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The Employment Effects of a Pandemic Wage Subsidy

Abstract

We estimate the causal effects of a pandemic-era wage subsidy program in Canada on job losses and business closures. Our estimates use administrative microdata and a regression discontinuity strategy to estimate the effects of marginal changes in the wage subsidy rate. The estimated net wage elasticity of employment was 0.11, implying a small aggregate employment effect of the program and an estimated fiscal cost per job saved of nearly $200,000 per year. Subsidy payments caused a small but persistent reduction in business closure rates during subsequent waves of the pandemic, and increased earnings of existing employees. In all, our results suggest the subsidies did little to preserve job matches, but played a greater role in the overall social insurance response to the pandemic.

JEL-Codes: H250, E320.

Keywords: Canada emergency wage subsidy, Covid-19, incrementality.

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1 Introduction

In response to the COVID-19 pandemic, many countries around the world engaged in a massive experiment with new labor market and social insurance policies. In Canada, the United States and other countries without existing work-sharing programs, labor market interventions took the form of a broad set of payroll subsidies to private-sector firms,[1] intended to reduce employment losses due to the pandemic and prevent business closures. Economy-wide payroll subsidies were likely attractive to governments during the pandemic because they permitted government assistance to be distributed quickly and broadly to the private sector (Corak, 2021). By the same token, broad payroll subsidies may not be a cost-effective mechanism for preventing job loss, because they are not targeted to employers in the greatest financial distress or jobs at the greatest risk.

This paper evaluates the effects on workers and firms of the Canada Emergency Wage Subsidy (CEWS), the largest component of Canada's social insurance response to the pandemic. Beginning in March 2020, CEWS offered subsidies up to 85 percent of eligible payroll costs to virtually all private-sector employers, at a total fiscal cost of five percent of GDP in the first year of the pandemic. To deal with the targeting problem, CEWS tied subsidy rates to an employer's "revenue drop," defined as the percentage reduction in eligible revenues relative to a pre-pandemic base period. Using administrative microdata, we exploit differences in subsidy rates among employers and over time to estimate the causal effect of subsidy rates on employment and the elasticity of employment with respect to net-of-subsidy wage costs, and the broader effects of the subsidy on workers and firms.

Briefly, our results are as follows. First, subsidy rates had small but statistically significant positive impacts on employment, particularly for mid-sized employers and those in service sectors most affected by social distancing restrictions. Because estimated effects are small, much of the spending supported inframarginal jobs that would have existed even in the absence of the subsidies. Based on the estimated elasticities of employment with respect to the net wage, we estimate the number of job-months in employment "saved" by the subsidy during 2020, and the fiscal cost of the program per job saved. From a social insurance perspective, the marginal effects on employment were likely too small to justify the fiscal cost of the program. Second, the subsidies had a small but persistent effect on business closures, reducing closure rates during the second and fourth waves of the pandemic during 2021. While the effect on layoffs through business closures is a separate channel through which employment increased, effects were again small, and the estimated fiscal cost of the program per job saved remains high. Third, we investigate the broader incidence of the subsidies using administrative data on the annual financial statements of (publicly traded and private) companies applying for CEWS. Firms receiving exogenously higher subsidies in 2020 did not report higher net business incomes by the end of the year, nor higher dividend payments to shareholders. Instead, the additional subsidies seem to have resulted in higher payments to suppliers and higher total payroll expenses, reflecting not only the extensive margin impacts on employment, but also intensive-margin increases in part-time work among existing employees.

Because subsidy rates were a mechanical function of revenue drop under the program, they were endogenously determined, reflecting business conditions and other factors facing the firm that simultaneously affected hiring and layoff decisions. To deal with endogeneity, we exploit a "safe harbor" provision of CEWS rules in July and August 2020, which guaranteed employers facing revenue drops

[1] Similar subsidies were introduced in Australia (Hamilton, 2020), and in Estonia, Ireland, Netherlands, and New Zealand (OECD, 2020).

of at least 30 percent sharply higher subsidy rates than those with smaller revenue drops. We use regression discontinuity (RD) methods to estimate the effect of the subsidy on employment at the 30 percent revenue drop threshold. We estimate that obtaining the safe harbor subsidy reduced employment losses by 4.4 log points at the cutoff, implying an elasticity of employment with respect to the net-of-subsidy wage of just 0.11.

This estimate is the local average treatment effect (LATE) of the subsidy rate for firms at the 30 percent eligibility threshold. Assuming that the average treatment effect for all firms is the same, we estimate the overall impact of the program. At the estimated net wage elasticity of 0.11, we estimate that the program saved 3.9 million months of employment during 2020, or about one job per subsidized employer, at an average fiscal cost of $15,602 per month.[2] This cost is large relative to the average salary of $4545 for employees covered by the subsidies, and equals $200,000 on an annualized basis.

Eligibility for the safe harbor depended on firm's revenue for the relevant months. If firms manipulated revenue drops, it might invalidate our inferences on the causal effects of the subsidy. While program administrators relied on firm's self-reported revenue drops to determine eligibility, these reports were subject to audit, and administrators have ready access to information on revenues from the monthly or quarterly value added tax returns remitted to the government by the majority of applicant firms. That said, we do find evidence of bunching of employers at the 30 percent threshold during the safe harbour periods, but not during other periods when there was no discontinuity in subsidy rates there. We also find imbalance in some covariates at the threshold, most notably in firm assets. To deal with manipulation, we employ employ several alternative strategies. We control for covariates and report alternative estimates from "donut RDs" which exclude data from the manipulation region. Most importantly, we focus on the impacts of the safe harbour on the change in log employment relative to twelve months earlier ("job loss") rather than on the level of employment. To the extent that manipulation is correlated with employment only through a firm fixed effect that is unchanging over time, such as firm size, then the RD effect of the safe harbor on job loss is a consistent estimate of the causal effect.

The safe harbor provision expired in September 2020, resulting in a sharp drop in subsidy rates for eligible employers. We therefore also estimate the dynamic treatment effects of the subsidy cut on employment through December 2020, where the treatment group is employers whose safe harbor subsidies were eliminated, and the control group is those with similar initial revenue drops who were ineligible for the safe harbor. In this context, too, revenue drop may be endogenous, and we control for period-specific local linear effects of initial revenue drop on subsequent employment. As such, our estimates are "difference-in-regression discontinuity" (DIRD) estimates, offering more robust inference in this context than traditional difference-in-difference estimates. To support the assumption that selection for treatment is ignorable, in spite of possible manipulation of the assignment variable, we find stable pre-trends in employment for treatment and control groups in the early months of the pandemic leading up to the safe harbor period.

Using the DIRD design, we estimate that expiry of the safe harbor caused employment to drop by 6.9 log points by December at treated firms, relative to the control group, implying net-wage elasticities of employment of 0.08, slightly smaller than the 0.11 estimated with the static RD design during the safe harbor period. While the estimated employment effects were therefore small overall,

[2]In this context, "month" means the four-week periods over which subsidies were calculated, and the estimates apply to claim periods from mid-April to the end of December.

there were greater effects in the food and accommodation and other service sectors, and for larger employers.

Our estimates complement existing evidence on the effects of the Paycheck Protection Program (PPP), the parallel general wage subsidy created during the pandemic in the United States. The PPP literature uses a variety of strategies to estimate its causal effects on employment and business closures.[3] Several researchers instrument PPP eligibility with firm size (since only firms with fewer than 500 employees were generally eligible) and find effects on employment of the same magnitude as we estimate for CEWS (e.g. Chetty et al., 2020; Hubbard and Strain, 2020; Autor et al., 2022a). In particular, Autor et al. (2022a) estimates employment effects of PPP somewhat larger than ours, but a fiscal cost per job saved that is somewhat larger too. A number of other researchers estimate effects from plausibly exogenous differences among regions and firms in the level and timing of PPP loans. Granja et al. (2022) and others (e.g. Faulkender et al., 2020; Bartik et al., 2021) use an event study approach that compares employment changes at firms receiving subsidies at different dates, where the timing of receipt may have reflected procedural delays in PPP as well as other factors. As such, they rely on differences in employment growth among recipients in a relatively brief period during the first wave of the pandemic. These papers find a wider range of estimated employment effects than emerge from comparisons at the 500-employee eligibility threshold, including some that are insignificantly different from a null effect, and some much larger than we estimate.

The PPP literature reports estimates of a different LATE than we estimate for CEWS. Our estimates represent the marginal effect of the subsidy rate at the 30 percent revenue drop cutoff, which was just below the median revenue drop in our sample. Because of the program's design, the PPP literature instead reports reduced-form aggregate impacts of PPP receipt. Because CEWS subsidy rates were explicitly varied among firms, whereas the terms of PPP loans were the same for all, our approach instead permits us to estimate the *marginal* effects of higher subsidy rates and to report results as net wage elasticities. Moreover, the reported effects in Autor et al. (2022a) and others are LATEs at the 500-employee size threshold, which is much larger than most firms receiving CEWS.

The small incremental effects on employment estimated by us for CEWS, and others for PPP, may reflect the unique product and labor market conditions facing firms during the early stages of the pandemic. As such, our estimates are not directly comparable to those for earlier and more targeted wage subsidy programs (e.g. Saez et al., 2019). In this view, pandemic wage subsidies were a social insurance policy rather than an active labor market policy, and their effects might best be judged relative to alternative social insurance programs, such as direct cash transfers to the unemployed and to illiquid firms in danger of closure.[4] In this vein, Hubbard and Strain (2020) refer to PPP as a "small-business revenue replacement program," and social insurance for businesses might have been an unstated policy objective for CEWS as well.

We use administrative data on the annual financial statements of applicants to investigate the broader impacts of the subsidies on firms, and their incidence on business owners and employees. Safe harbor employers received discontinuously greater total subsidies in 2020 than the control group, yielding plausibly exogenous variation in the level of transfers to firms, which we use to identify the broader business impacts. We find that safe harbor applicants were significantly less likely than

[3] The PPP literature was recently surveyed in Autor et al. (2022b).

[4] In addition to its existing unemployment insurance scheme, Canada instituted a new cash transfer to individuals experiencing earnings drops in 2020, which paid amounts equal to around two-thirds of the maximum subsidy per employee paid under CEWS (Cui, 2021). Other programs paid cash transfers to businesses in 2020, but none had a discontinuity in the level of support at the safe harbor threshold of CEWS.

control applicants to be closed in January-February 2021 during the second wave of the pandemic, and again in August-December 2021, when the highly transmissible Delta variant of the virus became dominant in Canada. In this sense, the subsidies seem to have insured business owners against the risk of closure during subsequent shocks. While this is a separate channel through which CEWS "saved" jobs, we estimate that the fiscal cost per job saved through the closure-layoff channel was more than $600,000.

While total subsidies received per employee were sharply higher at the cutoff for safe harbor rules, we find no evidence that net business income nor dividend payments to shareholders rises at the cutoff. This suggests that the additional cash received was paid to suppliers or employees. Indeed, we find that annual payroll expense per employee rises sharply at the cutoff by an amount equal to about 60 percent of the incremental subsidies received. This effect is too large to be explained by the extensive margin impacts on employment or business closures that we have estimated and suggests that there were likely also intensive-margin impacts on hours worked or wage rates among existing employees.

Results from the PPP literature on the incidence of subsidies have been mixed. Hubbard and Strain (2020) find that PPP eligibility increased firms' liquidity and ability to make loan and other recurring payments. Granja et al. (2022) find similar results using regional differences in PPP exposure. But they find only small effects of PPP exposure on business shutdowns, similar in magnitude to our estimates for CEWS. Bartik et al. (2021) find much larger effects of PPP on firms' self-reported subjective probabilities of survival. To our knowledge, no study has yet examined the impact of PPP on incomes of workers and business owners. However, Autor et al. (2022b) simulate the incidence of PPP and show that their estimated extensive-margin employment effects imply that at most 34 percent of PPP funds ultimately went to workers.

The plan of the paper is as follows. Section 2-4 respectively describe CEWS institutions, our regression discontinuity identification strategies, and our data. Section 5 presents static RD estimates of the effects of the safe harbor subsidy, and Section 6 DIRD estimates of the later impacts of its expiry. Section 7 presents estimates of impacts on business closures and employers' financial statements, and Section 8 concludes.

