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COVID-19 Wage Subsidy: Outcome evaluation

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Dean R. Hyslop, David C Maré, Shannon Minehan

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Author contact details

Motu Economic and Public Policy Research Trust

PO Box 24390, Wellington

New Zealand

Dean Hyslop: dean.hyslop@motu.org.nz

Dave Maré: dave.mare@motu.org.nz

Shannon Minehan: shanon.minehan@motu.org.nz

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Disclaimer

These results are not official statistics. They have been created for research purposes from the Integrated Data Infrastructure (IDI) and or Longitudinal Business Database (LBD) which are carefully managed by Stats NZ. For more information about the IDI and LBD please visit https://www.stats.govt.nz/integrated-data/. The results are based in part on tax data supplied by Inland Revenue to Stats NZ under the Tax Administration Act 1994 for statistical purposes. Any discussion of data limitations or weaknesses is in the context of using the IDI for statistical purposes, and is not related to the data's ability to support Inland Revenue's core operational requirements.
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Motu Economic and Public Policy Research

PO Box 24390 info@motu.org.nz +64 4 9394250
Wellington www.motu.org.nz
New Zealand

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Abstract

The COVID-19 pandemic has caused substantial disruption in social and economic activity since March 2020. The New Zealand Government reacted early, introducing stringent lockdowns to restrict the spread of the virus. At the same time, it introduced a series of economic policies designed to support the health response, the largest of which was the COVID-19 Wage Subsidy Scheme (WSS). The WSS was a high-trust policy that provided subsidy payments to firms who expected to have a substantial drop in revenues because of the pandemic. The objectives of the WSS were to avoid widespread layoffs, help firms maintain employment relationships with their workers, and maintain workers' incomes to help meet their essential needs during lockdown periods.

This paper analyses the impacts of the WSS on both firm and worker level economic outcomes. We adopt a 'doubly-robust' estimation approach, that uses propensity score methods both to match subsidy receiving firms to similar non-subsidised firms, and to weight the outcomes analysis. Our analysis focuses on the first four WSS-waves: the March 2020 (Original), Extension, Resurgence, and March 2021 waves.

First, we analyse whether the WSS reached the intended people and businesses. For the March 2020 wave, subsidised firms experienced substantially greater revenue declines than unsubsidised firms: the modal reduction in revenue for subsidised firms was about 50%. We also observe larger revenue losses relative to a year earlier for subsidised firms in the Extension and Resurgence waves, but revenue changes for the March 2021 wave are confounded by the March 2020 effects. As the subsidy payments were tied to firms, it was less effective in supporting more precarious jobs and workers.

Second, we analyse the effects of the WSS on firm survival and resilience over the short (6 months) and medium (12 months) term. We estimate that receiving WSS payments had a positive effect on firm survival rates over the following 12 months for three of the four WSS waves. However subsidised firms experienced slower subsequent employment growth than non-subsidised firms.

Third, we analyse the effects of the wage subsidy scheme on worker level outcomes. We estimate positive effects of WSS receipt on job-retention over both the short term (6-months) and medium term (12-months) for the March 2020, Extension and March 2021 waves; and roughly zero effects for the Resurgence wave. We also find positive employment effects for workers over the short term for the March 2020, Extension and March 2021 waves, and over the medium term for the March 2020 and Extension waves; and slightly negative effects for the Resurgence wave. However, conditional on being employed, we estimate that workers who

received March 2020 wage subsidy payments experienced slower subsequent monthly earnings growth than comparable non-subsidised workers. The estimates for the later waves are more mixed.

We find no compelling evidence that the WSS supported non-viable firms, although the higher survival rate and lower employment growth of subsidised firms suggests that the WSS may have kept firms with poorer growth prospects in operation. We also find no systematic evidence that firms did not comply with their obligations to pass on subsidy payments to workers and endeavour to pay them at least 80% of their usual earnings. However, we find that some subsidy receiving firms paid workers at either the part-time or full-time subsidy rate, or at 80% of their prior earnings, during periods of subsidy receipt. This was relatively more likely to occur during the original (March 2020) subsidy wave, and to a lesser degree the Extension-wave.

JEL codes

H25, J08, J20, J63

Keywords

COVID-19, Wage subsidy, firm survival, employment growth, earnings compliance, job-retention, employment continuity, earnings growth

Summary haiku

Wage Subsidy Scheme
kept firms and workers active
and softened the blow

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Tables and Figures

1 Introduction

The COVID-19 pandemic caused substantial disruption in social and economic activity in 2020. In its early response to the health threats, the New Zealand Government introduced a level-4 lockdown in late March to restrict the spread of the virus through ongoing social engagement. To help support these health focused measures, the government also introduced a series of policies to provide economic support for businesses, workers and the wider population. The largest of these policies was the COVID-19 Wage Subsidy Scheme (WSS) that provided subsidy payments to firms, in order to avoid widespread layoffs, maintain the employment relationships with workers and, in turn, as a means to maintain workers' incomes to help meet their essential needs. The policy adopted a high-trust approach, supporting firms that expected to experience a substantial drop in revenue due to the pandemic and lockdowns.

In this paper, we analyse the impacts of the WSS on both firm and worker level economic outcomes. In contrast to Graham and Ozbilgin's (2021) analysis of the macroeconomic impact of the pandemic, lockdowns and wage subsidy scheme, our approach uses a micro-level empirical counterfactual analysis of the impacts of the WSS on firms and workers that received subsidy support compared to similar firms and workers who didn't receive support payments. To do this we adopt a 'doubly-robust' estimation approach (Hirano and Imbens 2001), that corrects for bias from observable factors either affecting the selection into wage subsidy receipt or the outcomes of interest. In particular, the approach uses propensity score methods both to match subsidy receiving firms to similar non-subsidised firms, and also to weight the outcomes analysis.

The WSS included five distinct waves of support, as well as additional 'leave payments' for firms with workers on COVID-19-related leave. For the purposes of evaluation, the five waves are divided into two phases. Phase 1 was characterised by high levels of uncertainty in the early stages of the pandemic: this phase includes the original (March 2020) WSS wave that provided 12 weeks of support to firms, covering the period from late March until early June 2020; and the WSS-Extension wave that provided 8 weeks of support from mid-June. Phase 2 was characterised by more targeted assistance and monitoring: phase 2 includes the WSS-Resurgence wave from August 2020; the March 2021 wave; and the August 2021 wave. Due to reasons of data availability and follow-up lags, our analysis focuses just on the first four waves (i.e. the March 2020, Extension, Resurgence, and March 2021 waves), and does not cover the WSS leave payments.

As background to this project, in May 2021, the Office of the Auditor General (OAG) released an audit of the management of the COVID-19Wage Subsidy Scheme (WSS). The audit

recommended that the Ministry of Social Development (MSD), Inland Revenue (IR), the Ministry of Business, Innovation and Employment (MBIE), and The Treasury carry out timely evaluation of the development, operation, and impact of the WSS to inform preparation for future crisis-support schemes.

In August 2021, the Ministry of Social Development invited proposals from suppliers to undertake components of an evaluation of the COVID-19Wage Subsidy Schemes. There were three main components: a process evaluation, an outcome evaluation, and a synthesis of findings across process and outcome evaluations. The evaluation is being co-ordinated by MSD in partnership with IR, MBIE and The Treasury via cross-agency Working and Steering Groups. The outcome evaluation will focus on the first four WSS iterations, with the fifth iteration out of scope due to time constraints in the follow-up period.

The outcome evaluation component was to focus on the following three key evaluation questions:

1) To what extent did the WSS reach the intended people and businesses?
    a) To what extent did the WSS support employment attachment, business survival/resilience, employee income and other key outcomes in the short and medium term?
    b) How were these outcomes distributed across different population groups, firms, sectors, industries, and regions?
2) What was the value for money of the WSS?
3) What (if any) were the unintended outcomes/consequences/risks of the WSS? eg unfair or illegal treatment of employees by employers, support for non-viable firms, potential misuse of funds

The current paper addresses the first and third of these questions. Question 2 about value for money of the WSS is addressed in a companion report (Fyfe, Maré, and Taptiklis forthcoming).

The primary objective of the WSS was to support businesses affected by the impact of COVID-19 to maintain the employment attachment of their workers. Subsidy support was intended to target firms that expected to experience a substantial decline in revenue as a result of the pandemic and didn't have financial reserves to survive. For the original (March 2020) WSS-wave, the revenue decline test was at least a 30% decline relative to the previous year (or some more recent period), while for the subsequent waves, the test was a 40% decline.

This paper focuses on the first of the evaluation questions, which encompasses three separate sets of issues – take-up, outcomes for firms, and outcomes for workers. We address these three sets of issues sequentially. First, we address the question of did the WSS reach the intended people and businesses. To do this we first compare the revenue declines of subsidised and unsubsidised firms: there is a modal reduction in revenue of about 50% for subsidised firms

around the time of the March 2020 wave. We also estimate WSS-wave specific selection models for the probability that a firm received subsidy payments in terms of observable characteristics including the predicted decline in revenue, and assess the extent to which the predicted probabilities of subsidy-receiving firms were greater than those of non-subsidy receiving firms. This again shows that subsidised firms generally estimated to be more likely to receive subsidy payments than unsubsidised firms.

Second, we focus on the effects of the WSS on relevant firm-level outcomes, specifically firm survival and resilience over the short (6 months) and medium (12 months) term time horizon. We address this by estimating both how subsidised firms survival rates compared to similar firms that didn't receive WSS-payments, and how the firms' subsequent employment growth compared. We estimate that receiving WSS payments had positive effects on firm survival rates over the following 12 months: for example, we estimate that firms receiving the March 2020 wave subsidy had about 20% lower 'death' rates after 12 months than similar unsubsidised firms; the estimates are similar for the March 2021 wave, about 12% lower following the Resurgence wave, and not substantively different for the Extension wave. However, we also find that there was slower employment growth in subsidised than non-subsidised firms – e.g. we estimate that employment in March 2020 WSS wave subsidised firms was about 2% lower than in similar unsubsidised firms after 12 months.

Third, we focus on the WSS effects on worker-level outcomes. In particular, we estimate the effects on workers job-retention (continuity) with a firm, workers employment continuity (in any firm), and their monthly earnings when employed, over the 12-months following each WSS wave. We estimate positive effects of the WSS on job-retention over the short (6-months) and medium (12-months) term for the March 2020, Extension and March 2021 waves: our preferred estimates range from 3.5–9.3 percentage points (pp) after 6-months, and 1.2–4 ppt after 12-months; and small (approximately zero) effects for the Resurgence wave. Largely mirroring these results, we also find positive employment effects over the short term for the March 2020, Extension and March 2021 waves (workers 'receiving' subsidy payments had 2.0–9.5 ppt higher employment rates after 6-months). Over the medium-term, employment effects were positive for the March 2020 and Extension waves (1.3–5.3 ppt higher employment) and slightly negative for the Resurgence wave (-0.8 ppt after 6-months, and -0.5 ppt after 12-months). However, despite these positive job-retention and employment effects, we estimate that workers who received March 2020 wage subsidy payments experienced slower subsequent monthly earnings growth than comparable non-subsidised workers: our preferred estimated effects were 6.5% lower earnings growth after 6-months and 10.1% lower earnings growth after 12-months. The

estimates for the later waves are more mixed. The effects are generally smaller in magnitude and there is evidence of both positive and negative differences.

We have analysed the impacts and reported results across various firm- and worker-level subpopulations, including by firm age, size, ethnicity, region and industry, and by worker age, sex, ethnicity, region and industry. Although the effects vary across groups, the differences are complicated to summarise succinctly. For example, we estimate that Māori and Pacific peoples' employees were less likely to receive wage subsidy payments than Europeans, while Māori and Pacific peoples' sole traders (non-employees) were more likely to receive wage subsidy payments than Europeans. Furthermore, the estimated job-retention and employment effects are generally stronger for Pacific peoples than European workers across the waves, while the outcomes for Māori workers are comparable to those of European workers. Younger workers had lower subsidy take-up rates, but substantially higher job-retention and employment effects.

Finally, we also investigate the third evaluation question, concerning whether the WSS had possible unintended consequences, including supporting non-viable firms, interrupting optimal job reallocation flows, and whether businesses receiving wage subsidy payments complied with their obligations to pass on the subsidy payments to workers and endeavour to pay them at least 80% of their usual earnings. We find no systematic evidence that the scheme supported non-viable firms, or substantively affected the job reallocation process. However, our findings that subsidised firms had higher survival rates and lower employment growth than unsubsidised firms suggest that the WSS may have kept firms in operation that had poorer growth prospects. In addition, consistent with businesses complying with their obligations under the scheme, we find that some subsidy receiving firms tended to pay workers at the higher of the part-time or full-time subsidy rate, or 80% of their prior earnings, during periods of subsidy receipt: this was relatively more likely to occur during the original (March 2020) subsidy wave, and to a lesser degree the Extension-wave.

The rest of the paper is organised as follows. We begin by summarising the policy context and New Zealand's COVID-19 Wage Subsidy Scheme the next section. In section 3 we review the international literature on similar wage support policies during the pandemic. We then discuss the methodological approach we use for the outcome evaluation analysis in section 4. The analysis of the WSS impacts on firm outcomes is covered in section 5; and the analysis of worker outcomes in section 6. In section 7 we discuss the evidence on several possible unintended consequences associated with the wage subsidy scheme, then discuss how our main results compare with international findings in section 8, and discuss the implications of our analysis for

the key evaluation questions in section 9. The paper concludes with a summary discussion in section 10.

2 Policy background and the wage subsidy scheme

2.1 COVID-19 Wage Subsidy Support in context

COVID-19Wage Subsidy Support was a key component of the New Zealand Government's response to the COVID-19 pandemic. The first case of the disease in New Zealand was reported on 28 February 2020. On 17 March, the government announced a COVID-19 Economic Response Package, with the Wage Subsidy Scheme (WSS) as the single largest item of expenditure. The package provided substantial financial support for businesses and workers, with the stated aim "to support New Zealanders and their jobs from the global impact of COVID-19" (New Zealand Government 2020). The support package included a broad wage subsidy, initially for 12 weeks, to support businesses that experienced revenue losses as a result of the pandemic and the associated policy responses. The subsidy was intended to help firms maintain employment relationships and contribute to worker incomes while people were not able to work.

New Zealand's initial policy response was one of the most stringent in the world. Borders were closed on the 19th of March and the country was placed in a strict lockdown on 25 March 2020. Figure 1 shows that although New Zealand's initial policy response was among the most stringent, the stringency was comparatively short-lived. Other countries maintained a moderately high level of stringency for an extended period. The initial wave of WSS support was extended for 8 weeks in June 2020, reflecting the ongoing hardship being faced by firms. Further waves of subsidy support were introduced in response to subsequent restrictions and regional lockdowns to manage the risk of COVID-19 outbreaks. New subsidy waves were started when the alert level in Auckland was raised to level 3 in August 2020, (Resurgence Support), and March 2021. In August 2021, a new wave commenced when a national level-4 lockdown started on 17 August. The national alert level was lowered on 27 August, but Auckland remained at level-4 until 21 September.

