = -1	0.014	-0.120	7.556
	(0.017)	(0.591)	(43.453)
Placebo effect in t = -2	0.041	2.659***	278.713
	(0.030)	(0.999)	(260.641)
Controls	✓	✓	✓
Joint nullity of placebo (p-value)	0.351	0.021	0.561
Avg. in control (g,t) cells	0.86	37.4	$1,614
Obs. average total effect	9,503	7,589	6,212
Switchers avg. total effect	700	355	255

Difference-in-Differences intertemporal treatment effect estimates of experiencing a school disruption in the last 30 days relative to outcomes in the last non-disrupted HILDA wave (t=0). Estimates are produced using the Stata command `did_multiplegt_dyn` *from de Chaisemartin and D'Haultfeuille (2024), as described in Section III.C. Cluster-robust standard errors at the treatment group level in parentheses. ***, ** and * mark estimates statistically different from zero at the 90, 95 and 99 percent confidence level.*

Tab. A10. Effects of School Disruptions Using Respondents as Treatment Groups

	Effects of any school disruption in last 30 days on:		
Outcome:	**In labor force**	**Weekly hours worked (cond. on employed)**	**Weekly wages (cond. on employee)**
Average total effect	-0.046**	0.172	71.111
	(0.018)	(0.778)	(60.471)
Effect in t = 2	-0.047	0.484	-24.229
	(0.035)	(0.971)	(100.074)
Effect in t = 1	-0.040***	0.050	97.304*
	(0.015)	(0.779)	(56.589)
Placebo effect in t = -1	0.014	-0.120	7.556
	(0.017)	(0.591)	(43.453)
Placebo effect in t = -2	0.041	2.659***	278.713
	(0.030)	(0.999)	(260.641)
Respondents as g	✓	✓	✓
Joint nullity of placebo (p-value)	0.351	0.021	0.561
Avg. in control (g,t) cells	0.86	37.4	$1,614
Obs. average total effect	9,503	7,589	6,212
Switchers avg. total effect	700	355	255

Difference-in-Differences intertemporal treatment effect estimates of experiencing a school disruption in the last 30 days relative to outcomes in the last non-disrupted HILDA wave (t=0). Estimates are produced using the Stata command `did_multiplegt_dyn` *from de Chaisemartin and D'Haultfeuille (2024), as described in Section III.C. Cluster-robust standard errors at the treatment group level in parentheses. ***, ** and * mark estimates statistically different from zero at the 90, 95 and 99 percent confidence level.*

Tab. A11. Effects of School Disruptions including Non-Parents in Never-Treated

	Effects of any school disruption in last 30 days (incl. non-parents in never-treated groups) on:		
Outcome:	**In labor force**	**Weekly hours worked (cond. on employed)**	**Weekly wages (cond. on employee)**
Average total effect	-0.039**	-0.121	55.451
	(0.018)	(0.784)	(62.940)
Effect in t = 2	-0.038	0.062	-92.638
	(0.034)	(1.034)	(117.861)
Effect in t = 1	-0.035**	-0.163	103.232*
	(0.015)	(0.755)	(55.251)
Placebo effect in t = -1	0.009	-0.406	5.666
	(0.017)	(0.571)	(45.438)
Placebo effect in t = -2	0.026	1.906**	166.040
	(0.031)	(0.873)	(283.654)
Joint nullity of placebo (p-value)	0.666	0.025	0.603
Avg. in control (g,t) cells	0.74	37.4	$1,614
Obs. average total effect	27,769	7,589	6,212
Switchers avg. total effect	700	355	255

Difference-in-Differences intertemporal treatment effect estimates of experiencing a school disruption in the last 30 days relative to outcomes in the last non-disrupted HILDA wave (t=0). Estimates are produced using the Stata command `did_multiplegt_dyn` *from de Chaisemartin and D'Haultfeuille (2024), as described in Section III.C. Cluster-robust standard errors at the treatment group level in parentheses. ***, ** and * mark estimates statistically different from zero at the 90, 95 and 99 percent confidence level.*

Tab. A12. Effects of School Disruptions excluding Essential Workers from Never-Treated