2 The Canada Emergency Wage Subsidy

The Canada Emergency Wage Subsidy (CEWS) was introduced on March 27, 2020. Initially, CEWS paid subsidies equal to 75 percent subsidy of eligible payroll expenses to $1129 per employee per week (about the average industrial wage), with employers applying for the subsidy in a series of consecutive four-week periods that roughly corresponded to calendar months in 2020. Eligible employers were essentially all private-sector businesses, irrespective of size, that had experienced a 15 percent decline in revenues during the pandemic. The threshold revenue decline for eligibility was increased to 30 percent for April through June (periods 2-4).

The program then underwent several changes during 2020, which are central to our identification strategy and so described in detail here. The initial announcement indicated that the program would run from March 15 to June 6, 2020. On May 15, the government extended CEWS to August 29 (for periods 5-6), and announced a process for consulting employers about changes to the program. On July 17, the government again extended the program through December 2020 (period 10). At the same time, it announced changes to subsidy rates, which were retroactive to July 5 (period 5).

Table 1: CEWS parameters

Period	Effective date	Minimum revenue drop	Base subsidy rate	Maximum top-up rate
1	Mar. 15	15%	75%	0%
2-4	Apr. 12	30%	75%	0%
5-6	Jul. 5	0%	1.2R up to 60%	25%
7	Aug. 30	0%	1R up to 50%	25%
8-10	Sept. 27	0%	0.8R up to 40%	25%

Note: R is current period deemed revenue drop.

Under the new rules, all employers experiencing revenue declines were eligible for a base subsidy rate equal to 120 percent of the percentage revenue decline, with slightly higher "top-up" subsidy rates for revenue declines over 50 percent.[5] Base subsidy rates declined gradually in periods 7 and 8. The schedules of subsidy rates and revenue drop eligibility criteria applying in each period of 2020 are summarized in Table 1.

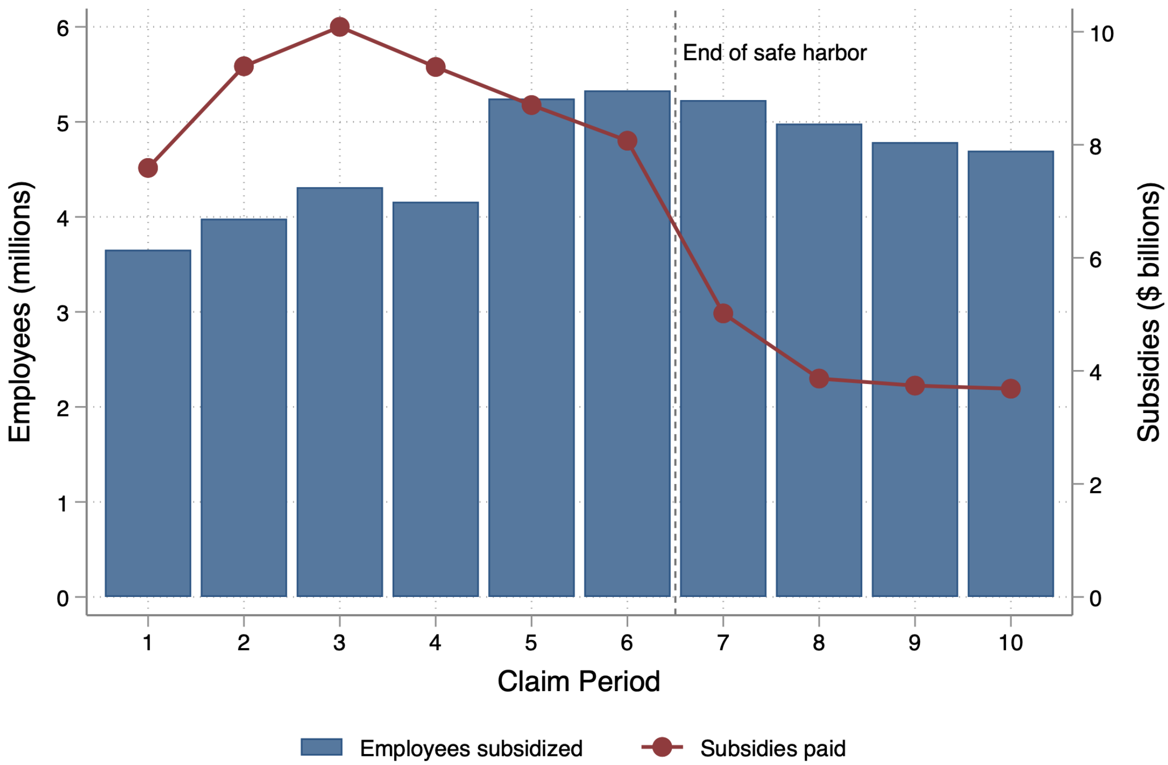
Under the new rules for period 5 and 6, subsidy rates dropped for some employers who had previously been eligible for the flat 75 percent subsidy rate. But those employers were eligible for a safe harbor provision, entitling them in periods 5 and 6 to the 75 percent subsidy rate they would have received under the old formula. The government announced the safe harbour provision would end in September, and that base subsidy rates would decline gradually in periods 7 through 10 (September to December).

The aggregate impacts of the reforms are depicted in Figure 1. With the period 5 expansion of CEWS eligibility, CEWS subsidies were paid for 5.2 million employees, or about 40 percent of pre-pandemic private sector employment. Total subsidies paid peaked at $10 billion in period 3, and declined only gradually through period 6, indicating that newly eligible employers under the more expansive Period 5-6 rules received smaller-than-average subsidies. Total payments drop sharply in period 7 (from $8 billion to $5 billion), reflecting the expiry of the safe harbor and resulting drop in subsidy rates for safe harbor employers.

There was flexibility in how the subsidy was calculated. Employers could elect to calculate revenue drop by comparing current-period revenue to January 2020 or to one year earlier. Moreover, the "deemed revenue drop" for the calculation in each month was the greater of the revenue drops in the current and prior periods. These forms of optionality were deemed necessary to give employers certainty about their subsidies in an environment of changing rules and rapidly changing economic circumstances. Subsidies were delivered each period as a refund paid directly to into the employer's income tax account with the Canada Revenue Agency. Note also that CEWS payments received were generally treated as taxable income for businesses.

[5] Beginning in period 5, the base subsidy applied to revenue drops up to 50 percent, and the top up subsidy was 125 percent of revenue drop in excess of 50 percent.

Figure 1: The evolution of CEWS in 2020



Note: Shows aggregate data for the first 10 claim periods of CEWS, roughly corresponding to the months of March through December 2020.
Source: Public data, Canada Revenue Agency

3 Empirical strategy

3.1 RD and difference-in-RD estimates

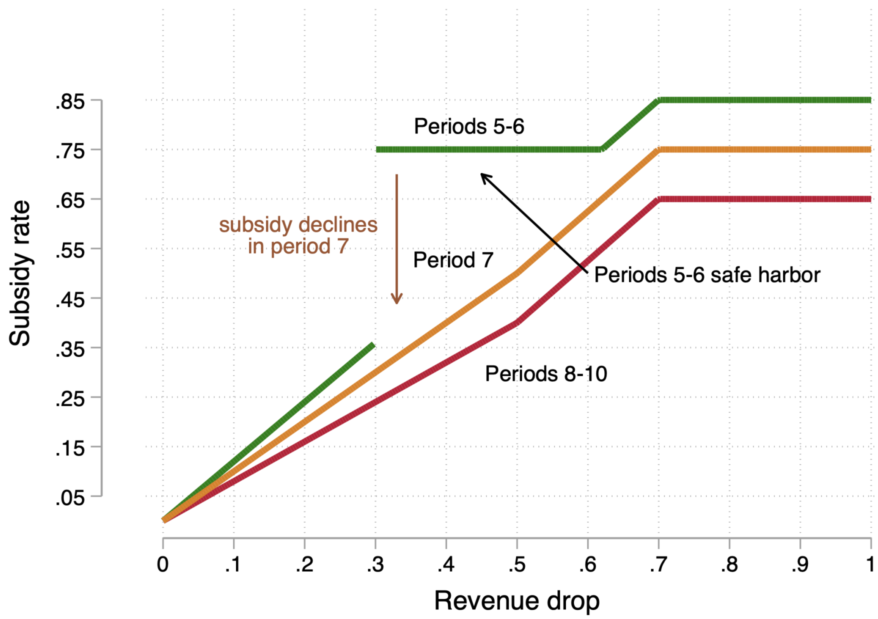
Our primary interest is in the impact of CEWS subsidies on employment at assisted firms. For employer i in claim period t, we therefore estimate specifications of the form

$$\Delta \log Y_{it} = a + b \log(1 - s_{it}) + u_{it} \tag{1}$$

where s_{it} is the CEWS subsidy rate and $\Delta \log Y_{it}$ is the change in log employment since one year earlier, which we refer to as "job loss" during the pandemic. We compare employment to the prior year, instead for example to January 2020 prior to the pandemic, in order to control for seasonality in employment (Jones et al., 2021). Since $1-s_{it}$ is proportional to the net-of-subsidy wage rate facing employers, the coefficient b is the elasticity of employment with respect to the net wage.

Under CEWS rules beginning in period 5, subsidy rates were a mechanical function of the reported revenue drop, and therefore correlated with the economic conditions facing the employers which are likely to be endogenous in (1). To deal with endogeneity, we exploit the safe harbor provision of the period 5-6 reforms, under which employers were entitled to the best of new and old rules.

Figure 2: The effect of the safe harbor rule



Periods 5-6: July 5-Aug. 29. Period 7: Aug. 30-Sep. 26. Period 8: Sep. 27-Oct. 24

Accordingly, as depicted in Figure 2, a deemed revenue drop of 30 percent or more entitled employers to subsidy rates of 75 percent (under the old rules), compared to a basic subsidy rate of up to 36 percent plus "top up" subsidy[6] for revenue drops below 30 percent (under the new rules).

We therefore estimate the effect of the subsidy using regression discontinuity (RD) methods, pooling data for periods 5-6 and estimating

$$\Delta \log Y_i = a + b \log(1 - s_i) + f(R_i) + v_i \tag{2}$$

where $f(R_i)$ is a local linear or polynomial function of revenue drop capturing factors correlated with revenue drop that influence the employment decision. Because subsidy rates varied among employers below the safe harbour region due to the top up subsidy, this is a fuzzy RD, and we instrument $\log(1 - s_{it})$ with the indicator $D_i = \mathbb{1}(R_i \geq .3)$ for safe harbor eligibility.

Safe harbor applicants in periods 5 and 6 received sharply higher total CEWS subsidies during 2020 than other similar applicants that were ineligible for the safe harbor rates. To explore the longer run effects of CEWS on workers and firms, we estimate sharp RD specifications analogous to (2) of the form

$$Z_i = \alpha + \beta D_i + \phi(R_i) + \epsilon_i \tag{3}$$

where Z_i is a measure of subsequent outcomes for the applicant, including annual net business income, dividends paid, and total payroll expenses for the 2020 tax year, and monthly indicators of business closures to the end of 2021. In this specification, β measures the causal effect on subsequent outcomes of the marginal total subsidy payments received by applicants eligible for the safe harbor

[6]The top up subsidy beginning in period 5 was 125 percent of revenue drops in excess of 50 percent. In periods 5-7, the top up subsidy rate was a function of the average revenue drop over the prior three months. Figure 2 depicts subsidy rates under the assumption that the three-month revenue drop is equal to the current-period deemed revenue drop.

rule.

With the expiry of the safe harbor in period 7, subsidy rates fell sharply for employers in the safe harbor region of revenue drops, as shown in Figure 2. To investigate the effect of the expiry on employment, we estimate dynamic difference-in-regression-discontinuity (DIRD) effects from the regression model

$$\log Y_{it} = a\ \mathrm{TREAT}_i + d_t + b_t \log(1 - s_{it}) + g_t(R_i) + e_{it} \qquad t = 5, \ldots, 10 \tag{4}$$

with the instrument $\mathrm{TREAT}_i = \mathbb{1}(\max\{s_{i5}, s_{i6}\} > .75)$ for employers receiving the safe harbor subsidy in period 5 or 6, and $g_t(R_i)$ is a time-varying local linear function of period-6 deemed revenue drop. In (4), b_t estimates the net wage elasticity of employment in period t, using changes in subsidy rates among safe harbor employers (the treatment group) relative to those with period-6 revenue drops just below the safe harbor range. The DIRD specification allow us to estimate the dynamic effects of the subsidy change over periods 7-10. Moreover, the DIRD estimates control for any remaining fixed differences between the treatment and control groups that might bias the static RD estimates of employment differences in periods 5-6.