Despite the intermittent nature of WSS support, New Zealand's WSS support represented one of the most substantial job retention policies in the OECD, both in terms of expenditure and in terms of the proportion of employment covered (Eichhorst et al. 2022; OECD 2022b). By December 31, 2020, the government had spent around NZ$18bn on COVID-19-related initiatives, including NZ$13.3bn (4.1% of GDP) on wage subsidies and leave payments and NZ$1.7bn on small business loans. Government spending on this scale has significant macroeconomic effects,

as well as microeconomic impacts on subsidised businesses and their workers. The NZ Treasury (2020a) report that the fiscal stimulus associated with the support package amounted to over 6% of GDP, and was expected to cushion the negative economic impacts of the pandemic and the associated health measures, including restrictions such as lockdowns.

The outlook for the labour market was expected to be bleak. Even factoring in the potential effects of up to $20 billion of support measures, Treasury (2020b) was envisaging scenarios in which the unemployment rate reached 17.5% in June 2020, and did not return to pre-lockdown levels for at least 4 years. The out-turn was considerably more positive, with the unemployment rate peaking at 5.3% in 2020q3, and dropping to below pre-lockdown levels by 2021q2. Similarly, it was posited that output (quarterly GDP) would remain below 2020 levels through until 2022, but returned after a one-quarter drop of 10% in 2020q2, and recorded annual average percentage growth of 5.3% in the year to June 2021.

The substantial fiscal stimulus associated with the COVID-19Wage Subsidy Support would almost certainly have contributed to the better-than-expected macroeconomic and labour market performance. It is difficult, however, to dismiss other contributing factors, such as the success of public health measures in reducing the health impact of the pandemic, or overly pessimistic scenarios formulated in the face of extreme uncertainty.

This outcome evaluation aims to identify the impact of wage subsidy support on outcomes for subsidised firms and the workers listed on their subsidy applications. We do this by comparing those outcomes with the outcomes of otherwise similar, but unsubsidised, firms and workers. This approach will fail to fully capture any positive macroeconomic effects of wage subsidy support as a fiscal stimulus. The difference in outcomes between subsidised and unsubsidised firms will not reflect any benefits that may accrue to unsubsidised firms. Stimulatory macroeconomic effects for unsubsidised firms are likely to be most pronounced in sectors that supply to the domestic market.

2.2 New Zealand's COVID-19 wage subsidy scheme

In response to the pandemic, the government swiftly implemented the 2020 COVID-19 Wage Subsidy Scheme (WSS) to combat the increasing unemployment which risked a deeper recession.[1] Entering the country wide lockdown in March 2020 at alert level-4, to minimise the risk of the contagion spreading and overwhelming the health system the government required the population to stay home in their 'bubbles'. Only essential workers were able to attend

[1] With consumption halted as stores remained unable to open and workers idle, the unemployment rate effectively doubled from 5.2% prior to lockdown to 10.5% by week 3 (Fletcher, Prickett, and Chapple 2021).

work.[2] The first iteration of the WSS (the 'Original' or March 2020 WSS) operated from 27 March through to 9 June 2020, to help firms by reducing the labour costs associated with retaining employees unable or impeded from working, and to help preserve jobs. It operated by providing financial assistance to businesses that had been negatively impacted by COVID-19. It paid firms a weekly lump sum payment of $585.80 per employee for full-time workers (working for more than 20 hours per week), and $350 per employee for part-time workers for an initial period of 12 weeks.[3] This equated to 57.6% of the median 2019 weekly New Zealand earnings. Employers were required to keep all employees whom they received a wage subsidy for, and were urged to maintain employee's regular wages, though these could be negotiated between the employer and employee to a minimum of 80% of the employee's regular wages. To be eligible for the subsidy, firms were required to show that business revenue was 30% lower in the previous 30 days compared to a similar time-period in the previous year. (MSD 2020a)

The second WSS iteration (the WSS 'Extension') was open from 10 June through to 1 September 2020. Together, the March 2020 and Extension waves represent phase-1 of the WSS, both being nationally focused. The WSS Extension had the same goals as the initial iteration, reflecting continued reduction in economic activity. The Wage Subsidy Extension required firms to show a 40% revenue decline in the last 30 days, offering the same payments as the March 2020 wave but in an 8-week lump sum payment. Employers could not apply for a subsidy for employees who had already been given a notice of redundancy, and repayment obligations applied for any self-employed worker who received a higher income on the subsidy than they would normally receive in its absence (MSD 2020c).

The second phase of the WSS, although available nationally, tended to be more regionally focused on Auckland's higher alert levels, and also involved more targeted support. This phase began with the third ('Resurgence') WSS iteration, available originally for the 14-day period from 21 August to 3 September 2020. To be eligible, firms had to show or predict a revenue decline of at least 40% for any 14-day consecutive period between 12 August to 10 September. In this phase, the subsidy duration was two weeks (MSD 2020d). The fourth iteration of Wage Subsidy Scheme occurred in March 2021, and included payments to support businesses, employers, and employees affected by the change in alert levels on 28 February 2021. Applications opened on 8 March 2021. To be eligible, firms had to show or predict a revenue decline of 40% for 14 consecutive days in the period between 28 February to 20 March 2021 compared to a two-week period between 4 January and 14 February 2021 (MSD 2021).

[2] Those who worked in food, medical supplies and other essential production and service industries (Maani 2021).
[3] Capped at $150,000 per firm initially, with the cap removed in subsequent reviews of the policy.

Overall, the various Wage Subsidy schemes had cost $19.0 billion to February 2022, including $0.16 billion for various leave support schemes. This covers a cumulative number of 2.6 million applications, of which 2.1 million (82.2%) were approved.[4] Refunds of $0.8 billion have been received from about 22,000 recipients.[5] The cost associated with the Original WSS was $11 billion over the first 12 weeks of the programmes (MSD 2020b). Together with the Extension WSS, the programme had supported 1.65 million jobs at a cost of about $14 billion (4.5% of GDP) by mid-2020 (MSD 2020e).

Although the WSS was by far the most substantial form of business support within the Government's COVID-19response, there were other components, including the COVID-19 Leave Support Scheme, and the Small Business Cashflow Scheme (SBCS). The Leave Support Scheme, which is still active at the time of writing (March 2023), allows employees to receive pay if they cannot come to work because they must self-isolate, or if they cannot work from home. It offers a flat rate of $600 a week for full-time employees (20+ hours a week) and $359 a week for part-time workers to employees who have been advised to self-isolate for at least 4 consecutive days. Subsequent payments are made if the employee must keep self-isolating for at least 11 calendar days or more, and again for every further seven days.

The SBCS was a programme aimed at supporting small to medium businesses dealing with loss of actual revenue attributable to the pandemic. Initially implemented in May 2020, this policy remains available for firms who meet eligibility requirements, with applications currently open until 31 December 2023. To be eligible, a firm must have 50 or fewer full-time-equivalent employees (FTEs), have been in business for at least 6 months, have experienced a 30% decline in actual revenue over a 14-day period relative to the same time period one year earlier, and must be viable. The loan must be used to pay for core operating costs such as rent, insurance, utilities, supplier payments, and rates. The maximum amount available to be loaned is $10,000, (from February 2022, the base loan was raised to $20,000 plus $1,800 per full-time equivalent employee, for up to 50 employees) and it is interest free if the loan is paid back within a year. Repayment is not required within the first 2 years, and payments one year after taking out the loan have an interest rate of 3% for a maximum term of five years.[6]

[4] We have not undertaken an exhaustive analysis of why around one in five applications was declined. The data that we had access to did not contain details about reasons for decline. However, a high proportion of declines appear to be for technical reasons, which we infer from the fact that many firms that were declined were subsequently approved when they re-applied. For instance, applications in the Resurgence wave were declined if they were submitted before the end of the period covered by Extension wave payments. Re-applications when eligible were generally approved.

[5] (MSD 2022). Given the time pressures of the scheme, the government opted out of individually assessing each application, instead broadening the eligibility to offer greater cover. Refunds may have been made because firms' income losses proved less severe, or they were unable to keep employment levels at their pre-pandemic rate. Some refunds were made voluntarily, while others were initiated or prompted by policy integrity investigations from government agencies.

[6] https://www.ird.govt.nz/covid-19/business-and-organisations/small-business-cash-flow-loan

3 COVID-19wage support schemes around the world

Several different economic policies were implemented around the world in response to the COVID-19 pandemic. These varied by the social and political schemes active in each country, as well as by eligibility, generosity, and the overall goals of the schemes. Job retention schemes, in particular, were deployed in many countries to offset expected job losses in response to the recession. In a review of job retention schemes used across countries, the OECD groups these policies into two categories (OECD 2020): short-time work (STW) compensation schemes, or some other wage subsidy scheme (WS).

New Zealand is relatively unusual among OECD countries in that it did not have a pre-existing STW scheme. The OECD (2020) summarises the nature of job retention schemes in 34 OECD countries (Further discussion and some examples are provided in the following sections). Of the 23 countries with pre-existing STW schemes, 20 enhanced the existing schemes by increasing access, coverage, or the level of benefits. The remaining three countries introduced a new wage subsidy scheme to address the challenges of the pandemic. Of the 11 countries without a prior STW scheme, seven introduced a new STW scheme, and three (New Zealand, Canada, and Ireland) introduced new wage subsidy schemes. Although New Zealand's path was unusual within the OECD, Eichhorst (2022) identifies six additional non-OECD countries that introduced wage subsidy schemes in response to the pandemic. New Zealand also stands out as the country with the highest take-up of job retention support, and relatively low reliance on unemployment benefit policy changes (Denk and Königs 2022; OECD 2020).

Job retention policies such as STW schemes and wage subsidy schemes were often complemented by other policies to further offset the risk of a deep recession. These included policies such as small business cash flow loan schemes; short-term absence payments, for employees who are at home awaiting test results; leave support schemes, for employees who have to stay home due to government imposed lockdowns but cannot, for various reasons, work from home; resurgence support payments; income relief payments, for employees who lost their jobs directly due to COVID-19; transition pay schemes; events transition support payments, to help facilitate/organise large events with the aim of boosting community spirit and support; business debt hibernation schemes, for companies who can only repay a fraction of their debt due to revenue losses; and industry specific recovery programs. This outcomes evaluation focuses primarily on the WSS policy itself, with some controls for other forms of business support. WSS support was, however, by far the largest component of business support in New Zealand.

3.1 Short-time work schemes

As the name suggests, short-time work (STW) schemes operate by directly subsidising firms to help cover, at least partially, their workers' earnings during periods of temporarily reduced hours of work. Thus, STW schemes require a reduction in hours worked to trigger a subsidy payment, and subsidy payments increase with the shortfall in hours worked. In contrast, other wage subsidy (WS) schemes were used to subsidise firms' labour costs, but these did not require a drop in hours worked (OECD 2020). Designed to subsidise firms' employment costs through the pandemic and lockdowns, WS schemes are targeted at firms and the self-employed to help continue paying staff and to protect jobs. Although WS eligibility typically requires some decline in business activity (e.g. loss of revenue), because the subsidy amount is not tied to a reduction in hours worked, WS schemes have a higher risk of deadweight loss than STW schemes. Such deadweight loss would occur if support were given to firms that could have maintained their employment levels even without a subsidy. However, because WS eligibility is typically based on passing some threshold and the subsidy does not increase with severity of the decline in activity, WS schemes may have lower risk of impeding Schumpeter's creative destruction and creating unviable ("zombie") firms (Laeven, Schepens, and Schnabel 2020).

Short-time work (STW) schemes directly subsidise firms for hours not worked by employees during a downturn in demand. They subsidise workers' loss of wages so they can reduce their work hours without facing the need to find alternative employment (OECD 2020). Typically, STW schemes operate as part of a country's unemployment insurance (UI) structure, and fund employees below what they would normally earn while working full-time, but above the expected earnings they would receive from an unemployment benefit (Cahuc 2019). This encourages employees to remain attached to their employer and to the workforce. Ordinarily, STW schemes operate with the overall goals of ensuring employment continuity and to act as insurance for eligible workers to hold onto their jobs.[7]

Countries with pre-existing STW schemes include Canada, the US, several European countries (e.g. France, Germany, Italy), and Japan.[8] Typically, these countries amended their respective schemes to relax the short-time eligibility criteria, broaden coverage of workers in non-standard employment, and increase the generosity of support: either through the subsidy rate to workers, the share of the subsidy paid by the UI system, and/or length of the subsidy

[7] Giupponi et al (2022) discuss the differing effects of insurance schemes and short term work schemes.

[8] Other countries that amended their short-time work schemes include Austria, Belgium, Chile, the Czech Republic, Finland, Korea, Luxemburg, Norway, Portugal, Slovak Republic, Spain, Sweden, Switzerland, and Turkey. The expansion of these schemes typically fell into three broad categories: simplifying access and extending coverage; extending coverage to non-permanent workers; and raising generosity.

entitlement period. For example, in Germany which had a long standing STW scheme (Kurzarbeit), the trigger point for firm support was reduced from 30% of the workforce affected by reduced hours to 10%, while the subsidy replacement rate to workers for hours not worked increased from 50% to 80% in 2020 (87% for workers with children),[9] and the social insurance contributions increased from 50% to 100% of the cost of the subsidy.

Similarly, France amended their STW scheme (Activité Partielle) to extend the maximum duration of the scheme from 6 to 12 months – an extension that was phased out in 2022. In lieu of COVID-19 subsidies, firms could apply for assistance for up to 30 days past the first reduction in hours observed (OECD 2020). Applicable to all contracted employees, permanent or otherwise, firms paid 70% of workers gross wage and did not bear any cost for hours not worked with the state reimbursing workers' pay at up to 4.5 times the hourly minimum wage (Eurofound 2020b; 2020a). Italy too amended their STW scheme (Cassa Integrazione Guadagni), to increase eligibility, with firms able to apply for an extended four months in response to reduced revenue due to the pandemic. Benefits were paid at 80% of gross wages, capped at varying levels, that resulted in an effective replacement rate of approximately 45% if hours were reduced to zero. These wages were subsidised for a maximum duration of 14 weeks, enabling the protection of workers' incomes (Eurofound 2021).