Outcome:	Effects of any school disruption in last 30 days (excl. HHs with essential workers from never-treated groups) on:		
	In labor force	**Weekly hours worked (cond. on employed)**	**Weekly wages (cond. on employee)**
Average total effect	-0.019	-0.522	16.750
	(0.018)	(0.830)	(65.802)
Effect in t = 2	-0.001	-0.618	-148.581
	(0.035)	(1.104)	(121.284)
Effect in t = 1	-0.022	-0.423	75.921
	(0.015)	(0.790)	(57.352)
Placebo effect in t = -1	-0.019	-0.104	24.296
	(0.018)	(0.635)	(49.757)
Placebo effect in t = -2	-0.016	2.071**	91.568
	(0.031)	(0.962)	(286.767)
Joint nullity of placebo (p-value)	0.547	0.096	0.855
Avg. in control (g,t) cells	0.38	34.9	$1,359
Obs. average total effect	7,826	2,533	1,807
Switchers avg. total effect	700	355	255

Difference-in-Differences intertemporal treatment effect estimates of experiencing a school disruption in the last 30 days relative to outcomes in the last non-disrupted HILDA wave (t=0). Estimates are produced using the Stata command `did_multiplegt_dyn` *from de Chaisemartin and D'Haultfeuille (2024), as described in Section III.C. Cluster-robust standard errors at the treatment group level in parentheses. ***, ** and * mark estimates statistically different from zero at the 90, 95 and 99 percent confidence level.*

Tab. A13. Effects of School Disruptions excluding Households with Children in Secondary School Only

Outcome:	**Effects of any school disruption in last 30 days (excl. HHs with children in secondary school only) on:**		
	In labor force	**Weekly hours worked (cond. on employed)**	**Weekly wages (cond. on employee)**
Average total effect	-0.049**	-0.968	19.654
	(0.021)	(0.837)	(65.284)
Effect in t = 2	-0.064*	-0.295	-119.681
	(0.039)	(1.073)	(125.162)
Effect in t = 1	-0.038**	-1.086	69.082
	(0.017)	(0.828)	(59.976)
Placebo effect in t = -1	0.020	-0.722	-8.348
	(0.020)	(0.628)	(49.646)
Placebo effect in t = -2	0.015	1.873**	234.973
	(0.036)	(0.948)	(320.464)
Joint nullity of placebo (p-value)	0.614	0.070	0.741
Avg. in control (g,t) cells	0.74	36.4	$1,467
Obs. average total effect	7,584	5,994	4,972
Switchers avg. total effect	578	298	225

Difference-in-Differences intertemporal treatment effect estimates of experiencing a school disruption in the last 30 days relative to outcomes in the last non-disrupted HILDA wave (t=0). Estimates are produced using the Stata command `did_multiplegt_dyn` *from de Chaisemartin and D'Haultfeuille (2024), as described in Section III.C. Cluster-robust standard errors at the treatment group level in parentheses. ***, ** and * mark estimates statistically different from zero at the 90, 95 and 99 percent confidence level.*

Tab. A14. Effects of School Disruptions Across Different Exposure Windows

	Effects of any school disruption (across different exposure windows) on:		
Outcome:	**In labor force**	**Hours worked**	**Weekly wages**
Average total effect for disruptions:			
In the last 60 days	-0.049***	-0.121	55.451
	(0.018)	(0.784)	(62.940)
Joint nullity of placebo (p-value)	0.609	0.064	0.841
In the last 90 days	-0.050***	-0.123	43.451
	(0.016)	(0.710)	(55.586)
Joint nullity of placebo (p-value)	0.657	0.060	0.795
Since the start of the pandemic	-0.034***	1.084**	17.118
	(0.012)	(0.534)	(42.046)
Joint nullity of placebo (p-value)	0.910	0.418	0.816

Difference-in-Differences intertemporal treatment effect estimates of experiencing a school disruption in the last 30 days relative to outcomes in the last non-disrupted HILDA wave (t=0). Estimates are produced using the Stata command `did_multiplegt_dyn` *from de Chaisemartin and D'Haultfeuille (2024), as described in Section III.C. Cluster-robust standard errors at the treatment group level in parentheses. ***, ** and * mark estimates statistically different from zero at the 90, 95 and 99 percent confidence level.*