3.2 Manipulation and consistency

As shown by Lee and Lemieux (2010), the estimator of b in (2) consistent if the density of u_i is continuous at the 30% cutoff where the safe harbor applied. If applicants have precise control over revenue drop, and so can manipulate to report a revenue drop above the cutoff to obtain the safe harbor subsidy, then this condition may be violated. If manipulators above the cutoff experienced greater job loss than non-manipulators, our estimates of b would therefore be biased toward zero. To deal with manipulation, we (i) include parametric controls for covariates in (2), and (ii) report results of "donut RDs" which exclude applicants in the immediate neighborhood of the cutoff. As a check on the static RD estimates in the safe harbor periods 5-6, we also report results of a placebo specification that regresses job loss for applicants in periods 1-4 on the period-6 running variable. Since there was no there was no discontinuity in the subsidy in periods 1-4, the placebo specification provides a check on the extent to which potential manipulators during the safe harbor periods had previously experienced different job losses than those below the cutoff.

More generally, to deal with manipulation, we rely on the panel structure of the data. We focus on differences in regression discontinuities over claim periods where the discontinuity in the subsidy rate at the cutoff varied. As shown by Grembi et al. (2016), difference-in-RD estimates of this kind deal with selection bias at the cutoff. Our estimate of the dynamic effects of eliminating the safe harbor in (4) includes a fixed effect for safe harbor firms and so yields a consistent estimate of b if unobservable factors affecting manipulation of the period 6 revenue drop are uncorrelated with shocks to employment *in later periods*.

4 Data

Our analysis uses administrative microdata on CEWS created by the Canada Revenue Agency (CRA) and supplied to Statistics Canada. For each applicant employer[7] and each four-week claim period, the dataset records the employer's CEWS payment, eligible payroll expense, and the number of employees for which the employer applied for subsidies, based on information reported by employers on the CEWS application form. Beginning in period 5, CEWS subsidy rates varied among applicants, and the dataset also records the relevant revenue drops and the base and top-up subsidy rates that were paid.

We have linked the CEWS administrative data to Statistics Canada's PD7 payroll data, which themselves are derived from CRA's payroll and income tax withholding systems. The PD7 data also record employment counts and total payroll expenses for each employer on a monthly basis, and we use the PD7 data from 2019 through 2021.[8] For some individual employers and months, PD7 and CEWS measures of employment differ. In what follows, we analyze impacts of the program on PD7 measures of employment; however, results for the CEWS measures are quite similar. We have also linked the CEWS data to employers' income statements and balance sheets for the 2018 through 2020 tax years. This information comes from T2 corporate tax returns of employers (and T1 Financial Declarations for unincorporated businesses), as recorded in Statistics Canada's National Accounts Longitudinal Microdata File (NALMF). In particular, we draw from NALMF applicants' total assets and its primary NAICS code in 2019, province of residence, and financial statement variables for the 2020 tax year.

On average, CEWS subsidized 492,026 employers per period, paying \$25,892 to each, or \$1,772 per subsidized employee. Summary statistics for all CEWS applicants in 2020 are reported in Appendix Table A1. As those figures suggest, most applicants were small. In 2020, 78.2% of applicants had 25 employees or fewer, and only 1.3% had 250 or more employees.[9] Not surprisingly, applicants had experienced employment losses in 2020. On average, applicants' employment was 12 log points lower than recorded in the PD7 data 12 months earlier.

Our static RD analysis reported below relies on the universe of CEWS applicants in periods 5 and 6, when the safe harbor rule operated. For the difference-in-RD (DIRD) analysis of the expiry of the safe harbor in periods 7-10, we focus on a balanced panel of employers who applied for CEWS in each of periods 2-10. The focus on the balanced panel ensures our results are not influenced by sample selection effects as employers move in and out of the program over time, or cease operating altogether. Restricting attention to the balanced panel seems unlikely to affect results, as repeat claim behavior was common. Among employers who ever applied for CEWS in 2020, the probability of applying in a period was 64 percent, and 133,960 employers or 27 percent applicants applied in all claim periods of 2020.[10] That said, restricting attention to the balanced panel implies that our DIRD results apply only to the subsample. We return to the impact of CEWS on business closures below. Compared to the full sample, employers in the balanced panel are somewhat larger by assets and employment and somewhat more likely to operate in service industries.

[7]Firms could apply for CEWS in respect of individual business divisions. In such cases, we aggregate the applicant data to the firm level using CRA's Business Number identifiers.

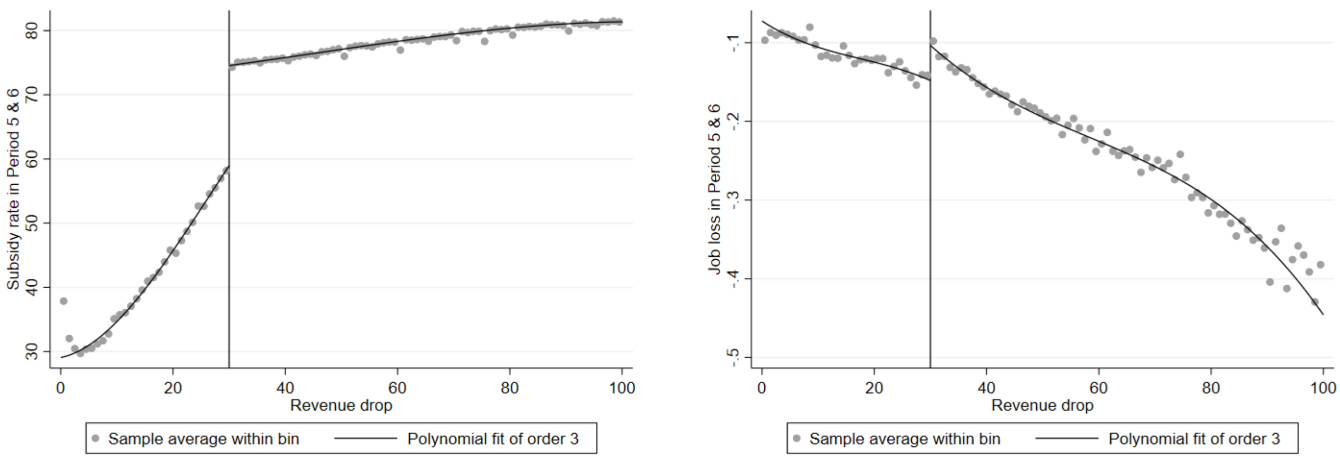
[8]While the CEWS data are for four-week claim periods and the PD7 data for calendar months, these largely coincided during periods 5-8, which are the main focus of our analysis. Note that period 5 began on July 5, period 6 on August 2, period 7 on August 30, and period 8 on September 27.

[9]These statistics are based on aggregates of the administrative data publicly released by CRA.

[10]Summary statistics for the balanced panel are reported in column 2 of Table A1.

Figure 3: Effect of subsidy rates on job loss

(a) Subsidy rate (b) Change in log employment



Note: The figures depict mean subsidy rate and year-over-year change in log employment ("job loss") by one percentage point bins of revenue drop, for all CEWS applicants in periods 5 and 6. The vertical line at $R = 30\%$ denotes the threshold at which the safe harbor rule applied.

5 Static RD estimates

Figure 3 provides a first look at how the safe harbor affected subsidy rates and job losses. The left panel depicts a binned scatterplot of the mean CEWS subsidy rate for each one percentage point bin of current-period revenue drop, for the pooled period 5 and 6 data – the first stage of our fuzzy RD estimate. The mean rate rose sharply at the 30 percent threshold at which the safe harbor applied, from 58 to 75 percent on average. The right panel shows the corresponding mean changes in log employment relative to 12 months earlier, our measure of (minus) job loss. Job loss also exhibits a sharp albeit small discontinuity at the safe harbor threshold, equal to 5 log points.

Table 2 reports the estimated RD effect on the log net-of-subsidy wage (the first stage) in column (1), the reduced form effect on log job loss in column (2), and the IV estimate of the elasticity (the fuzzy RD) in column (3). Rows of the table report results for alternative specifications of the model. In the base specification reported in the first row, estimates use local linear controls for the running variable, and the optimal bandwidth based on the method of Calonico et al. (2014), while in the second row we use cubic polynomial controls for the effect of the running variable. The optimal bandwidth is rather small, reflecting the tight fit depicted in Figure 3. The third row reports results using the local linear specification and all of the data in the interval of revenue drops between 15 and 45 percent.

In all specifications, results show a robustly estimated increase in employment at the cutoff between four and five log points in the reduced form, as reported in column (2). Meanwhile, the mean net-of-subsidy wage dropped by 35 to 38 log points at the cutoff. The corresponding estimated net-wage elasticities of employment are small but precisely estimated, and lie between -0.10 and -0.13 for the various specifications. In row 4, the regression includes controls for log total assets, two-digit industry, and province of residence – covariates that may be correlated with obtaining the safe harbor rule. The estimate of the RD is essentially unchanged.

Table 2: Estimated static RD effects of the safe harbor subsidy

	Log net-of-subsidy wage	Log job loss	
	First stage	Reduced form	Fuzzy RD
	(1)	(2)	(3)
1. Base	-0.357***	0.0510***	-0.130***
	(0.009)	(0.006)	(0.020)
2. Local cubic	-0.365***	0.0463***	-0.127***
	(0.006)	(0.005)	(0.015)
3. Local linear bandwidth=15	-0.384***	0.0413***	-0.107***
	(0.004)	(0.004)	(0.011)
4. Includes covariates	-0.357***	0.0441***	-0.107***
	(0.008)	(0.006)	(0.021)

Notes: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Each cell reports estimated discontinuity from an alternative model specification.
Standard errors use heteroskedasticity-robust nearest neighbour variance estimator with a minimum of 3 neighbours.

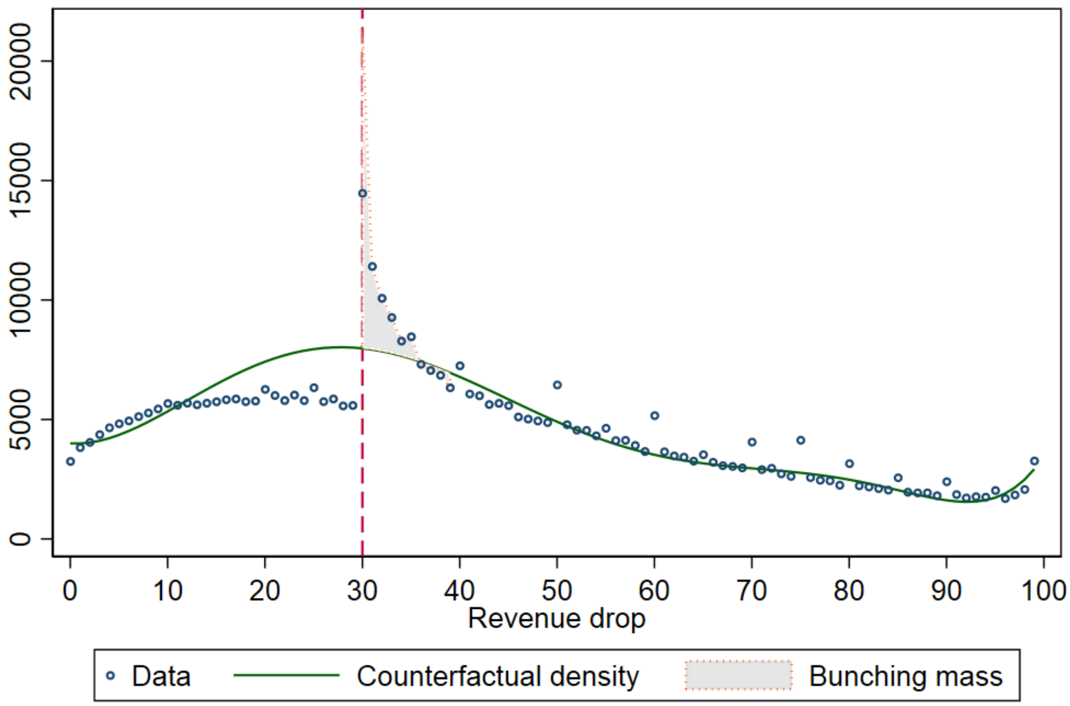
5.1 Manipulation and the safe harbor

Some employers may have had opportunity to retime revenues to later periods, resulting in a larger revenue drop during the safe harbor periods. Moreover, because revenue drop was self-reported by firms, there was a potential for false reporting. But CEWS was administered by the Canada Revenue Agency, which had ancillary information on revenues from firms' monthly or quarterly value-added tax returns,[11] and there were penalties for false reporting. That said, there is clearly potential for our running variable to be subject to manipulation that could invalidate our inferences about the causal effect of the safe harbor subsidies. To estimate the extent of manipulation, we employ the bunching estimator of Chetty et al. (2011). We assume that manipulation affects the density of revenue drops in the interval of revenue drops from 20 to 40 percent, where the incentives for manipulation draw employers that would otherwise have experienced revenue drops $R_i \in [20, 30)$ to instead report $R_i \in [30, 40]$. We calculate the counts of employers in the integer bins of revenue drops, say N_i, and estimate the smooth counterfactual density of revenue drop as a seventh-order polynomial function of R_i by applying least squares to the data outside the bunching interval. The counterfactual density is then estimated by reallocating excess mass in $[30, 40]$ to the $[20, 30)$ interval. The estimated counterfactual density is depicted in Figure 4, together with the histogram of revenue drops. The estimated excess bunching mass is depicted as the grey-shaded area between the histogram and the counterfactual density. We estimate that 21,075 employers (s.e. $= 318$) manipulated revenue drop to obtain the safe harbour subsidy rate, which is about 4 percent of all applicants in periods 5 and 6.[12]

[11] Not all firms are subject to the value-added tax, and Auditor General of Canada (2021) reports that 15 percent of CEWS applicants did not file a value-added tax return during 2020.