The US used a multi-pronged approach in response to the effects of the pandemic, through the Coronavirus Aid, Relief, and Economic Security (CARES) Act. The federal government fully funded the STW (Short-Time Compensation, STC) payments for 26 states with existing schemes, and 50% of payments for states that introduced a new STC scheme (Holzer, Hubbard, and Strain 2021; OECD 2020). In addition, all workers who received STC support were entitled to additional weekly benefit payment of USD600 (the same received by those on unemployment benefits). The US also introduced two limited wage subsidy schemes, the Paycheck Protection Programme (PPP) and the Employee Retention Tax Credit (ERTC) (OECD 2020). The PPP functions by providing small businesses (<500 employees) loans to pay their employees during the crisis,[10] regardless of the effect the pandemic has had on their sales. These loans were to be forgiven if the companies conform to certain requirements such as spending 60% of the loan on payroll, not reducing the number of employees from pre-pandemic levels, and not reducing employee wages by more than 25% of the pre-pandemic rate. If these conditions were not met, some of the loan amounts would need to be repaid. The ERTC was a refundable tax credit available until the end of 2021 covering 50% of all employees' wages, available to employers who saw a decline in sales

[9] These increased in three stages: 60% in March, 70% in April and 80% in July 2020.
[10] Under the programme, businesses can borrow up to 2.5 times their average monthly payroll costs, capped at USD$10 million.

of over 50%. It was also available to larger firms but only for the wages of workers who do not work during the crisis (Autor et al. 2022b). Finally, the USA also implemented two unemployment insurance schemes: the Pandemic Unemployment Assistance (PUA), which extended eligibility of unemployment benefits to workers who would otherwise be ineligible; and the Federal Pandemic Unemployment Compensation (FPUC), which added a federally funded USD600 weekly supplement on top of existing standard unemployment insurance benefits.

Canada responded to the pandemic by utilising an existing STW scheme, the Canada Recovery Hiring Program (CRHP), to counteract expected job losses. The CRHP works by covering part of employees' wages if firms hire new employees while also increasing existing employees' wages or hours (Canada Revenue Agency, 2021). Specifically, it "supports employers with a subsidy of up to 50% on incremental remuneration paid to eligible working employees" to encourage businesses to hire and grow as the economy recovers. To complement this, the Canadian government also implemented a new WS (the Canada Emergency Wage Subsidy, CEWS) to further aid the country. The CEWS was on offer from March 2020 until September 2021 to qualifying employers who suffered a drop in revenue attributable to COVID-19. It worked by subsidising employers a percentage of their employees' pay based on their observed revenue loss (Canada Revenue Agency, 2021).[11]

Japan also amended its existing STW scheme (Koyo Chosei Jyoseikin), to simplify access, expand its coverage, and increase benefits. In particular, the reduced production eligibility criterion for firms was reduced from a 10% reduction in production for more than three months to a reduction of 5% over one month, and the subsidy rates for hours not worked increased to 100% for SMEs and 80% for larger firms. Additionally, the programme now covers non-regular workers who are not covered by employment insurance (Hamaguchi 2020; Tsuruga 2020).

Several countries introduced new STW schemes, including Denmark and the UK.[12] In Denmark, the new STW scheme (Arbejdsfordeling) replaced an older and rarely used scheme, and operated by job-sharing reduced hours of work (e.g. two workers sharing a 40 hour per week job). During employees' reduced days of the week, the employee was then eligible for unemployment insurance.[13] This was complemented by a supplementary scheme, the Lønkompensation (Wage Compensation), designed to reduce pandemic related layoffs (Kvist

[11] In October 2021, the CEWS was replaced by the Tourism and Hospitality Recovery Program (THRP), the Hardest-Hit Business Recovery Program (HHBRP), and the CRHP. Eligible employers can claim either the higher of the CRHP or the wage portion of the THRP or HHBRP

[12] Other countries that employed new short-time work schemes include Greece, Hungary, Iceland, Latvia, Lithuania, and Slovenia.

[13] This policy gained much traction, increasing more than 50 times its average use of 102 persons in March 2015 and 2019 to 5,471 in March 2020.

2021). The policy was offered to companies who intended to dismiss 30% of their workers, requiring firms to send employees home instead. Furloughed employees were then paid using a monthly benefit payment given to the employer of EUR2,085 for white collar employees and EU3,490 for blue. These were later both lifted to EUR4,025 per month, covering 75% of the salaries of white-collar workers and 90% of the wages of blue-collar workers overall.[14] These schemes were further complemented by amendments to unemployment insurance, income compensation schemes for the self-employed and freelancers, sickness benefits, etc.

Similarly, the United Kingdom implemented the Coronavirus Job Retention Scheme (CJRS). This scheme offered UK employers a grant covering up to 80% of the wages of employees who would otherwise have been laid off because of the pandemic. These employees were placed on 'furlough' whereby they didn't work but continued to be paid (Mayhew and Anand 2020).[15] In July 2020 it was replaced by a new phase known a 'flexible furlough'. This phase amended the scheme such that furloughed staff were now able to work part time with employers and still maintain access to the grant for their non-work hours.

3.2 Other wage subsidy schemes

Other wage subsidy (WS) schemes were used to subsidise firms' labour costs, but were not tied to a reduction in hours worked (OECD 2020). New wage subsidy schemes were also implemented around the world to replace existing STW schemes. Ireland created the Pandemic Unemployment Payment (PUP), a policy which entitled €350 per week to workers who lost their jobs due to COVID-19 (Byrne et al. 2020). Furthermore, the Irish government also implemented the Temporary Wage Subsidy Scheme (TWSS), enabling employers to directly support workers and keep them on payroll for when business picked up post-crisis. The Employment Wage Subsidy Scheme (EWSS) replaced the TWSS in September 2020, having been introduced in July and working parallel to the TWSS until August 2020. The EWSS differed to the TWSS in that it provided a flat-rate subsidy to eligible firms based on the number of eligible employees on their payroll.

Comparably, the Netherlands initially utilised WerkTijdVerkorting (WTV), an existing STW scheme which provided extended unemployment benefits to firms who kept employees through the crisis. This scheme, however, was superseded by the Noodmaatregel Overbrugging Werkgelegenheid (NOW) which operated by providing compensation for the wage costs of

[14] The worker receives full wage compensation but must take holiday meaning the employer only has to pay 10-25% of the wages.

[15] Under the policy's guidelines, employees could be furloughed when they were unable to work due to shielding in line with public health guidance or if they had caring responsibilities resulting from the pandemic (including caring for children).

employers who expected a loss of revenue of at least 20% over a three consecutive month period. Specifically, it subsidised up to 90% of labour costs for firms who realised these predicted reductions in turnover (Groenewegen, Hardeman, and Stam 2021). These policies were coupled with schemes such as the Tegemoetkoming Ondernemers Getroffen Sectoren (TOGs) COVID-19 and Tegemoetkoming Vaste Lasten (TVL) which provided SMEs with tax-free allowances to pay fixed costs such as rent.

Australia also introduced a new WS, the JobKeeper (JK) Payment. In its initial phase, the WS paid a flat subsidy of $750 per week to employees and the self-employed from 30 March 2020. To qualify, employees needed to have been working at a firm before 1 March 2020 or reattached to the firm if they were laid off prior to the announcement. Firms also needed to have experienced or anticipated a reduction in turnover relative to a comparable period a year ago of a set amount depending on average turnover.[16] Once eligibility was established, an organisation received the WS through to 27 September, irrespective of subsequent turnover. Phase 2 amended the size and eligibility of the WS, reducing its generosity to better reflect hours actually worked by employees (Bishop and Day 2020; Borland and Hunt 2021).

Outside the OECD, Eichhorst et al (2022) document that Egypt, Peru, Philippines, Serbia, South Africa, and Vietnam also introduced new or adapted wage subsidy schemes in response to COVID-19.

4 Approach to outcome evaluation

4.1 Evaluation rubrics

Rubrics for the WSS outcome evaluation were developed in consultation with MartinJenkins, with consideration given to input from workshops with key agencies in January 2022. The following table summarises the key evaluation criteria and factors that were identified for consideration.

In the analysis that follows, we have examined outcomes across all of the identified criteria. Due to limitations in data availability, there are some of the components that have not been examined. We have not examined the effect of WSS support on household income or hardship. Such an analysis would have required reliance on a relatively small sample from a household survey such as the HLFS in order to reliably identify household. Firms were permitted to reallocate excess subsidy payments to other subsidised workers, but it is not feasible to

[16] These reduction boundaries are 15 percent reduction for a registered charity; and a 30 or 50 percent reduction for businesses with turnover respectively less or more than $1billion.

identify whether this occurred as the subsidy portion of worker earnings are not separately identified. The analysis of tax payments from firms would require full-year tax data which are available only after the end of the tax year, and which were not comprehensively available in the LBD at the time of analysis.

CRITERION	COMPONENTS FOR CONSIDERATION	EXPECTED DIFFERENCES BETWEEN SHORT TERM AND MEDIUM TERM

Firm survival	• Business closures
	• Business turnover
	• Potential unintended consequences:
	  • Prolonged survival of non-viable firms/ delayed firm exits
	  • Money supported businesses but not employees
	  • Number of firms that received more than needed to replace lost earnings;
	  • average excess subsidy received; whether or not excess subsidy was repaid
		Short term reduction in closures; medium term return to baseline rates of firm birth and closure

Employment continuity / job attachment	• Job tenure: whether people stay with the same firm? • Redundancies / job losses	Short term increase in tenure; medium term return to baseline rates of job turnover
Labour market attachment	• Employment rate (probability that a person has a job?) • Transitions into and out of employment • Incidence of benefit receipt • Potential unintended consequences • Lower than usual worker turnover	Proportion of workers employed remains high in short and medium term

Earnings / employee income	• Wage and salary earnings
	• Individual income
	• Household income
	• Household hardship
	• Potential unintended consequences
	  • Subsidised earnings higher than previous earnings
	  • Average excess subsidy received
		Short term earnings remain above 80% of prior earnings
		Limited and evenly experienced increase in hardship

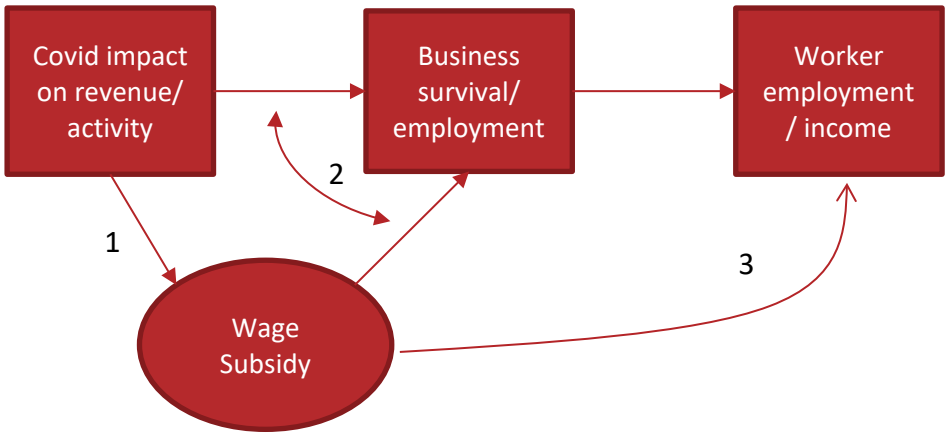
Business confidence	• New jobs created / firm willingness to hire • Job creation rate/ accession rate/ net employment growth	Medium term: Firm expansion and hiring rises to baseline levels
Value for money	• Number of firms saved; cost per firm saved • Number of jobs saved; cost per job saved	Short term net cost; Positive net benefit in medium term The benchmark is not clear. Success will be gauged relative to international experience of similar policies
Increased engagement with the tax system	• Potential unintended consequence • Number of firms paying tax; amount of tax paid	No difference expected in the short term

4.2 Overall design of outcome evaluation

The outcome evaluation takes a quantitative approach to assessing the effects of the first four wage subsidy waves on various firm and worker-level outcomes. The insights from this analysis will be enriched when combined with insights from the associated process evaluation.

The key assumption that is tested in the outcome evaluation is that wage subsidy support led to differential outcomes for subsidised and unsubsidised firms, and hence, on the employees in such firms. Specifically, we investigate whether wage subsidy support changed the way that affected firms altered employment levels and composition in response to COVID-19-related changes in revenue or activity. We emphasise that the focus on the evaluation is on the effects of the WSS, not on the effects of the COVID-19 pandemic or lockdown response *per se*. Thus, one of the key challenges is to identify the impacts of the wage subsidy support separately from the effects of the COVID-19 pandemic itself and of the associated lockdowns and restrictions. The key elements of the analysis are summarised in the following diagram:

- Eligibility selection (1): the initial impact of COVID-19 and the associated public health policy measures was to affect business activity and revenue. Because the WSS was targeted assistance, firms that applied for and received support are a selective sample of firms. We will analyse this pattern of selection with a view to identifying firms that experienced a similar impact from COVID-19/ lockdown, some of which received WSS and some that did not.
- Counterfactual business outcomes (2): A comparison of business outcomes (business survival; employment levels) of comparable subsidised and unsubsidised firms will provide the primary basis for evaluating the impact of WSS on businesses.
- Determinants of worker outcomes (3): Whether workers received WSS will depend on the same factors that affect business selectivity. In addition, worker receipt of WSS may also depend on characteristics of the workers themselves. To identify the impact of WSS on worker outcomes, we will document the incidence of WSS for different groups of workers, and then compare outcomes of subsidised and unsubsidised workers with similar exposure to firm outcomes and with similar characteristics.



We estimate the nature, strength, and variability of these contributors to business and worker outcomes using microdata on individual firms and individual workers. This involves a substantial amount of analysis of data sources held within Statistics New Zealand's Integrated Data Infrastructure (IDI) and Longitudinal Business Database (LBD).

The first key outcome evaluation question involves several aspects, which we broadly summarise in terms of:

1) To what extent did the WSS reach the intended workers and firms, and what was the selectivity of who received the WSS payments?
2) What effects did the WSS have on firms' resilience and survival?
3) What effects did the WSS have on workers' employment retention by firms, and on workers' incomes and other key (labour market related) outcomes?
4) How were the outcome effects of WSS distributed across population groups, and across firms, industries, sectors and regions?

The key challenge for the outcome evaluation is that outcomes for subsidised and unsubsidised firms and workers will differ for reasons other than subsidy receipt. Subsidy support was targeted towards firms with larger revenue declines, and some firms such as those in 'essential industries' were less adversely affected by the COVID-19 lockdowns. Our general approach to the outcome evaluation is to model the selectivity of subsidised firms, and compare outcomes of subsidised firms with average outcomes for unsubsidised firms that are similar to the subsidised firms.[17] The factors included in the probability equation capture differential exposure to the adverse effects of the COVID-19 lockdown, which varied across industry and region, differential ability of firms to operate without subsidy support (prior growth, firm size and age), and possibly differential knowledge of or access to subsidy support, which could vary by any of these factors, or by the ethnicity of the firm.

Probability equation (1)

$$P(Subsidised_i) = f(industry_i, region_i, size_i, prior\ growth_i, ethnicity_i, age_i)$$

We use the estimated probabilities from an equation such as (1) to reweight data on unsubsidised firms – giving more weight to unsubsidised firms with a high predicted probability of being subsidised. The probability equation also sheds light on the sort of firms that had higher subsidy take-up, and the characteristics of firms that were less likely to have received subsidy support.