Tab. A15. Effects of Experiencing School Disruptions on Potential Mechanisms

	Effects of any school disruption in last 30 days on:				
Outcome:	**Mostly WFH**	**Health limits work**	**HH members > 60yo**	**Grand-parent childcare**	**Weekly childcare cost**
Average total effect	0.192***	-0.030*	0.014	0.000	-3.547
	(0.033)	(0.018)	(0.010)	(0.007)	(6.801)
Effect in t = 2	0.345***	-0.036	0.003	-0.005	-6.520
	(0.058)	(0.024)	(0.015)	(0.007)	(11.114)
Effect in t = 1	0.119***	-0.024	0.015*	0.002	-2.122
	(0.029)	(0.017)	(0.009)	(0.007)	(5.996)
Placebo effect in t = -1	-0.025	-0.026*	0.015	0.010	5.760
	(0.027)	(0.015)	(0.012)	(0.008)	(6.306)
Placebo effect in t = -2	-0.043	-0.031	-0.010	0.002	-10.967
	(0.044)	(0.022)	(0.021)	(0.011)	(14.439)
Joint nullity of placebo (p-value)	0.487	0.101	0.364	0.429	0.390
Avg. in control (g,t) cells	0.13	0.10	0.05	0.02	$20
Obs. average total effect	7,578	9,492	9,503	9,503	9,503
Switchers avg. total effect	355	699	700	700	700

Difference-in-Differences intertemporal treatment effect estimates of experiencing a school disruption in the last 30 days relative to outcomes in the last non-disrupted HILDA wave (t=0). Estimates are produced using the Stata command `did_multiplegt_dyn` *from de Chaisemartin and D'Haultfeuille (2024), as described in Section III.C. Cluster-robust standard errors at the treatment group level in parentheses. ***, ** and * mark estimates statistically different from zero at the 90, 95 and 99 percent confidence level.*

Tab. A16. Effects of School Disruptions on Mechanisms Using Two-Way Fixed Effects

Panel A: Mostly WFH					
Any school disruption in last 30 days	0.181***	0.095***	0.088**	0.088***	0.082**
	(0.034)	(0.034)	(0.034)	(0.034)	(0.033)
Outcome mean	0.14	0.14	0.14	0.14	0.14
Obs.	18,748	18,748	18,574	18,685	18,505
Panel B: Health limits work					
Any school disruption in last 30 days	-0.017	-0.020	-0.009	-0.019	-0.008
	(0.013)	(0.013)	(0.013)	(0.014)	(0.013)
Outcome mean	0.11	0.11	0.11	0.11	0.11
Obs.	23,300	23,300	23,283	23,226	23,206
Panel C: HH members > 60yo					
Any school disruption in last 30 days	0.013	0.009	0.007	0.013	0.008
	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)
Outcome mean	0.05	0.05	0.05	0.05	0.05
Obs.	23,320	23,320	23,304	23,244	23,225
Panel D: Grandparent childcare					
Any school disruption in last 30 days	-0.002	-0.003	-0.004	-0.001	-0.002
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Outcome mean	0.02	0.02	0.02	0.02	0.02
Obs.	23,320	23,320	23,304	23,244	23,225
Panel E: Weekly childcare cost					
Any school disruption in last 30 days	-4.500	-5.146	-6.040	-6.175	-7.042
	(5.999)	(6.464)	(6.573)	(6.664)	(6.617)
Outcome mean	$21	$21	$21	$21	$21
Obs.	23,320	23,320	23,304	23,244	23,225
Treatment group FE	✓	✓		✓	
Wave FE	✓	✓	✓		

Controls	✓	✓	✓	✓
Person FE		✓		✓
Interview date FE			✓	✓

*Ordinary Least Squares regression estimates of experiencing a school disruption in the last 30 days. Cluster-robust standard errors at the treatment group level in parentheses. ***, ** and * mark estimates statistically different from zero at the 90, 95 and 99 percent confidence level.*

Online Appendix B: Variable Definition

A list of all variables used in the analysis, along with mean values for the estimation sample and relevant sub-samples, is provided in Online Appendix Table A2. As can be seen, we consider three different outcome variables: (i) labor force participation (1 = either employed or unemployed during the week preceding interview, and 0 if otherwise[8]); (ii) the number of hours usually worked per week in all jobs (conditional on being employed); (iii) current usual weekly gross (i.e., before tax) earnings from wages and salaries (conditional on wage and salary earner employment).[9] Outcome variable (i) measures the impact of school disruptions on the extensive margin of labor whereas outcome variables (ii) through (iii) measure the effect at the intensive margin.