[12] Figure 4 also shows excess mass at multiples of 10 percent above 30, and at 75 and 85 percent, which is an example of "left-digit bias" common in administrative data. All of our estimators include dummy variables at these values to control for differences among applicants subject to left-digit bias.

Figure 4: Bunching in revenue drops



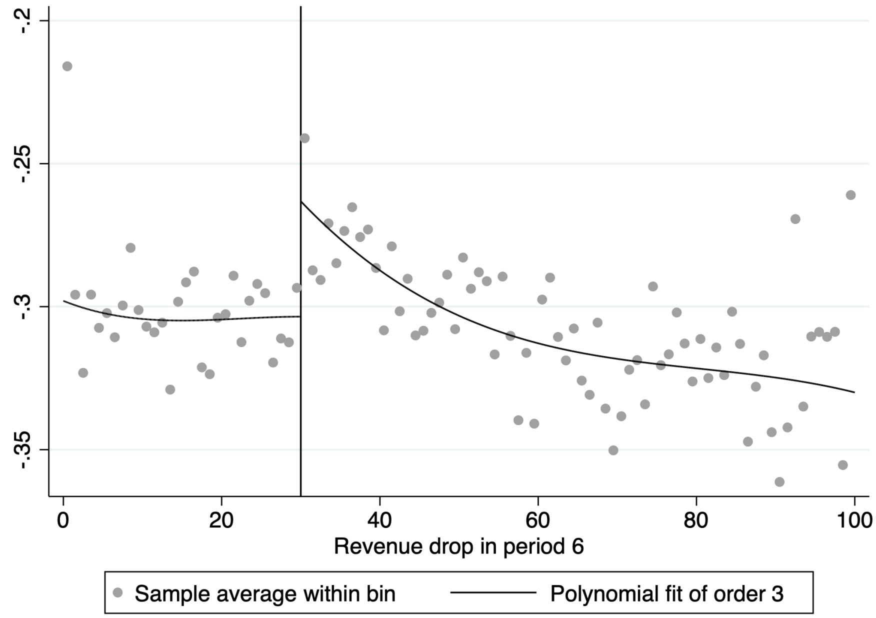
In the appendix, we report on covariate balance tests.[13] These investigations show that applicants above the cutoff had larger total assets in 2019, were more likely to operate in manufacturing industries, and were somewhat more likely to operate in the province of Ontario than applicants just below the cutoff. Controlling for log total assets, however, the remaining imbalance in these covariates is small and generally not statistically significant. This suggests that treatment with the safe harbor subsidy was essentially ignorable, after conditioning on pre-pandemic firm size.

The panel nature of our data permits a placebo falsification test of the effects of manipulation. In periods 1 through 4 of the program, there was no safe harbor rule and no discontinuity in the subsidy. If economic factors influencing job loss are autocorrelated, then any discontinuity in job loss in earlier periods at the cutoff is informative about counterfactual job loss among manipulators during the safe harbor periods. Figure 5 depicts the mean log employment loss for applicants in periods 1-4, for integer bins of revenue drop in period 6. Inspection of the figure suggests a small *positive* discontinuity in employment (reduction in job loss) prior to the safe harbor periods at the cutoff. As reported in the appendix, the discontinuity is insignificantly different from zero, if applicants with period-6 revenue drop in $[30, 31]$ are excluded. If the lower job loss among manipulators persisted during the safe harbor periods, then our estimates of the causal effect of the subsidy on employment, while nearly zero, are if anything too large.

An alternative method to deal with manipulation proposed by some researchers is to exclude from

[13]Figure A2 shows binned scatterplots of selected key covariates, including indicators for industry and providence of residence, and log total assets. Table A2 shows the estimated discontinuities in covariates at the cutoff.

Figure 5: Placebo test of job loss in periods 1-4



Note: The figure depicts mean job loss in periods 1-4, when there was no discontinuity in the subsidy, among applicants in those periods, by bins of revenue drop in period 6.

regressions the revenue drops in the immediate neighborhood of the cutoff, where manipulation is thought most likely to occur (Barreca et al., 2016). Such "donut RDs" are somewhat unsatisfying, inasmuch as they are inconsistent with the local nature of the estimator, but they do provide a falsification test for the main results. Table 3 reports donut RD estimates. All these specifications employ local linear controls for the running variable, and a fixed bandwidth of revenue drops from 15 to 45 percent, excluding successively larger intermediate intervals. As expected from Figure 3 above, the estimated discontinuity in job loss grows modestly smaller through donutting, while the first stage discontinuity in subsidy grows somewhat larger, resulting in a modest decline in the estimated elasticity. But the elasticity remains a precisely estimated (but very small) 0.06, even when revenue drops in $[25, 35]$ are omitted.

6 Expiry of the safe harbor

We turn now to the evolution of employment in the autumn of 2020, after expiry of the safe harbor rule, which induced large reductions in the subsidy rates paid to firms above the threshold. Initially, we estimate reduced-form changes in subsidy rates and log employment for the treated group of employers that received the safe harbor subsidy in period 6 (August) compared to other period-6 applicants ineligible for the safe harbor, which therefore did not experience the same drop in subsidy rates in the September-to-December period. In defining these treatment and control groups, to eliminate possible attrition bias, we restrict attention to a balanced panel of firms that applied for CEWS in every period from period 2 (April/May) until the end of the year. To ensure comparability of the

Table 3: Estimated static RD effects excluding "donut" applicants

	Log net-of-subsidy wage	Log job loss	
	First stage	Reduced form	Fuzzy RD
	(1)	(2)	(3)
1. Linear bandwidth=15	-0.384***	0.0413***	-0.107***
	(0.004)	(0.004)	(0.011)
2. Excludes revenue drops 30-31	-0.392***	0.0343***	-0.0874***
	(0.004)	(0.004)	(0.011)
3. Excludes revenue drops 28-32	-0.402***	0.0357***	-0.0884***
	(0.005)	(0.006)	(0.014)
4. Excludes revenue drops 25-35	-0.430***	0.0256**	-0.0590**
	(0.009)	(0.009)	(0.023)

Notes: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Each cell reports estimated discontinuity from an alternative model specification. Rows 2-4 are "donut RDs" which employ a fixed bandwidth of $[15, 45]$, excluding the specified interval in the neighborhood of the cutoff, and a local linear control for the running variable.
Standard errors use heteroskedasticity-robust nearest neighbour variance estimator with a minimum of 3 neighbours.

treatment and control samples, we restrict attention to applicants with period-6 revenue drops in the $[22, 38]$ interval, a bandwidth of 8 percentage points centred on the cutoff for safe harbor eligibility. Likewise, we control for local linear functions of period-6 revenue drop on each side of the cutoff. The resulting estimates are therefore difference-in-RD estimates of the effect of safe harbor expiry.

Figure 6 depicts the resulting estimates of the DIRD changes in subsidy rates (the dotted line) and log employment (the solid line), for size groups of firms defined by employment counts in January 2020, before the pandemic. These are reduced-form treatment effects underlying the elasticity estimates from (4), which are presented below. The unconditional means of log employment by size, month, and treatment status are reported in the appendix, for both the regression sample and an alternative sample with a wider bandwidth of revenue drops.

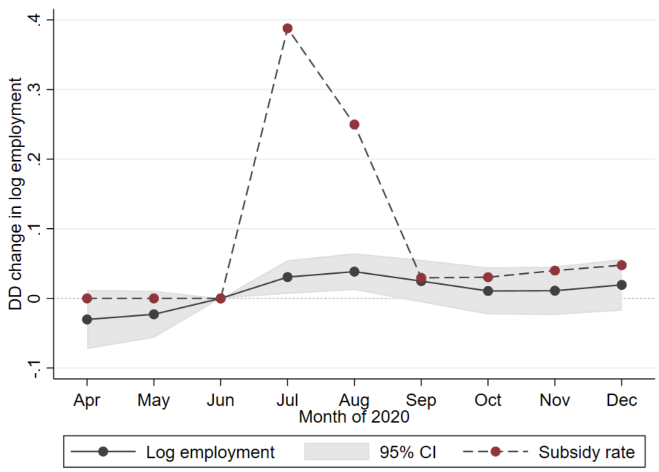
Pre-treatment differences in employment changes between treatment and control groups are insignificant in April through June, when there was no discontinuity in subsidy rates. This lends support to our assumption that treatment is ignorable in this setting. Employment at treated applicants rises with the safe harbor rule in July and August, and then declines gradually after its expiry in September, leaving no significant difference in employment changes at treated and control firms between June and December. This too offers reassurance on the validity of our identification strategy for both the safe harbor subsidy treatment in July-August, and the expiry treatment in September.

Figure 6 shows employment at treated firms rose by just 4.5 log points relative to control firms in the smallest size category, but a larger treatment effect (9 to 16 log points in August) at larger firms. Column (4) adds controls for period-specific shocks at the two-digit NAICS level. The estimated RD effects are essentially unchanged for all employment size groups except the largest one, for which the estimated effect becomes small and statistically insignificant.