This weighting is used when estimating the relationship between subsidy receipt and firm or worker outcomes. Equation (2) illustrates the structure of the outcome equations that we estimate and report:

Outcome equation (2)

$$Outcome_i = f(subsidy\ receipt_i, other\ controls_i).$$

The specific form of the equation, and the choice of controls, vary across the different outcomes that we examine, and are described in more detail in later sections of this report.

[17] The resulting estimates provide an indication of the effect that subsidy support had on supported firms (referred to as the "effect of treatment on the treated" in the economics evaluation literature). We do not estimate the effect that subsidy support would have had on firms that did not receive subsidy support.

Key firm-level outcomes are firm survival (section 5.4) and levels of employment (section 5.4.3). Because subsidy receipt is effectively determined at the firm-level, we use reweighting based on firm selectivity for the analysis of worker level outcomes as well. For workers, we focus on outcomes of job-retention (section 6.3.1), whether they continue to be employed (possibly in other jobs) (section 6.3.2), and their earnings levels when employed (section 6.3.3)

For both worker and firm outcomes, we investigate subgroup-specific probabilities of receiving subsidy support, as well as subgroup-specific differences in the effect of support on outcomes. For firms, we examine differences by industry, region, firm ethnicity,[18] size, and age. For workers, we examine differences by age, sex, ethnicity, region of residence, and industry.

4.3 Analytical approach

The objective of our analysis is to provide causal estimates of the effects of receiving wage subsidy support payments on firm outcomes, such as survival and employment growth, and worker outcomes, such as job retention, employment continuity and earnings growth. In this section we describe the approach used for analysing the effects of receiving wage subsidy support on such outcomes of interest.

The essential problem in attempting to estimate the causal effects of the wage subsidy on firms and workers outcomes concerns possible bias caused by non-random selectivity of the wage subsidy support across firms and workers. To the extent that resulting bias is associated with observable characteristics, two common methods have been adopted to control for bias. First, using observable covariates in multivariate regressions for the outcomes of interest to control for differences that are correlated with the wage subsidy receipt; and second, using (typically propensity score) matching methods to control for the selectivity bias by balancing subsidy-receiving and non-receiving subgroups. We adopt the approach of Hirano and Imbens (2001) that combines these commonly used regression-adjusted and propensity score methods to provide a "doubly-robust" approach to controlling for bias in the estimated causal impacts of the wage subsidy. That is, it provides a relatively flexible way to control for selection bias by controlling for the effects of observable characteristics both on receipt of subsidy payments and on the outcomes of interest.

We focus on an "average treatment on the treated" (ATT) analysis to measure the average effects across firms (or workers) that receive wage subsidy payments in each wave. Our approach uses a two-stage method to estimate the effects of COVID-19 wage subsidy support for each wave of WSS support, and separately for (ever) employing firms and for sole-trader firms.

[18] The approach to defining and identifying firm ethnicity is summarised in Appendix 1: Firm ethnicity.

In the first stage, we estimate the probability that each firm receives subsidy support in a particular wave (i.e. the 'propensity score'), based on their characteristics in the first month of the support and in the previous 14 months. Then, in the second stage, we estimate weighted regressions for the outcomes of interest, in which the non-subsidised firms are weighted using a function of the estimated propensity scores to provide a balanced comparison sample to the subsidised firms.

Specifically, in the first stage we estimate the probability that each firm receives subsidy support in a particular wave. This "propensity score" estimation is based on a logistic regression model:

$$P\big(Subsidised_{i,t=0}\big) = f(X_{it}, \{X_{i,t-j}\}; \beta) \quad (3)$$

where X_{it} includes indicators for industry, the proportion of employees (or of business locations if no employees) in each regional council area, the log change in average monthly sales (and employment if the firm had employees) for the two months prior to the subsidy wave starting, relative to the average level in the preceding year (3-15 months prior to the wave), the log of average monthly sales in the prior (3-15 months earlier) period, indicators for employee-size categories for employing firms, the age of the firm at the start of the wave, and separate indicators for whether the firm is a Māori, Pacific, Asian, MELAA, or European/other firm.[19] The predicted probability from this model is firm-*i*'s propensity score ($\hat{P}_i$).

In the second stage of our approach, we use the predicted probability of receiving support to derive weights for unsubsidised firms, to provide appropriate counterfactual analysis. If $\hat{P}_{iw}$ is the estimated propensity score that observation-*i* (either firm, job or worker) received wave-*w* wage subsidy, we estimate the weight $\omega_{iw} = 1$ for WSS-recipient observations, and $\omega_{iw} = \hat{P}_{iw}/(1 - \hat{P}_{iw})$ for WSS non-recipient observations. To estimate the average WSS treatment effect for WSS recipients (ATT) on an outcome of interest y_i, we then use these weights to estimate regressions of the form:

$$y_i = \beta CWS_{iw} + X_i'\gamma + \epsilon_i \quad (4)$$

where CWS_{iw} is an indicator variable for whether observation-*i* received wave-*w* wage subsidy support, X_i' is a vector of control variables, and ϵ_i captures idiosyncratic unobservable effects. In equation (4), the coefficient β captures the WSS treatment effect of interest.

This two-stage approach is designed to control for selection on observables. It provides valid estimates only if certain conditions are met. In the terminology of Rosenbaum and Rubin (1983), these conditions are referred to as "unconfoundedness", and require that, once

[19] Our characterisation of firm ethnicity is similar to those used by Te Puni Kōkiri & Nicholson Consulting (2020): for details see the Appendix 1: Firm ethnicity.

differences in observed characteristics have been taken into account, whether or not a firm receives a subsidy is not related to their subsequent outcomes. In this case, (conditional) subsidy receipt is effectively random, so that differences in outcomes can be attributed to the effect of the subsidy. In practice, it is unlikely that observed characteristics perfectly account for all of the differences between subsidised and unsubsidised firms. There may be 'selection on unobservables', meaning that even among firms with the same observed characteristics, subsidised firms may differ in unobserved ways, which could account for differences in subsequent outcomes. If subsidised firms are more adversely affected by COVID-19 lockdowns than unsubsidised firms with the same observed characteristics, estimates of subsidy effects will be negatively biased from attributing some of the adverse lockdown effects to the subsidy. The validity of our treatment effect estimates is predicated on the (untestable) assumption that controlling for observed characteristics is effective at removing most, if not all, of the bias resulting from the fact that the average subsidised firm had different prospective outcomes than the average unsubsidised firm.

4.3.1 The estimated propensity score

Beyond the concern about selection on unobserved characteristics, there are two main potential issues of concern with the estimation approach. First, is it able to distinguish firms that were more or less likely to have received wage subsidy payments. This is important for being able to identify which firms were more likely to receive subsidy payments (assuming there was variation in the underlying probability of receipt). Second, is there sufficient overlap of propensity scores between subsidised and unsubsidised firms. This is important for being able to construct counterfactuals for subsidised firms' experiences based on comparable non-subsidised firms' experiences.

To address these issues, we provide a brief summary of the propensity score estimation patterns for the original March 2020 wave. In addition, this will address the evaluation question: 'did the WSS reach the intended firms?'.[20] Figure 2 illustrates the distribution of propensity scores for subsidised and unsubsidised firms. The left-hand panel shows the patterns for firms that employ workers; and the right-hand panel shows the patterns for non-employing sole traders.

First, both panels show there is dispersion in the propensity scores across firms. For employing firms, there is a pronounced peak for subsidised firms at around 0.9, which indicates that the characteristics included in the prediction equation (3) are strongly correlated with

[20] There is a trade-off between the policy objective of wanting the subsidy to go (only) to firms that needed it, and the evaluation need to have similar firms (in terms of 'need') that did and didn't receive the subsidy.

subsidy receipt. In contrast, for unsubsidised employing firms there is a modal propensity value of around 0.2. This implies that employing firms with a greater need (higher propensity of wage subsidy support) were predominantly subsidy-receiving firms: in this sense, WSS did tend to reach the intended firms. The propensity score estimation equation shown as equation (3) includes controls for industry, location, firm age and ethnicity, as well as the level and growth of sales and, for employing firms, also the level and growth of firm size (employees). Each of these sets of controls is statistically significantly related to the probability of subsidy receipt.[21]

For sole traders, the observed characteristics are more effective at identifying which firms do not receive support, with a modal propensity score of around 0.1, and 84% of unsubsidised firms having a predicted probability of less than 0.2, while the propensity scores for subsidy-receiving sole traders was widely dispersed, and (roughly) uniformly distributed between 0.1 and 0.9. In this case, it is less clear that the WSS reached the intended sole traders: it appears that sole traders that didn't receive WSS-payments did not need them; however, many subsidy-receiving sole traders also have low propensity scores.

Second, there is also a moderately high proportion of unsubsidised employing firms with a relatively high predicted probability of receiving subsidy support. For example, about 50% of unsubsidised firms have predicted probabilities greater than 0.6, and 20% greater than 0.8; compared to 89% and 62% respectively for subsidised firms. As discussed above, such overlap is important for our estimation approach because it implies that there are sufficient numbers of unsubsidised firms with similarities to subsidised firms. This enables us to reweight the outcomes of the unsubsidised firms to create a credible counterfactual for the subsidised firm outcomes. For sole traders, there is also some overlap in the subsidised and unsubsidised distributions, due to the variation in predicted probabilities for the subsidised sole traders. Again, this overlap provides support for the reweighting approach.

5 WSS Impacts on firm outcomes

In order to evaluate the impact of COVID-19 wage subsidy support on firm outcomes, we need to control for changes in outcomes that affected both subsidised and unsubsidised firms, and account for the fact that subsidised firms differed from unsubsidised firms in many respects. The targeting of subsidy assistance to firms with relatively large revenue losses is one of the reasons that outcomes for subsidised and unsubsidised firm are expected to differ in the months

[21] Table A1 reports R^2 goodness of fit statistics from linear probability models analogous to the logistic regressions used in estimation. In the first 6 columns, each set of regressors is entered separately. In the final column, all covariates are included (including indicators for prior subsidy receipt for waves later than March 2020). Overall R^2 values are between 23.1% and 41.5%. Industry, region, and firm ethnicity have the strongest explanatory power.

following subsidy waves. In this section, we describe general patterns of change before and after the first major lockdown and examine the selectivity of which firms received subsidy support, before presenting estimates of the impact of wage subsidy support on firm survival and employment levels. An initial necessary step is to derive a consistent definition of firms based on the available data. This step is outlined in the next section.

5.1 Defining firms and WSS recipients

Data on COVID-19 wage subsidy support is organised primarily by firm. Subsidy applications were submitted by firms, with an associated list of employees provided if the firm had employees. Strictly speaking, we observe whether a firm received support for a listed employee, but we do not observe whether an employee received subsidy support per se. However, for our subsequent worker level analyses, we refer to employees as subsidy recipients if they were listed on a firm's paid wage subsidy application, and compare outcomes for recipient (listed) and non-recipient (unlisted) employees.

There were two types of COVID-19 Wage Subsidy application – one for employers, and one for sole traders. This distinction does not completely align with our classification of firm types into 'Employing' and 'Sole Trader' firms. We need a classification of firms that can be applied to unsubsidised as well as subsidised firms, so we cannot rely on a classification based on the type of subsidy application. Table 1 shows how application types relate to firm types. Of the 787,900 paid applications in the first four waves of COVID-19 subsidy support (March 2020, Extension, Resurgence, or March 2021), over half (53%) were sole trader applications. Among sole trader applications, 16% were from firms that we classify as Employing – being firms that had employees other than the working proprietor at some stage. Almost all (96%) of the Employer applications were from firms that we classify as employing, with the remaining 4% being applications where the only listed employee had the same tax-based identifier as employer.

The initial (March 2020) wave of subsidy support accounted for over half (56%) of all the applications in the first four waves, and 78% of subsidy payments. Table 2 summarises the timing and extent of support for each wave. The first wave provided the longest period of support – lasting for 84 days. It also provided support to 95% of the firms that received any COVID-19 Wage Subsidy. Table 3 summarises the number of firms, number of workers listed (with sole traders counted as one person), value of paid applications for each wave, and available repayments data, separately by firm type.[22] Although sole trader firms account for 46% of

[22] Even though (for waves other than the Resurgence wave), firms could be paid only once for any employee, there are many cases of multiple applications per firm within a wave. These may cover different employees; may represent changes

applications, they account for only 15% of subsidy payments ($1.7b out of a total of $14.2b). Employing firms in the March 2020 wave alone accounted for $9.68b in payments. Table 4 and Table 5 show that most recipient firms received wage subsidy support in the March 2020 wave, so there is considerable overlap in the populations of recipients across waves.

The analysis uses data from Statistics New Zealand's Integrated Data Infrastructure, combined with information from the Longitudinal Business Database, and Wage Subsidy information.[23] Table A2 summarises the source tables from which our analytical datasets are constructed for the analysis of monthly firm outcomes.

We distinguish 'employing' firms,[24] being firms that ever report employees in monthly EMS data or in COVID-19 Wage Subsidy applications, from 'sole trader' firms, which never report employees. We restrict attention to firms that are 'active', where we classify a firm as active if administrative data indicate non-zero GST sales, GST purchases, or employees. We also include in some of our analyses firms that are covered by wage subsidy payments, even when they do not appear as active in administrative data, so that all we know about the firm is that they received a subsidy payment. Table 6 summarises the sizes of the different groups of firms for each subsidy wave.

5.2 Background patterns of firm and employment change

In the months prior to the March 2020 lockdown, there were about 290,000 active sole trader firms, where activity is identified by having positive GST sales or purchases. There were around 275,000 active employing firms, with positive sales, purchases, or employees per month. Panel (a) of Figure 3 shows the growth in the number of employing firms since 2017, with regular seasonal variation that was interrupted in March 2020. In March 2020, there were also an additional 20,000 employing firms that received paid WSS employer applications for some form of COVID-19-related support (subsidy, leave payment, etc). In March 2020, there was also a

in the classification of employees between full-time v part-time, represent small adjustments to payment amounts, or result from a working proprietor being paid on a sole-trader application and their employees being paid on an employer application. In the first (March 2020) wave, there were also many multiple applications because firms were eligible to receive additional payments when an initial $150,000 per firm cap was removed on 23 March 2020.

[23] Data access was granted under Microdata Access Agreement MAA2018-97 between Statistics New Zealand and MBIE. Access to the data used in this study was provided by Stats NZ under conditions designed to give effect to the security and confidentiality provisions of the Statistics Act 1975 and the Data and Statistics Act 2022. The results presented in this study are the work of the authors, not Stats NZ or individual data suppliers.

[24] There is no unique business identifier that readily links data from these various sources. We define a firm based on two different views of a firm's identity – the first a confidentialised identifier based on a business tax number, and the second a confidentialised identifier of a business enterprise, as defined in Statistics New Zealand's Longitudinal Business Database (LBD). Data on Covid Wage subsidy applications are identified only by the former identifier, whereas other firm information such as industry is most appropriately captured as a characteristic of an enterprise. Because a tax number can be associated with more than one enterprise, and an enterprise can be associated with more than one tax number, we apply a grouping algorithm (Abowd, Creecy, and Kramarz 2002) to combine tax numbers and enterprises that overlap. We use the term 'firm' to refer to the combined groups.

sizeable increase in the number of sole traders captured in the administrative data, due mainly to the appearance of around 110,000 firms that do not appear to be active in administrative data except as recipients of COVID-19-related support. The change in the number of sole-trader firms is shown in panel (b) of Figure 3.