Our main measure of the treatment ($Disrupted$) is a binary variable that conceptually identifies persons who, at the time of the interview, had ever been affected by either restrictions on teaching mode within schools or the presence of government recommendations not to use formal child care (i.e., day care centers), which we collectively refer to as school disruptions. The presence of school disruptions is determined by the presence of any resident children of daycare age (0 to 4 years old), primary school age (5 to 11 years old), or secondary school age (12 to 17 years old) during the pandemic and is also determined by whether any member of the household was classified as an 'essential worker' during the pandemic. The coding of school disruptions also considers variations in exposure across locations (i.e., LGAs) and over time (as measured by interview date). Households with no essential workers are assigned to the

[8] The definitions of employment and unemployment used in the HILDA Survey follow ILO conventions. Employment thus includes persons who did not work during the one-week reference period because of a temporary absence (e.g., sickness or holidays), while unemployment requires both active job search and being available to work.

[9] As per ABS practice, wage and salary earners include both employees and owner managers of incorporated businesses.

treatment group if, in their LGA of residence and at the time of survey, there have been disruptions in relevant school years according to their children's age. Thus, a respondent with a 7-year-old child will be coded as treated by school disruptions if, at the time of survey and in their LGA of residence, there have been disruptions to primary schools. A respondent will no longer be coded to the treatment group if at time *t* they were residing in an LGA not affected at that time by disruptions to schooling or day care. Furthermore, respondents living in affected LGAs will only be coded to the treatment group if the restrictions in place apply to the type of school or child care provider that any of their children could potentially be attending.[10]

For mechanisms we used five variables: (i) a binary variable indicating whether the proportion of hours worked from home is more than 50%; that is, whether the number of hours usually worked each week from home in the main job exceeds 50% of the total usual weekly working hours in the main job; (ii) an indicator of whether a person has a health condition that limits working abilities partially or completely; (iii) the number of household members who are aged 60 years or more at June 30 of the interview year; (iv) a binary variable indicating whether grandparents are cohabiting and providing childcare support; and (v) the weighted average of weekly childcare expenditure ($) for school-going and non-school-going children during term time while parents work. To adjust for differences in individual characteristics, we include controls for gender (0 = male, 1 = female), age, Australian indigeneity (0 = non-Indigenous Australian, 1 = Indigenous Australian), country of birth (1 = Australia, 2 = main English-speaking country, 3 = other country), and educational attainment (1 = year 11 or below, 2= Year 12, 3 = graduate diploma/certificate, 4 = University degree or higher). In our models, we also

[10] An alternative treatment variable we consider measures the cumulative number of days up to the date of interview that a respondent has been affected by these restrictions. The construction of this variable is analogous to the binary variable, and thus is also determined by location, presence and age of children, and whether essential workers are present in the household. This variable provides a measure of the intensity of treatment.

include if respondents were exposed to stay-at-home orders in last 30 days at the time of the interview, which varies based on both the date of the interview and location. In theory, the inclusion of this variable is needed to isolate the effect of school disruptions from any broader effect of lockdowns. At the same time, we recognize that this variable might be expected to be highly correlated with the treatment variable. As it turns out this is not true: The simple correlation with our treatment variable is just 0.35. This simply reflects the fact that while most Australians were exposed to stay-at-home orders at some point during the pandemic, relatively few were directly affected by school disruptions, either because they were not parents or because someone in the household was an essential worker. This also highlights how our identification strategy allows us to disentangle the effects of school disruptions from the broader effect of the pandemic from other pandemic-related policies.

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