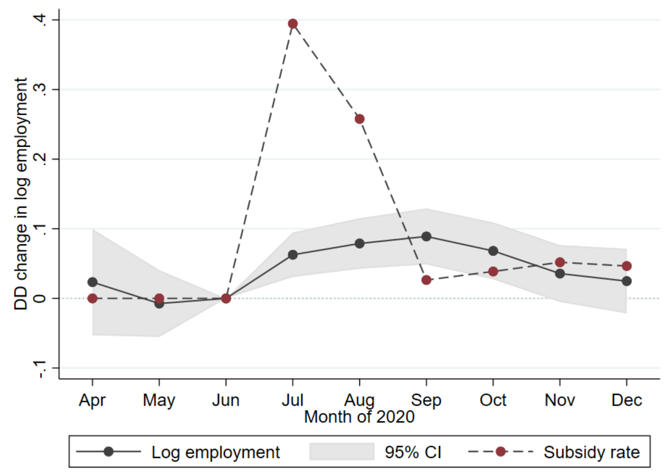
Table 4 presents the associated DIRD estimates of elasticities of December employment with re-

Figure 6: Dynamic difference-in-RD estimates, by firm size

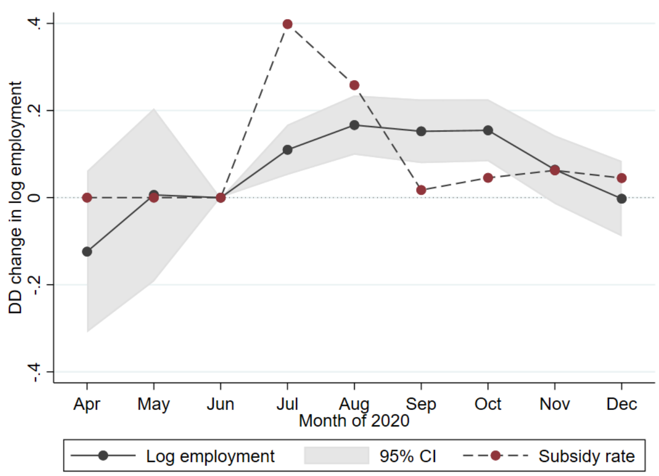
(a) 0-9 employees



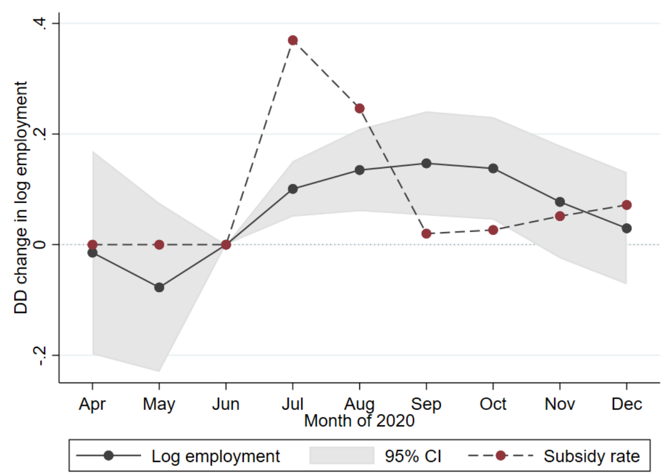
(b) 10-49 employees



(c) 50-99 employees



(d) 100+ employees



Note: The figures depict dynamic reduced-form difference-in-RD estimates of the impact of safe harbor subsidy rates by groups of applicants classified by January 2020 employment. The excluded month is June, before the safe harbor discontinuity in subsidy rates. The dotted line shows difference-in-RD changes in subsidy rates, and the solid line difference-in-RD changes in log employment. The regression sample is a balanced panel of applicants in periods 2 through 10 which reported a deemed revenue drop in period 6 in [22,38]. Treated firms are those that received the safe harbor subsidy rate in period 5 or 6. Estimates are weighted by January 2020 employment, winsorized at the 99th percentile. Standard errors are clustered by applicant. The grey-shaded areas are 95% confidence intervals.

Table 4: Difference-in-RD elasticities from safe harbor expiry

	(1)	(2)	(3)	(4)
All applicants	-0.079***	-0.087***	-0.076*	-0.047
	(0.018)	(0.026)	(0.031)	(0.032)
N	128103	75603	62589	62589
By firm size:				
Fewer than 10 employees	-0.014	-0.049	-0.052	-0.055
	(0.021)	(0.027)	(0.029)	(0.028)
N	60639	35152	28758	28758
10-49 employees	-0.064***	-0.079***	-0.069**	-0.060*
	(0.019)	(0.022)	(0.024)	(0.024)
N	48079	28884	24294	24294
50-99 employees	-0.121***	-0.122***	-0.137**	-0.107*
	(0.030)	(0.034)	(0.042)	(0.045)
N	8878	5266	4379	4379
100 or more employees	-0.093*	-0.109*	-0.102	-0.034
	(0.039)	(0.052)	(0.062)	(0.068)
N	6508	4024	3302	3302
Bandwidth	[10,50]	[20,40]	[22,38]	[22,38]
Include period-by-NAICS controls	No	No	No	Yes

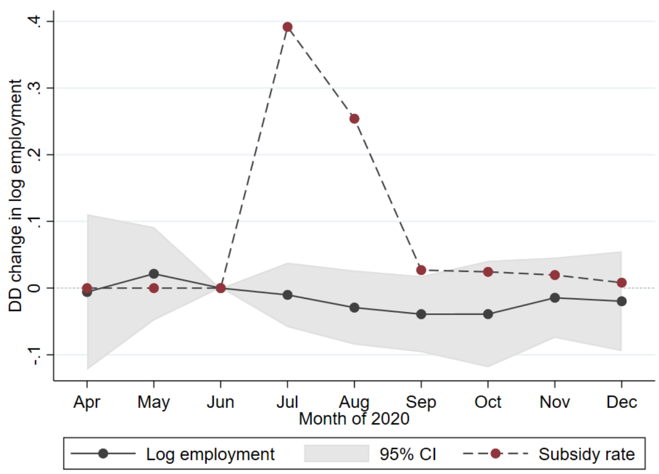
Notes: * $p< 0.05$, ** $p< 0.01$, *** $p< 0.001$
The table reports difference-in-RD elasticity estimates of (4) for period 10 (December) relative to safe harbor periods, where coefficients for the safe harbor periods are constrained to be zero. Each cell reports the estimated elasticity from an alternative model specification. Estimates are weighted by January 2020 employment, winsorized at the 99th percentile. In parentheses are robust standard errors, clustered by applicant.

spect to the net-of-subsidy wage. To estimate the elasticities, we use the data beginning in July and constrain the b_t coefficients for July and August (the pre-treatment period for safe harbor expiry) to equal zero. In view of the heterogeneity shown in Figure 6, estimates are weighted by applicants' employment in January 2020, winsorized at the 99th percentile. Columns (1)-(3) of the table report the resulting elasticity estimates for alternative bandwidths, with column (3) corresponding to the data reported in Figure 6. The elasticity estimates confirm the heterogeneous response to the subsidy of size groups, with elasticities in column (3) ranging from an insignificant 0.049 for the smallest employers to 0.137 for employers in the 50-99 employee range. The average treatment effect elasticity is 0.076, somewhat smaller than the 0.107 elasticity estimated from the static RD comparisons in July and August.

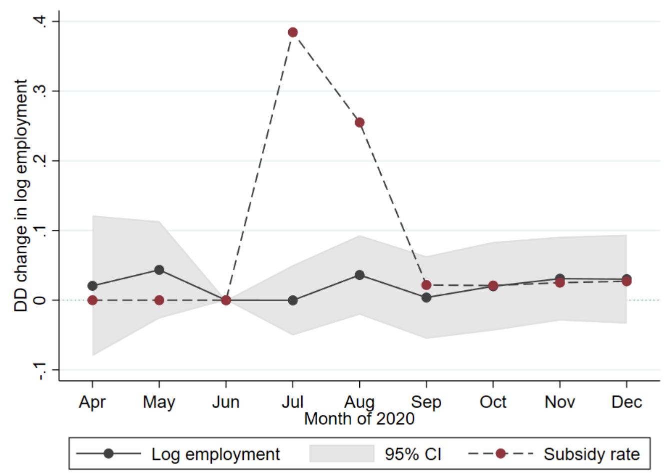
Heterogeneous responses occurred across industries as well. Figure 7 depicts reduced-form treatment effects on employment by month for broad industrial categories. There was no significant treatment effect of the safe harbor subsidy or its expiry in the primary and manufacturing sectors (NAICS one-digit codes 1-3), or in the trade and transportation sectors (NAICS code 4). Responses were largest in the accommodation and food services sector, and small but positive in other service

Figure 7: Dynamic difference-in-RD estimates, by industry

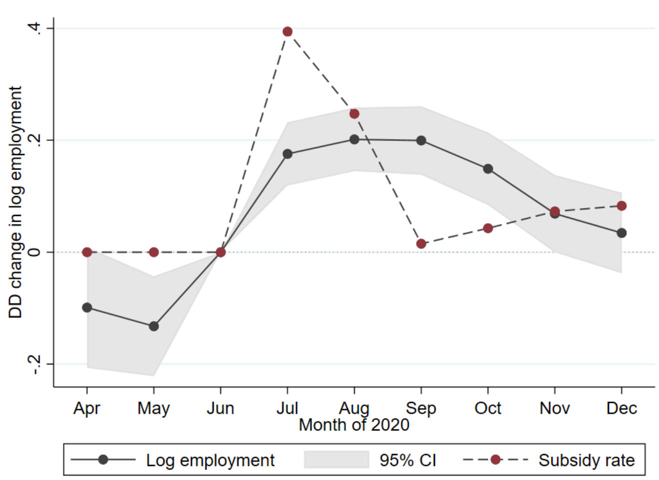
(a) Primary and manufacturing



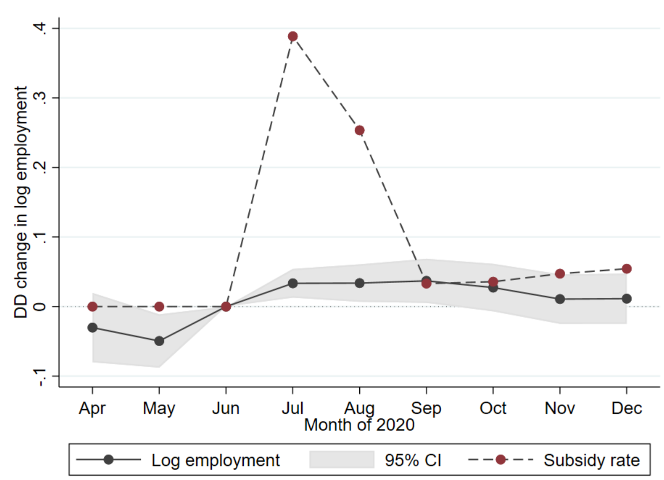
(b) Trade and transportation



(c) Accommodation and food



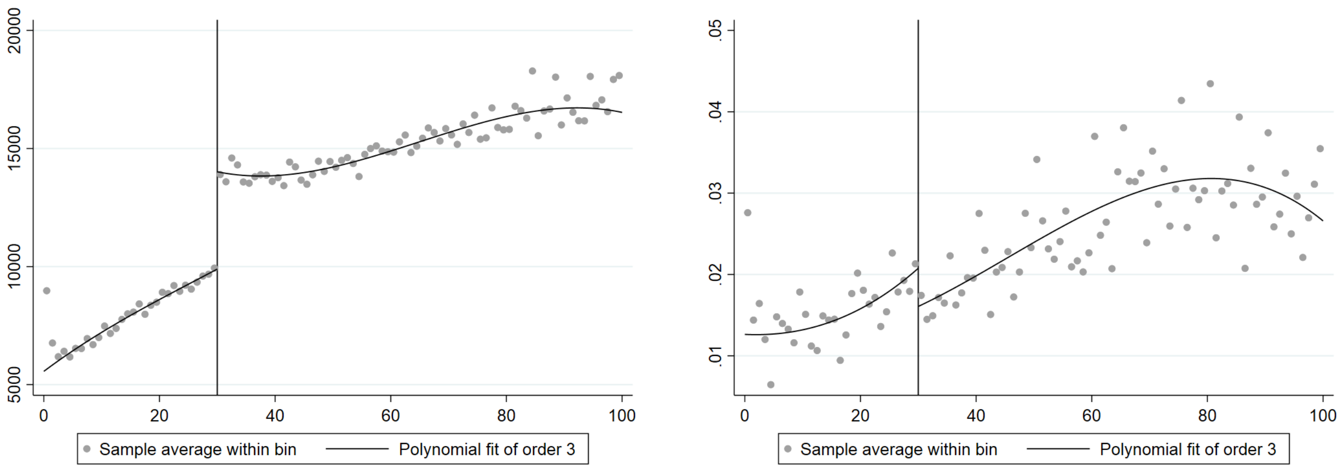
(d) Other services



Note: The figures depict dynamic difference-in-RD estimates of equation (4) by broad industry groups. Primary and manufacturing is NAICS codes 1-3, trade and transportation NAICS code 4, and accommodation and food services NAICS code 72. For additional details, see the notes for Figure 6.