An obvious question is whether the WSS-only sole-trader firms are genuine firms that simply do not appear in the administrative data, or if they represent ineligible subsidy claimants. Figure 3(b) includes a line showing the number of zero-employee firms in February each year, from Statistics New Zealand's Business Demography data. In February 2020, there were 406,000 such firms, only slightly above the number of active or WSS sole-trader firms in our data.[25] In March-June 2020, the entry of WSS-only firms increased the number of sole-trader firms to around 400,000, roughly equal to the number of zero-employee firms on the Statistical Business Register (SBR). The SBR is, however, restricted to economically significant enterprises, so will exclude some genuine firms.[26] Table 7 reports which other data sources the inactive 'WSS-only' firms appear in, based on data from 2017 onwards. Over 96% of WSS-only sole trader firms and 72% of WSS-only employing firms appear as persons on the IDI spine, meaning that they appear in other person-identified data. The majority (64%) of the WSS-only sole trader firms appear in personal income tax (IR3) data, with self-employed earnings, suggesting that many of these firms could be sole traders with GST sales below the compulsory registration threshold. Within the constraints of using confidentialised administrative data, we cannot draw any more definitive conclusions about the nature of these firms.

Because the WSS-only firms are not observed in other administrative data sources, there is no reliable way to identify outcomes for them. We therefore exclude these firms from analysis when estimating the impacts of the WSS on firm survival or subsequent employment. Table 8 summarises the GST sales, employment, and subsidy payments for each of the groups identified in Table 6.

Panel (c) of Figure 3 shows variation in the level of employment and monthly GST sales since January 2017, totalled over all firms. For both employment and GST sales, there were pronounced declines between March and April 2020, when the first lockdown occurred. GST sales dropped from around $75bn per month to $55bn in April, before returning to pre-lockdown levels in May. In contrast, the decline in employment in April, from 2.52m in March to 2.42m in April, did not return to pre-lockdown levels until late in 2020. The changes can be more

[25] Some of this difference will be due to different firm definitions. We combine some firms, as outlined above.
[26] For zero-employee firms, the SBR economic significance threshold is $30,000 of GST sales, with only partial coverage of firms with sales below the threshold for compulsory GST registration ($60,000 from 1 April 2019), https://datainfoplus.stats.govt.nz/Item/nz.govt.stats/bdb02aa2-866e-418f-83e8-342234867a0f.

clearly seen in the panel (d) of Figure 3, which adjusts employment and sales levels for seasonality and trend growth.[27] Employment had declined by 100,000 (4%) by May 2020, and even by March 2021, was still well below the level that would have been expected if trend employment growth had continued. Monthly GST sales declined by $16bn (27%) between March and April 2020 (following a $6bn decline between February and March 2020), before returning to trend in the following month.

Figure 4 decomposes the changes in employment and sales into contributions from firm entry and exit, and from expansions and contractions among continuing firms (all series are adjusted for seasonal and trend variation). The dashed line in each figure shows an index of monthly employment or sales growth.[28] The first panel shows that the net employment growth of -3.4 was a result of lower job creation (contributing -0.8) and higher job destruction (contributing -2.4) within continuing firms. The lower left figure shows that the contributions were relatively small from lower firm entry (-0.1) and lower firm exit (0.1). The initial job creation and destruction changes were partially reversed in June 2020, suggesting that some job creation was delayed until after the lockdown, and that some contractions were only temporary.

The analogous figures for sales growth are shown in the right column of Figure 4. The initial contraction between March and April 2020 (-25.0) was almost entirely due to reductions within continuing firms (-24.5), with only a small contribution from a lower level of expansions in continuing firms (-0.6). Symmetrically, the reversal in May and June 2020 was predominantly due to increased expansions within continuing firms. The contribution ns of higher firm entry (0.5) and higher firm exit (-0.3) to sales growth are relatively small.

In Figure 5 we document the monthly job exit (in panel (a)) and entry (panel (b)) patterns over the period January 2019 until April 2022. For this we stratify jobs according to whether they were in firms that received no WSS-payments ("Non-WSS"), firms that received only WSS-payments during the original ("March 2020") wave, or firms that received WSS-payments in other waves ("Misc-WSS").[29] The figures show quite strong seasonal patterns, with high exit and entry rates in December for each series, and also relatively high rates around March (particularly the Misc-WSS sample). The exit and entry rates are generally similar for the March 2020 and

[27] The adjusted variable is the residual from a regression of the raw variable on calendar month dummies and a linear time trend, estimated over June 2017 to February 2020.

[28] The net growth index is calculated as $(x_t - x_{t-1})/\bar{x}$, where $\bar{x} = (x_t + x_{t-1})/2$. This converts growth rates into an index between -2 and 2, and facilitates decomposition into creation (expansions within continuing firms), destruction (contractions within continuing firms), firm entry, and firm exit (Davis and Haltiwanger 1992).

[29] The miscellaneous sub-sample generally includes jobs in firms that received WSS-support during March 2020 as well as at least one other WSS-wave. The underlying samples of firms change over time as firms enter and exit; however the job-exit and entry patterns are similar if we restrict the analysis to firms that exist over the two year period from April 2019 to March 2021. We calculate the exit and entry rates relative to the number of jobs in the reference month in order to use a consistent base – e.g. in March 2020 we calculate the exit rate as the number of March jobs that don't appear in April, and the entry rate as the number of March jobs that didn't appear in February, relative to the number of March jobs.

Non-WSS firm samples, and usually about 3 percentage points lower than for the Misc-WSS sample. The job-exit rate for each group was relatively high in March 2020 (particularly for jobs in Misc-WSS firms), perhaps reflecting some job-shedding associated with the pandemic and initial lockdown; and both the exit and entry rates were noticeably lower in the following two months. In addition, there was relatively stronger job growth in non-subsidised firms than subsidised firms over the period, from both a relative drop in job-exit and an increase in job-entry rates compared to WSS-firms. For example, the average net monthly job growth (the entry-exit rate) in Non-WSS firms was 0.9% higher over the year to February 2022 compared to the year to February 2020, compared to net job loss of 0.5% and 0.9% for the March 2020-WSS and Misc-WSS firms respectively.[30]

5.3 Selectivity – which firms were paid the subsidy?

A direct comparison of the outcomes of subsidised and unsubsidised firms is unlikely to provide a reliable measure of the effects of the subsidy. Subsidised firms are not simply a random sample of firms, so it is not valid to assume that their outcomes in the absence of the subsidy would have been the same as the observed outcomes of unsubsidised firms. The eligibility criteria for the first (March 2020) wave of the COVID-19 Wage Subsidy included the requirement that applicants were anticipating a 30% revenue decline attributable to COVID-19. This requirement alone suggests that the prospective outcomes for subsidised firms were less positive than those of firms with expected revenue increases or smaller expected revenue declines.

Figure 6 confirms that revenue (as captured by GST sales) did generally decline by more for subsidised firms than for unsubsidised. The figure shows the growth in revenue in the two months of April and May, relative to revenue for February and March.[31] For subsidised firms, there is a modal reduction of around 50%. For unsubsidised firms, the distribution is bimodal, with peaks at 0% (unchanged sales revenue) and for reductions of between 20% and 25% - smaller than for the subsidised firms. For both groups, there is however considerable variation in sales growth. Around 64% of subsidised firms had revenue declines greater than 30%, whereas this proportion was only 41% for unsubsidised firms. Similarly, only 15% of subsidised firms experienced an increase in revenue for April and May relative to February and March. Almost 27% of unsubsidised firms experienced a revenue increase.

[30] For Non-WSS firms the average job-exit (entry) rate was 6.6% (7.3%) over the 12 months to February 2022, compared with 7.1% (6.9%) over the 12 months to February 2020; while for March 2020-WSS firms, the average rates were 6.4% (6.4%) compared to 6.5% (7.0%); and for Misc-WSS firms 9.0% (8.6%) compared with 9.3% (9.8%)

[31] A 2-monthly window was chosen for this illustration because many firms report GST sales on a 2-monthly basis. We transform GST returns that cover multiple (2 or 6) month periods into monthly sales by spreading them in proportion to aggregate monthly sales for monthly returns over the same period, for firms in the same 2-digit ANZSIC industry group. Firms with a more than doubling of sales have their growth top-coded at 100%.

Table 9 summarises the revenue changes for the March 2020 and other waves, separately for subsidised and unsubsidised firms. The upper panel shows short term revenue change, being the change in revenue around the reference month. The lower panel shows change in monthly revenue compared to 12 months earlier, evaluated in the month after the reference month. Consistent with the patterns in Figure 6, both the short-term and 12-month changes for the March 2020 wave were more strongly negative for subsidised than non-subsidised firms. The patterns for the other waves are more complicated because the revenue losses around the March 2020 wave act to confound the patterns for the subsequent waves: this is particularly the case for the short-term changes for the Extension and Resurgence waves, and the 12-month changes for the March 2021 wave.[32] However, there were fairly strong 12-month revenue declines for the WSS-receiving firms in the Extension and Resurgence waves, compared to small positive increases for non-WSS firms. For example, subsidy receiving firms experienced 15-20% mean (21-26% median) revenue losses relative to the previous year, compared to 0-5% mean and median revenue gains for non-subsidised firms over these waves. Although it is difficult to conclude much for the March 2021 wave because of possible confounding effects on both the short-term and 12-month changes, we estimate that a quarter of subsidised firms had short-term revenue declines of at least 18%.

In our comparison of outcomes for subsidised and unsubsidised firms, we aim to put more weight on unsubsidised firms that more closely resemble subsidised firms. Our approach is to predict which firms received subsidy support in each wave, based on information about them at the start of the subsidy wave and from the preceding 14 months (including prior revenue change), and then statistically reweight unsubsidised firms so that they are more similar to subsidised firms. Our method is outlined in more detail in section 4.3.

5.3.1 Patterns of selectivity

Table 10 and Table 11 summarise some of the WSS take-up patterns for employing firms and sole traders respectively. In these tables, and throughout this study, take-up rates are measured as the number subsidised out of the full population. While it is common to measure take-up as a proportion of eligible firms or workers, we cannot reliably determine eligibility for non-recipients, so we rely on this population-linked measure. Each table documents both the 'raw' and 'adjusted' take-up rates. The adjusted take-up rates are derived from a linear probability regression that simultaneously controls for all the characteristics shown in the table. Within each

[32] That is, the short-term revenue changes for the Extension and Resurgence waves each have base periods following April 2020; and for the March 2021 wave, the 12-month change compares sales in April 2021 with the low level of sales in the first month of the first lockdown in April 2020.

block, rows are sorted (approximately) from lowest to highest take-up rates and are in the same order in both tables.[33] The first row of each table shows the overall take-up rates for each wave.[34] All other table entries are expressed as deviations from the average rate.[35]

For employing firms (Table 10) in the March 2020 wave, the raw and adjusted variation by industry is very similar. Take-up rates are low for primary industries, finance and insurance, and education. High take-up is evident for construction, accommodation and food, and manufacturing. The fact that the raw and adjusted statistics are similar indicates that industry variation is not merely reflecting other factors that vary across industry. In contrast, adjusted take-up rates by region vary less than raw take-up rates, indicating that regional variation in take-up is to a large extent reflecting variation in industry mix and other characteristics. Southland, for instance, has raw take-up rates that are 18 percentage points (ppt) below average, but an adjusted rate that is only 0.5 ppt below average. The ranking across regions is, however, similar whether raw or adjusted measures are used. Take-up is low in Gisborne, Taranaki, and West Coast, and consistently high in Auckland. The low raw take-up rates for Māori firms (-3.8 ppt) and European/ other firms (-1.4 ppt) are in part due to differences in industry, region, firm size and age. Adjusted rates are -1.4 ppt and -0.1 ppt below average, respectively. By firm size, the high take-up rate of 5.5 ppt above average for firms with 6-10 employees is more than accounted for by other firm characteristics (adjusted rate of 0.7 ppt below average).

In later waves, the ranking of take-up rates across groups is similar to the ranking in March 2020. The high relative take-up in Auckland is more pronounced, with even adjusted rates at least 7 ppt above average. Industry variation is less pronounced in the later waves, as is variation by firm size. Asian firms and MELAA firms have consistently high relative take-up rates, and even adjusted take-up for Māori firms is low in the Resurgence (-1.0 ppt) and March 2021 (-1.7 ppt) waves.

The patterns of take-up for sole trader firms (Table 11) are similar to the patterns for employing firms. Raw and adjusted industry patterns for sole traders are similar in the March 2020 wave, although the pattern of which industries have high and low take-up rates is not the same as for employing firms. For instance, sole traders in the accommodation and food industry had relatively low take-up rates. Sole trader take-up is relatively high Auckland for all waves,

[33] Table A3and Table A4 summarise raw take-up rates, and the share of firms accounted for by each group.
[34] Although each wave is summarised separately, there is considerable overlap due to the fact that most firms that received support after the March 2020 wave had also received support in March 2020. See Table 4 and Table 5.
[35] Table entries are relative to the weighted mean rate within each block. Thus, for regional variation, the entries show deviations from the weighted mean take-up rate excluding firms for which regional information is unavailable. For firm ethnicity, the weighted mean is based on the weighted sum, which double counts multiple-ethnicity firms. The impact of double counting on the calculation of the mean is small.

whether measured as raw or adjusted. By firm ethnicity, take-up among sole traders was high for Asian and MELAA firms, and low for European/ Other firms. Māori sole traders had high relative take-up in the March 2020 wave, and low relative take-up in the Resurgence and March 2021 waves.

5.4 Firm survival

In this section, we estimate the effect of subsidy support on firm survival. We first document patterns of firm births and deaths before and after the first COVID-19 lockdown, before focusing on the probability of survival among cohorts of firms that are active at the beginning of each wave. We estimate the impact of wage subsidy support using the reweighting approach described in section 4.3, and using both empirical (Kaplan-Meier) survival patterns, and parametric (proportional hazards) regression estimates. We use the parameter estimates to derive estimates of the number of firm deaths that were prevented by the wage subsidy support.

5.4.1 Firm births and deaths

Figure 7 shows the pattern of firm births and deaths by month, as captured by monthly administrative data. A firm is classified as active if it has non-zero GST sales or purchases, or employees in a month. A birth is identified as occurring in the first month in which a firm is observed as active, and a death is identified as the final month in which a firm is observed as active. Monthly GST data are incomplete from August 2021, and 'pent'-based measures of employment are reliably recorded only until June 2021.[36] We therefore do not treat final months as deaths for these specific cases. Survival analysis consequently excludes months after June 2021.