Figure 8: Effect of subsidies on business closures

(a) Subsidies paid per employee (b) Business closures in January 2021



Note: The figures depict mean total CEWS subsidies paid in 2020 and the proportion of period 6 applicants that were closed in January 2021, by integer bins of revenue drop in period 6. The vertical line at $R = 30\%$ denotes the threshold at which the safe harbor rule applied. Closure is defined as six consecutive months with zero employees.

sectors. These differences are perhaps unsurprising. By July 2020, aggregate employment in sectors other than accommodation and food services had recovered to 95 percent of prior year levels,[14] and employment at many applicant firms had already returned to its pre-pandemic level. It would have been difficult to add employment for these firms, given that most employees laid off in the first wave of the pandemic had been recalled, and that job posting by firms and job search by the remaining unemployed remained severely restricted (Jones et al., 2021). Nevertheless, these sectors continued to benefit financially from the subsidy program, with more than 50 percent of aggregate subsidies paid to applicants in the primary, manufacturing, trade, and transportation sectors in the last six months of 2020.[15]

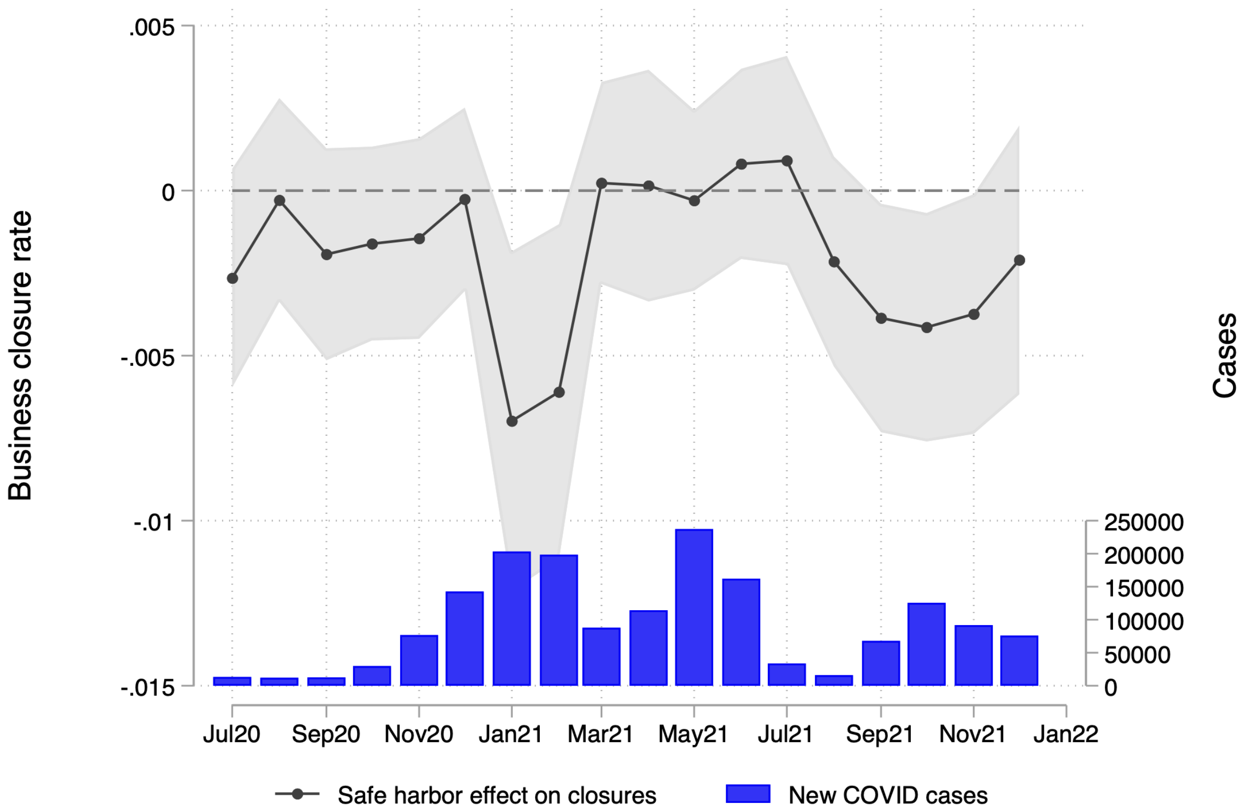
7 Longer run effects on firms

Wage subsidies did more than subsidize payroll costs of marginal workers. They paid cash to firms that might have provided liquidity needed to weather subsequent shocks, preventing business closures. Moreover, subsidies paid in respect of inframarginal employees whose jobs were not at risk constituted net revenue to firms that may have increased incomes of business owners. In this section, we investigate these broader impacts of the subsidies on businesses.

To do so, we again exploit the safe harbor rule, which paid larger total subsidies in the summer of 2020 to eligible than ineligible firms facing similar economic conditions. We therefore again use the static RD design to estimate discontinuities in subsequent outcomes for firms at the 30 percent threshold of deemed revenue drop that made applicants eligible for the safe harbor rule in period

[14]Aggregate employment statistics are presented in the appendix.
[15]Aggregate subsidy payments by industry are presented in the appendix.

Figure 9: Estimated effect on business closures over time



Note: The figure depicts the estimated RD effect of the safe harbor on business closures by month. Grey-shaded areas are 95 percent confidence intervals. Standard errors use heteroskedasticity-robust nearest neighbour variance estimator with a minimum of 3 neighbours.

6. Figure 8 depicts in its left panel the binned scatterplot of annual total CEWS payments received in 2020 by integer bins of revenue drop. The figure shows that subsidies received rise sharply at the cutoff by approximately $4000 per employee,[16] which is the "first stage" estimate that we use to investigate the impacts of subsidies on subsequent outcomes.

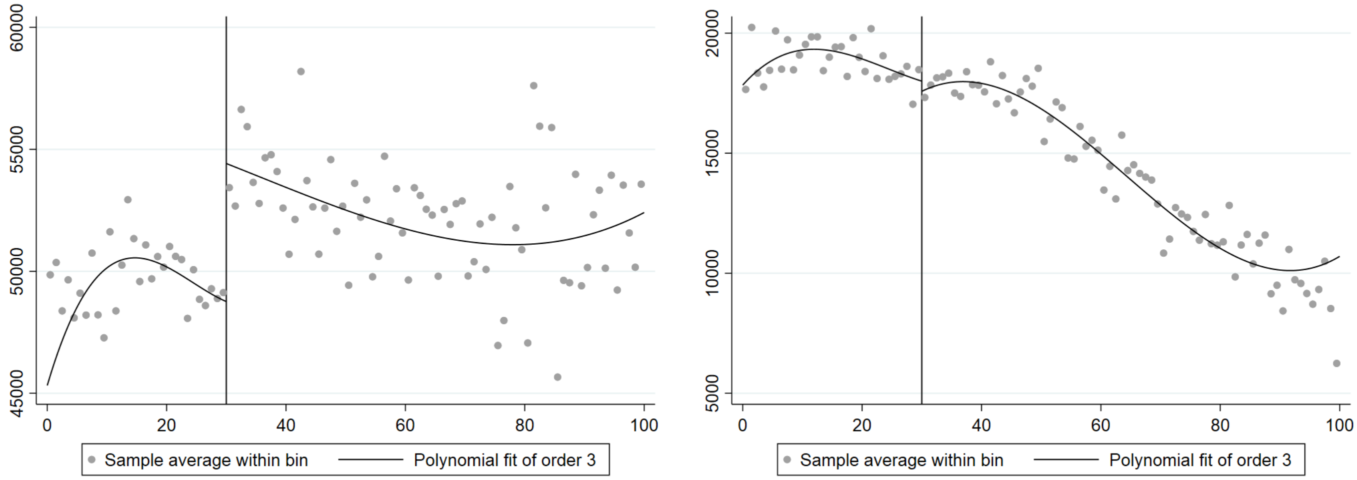
We define a business as closed in a given month if it had no employees in the current and the five prior months. (By this definition, some firms were closed even when they applied for CEWS in August 2020, because firms could apply for CEWS in respect of salary payments to owner-managers, even without other employees in the current period.) The right panel of Figure 8 shows the RD plot for the January 2021 business closure indicator. Business closures were in fact less common in 2020 than in prior years, particularly among those applying for CEWS (Leung and Liu, 2022), and the figure shows that just two percent of applicants below the cutoff were closed in January. The proportion closed drops sharply at the cutoff, leading us to infer the the incremental safe harbor subsidies paid in the summer reduced the propensity of businesses to close subsequently.

Figure 9 depicts the estimated RD effect on business closures in each month to the end of 2021.

[16]Employee counts are measured in January 2020, before the pandemic.

Figure 10: Effect of safe harbor on annual payroll and business incomes

(a) Payroll expense per employee (b) Net business income per employee



Note: The figure depicts mean annual payroll expense and net income before taxes for the year 2020, by integer bins of revenue drop in period 6. Dependent variables are normalized by dividing by employment in January 2020. The vertical line at $R = 30\%$ denotes the threshold at which the safe harbor rule applied.

Safe harbor firms were significantly less likely to close in January-February 2021, during the second wave of the pandemic, and again beginning in September, during the fourth "Delta" wave. While the estimated discontinuity is small, because closures were rare below the cutoff, the estimated impact on the log odds ratio for closures is large. In this sense, the subsidies seem to have insured business owners against the risk of closure during subsequent shocks, and employees against the risk of layoffs. That said, as we discuss further below, the layoff effect is small.

To study the economic incidence of the subsidies, we examine the year-end financial statements of applicant firms, which are filed together with business tax returns and so are part of our administrative dataset. Given the sharp increase in total subsidies received per employee for safe harbor applicants, how did this affect the incomes of workers and shareholders? Figure 10 shows RD plots for applicants' annual total payroll expense per employee and net income before taxes per employee, a rough but comprehensive measure of profitability. While there is no discontinuity in net income, payroll expense rises sharply at the cutoff by a magnitude comparable to the increase in subsidies there. In the appendix, we report estimates of the RD effect in these and other annual financial variables, while controlling for covariates that may be imbalanced at the cutoff. The estimated increase in payroll expense is $2447 per employee, or about 64 percent of the associated increase in subsidies per employee. Moreover, there is no evidence that retained earnings or dividends paid to shareholders increased with the safe harbor. In this sense, much of the additional cash received under the safe harbor rule went to employees. This effect is too large to be explained by the extensive margin impacts on employment or business closures that we have estimated and suggests that there were likely also intensive-margin impacts on hours worked or wage rates among existing employees.

8 Concluding remarks

The safe harbor rule in Canada's pandemic wage subsidy program caused sharply higher subsidies to be paid to eligible employers than others facing similar economic conditions. We use this fact to identify the elasticity of employment with respect to the subsidy rate, and its impacts on business closures and business incomes. The estimated employment responses are small. Conceptually, the program could affect employment through three channels: extensive margin responses in employment at operating firms, layoffs resulting from business closures, and intensive margin changes in hours worked by workers already employed at operating firms.

To estimate the extensive margin impacts on jobs "saved" through the subsidies, we simulate counterfactual employment in the absence of the subsidies, say E^0_{it}, from the log-linear specification of (1) as

$$E_{it} = (1 - s_{it})^b E^0_{it}$$

Our preferred estimate of the net wage elasticity of employment is $\hat{b} = -0.11$, a LATE for the safe harbor subsidy in period 5-6. Assuming that the same elasticity describes responses to the subsidy by all employers in all periods, we estimate job-months saved as

$$E_{it} - \hat{E}^0_{it} = \left[1 - (1 - s_{it})^{-\hat{b}}\right] E_{it}$$

We then sum estimates over all applicants in periods 2-10 to arrive at an aggregate estimate that 3.9 million job-months of employment were preserved through the subsidies in those periods. The aggregate fiscal cost of those subsidies was \$61.0 billion, implying a fiscal cost per job-month saved of \$15,602. This cost is large relative to the average payroll expense of \$4545 for employees covered by the subsidies, or the \$2000 per period paid under Canada's COVID benefits to those experiencing income loss not covered through regular unemployment insurance benefits. In this context, "month" means the four-week claim period over which CEWS subsidies were calculated. Multiplying by 13 gives an annualized estimated cost of \$201,043 per job saved.

We also found evidence that the subsidies reduced the risk of business closures, which in turn resulted in fewer jobs lost through layoffs, and so a lower aggregate fiscal cost of preserving jobs through the program. But this effect too is small. The safe harbor rule caused about \$4000 in additional subsidies per employee to flow to eligible firms. At its peak in January 2021, the safe harbor effect was to reduce closures by just 0.6 percentage points, implying a fiscal cost of jobs saved through the layoff channel of more than \$600,000.

Our data do not permit us to estimate intensive margin changes in hours worked directly. But we do find evidence that annual payroll expense at employers eligible for the safe harbor rule increased by 60 percent or more of the additional subsidy payments received. We also find no evidence of increases in business incomes or dividends paid to shareholders. This suggests that much of the incidence of the subsidy was ultimately on workers. The effect is too large to be explained by the rather small extensive margin employment elasticity that we have estimated.