As can be seen in the upper panels of Figure 7, there is a strong seasonal pattern in monthly births and deaths, dominated by the prevalence of firms with two-monthly GST returns. The proportion of firms dying (becoming permanently inactive) is highest in March each year, with an associated peak in firm births in April each year. Some of this reflects administrative factors such as the alignment of business transitions to March tax years. The changes that occurred around the time of the first COVID-19 lockdown can be more clearly seen in the middle panels of Figure 7, which adjust the series for seasonal (calendar month) effects and a time trend. In April 2020, there was a pronounced decline in firm births for both employing and sole

[36] In the labour tables that we use (Fabling and Maré 2015), firms are identified by a 'permanent enterprise number' or 'pent' (Fabling 2011). At the time that our analysis was done, the labour tables did not extend beyond June 2021.

trader firms. In contrast, firm death rates were particularly high only a year later - in March 2021.

The bottom panels of Figure 7 show the number of employing and sole trader firms active in each month. The decline in the number of active firms in April 2020 is more pronounced than the rise in firm deaths, especially for sole trader firms. This reflects the fact that many of the firms that were inactive around April 2020 were observed again with employees or non-zero GST sales or purchases in later months.

5.4.2 Following cohorts of firms

For each wave, we analyse firm survival for cohorts of firms that were active (non-zero GST sales or purchases, employees, or WSS subsidy coverage) during the first month that the subsidy was paid (See Table 6). The survival rates for the March 2020 cohorts are shown in the top rows of graphs in Figure 8.[37] The heavy line is the survival curve for subsidised employing firms. By March 2021, 94.6% of the cohort were still active. This compares with 90.9% of the unsubsidised firms in the cohort (light dashed line). The lighter solid line shows the survival curve for the reweighted unsubsidised firms. Because subsidy assistance was targeted to firms that were expecting poorer outcomes (including greater expected revenue declines), reweighting the unsubsidised firms to more closely resemble subsidised firms has the effect of putting more weight on unsubsidised firms with poorer expected outcomes. The weighted survival curve for the unsubsidised firms therefore lies below the unweighted curve, implying that only 89.9% of firms that are comparable to subsidised firms would have remained active in the absence of the subsidy.

For sole trader firms, the proportion of firms surviving until March 2021 was the same for subsidised and unsubsidised firms (88.1%). However, unsubsidised firms had more favourable survival prospects prior in March 2020, so when they are reweighted to more closely resemble the sort of firms that were subsidised, their weighted survival rate is only 84.6%.

Although the reweighting of unsubsidised firms provides a plausible counterfactual against which to compare the survival rates of subsidised firms, a more robust estimate can be obtained by regression adjusting the estimates to take account of any remaining differences in observable characteristics that may be related survival. For analysing survival, we estimate a weighted proportional hazard (PH) regression model, as summarised in equation (5):

$$P(final\ active\ month_i) = h_0(t) f(X_i; \gamma). \quad (5)$$

[37] Survival rates are calculated using a Kaplan-Meier estimator.

The 'hazard' is the likelihood that a month is the firm's final month and the firm's likelihood of survival can be calculated from the sequence of monthly hazard rates. The specification in (5) allows for a common baseline hazard $h_0(t)$ (and therefore a common baseline survival curve), which can be scaled proportionally for firms with different observable characteristics (*X*). Our primary interest is in the scaling coefficient on subsidy receipt, which shows the hazard rate for subsidised firms as a proportion of the hazard for unsubsidised firms. The included covariates (X_i) include all of the covariates that are included in equation (3), plus time-varying covariates to control for receipt of subsidy assistance in other waves,[38] receipt of small business cashflow loans or resurgence support, or repayment of subsidy assistance. For waves other than the March 2020 wave, indicators are also including to control for whether the firm had received subsidy assistance in each of the prior waves.

Table 12 summarises the estimated subsidy coefficients from PH regressions for each subsidy wave, and separately for employing and sole-trader firms. The table reports 'raw' estimates, from unweighted regressions with no covariates other than a subsidy-receipt indicator, and a preferred 'full' specification that includes all covariates and is inverse-probability-weighted by w_i (derived from equation (3)).

The March 2020 raw estimates reflect the patterns shown in Figure 8. The 'raw' coefficient for employing firms in the March 2020 wave is 0.56, implying that, in each month, subsidised firms have a little over half the probability of dying compared with unsubsidised firms. The estimate from the full specification is a relative hazard of 0.60. The cumulative effect is that the proportion of firms surviving is consistently higher for subsidised firms. Sole trader firms that received subsidy support in March 2020 also experienced higher survival rates than otherwise similar unsubsidised sole trader firms. The raw relative hazard coefficient is 0.86, and the full-specification estimate is 0.80, implying a 20 percent lower death rate. The March 2021 wave of support had a similar positive impact on firm survival, with subsidised firms having death rates relative to comparable unsubsidised firms that were only 67% (for employing firms) or 81% (for sole trader firms) as high. The impacts on firm survival for the Extension and Resurgence waves were more muted. For employing firms, the relative hazard rate was barely significantly different from 1, meaning that subsidised and unsubsidised firms had similar death rates. For the Extension wave, death rates for subsidised firms were estimated to be 4.9% higher than for subsidised firms and for the Resurgence wave, only 5.3% lower. For sole trader firms, only the

[38] Time varying covariates are included by adding terms $\left[\gamma^j\left(\ln(t) * x_t^j\right)\right]$, with associated parameters γ^j to the function $f(X;\beta)$.

resurgence wave had a statistically significant positive effect – lowering the death rate to 88.3% of the rate for comparable unsubsidised firms.

The estimates in Table 12 can be used to calculate the change in the number of firm deaths attributable to subsidy support. The actual survival rate for subsidised firms $S(t) = \prod_{r=1}^{t}(1 - h_r)$ where h_t is the observed hazard rate, as shown in Figure 8 for the March 2020 wave. The PH regression estimates imply that $h_t = \gamma^s h_t^*$, where γ^s is the coefficient reported in Table 12, and h_t^* is the counterfactual hazard rate that the firm would have experienced in the absence of subsidy support. The counterfactual survival rate can be calculated as $S^*(t) = \prod_{r=1}^{t}\left(1 - \frac{h_r}{\gamma^s}\right)$, with confidence intervals based on the confidence interval estimates for $\widehat{\gamma^s}$. The actual survival rate $\big(S(t)\big)$, and the counterfactual survival rate (with confidence intervals) are shown in Figure 9, for each wave, and separately for employing and sole trader firms. The gap between these curves shows the effect of subsidy assistance on survival rates.

The implied effect of the subsidy on the number of surviving firms can be calculated as $\Delta deaths = N_0\big(S(t) - S^*(t)\big)$. Table 13 summarises the implied impact on the number of firms surviving to 3, 6, 9 and 12 months after the start of each wave. The starting number of firms (N_0) in the calculation is the actual number of subsidised firms – not just the restricted sample used for estimation. It thus includes WSS-only firms, on the assumption that their behaviour and survival follow the same pattern as those of firms in the estimation sample but that they are not fully observed in the administrative data.

The largest impact was on the number of employing firms as a result of the March 2020 wave of subsidy support. The estimates imply that after 3 months, there were 674 more subsidised firms surviving that would not have survived without subsidy support. After 12 months, the estimated cumulative impact is that there were 7664 more surviving firms than there would have been in the absence of subsidy support. For sole trader firms, the estimated impact on survival is of similar magnitude, with 393 additional surviving firms after 3 months, and 5326 more after 12 months. These figures include the estimated impact on WSS-only firms, whose survival is not reliably observed. Without these firms, the estimated impact for sole traders would be roughly halved.

For the Extension wave, the estimated changes to firm survival are small, and none of the estimated impacts is statistically significantly different from zero. The survival effects for the Resurgence wave are statistically different from zero, but are small, amounting after 12 months to only 221 employing firms and 740 sole trader firms. The March 2021 impacts are estimated for only 6 months after the reference month, and are small relative to the March 2020 impacts.

5.4.3 Heterogeneous impacts

The estimated impacts of COVID-19 Wage Subsidy support on firm survival (section 5.4) summarise the overall patterns but do not provide evidence on whether the support had a greater impact for some types of firms. In this section, we estimate impacts separately for 29 different groupings of firms, to detect heterogeneous impacts by industry, region, firm ethnicity, firm age, and firm size. The groups are summarised in the separate box in this section. The groupings are aggregations of standard classifications, where the aggregations have been chosen based on similarity of take-up rates and to ensure provide a parsimonious summary of heterogeneous effects. The approach to defining and identifying firm ethnicity is summarised in Appendix 1.

Separate hazard regressions are estimated for each subgroup, so the estimates that are summarised in Table 14 (for employing firms) and Table 15 (for sole trader firms) capture the differences in firm survival between subsidised and unsubsidised firms within each subgroup. The first rows of Table 14 and Table 15 repeat the overall estimates from Table 12. For many subgroups, the impact of subsidy support on survival was similar to the overall impact.

Among employing firms, subsidy support in the March 2020 wave had favourable and significant effects on firm survival for almost all groups, as is evident in the fact that the relative hazard is smaller than 1. There were relatively strong favourable effects on firm survival (a low relative hazard) for medium sized firms (6-50 employees) (exit probabilities that are 20-25% of the probability for comparable unsubsidised firms), construction industry firms (41%), retail/ accommodation/ food firms (44%). Even groups that benefited less from subsidy support had survival rates that were significantly higher than comparable unsubsidised firms, with relative hazards below 100% (all below 82%). The only exception was very young firms (less than 12 months), with survival rates almost identical to that of unsubsidised young firms. In contrast, subsidised employing firms were estimated to have worse survival outcomes from the extension wave (controlling for the positive impact of the March 2020 support, which the majority of Extension recipients also received). The only groups to have significantly lower death rates from subsidy support were construction firms (84%) and firms with 11-50 employees (76%). Survival impacts from the Resurgence wave of support were small and barely significant. In the March 2021 wave, there were significant favourable effects on survival overall, and also for small firms, older firms, Asian firms (59%), construction firms (60%), and firms in Auckland. In Wellington,

Subgroup definitions

Industry groupings

- Primary and Mining (ANZSIC06 A &B)
- Manufacturing (ANZSIC06 C)
- Construction (ANZSIC06 E)
- Retail/ Accommodation and Food Services (ANZSIC06 G & H)
- Network Industries: Electricity Gas and Water/ Wholesale/ Transport Postal and Warehousing/ Information Median and Telecommunications (ANZSIC06 D,F,I,J)
- FIRE: Financial and Insurance Services/ Rental hiring and real estate services (ANZSIC06 K,L)
- Public and Social: Public administration and safety/ Education and Training/ Health Care and social assistance (ANZSIC06 O,P,Q)
- Professional Services (ANZSIC06 M)
- Other Services: Admin and support/ Arts and Recreation/ other (ANZSIC06 N,R,S,T)

Regional groupings

- Auckland Region
- Wellington Region
- Canterbury Region
- Waikato Region
- Upper North Island: Northland, Bay of Plenty, Gisborne, Hawke's Bay
- Lower North Island: Taranaki, Manawatu-Wanganui
- Lower South Island: Otago, Southland
- Other South Island: Nelson, Tasman, Marlborough, West Coast

Firm ethnicity groupings

- European/ Other Firm
- Māori Firm
- Pacific Firm
- Asian Firm
- MELAA Firm

Firm Size groupings

- (0,5]: 1-5 employees
- (5,10]: 6-10 employees
- (10,50]: 11-50 employees
- More than 50 employees

Firm Age groupings

- New: Fewer than 12 months
- Young: 12-35 months
- Older: 36 months and over

there is a very large adverse impact on survival, reflecting the high survival rate among unsubsidised firms, albeit over the relatively short observed period following support payments.

Among sole trader firms (Table 15) the March 2020 wave again stands out, with favourable survival effects for most groups. In the Extension wave, the overall impact was insignificantly different from one but there were significant adverse effects for retail/ accommodation/ food firms, albeit controlling for a strong favourable effect of the March 2020 support. The strongest favourable effect of Resurgence support on survival was for Asian firms, and for none of the groups was there a significant effect on survival from the March 2021 wave of support.

5.5 Firm employment growth

Firm survival is one of the two key firm-level outcomes that we examine. The other is employment – estimating whether employment in subsidised firms was higher than it would otherwise have been as a result of subsidy support. We estimate the employment impact of COVID-19 wage subsidy support based on a log-linear regression model of employment levels. By definition, sole trader firms have no employees, so the employment impacts are equivalent to the firm survival impacts described in the previous section. The estimates summarised in the current section are thus restricted to employing firms.

For each wave, we select firms that were active with employees in the first month of the wave, and track how many employees that cohort collectively employed in each month from January 2017 through until June 2021. To increase comparability of the subsidised and unsubsidised firms, unsubsidised firms are reweighted using a weight w_i derived from propensity scores estimated from equation (3). A consequence of this selection is that the number of firms is highest in the reference month. The total employment levels over time in these cohorts of firms is affected by the selection, contributing an increase in employment leading up to the reference month and a decline afterwards. However, the effect of selection is expected to be similar for subsidised and (reweighted) unsubsidised firms.

The employment levels for the subsidised and (reweighted) unsubsidised firms are shown in Figure 10. Both series are indexed relative to the value in the month immediately prior to the reference month. Despite the reweighting, the patterns of growth differ across the subsidised and unsubsidised firms. Firms that received subsidy support in the March 2020 wave and were active in the reference month had stronger employment growth leading up to the reference month than comparable unsubsidised firms. The decline in employment between March 2020 and April 2020 of was more pronounced for the subsidised firms (6%) than for the unsubsidised (3%), and employment remained at around the April 2020 level through until May 2021. In contrast, the cohort of unsubsidised firms showed growth in employment from mid-2020.

Following subsidy support in the Extension and Resurgence waves, employment growth in the cohort of subsidised firms continued to decline, whereas employment for unsubsidised firms stabilised or grew. There is no distinguishable difference in employment growth following the March 2021 wave of support, although the follow-up period is very short.

Employment impacts are estimated using a weighted regression as shown in equation (6):

$$\ln Emp_{it} = [\,\alpha_t + (\beta_S + \beta_{St}) * S_i] + X_{it}\beta + \beta_{lagE} \ln Emp_{i,t-12} + \varepsilon_{it}. \qquad (6)$$

The inclusion of lagged employment and covariates in addition to the reweighting is intended to control for remaining differences between subsidised and unsubsidised firms. The vector X_{it}

contains additional covariates: industry intercepts, employment shares by regional council, a quadratic in firm age (top coded at 36 months), and indicators for whether the firm is European/other, Māori, Pacific, Asian, or MELAA. For each wave, fixed effects are included to capture whether a firm had received subsidy support in a previous wave, and the terms in square brackets in equation (6) are a full set of month dummies to absorb period effects that are assumed to be common across subsidised and unsubsidised firms, and, for subsidised firms ($S_i = 1$), an overall mean level difference (β_S), and month-specific coefficients (β_{St}). One of the month-specific coefficients – for the month immediately prior to the start of the subsidy wave - is omitted, so that all other subsidy effects are relative to that month. This normalisation is arbitrary. Ideally, the month-specific coefficients would be insignificantly different from zero prior to the reference month. Table 16 summarises the pattern of (β_{St}) coefficients for 3, 6, 9, and 12 months after the reference month, which are uniformly negative for all waves. Coefficients are also reported for the reference month itself, which show the difference from the prior month, and indicate any pre-support growth or decline.

The month-specific coefficients (β_{St}) can be used to get estimates of the month-by-month effect of receiving subsidy support on employment levels. Observed employment in subsidised firms in a month is $E_t = E_t^*(1 + \widehat{\beta_{St}})$, where E_t^*.is the counterfactual employment level – the level of employment that would have been experienced in the absence of subsidy support. Figure 11 plots actual employment (E_t) by month, together with the implied counterfactual employment $\left(E_t^* = E_t/(1 + \widehat{\beta_{St}})\right)$ with 95 percent confidence intervals. The effect of subsidy receipt is the vertical distance between these two lines $\left(\Delta Emp_t = \left(E_t - \widehat{E_t^*}\right)\right)$, which is plotted for each wave in Figure 12. The estimated employment impacts of subsidy receipt for the March 2020 wave, shown in the first panel of Figure 12, are significantly positive prior to February 2020, indicating that the cohort of subsidised firms had a relative decline in employment prior to the subsidy support starting. This relative decline continued after March 2020, reflecting the pattern seen in Figure 10 in which employment in unsubsidised firms grew while employment in subsidised firms was stable. The patterns of relative post-support declines for subsidised firms is also evident for the Extension and Resurgence waves of subsidy support. Estimates for the impact of the March 2021 wave imply a decline in the reference month, which is reversed in the following month. Due to the relatively small number of firms and short follow-up period, these estimates may be somewhat unreliable.

The relative declines in employment can be aggregated across months to calculate an aggregate of employment-months gained or lost in subsidised firms as a result of the subsidy. The results of this calculation are presented in Table 17, expressed in terms of full-year

employment. In the 12 months following March 2020, subsidised firms are estimated to have had 28,400 person-years less employment than otherwise comparable unsubsidised firms.

The effects of the Extension and Resurgence waves were about half as large, but also negative – implying around 8,000 fewer FTE years of employment after 9 months and, for the Extension wave, 12,500 fewer after 12 months.

5.5.1 Heterogeneous impacts

As was done for the estimation of firm survival impacts, separate employment regressions were estimated for each of 29 subgroups of firms – as described in section 5.4.3. Regression coefficients for relative employment 9 months after the reference month of each wave for each group are shown in Table 18 (estimates for the March 2021 wave are shown for 3 months after the reference month because of the short follow-up period). The first row of the table reproduces the all-firms estimates from the final row of Table 16. The estimated adverse effect of subsidy support on subsequent employment is evident for almost all groups. Taken together with the generally favourable survival effects, these estimates imply that even though subsidy support may have improved firms' chances of survival, subsidised firms still struggled to grow in the months following support – at least relative to comparable unsubsidised firms.

6 WSS Impacts on worker outcomes

In this section we focus on the effects of the WSS on various worker-level outcomes. This involves three distinct sets of analyses. First, in subsection 6.1, we document and analyse the subsidy receipt patterns across waves and by worker characteristics. Second, in subsection 6.2, in order to address the question of whether subsidy-receiving firms complied with their obligations to, at least, maintain the wages of their workers at the subsidy-rate received and to endeavour to pay at least 80% of their normal earnings during subsidy periods, we analyse patterns of weekly earnings over period during and following receipt of subsidy payments. Third, in subsection 6.3, we focus on our main analysis of the impact on the scheme on worker-level outcomes.

Each of these analyses is based on distinct, although related, data extracts. The first analysis in section 6.1 uses data on workers, collated from IR's IDI main release Employer Monthly Schedule (EMS) tables, MSD's Ad-hoc WSS (COVID-19 Wage Subsidy) tables, and LBD tables on working proprietors. As the obligations of subsidy-receiving firms was in terms of workers' weekly earnings rates, for the second analysis of earnings compliance in section 6.2, we use earnings data from IR's payday-filing (Employment Information – Employee, or EIE) tables,

that are available since April 2019. This data enables us to construct a 'weekly earnings' rate that can be used to assess firms' compliance with their obligations. Finally, for our main analyses of the impacts of the WSS on worker outcomes in subsection 6.3, we use data from the EMS tables, over the period from January 2019 to April 2022. This analysis is restricted to workers with PAYE tax-withheld wage and salary employment.[39]

6.1 Subsidy receipt across workers

We begin by describing the relative wage subsidy take-up (i.e. receipt or listed-on successful subsidy application) rates of workers across different dimensions. To identify the population at-risk of receiving wage subsidy payments, we collate individuals observed over the period from January 2019 to March 2022, from several sources: first, employees who receive tax-withheld payments in the EMS tables or are listed in the COVID-19 Wage Subsidy (CWS) tables; and second, employers in either the EMS or CWS, sole traders in the CWS, or working proprietors in the LBD tables, who appear as people in the IDI personal details table. We use the same population construct to analyse take-up patterns across all waves.

We summarise the WSS take-up rates across all waves in Table 19. The overall take-up rate across all waves is 47.4%.[40] The first column documents the raw take-up rates across each of the characteristics dimensions: separately by sex, ethnicity, age, prior earnings, and geographic location. This shows, for example, that 53% of males received WSS subsidy in at least one wave, compared to 42% of female; receipt rates among European (49%) and Asian (54%) workers was higher than among Māori (38%) and Pacific peoples (42%); similarly, prime aged workers had higher take-up rates than young workers (e.g. 53% of 40-54 year olds, compared to 37% of under-25s); take-up was highest in the Construction industry (83%), and also high in Manufacturing, Wholesale, Accommodation and Food, Rental and Real Estate, Arts and Recreation, and Other Services industries;[41] and take-up among Auckland workers (53%) was higher than for workers in other regions: Canterbury was next highest at 50%, while Gisborne was lowest at 38%.

In column 2, we present coefficients from a multivariate regression for wage subsidy receipt, including covariate controls for each of the characteristic dimensions. The estimated coefficients provide conditional effects relative to the omitted base-group for each

[39] Most workers are employees, but this may also include self-employed workers if they are paid PAYE-withheld wage and salary earnings.
[40] All take-up rates are calculated as proportions of all 3.4m individuals observed from January 2019 to March 2022. Total employment (HLFS) averaged about 2.7m during the period of the wage subsidies, so the take-up rate as a proportion of filled jobs was 59% (=0.47*3.4m/2.7m).
[41] Because workers can work in several industries, these estimates are based on the fraction of the period a worker works in each industry.

characteristic. For example, conditional on other characteristics, female workers had 3.0 percentage points (ppt) lower WSS take-up rates than males, compared to the raw difference of 11.5 ppt in the first column.

In the final two columns we present analogous regression estimates for workers stratified by whether or not they were employees.[42] Although the overall relative take-up rates are dominated by employees, these columns show some marked group differences for non-employees. In particular, among non-employees, non-European workers had 1-7 ppt higher wage subsidy receipt rates than Europeans, whereas among employees, non-European workers had take-up rates that were 1-9 ppt lower.

In Table 20 we present regression adjusted estimates for any wave (repeated from Table 19), and separately for each of the four subsidy waves in the outcome evaluation. In the first row, we list the overall take-up rates for each wave, which declined steadily from about 45% for the original (March 2020) wave down to less than 5% for the March 2021 wave. These rates imply that the vast majority (96%) of workers who received any subsidy support received support during the March 2020 wave: in contrast, only about a third received support during the Extension, one-sixth during the Resurgence, and 10% during the March 2021 waves.

Largely mirroring this decline in take-up rates across waves, the estimated percentage point differences in subgroup take-up also generally decline.[43] In contrast, the subgroup relative take-up rates, calculated as the subgroup take-up relative to the overall take-up for a wave, vary less systematically. For example, while female subsidy receipt of the March 2020 wave was 10.5 ppt lower than for males, and this difference declined across the waves to 5 ppt (Extension), 1.2 ppt (Resurgence) and 0.6 ppt (March 2021), the relative take-up rates for females fluctuated between 93% and 97% across the waves. While the March 2020 wave take-up for Māori (only) workers was 9.2 ppt lower than for European workers, and this difference declined to 3.6pp, 2.1pp, and 1.2 ppt across the subsequent waves, the Māori relative take-up rates declined steadily from 88% to 73% across the waves. Similarly, the relative take-up rates for Pacific peoples fell from 85% to 44% across the four WSS-waves; and the relative take-up rates for European workers also declined from 103% to 88%. These declining ethnic relative take-up rates are associated with a strong increase in the relative subsidy take-up by Asian workers, from 100% for the March 2020 wave to 178% for the March 2021 wave.

[42] We define a worker as an employee if they (ever) appear as a worker in the EMS data or listed as an employee in the CWS data, and as a non-employee otherwise.

[43] Regional location was the one noticeable dimension where this wasn't the case: not surprisingly, as the Resurgence and March 2021 (as well as the later, out of scope, August 2021 subsidy wave) concentrated support on the predominantly Auckland lockdowns.

In contrast to the apparently worsening subsidy receipt patterns across these demographic dimensions of equity, the patterns across workers stratified by their earnings are quite different. In particular, the relative take-up rates increased steadily across waves for workers in the two lower quartile: from 106% and 111% for those in the bottom and second quartile of earnings for the March 2020 wave, to 139% and 140% for the same quartiles for the March 2021 wave; while the relative rates for workers in the third and top quartiles fell from 102% and 80% for the March 2020 wave, to 86% and 35% for the March 2021 wave respectively.

6.1.1 WSS take-up and precarious jobs

In order to shed some light on the extent to which the WSS reached precarious jobs and workers, we examine the characteristics of the population of EMS wage and salary jobs in March 2020, stratified by March 2020 wave WSS-receipt status. Note that this sample differs from that used for the take-up analysis in Table 19 and Table 20: in particular, it focuses on workers at a point-in-time rather than over a period, on wage and salary employment only, and on jobs rather than workers; however, the patterns are broadly consistent across the samples.

First, in Table 21 we describe the characteristics of all jobs in column (1), jobs in non-WSS firms in column (2) (these are necessarily non-subsidised jobs), all jobs in WSS-receiving firms in column (3), and non-subsidised and subsidised jobs in WSS-firms in columns (4) and (5) respectively. Comparing columns (2) and (3) shows some differences between jobs in subsidised and non-subsidised firms. These include that workers in subsidised firms are younger and less likely to be female; in addition, the tenure and earnings (both in March, averaged over the previous year, and worker totals) of jobs in subsidised firms are noticeably lower than those in unsubsidised firms.

Perhaps of more interest is the comparison between unsubsidised and subsidised jobs in subsidised firms in columns (4) and (5). This shows workers in unsubsidised jobs are younger, more likely to be female, and of Māori or Pacific peoples ethnicity. In addition, the characteristics of the unsubsidised jobs are consistent with common dimension of job-precarity, such as lower job tenure, workers having higher numbers of jobs, lower employment propensity and more likely to receive welfare benefits, and have lower earnings.

Second, in Table 22 we focus on the samples in columns (2) (all jobs in non-WSS firms), (4) (non-WSS jobs in WSS-firms) and (5) (WSS-jobs in WSS-firms), and stratify each sample by whether the job ends in March or continues to April (at least). Across each sample, jobs that end in March tend to have shorter tenures and lower earnings; and be associated with younger workers, and those who have higher job turnover, and benefit receipt. These differences are particularly stark for unsubsidised jobs in subsidised firms. In addition, the March-end rate of

these unsubsidised jobs is substantially higher (34%) than for the other groups (4.2% for jobs in unsubsidised firm, and about 0.5% for subsidised jobs in subsidised firms). Given the relative precarity of these jobs, there are two possible interpretations: first, that the firms tended to 'let go' workers in such jobs rather than apply for wage subsidy support for them; second, many of these jobs may have either ended or were scheduled to end before the subsidy period which caused the firm to not apply for subsidy support for them. Although it is not possible to disentangle these interpretations given the available data, we suspect both interpretations are at play. However, this issue does highlight the potential weakness and difficulty of using a wage support scheme delivered through firms to reach workers employed in casual or precarious jobs. In addition, the relatively high March-ending associated with non-WSS jobs in WSS-firms will contribute to the lower employment growth in subsidised firms discussed in subsection 5.5 above.

6.2 Compliance of firms' wage obligation

In this subsection we analyse the earnings paid by firms receiving WSS payments to their workers. In particular, subsidy receiving firms were obliged to "use best endeavours to pay at least 80% of each named employee's normal wage and salary for the duration of the subsidy", and "pass on at least the full amount of the subsidy claimed, to the employee, for the subsidised period, but if the employee's ordinary wages are less than the subsidy, pay them their usual wages" (WINZ 2021). Given this pair of obligations, we first analyse the extent to which the weekly job earnings distribution tends to show spikes at either the part-time or full-time subsidy rates, or at 80% of the 'normal' earnings rate. We then analyse what characteristics are associated with any such spiking.

This analysis uses 'Payday filing' earnings data from IR's Employment EIE tables, which provides employer-employee (job-level) earnings spell data. The EIE data have a significant advantage over IR's EMS data for the current analysis, in that each EIE observation includes both the pay frequency, and the start and end dates, of each earnings spell observation, which enable standardised weekly earnings rates to be estimated. Using these data, we construct a monthly panel sample of job earnings by sampling all earnings spells that span the 15th of each calendar month. This sample is then matched to firm characteristics such as industry and importantly WSS receipt, and worker characteristics such as age, gender and ethnicity. The analysis is restricted to wage and salary employees.[44]

[44] This is defined as observations with income_source_code equal to "WS" (wage and salary) and tax_code not equal to "WT" (withholding tax).

For each observation, we estimate the job's weekly earnings rate for job-*j* in month-*t* as:

$$weekly\_earnings_{jt} = 7 * \frac{gross\_earnings_{jt}}{spell\_duration_{jt}}$$

where the earnings spell duration is measured in days: $spell\_duration_{jt} = end\_date_{jt} - start\_date_{jt} + 1$. There is a strong, albeit imperfect, concordance between spell duration and the employee pay frequency indicator in the EIE data. For example, the most common pay frequencies are fortnightly (FT) and weekly (WK), accounting for 49% and 38% of all records respectively; and, for each of these frequencies, the vast majority (about 98%) of spell durations are 14 days and 7 days respectively.[45]

As there are two dimensions to firms' wage subsidy obligations with respect to paying wages, we examine and document both the extent to which the wage subsidy amounts are prevalent in wage earnings, and the extent to which 80% of normal earnings are prevalent in relative weekly earnings. For the latter, we measure a job's 'normal' earnings as the average of its weekly earnings rate in February and March 2020,[46] which we refer to as pre-earnings; and then measure its weekly earnings in subsequent months relative to these pre-earnings.