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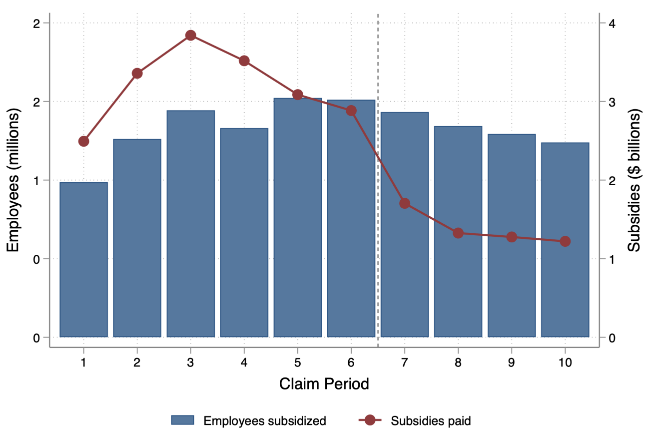
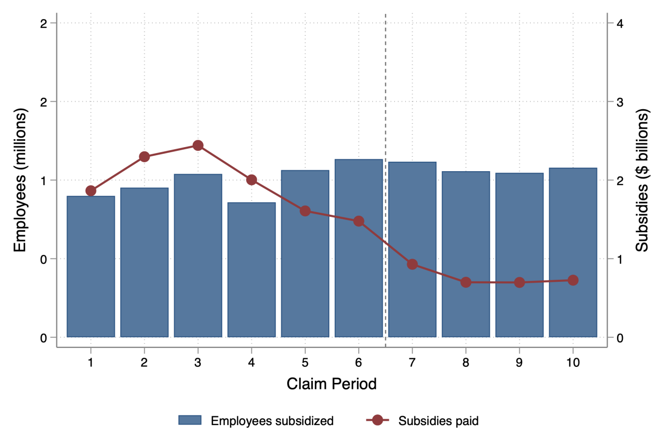
Figure A1: Aggregate subsidies paid, by industry and month

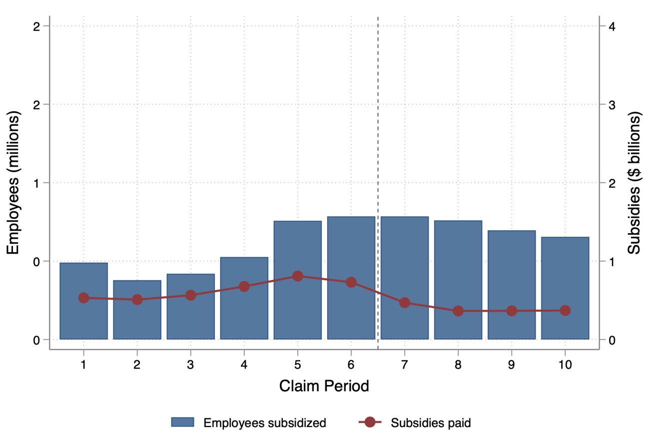
(a) Primary and manufacturing



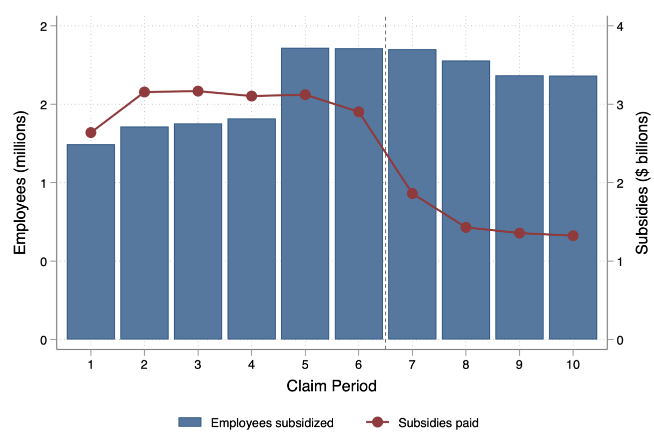
(b) Trade and transportation



(c) Accommodation and food



(d) Other services



Note: The figures depict aggregate subsidies paid and the number of employees at applicant firms by month, for the four broad industry groups: Primary and manufacturing (NAICS codes 1-3), trade and transportation (NAICS code 4), accommodation and food services (NAICS code 72), and other services. Public-sector employers are excluded.

Table A1: Summary statistics

	All applicants		Balanced panel	
	Mean	(s.e.)	Mean	(s.e.)
Subsidy Rate (%)	58.77	(23.96)	61.09	(21.81)
log Net-of-Subsidy Rate	-1.03	(0.52)	-1.08	(0.50)
Total CEWS Subsidies	25,892	(249,345)	35,948	(363,988)
Total CEWS per Employee	1,772	(2,527)	1,887	(2,316)
Applied in current period (%)	0.64	(0.50)	0.98	(0.13)
log Employment	1.74	(1.25)	1.98	(1.30)
log Employment Loss	-0.12	(0.52)	-0.19	(0.57)
log Total Assets in 2019	13.12	(1.82)	13.39	(1.87)
Payroll per Employee	4,545	(7,356)	4,585	(6,787)
Industry indicators:				
Primary Sector and Manufacturing	0.24	(0.43)	0.22	(0.41)
Trade and Transportation	0.19	(0.39)	0.16	(0.36)
Accommodation and Food Services	0.12	(0.32)	0.15	(0.36)
Other Services	0.46	(0.50)	0.48	(0.50)
Number of Observations	4,920,264		1,205,640	

Figure A2: Covariate balance plots

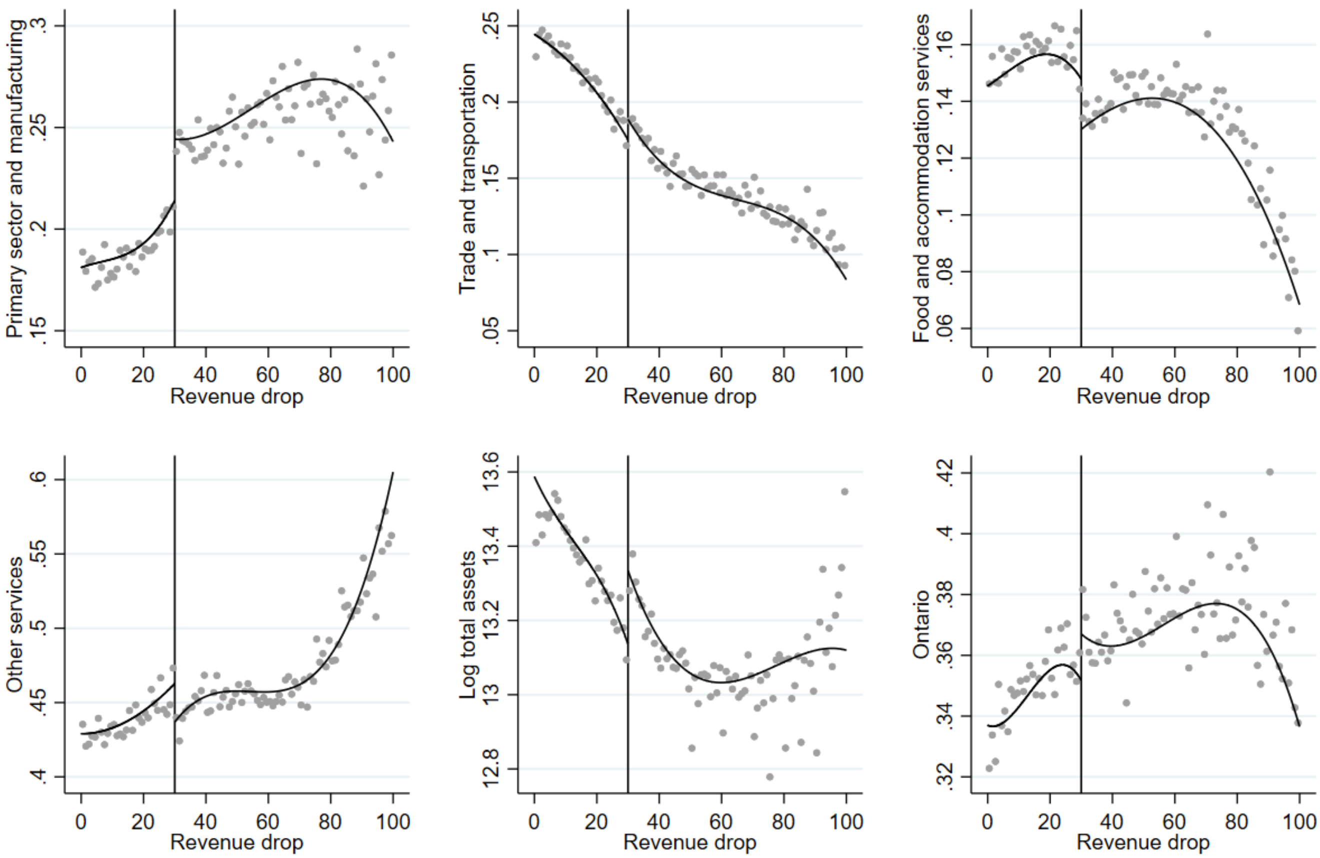


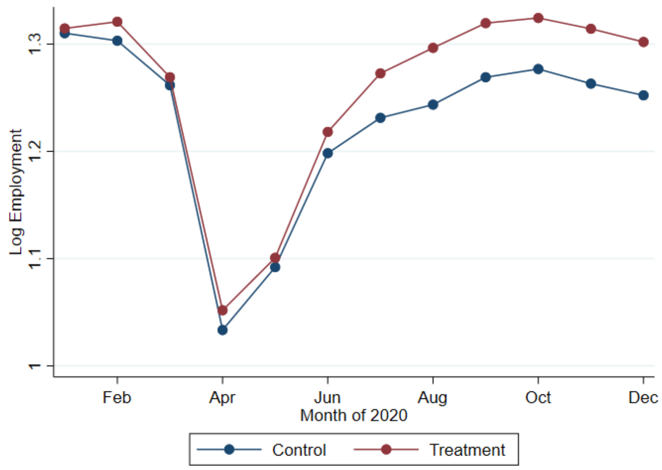
Table A2: Selected covariate balance tests

	Estimated discontinuity		
log Total Assets	.201		.349***
	(0.035)		(.041)
Province indicators:			
Alberta	-.007	-.008	-0.007
	(0.005)	(0.005)	(0.005)
British Columbia	-.010*	-.001	-0.009
	(0.005)	(0.005)	(0.006)
Ontario	.033***	0.000	0.026**
	(0.008)	(0.000)	(0.009)
Quebec	.005	.002	0.003
	(0.006)	(0.001)	(0.007)
Industry indicators:			
Primary and manufacturing	.034***	.022**	0.04***
	(0.007)	(0.008)	(0.008)
Trade and transportation	.010	.007	0.012
	(0.007)	(0.007)	(0.007)
Food and accommodation services	-.027***	-0.010	-0.02**
	(0.007)	(0.007)	(0.007)
Other services	-.017**	-.015*	-0.030***
	(0.006)	(0.007)	(.008)
Placebo: Log employment loss in periods 1-4	.051***	.045***	-.016
	(0.010)	(0.009)	(.013)
Control for log total assets	No	Yes	No
Exclude revenue drops [30, 31]	No	No	Yes

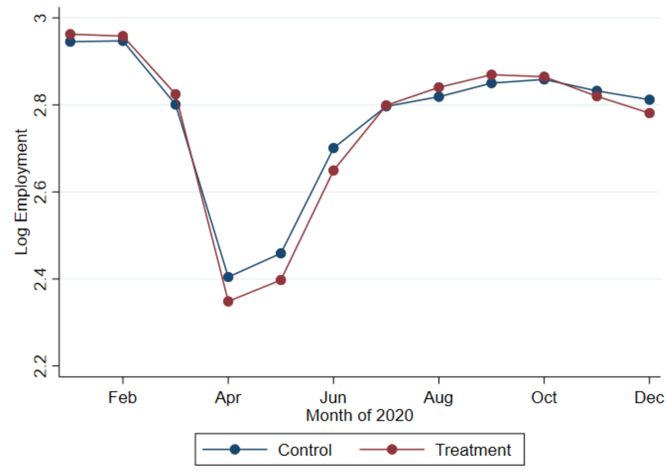
Notes: * $p< 0.05$, ** $p< 0.01$, *** $p< 0.001$
The table reports RD estimates of discontinuity in mean of selected covariates at the cutoff revenue drop in periods 5-6. Standard errors in parentheses use heteroskedasticity-robust nearest neighbour variance estimator with a minimum of 3 neighbours.

Figure A3: Mean log employment by month and firm size, revenue drop ± 8 from the cutoff

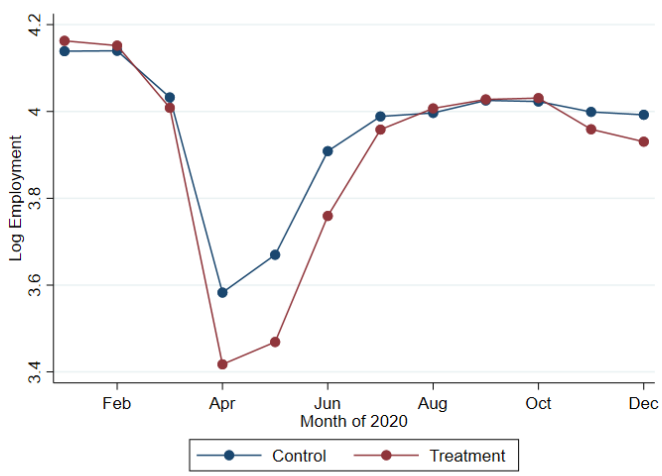
(a) 0-9 employees



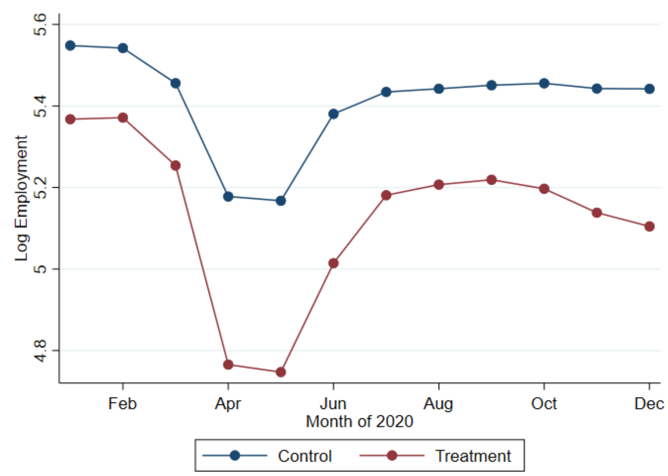
(b) 10-49 employees



(c) 50-99 employees



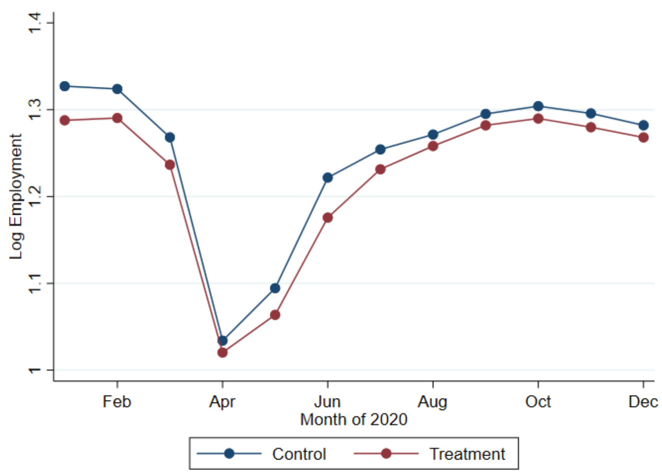
(d) 100+ employees



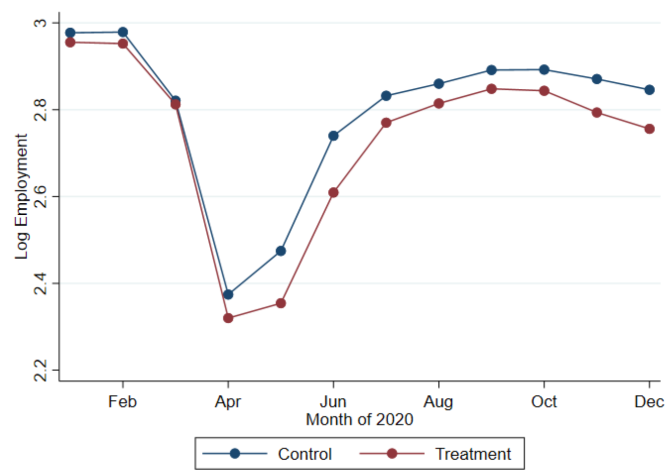
Note: The figures depict mean log employment by month by groups of applicants classified by January 2020 employment. The sample is a balanced panel of applicants in periods 2 through 10 which reported a deemed revenue drop in period 6 within 8 percentage points of the safe harbor cutoff, i.e. in [22,38]. Treated firms are those that received the safe harbor subsidy rate in period 5 or 6.

Figure A4: Mean log employment by month and firm size, revenue drop ± 20 from the cutoff

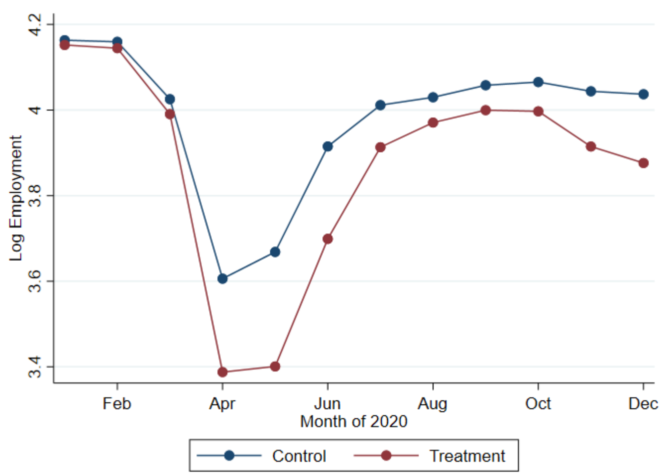
(a) 0-9 employees



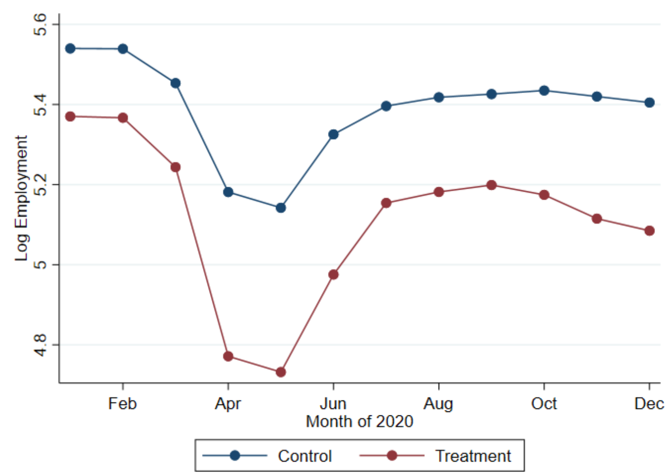
(b) 10-49 employees



(c) 50-99 employees

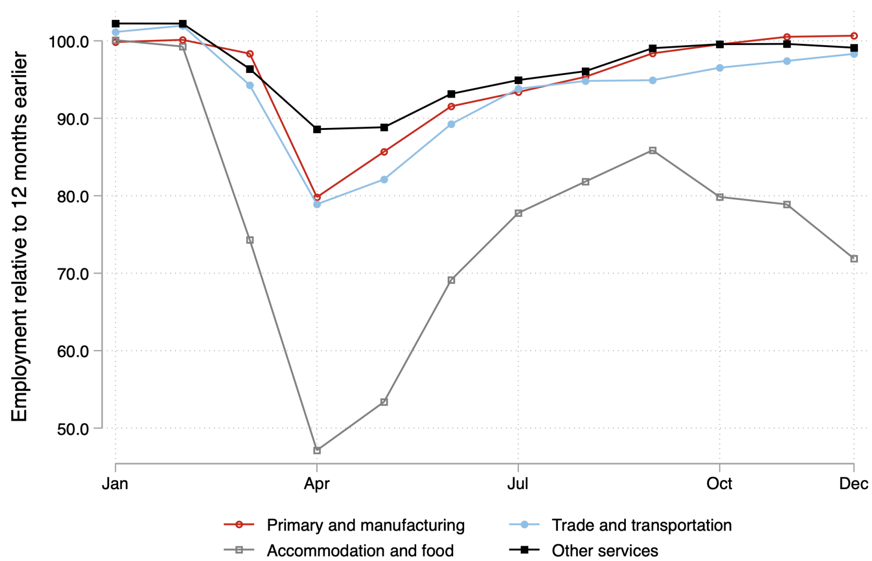


(d) 100+ employees



Note: The figures depict mean log employment by month by groups of applicants classified by January 2020 employment. The sample is a balanced panel of applicants in periods 2 through 10 which reported a deemed revenue drop in period 6 within 20 percentage points of the safe harbor cutoff, i.e. in [10,50]. Treated firms are those that received the safe harbor subsidy rate in period 5 or 6.

Figure A5: Aggregate employment by industry and month, 2020



Source: Statistics Canada Table 14-10-0376

Note: The figure depicts aggregate employment relative to one year earlier for private-sector firms, regardless of CEWS applicant status, excluding the self-employed, by broad industry group by month in 2020.

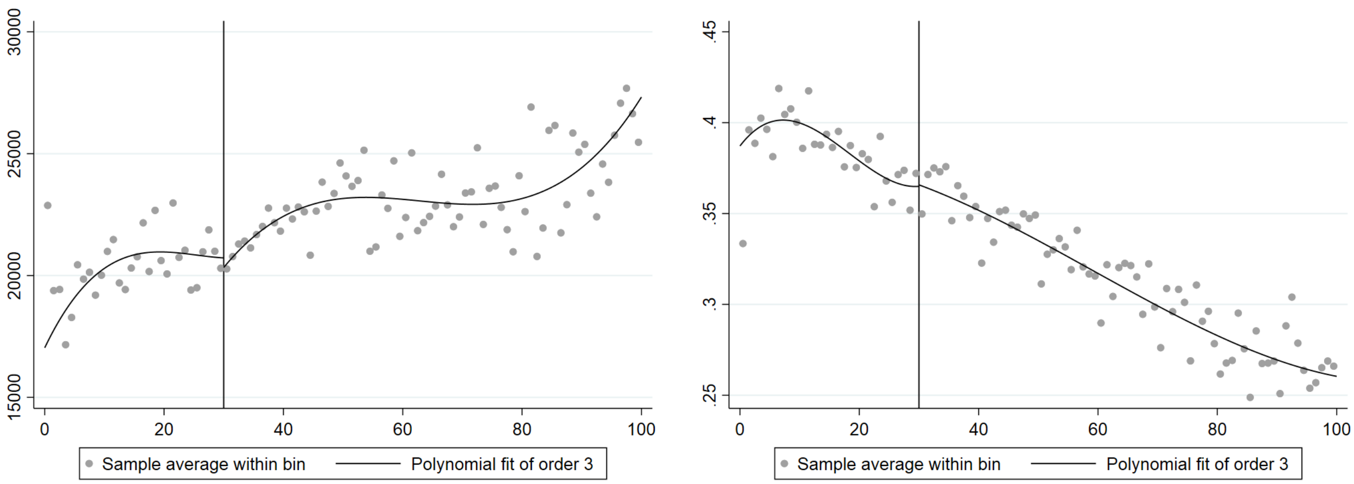
Table A3: The economic incidence of safe harbor subsidies

	RD effect	RD effect
CEWS per employee	4046.8***	3784.8***
	(359.1)	(369.7)
Payroll per employee	4854.8**	2447.9
	(1443.4)	(1491.8)
Non-zero dividends	-.018	-.025*
	(.012)	(.012)
Net income per employee	-1177.9	-2035.9
	(1111.5)	(1140.5)
Retained earnings per employee	145.4	1124.3
	(897.6)	(972.1)
Dividends declared per employee	-646.9	-1140.6*
	(527.1)	(642.1)
Include parametric controls	No	Yes

Figure A6: Effect of safe harbor subsidies on dividends

(a) Dividends declared per employee

(b) Dividends declared indicator



Note: The figure depicts mean dividends declared (if non-zero) divided by the firm's employee count in January 2020, and the proportion paying dividends, by integer bins of revenue drop in period 6. The vertical line at $R = 30\%$ denotes the threshold at which the safe harbor rule applied. Dividends per employee are winsorized at the 99th percentile.