6.2.1 Describing the weekly earnings distribution over time

We first document the patterns of weekly earnings just before (March) and after (April) the introduction of the Original WSS in March 2020. Figure 13(a) presents the distributions of weekly earnings for jobs, separately in March and April 2020 and stratified by whether or not the job was listed in an Original (March 2020) WSS-application, and Figure 13(b) presents the distributions of relative earnings in April, separately for listed and non-listed jobs. For each of these (and subsequent) analyses, we have left and right censored weekly earnings at $50 and $3,000 respectively; and similarly left and right censored relative weekly earnings at 0.25 and 1.75 respectively.[47]

Focusing on the distribution of weekly earnings in panel (a), we make several observations. First, there are clear spikes in the distribution of earnings around the part-time ($350/week) and the full-time ($585.50/week) subsidy rates for WSS-listed jobs in April, with the spike at the full-

[45] Some jobs have multiple earnings spells that span the 15th of a month. We are not sure the reasons for these multiple spells, but suspect they reflect a combination of duplicate observations and genuine multiple earnings payments. For these cases, we calculate the weekly earnings rate associated with each observation, sum these to provide a combined weekly earnings rate for the job-month, and then keep a single job-month observation.

[46] Although many earnings spells spanning the 15th of March likely also included the initial wage subsidy announcement date of the 17th and may be affected by either the announcement or factors related to the pandemic or impending lockdown, we observe little apparent effect on the distribution of March earnings. For this reason, we treat March as part of the pre-period.

[47] This censoring protects the confidentiality of small and large weekly earnings, and aids the visual presentation of the areas of interest in the distributions by limiting the effects of earnings outliers. Such censoring affects approximately 1% of monthly observations in each tail of the distribution.

time rate being particularly prominent. There is almost no evidence of bunching around these points in the either the March distribution for WSS-listed jobs, or the March or April distributions for non-listed (i.e. non-subsidised) jobs: this suggests the April WSS-spikes are related to WSS-receipt. Second, comparing the March and April distributions for WSS-listed jobs, the distributions below the part-time subsidy rate, and between the part-time and full-time rates, appear very similar, and most of the April spikes were created by downward-shifts of density relative to the March distribution.

Third, although the March and April distributions for non-listed jobs appear broadly similar, there is more mass in the lower earnings (below about $500/week) in March. This suggests there were relatively fewer low wage jobs in April than March, consistent with the prevalence of unsubsidised jobs ending in March. In fact, the number of non-listed jobs fell by 5% between March and April, while the number of WSS-listed jobs increased 4%. These differences may reflect the effects of the pandemic or lockdown on subsidised versus non-subsidised jobs, but may also be associated with seasonal job turnover patterns. Fourth, comparing the pre-subsidy (March) distributions of earnings for WSS-listed and non-listed jobs, it is apparent that WSS-listed jobs had predominantly lower earnings than non-listed jobs.

Next, Figure 13(b) shows the distributions of jobs' relative weekly earnings in April compared to their pre- (February and March average) earnings for WSS-jobs and non-WSS jobs. First, there is a clear spike in WSS-listed relative earnings at 0.8, corresponding to 80% of pre-subsidy earnings, and (almost) no spike at this point for non-listed job earnings. Not, surprisingly, there are also dominant spikes at 1.0 in both distributions, associated with jobs that have stable earnings rates.

Second, there is a degree of dispersion in the relative earnings for both WSS-list and non-listed jobs. For non-listed jobs, the dispersion is centred on 1.0, and roughly symmetrical around this point: although some of this observed dispersion may be related to the pandemic or lockdown, we suspect it predominantly reflects secular changes in job patterns over time. In contrast, for WSS-listed jobs, the dispersion is much broader, around both spikes at 0.8 and 1.0. In order to provide some sense of the degree of variation in job earnings over time in the absence of the wage subsidy, appendix Figure A1 plots the distributions of relative earnings in March for WSS-listed and non-listed jobs. This figure shows a degree of natural variation in both sets of jobs, with a little more variation in the distribution of WSS-listed jobs.[48]

[48] This figure understates the true variation in earnings, as March earnings enter both the numerator and denominator in the relative earnings calculation. Allowing for this effect, the variation in non-listed jobs' relative earnings in April appears broadly consistent with the expected variation in the absence of any pandemic effects. The spikes just below 1.0 (0.96) are because February and March have different numbers of days which affects the estimated weekly earnings for workers who are paid a fixed monthly salary.

Third, as well as the mass point at 0.8, there is also noticeably greater prevalence of relative earnings for WSS-listed jobs across the range below about 0.9 and correspondingly less prevalence above 1.0. This is consistent with WSS-firms paying workers considerably less than their pre-lockdown earnings, either at 80% of their pre-earnings, the full-time or part-time subsidy rate, or some other amount.

We next consider the progression of WSS effects on job earnings over time. To do this, we first stratify jobs into three groups, according to whether they were listed on any wage subsidy applications and, if so, for which waves. Specifically, the first group consists of jobs that were never listed on a WSS application; the second group consists of jobs that were listed on only a March 2020 wave application; and the third group consists of other jobs that were listed on miscellaneous WSS-wave applications: the latter group predominantly includes jobs that were listed on a March 2020 application and one or more subsequent wave applications. For each of these groups, we then document how the distribution of weekly earnings evolve month-by-month. In order to abstract from possible sample-selection effects associated with changes in jobs, we have conditioned on jobs that existed (i.e. were observed) in or before March 2020 and in or after June 2021.[49]

Figure 14 documents the evolution of each group's earnings over the four-month period from March to June 2020. Figure 14(a) shows the evolution of earnings for the never WSS-listed jobs. Consistent with the pattern in Figure 13(a), there is no evidence of spikes in weekly earnings at either the part-time or full-time subsidy rate across the four months.

Next, Figure 14(b) similarly shows no spikes in March for March 2020 WSS jobs, a large spike at the full-time rate and smaller secondary spike at the part-time rate in April. There is also more mass above the full-time subsidy rate and the mode of the earnings (around $900), and a deficit above this point, implying the leftwards displacement of earnings was relatively common. Each of these spikes fall over time: for example, the scale of the density at the full-time spike falls from about 1.5 in April to about 0.8 in May and 0.5 in June, compared to a density of about 0.35 at that point in March. In subsequent months (not shown), these spikes continue to dissipate for this group and the earnings distribution converges towards what it was in March.

The patterns for the third group of WSS-listed jobs in Figure 14(c), show larger spikes and density displacement effects than for the March 2020 only jobs in Figure 14(b). Also, while there is a similar convergence over time towards the original March 2020 distribution, the correction is

[49] As we don't require these jobs to have earnings in all months over this period, there will still be some month-to-month variation in the sample, but we expect the sample to be comparatively stable.

noticeably slower – e.g. compared to the relative density at the full-time rate in March of about 0.45, it was 3.3 in April, 2.1 in May, and still 1.25 in June.

We document the continued evolution for this group in Figure 15, from July to October in panel (a), and November to April 2021 in panel (b). The relative density at the full-time spike continued to be about 1 in July and August, by which time the WSS Extension wave was ending. The size of the spike continued to decline gradually over time, but was still noticeable in April 2021, by which time all wage subsidies (under our consideration) had ended.

Similar temporal patterns in the degree of spiking at 80% of pre-subsidy earnings are observed in the distributions of relative weekly earnings. There is strong evidence of bunching at the 80% point in April 2020, and the size of the spike dissipates over time, and appears to be less prevalent with the later subsidy waves. Taken together, these patterns suggest that prevalence of earnings spikes at the subsidy rates was especially strong in the initial phase of the wage subsidy and dominated among firms receiving multiple waves of support. The degree of spiking gradually dissipated over the period and appears relatively small by the end of the subsidies in April 2021.

6.2.2 Analysis of the extent of earnings spikes

In this section we now attempt to quantify the magnitude of spiking effects at the part-time and full-time subsidy rates, and at 80% of pre-subsidy earnings. The analysis is inherently complicated as it requires an analysis of the entire wage distribution. We take three related approaches to identify the extent and nature of spiking. We first construct separate binary indicator outcome variables for whether a job's weekly earnings are at the part-time, the full-time rate, and whether the weekly earnings are at 80% of the pre-subsidy baseline earnings.[50] We then estimate separate sets of difference-in-difference type linear probability regressions for each of these outcome variables. Finally, we construct a multinomial outcome variable for whether earnings are at the part-time subsidy rate, the full-time subsidy rate and/or at 80% of baseline earnings, and estimate multinomial logit model specifications for this combined outcome variable.

We begin by estimating whether a job's weekly earnings rate is at a spike point using difference-in-difference type regressions. Specifically, we estimate regressions of the form:

$$W_{jt} = \beta_0 + \beta_1 CWS_j + \beta_2 Post_{jt} + \beta_3 CWS_j * Post_{jt} + \beta_4 CWSactive_{jt} + X'_{jt}\gamma + \epsilon_{jt} \quad (7)$$

[50] To allow for possible rounding in earnings, we measure spiking at the part-time or full-time subsidy rate as whether weekly earnings are within +/- $10 of either rate; and we measure spiking at 80% of pre-earnings as whether relative earnings are within +/- 1 percentage point (ppt) of 0.8.

where W_{jt} is an indicator for whether the weekly earnings of job-*j* in month-*t* is at a spike (either at the part-time or full-time subsidy rate, or at 80% of pre-subsidy earnings), CWS_j is an indicator variable for whether job-*j* was listed on any of the WSS-wave applications, $Post_{jt}$ is an indicator for whether the observed job-*j* in month-*t* is after March 2020 (after the introduction of the WSS), $CWSactive_{jt}$ is an indicator for whether the job-*j* is listed on an active WSS-application in month-*t*,[51] and X'_{jt} is a vector of job-level variables, including worker demographic characteristics (age, sex and ethnicity), firm industry and region, firm-size and average earnings, and other job-specific characteristics such as the location of pre-subsidy earnings relative to the part-time, full-time and 80% focal spike points.

Equation (7) specification allows spiking to occur independently of the WSS policy, and allows the prevalence to vary before and after the subsidy was introduced in March 2020, as well as across jobs that were listed or not on a WSS-application, and also across job characteristics. Our main interest is in the coefficients on the $CWS_j * Post_{jt}$ interaction (β_3) and the $CWSactive_{jt}$ (β_4) variables. For example, $\beta_3 > 0$ would suggest there was a general increase in the incidence of earnings being at the focal spike points for WSS-listed jobs after the wage subsidy was introduced; while $\beta_4 > 0$ would suggest an increase in earnings-spiking during active wage-subsidy periods. All models are estimated using job-earnings observations over the period April 2019 – April 2022 for a 25% random sample of workers. We have estimated regressions both unweighted and weighted by the inverse of the estimated propensity score of a job being listed on a March 2020 WSS-application: the unweighted and weighted results are similar, and we report the estimates from the weighted regressions.

We begin by estimating regressions for whether a job's weekly earnings rate is at the part-time or full-time wage subsidy rate, and the results are collated and presented in Table 23. Column (1) presents the estimates for the simple model without any covariates. These results suggest the incidence of weekly earnings at the part-time subsidy rate increases slightly (0.3 ppt) for WSS-listed jobs after March 2020 relative to non-listed jobs.[52] In addition, the incidence of spiking is 0.8 ppt higher for WSS-jobs during active WSS-periods. In column (2), we allow the WSS-active period effects to vary across each WSS-wave. This shows the prevalence of earnings spiking at the part-time subsidy rate was highest during the Extension-subsidy wave (1.1 ppt), and was broadly similar across the other waves (ranging from 0.5–0.7 ppt). For sample comparability with the "at-80% of pre-earnings" analysis, in column (3) we restrict the sample to

[51] We assume the active subsidy periods were April and May 2020 for the Original WSS, June – August for the Extension WSS, September for the Resurgence WSS and March 2021 for the March 2021 WSS.

[52] Coefficients not reported in Table 23 show the incidence of earnings at the part-time wage subsidy rate for non-listed jobs before the WSS-period is about 1.1 percent, and the incidence is slightly (0.24 ppt) lower for WSS-listed jobs before March 2020. Also, the incidence is slightly (-0.3 ppt) lower for non-listed jobs after March 2020.

jobs that existed in the "pre-earnings" period of February and March 2020: the estimates are broadly similar to those in column (2), although the post effect is smaller (0.05 ppt) and the active WSS-period effects now decline across the waves, from 0.9 ppt during the March 2020 wave period (April and May 2020) to less than 0.3 ppt during the March 2021 wave period (March 2021).

In column (4), we control for job characteristics. The estimated coefficients are somewhat lower, which implies the WSS-status of jobs is correlated with the job characteristics. The estimated spiking effects associated with the WSS post effect is marginally negative (-0.1 ppt), and the active WSS-period effects are also smaller, and small and statistically insignificant for the Resurgence period, and negative for the March 2021 period (-0.3 ppt).

In the final specification, in column (5), we include mutually exclusive indicator variables for which focal spike point the job's pre-earnings are closest to.[53] Comparing the coefficients on these variables shows clearly that WSS-jobs with pre-earnings close-to and above the part-time subsidy rate were more likely to have earnings at the part-time rate after March 2020, and jobs with higher pre-earnings had monotonically lower incidence of being at the part-time spike.

In Table 24 and Table 25, we repeat this set of specifications for the binary outcome variables for whether a job's earnings were at the full-time rate and at 80% of their pre-earnings respectively. For the full-time rate spike in Table 24, we estimate similar main WSS*Post effects (about 0.3 ppt in column (1) and (2), and essentially zero when covariates are included in columns (4) and (5)) to those for spikes at the part-time rate. We also estimate substantially larger effects associated with the active WSS-periods which, in column (4), decline monotonically from 6.9 ppt during the March 2020 period, to 3.6 ppt during the Extension period, 0.7 ppt in the Resurgence period, and essentially zero in March 2021. The estimates associated with the indicator variables for relative pre-earnings are included in column (5), imply that the strongest spiking effect is for jobs with pre-earnings close-to and above the full-time subsidy rate; there are also noticeable effects for jobs with pre-earnings near-to but below the full-time rate (i.e. "between PT & FT"), and also in the "close to 80%" range (i.e. where $0.8 * preearns \geq \$585.50$ & $0.8 * preearns < 2 * \$585.50$).

For the "at 80% of pre-subsidy earnings" analysis in Table 25, we find similar patterns. Restricting the analysis to jobs with (February or March 2020) pre-earnings, in columns (3) – (5), we estimate WSS*Post effects of about 0.1pp, and again larger and declining effects during the

[53] Specifically, we define the following five indicator variables: (i) $ClosePT = (preearns \geq \$350$ & $0.8 * preearns < \$350)$; (ii) $BetweenPT\_FT = (0.8 * preearns \geq \$350$ & $preearns < \$585.50)$; (iii) $CloseFT = (preearns \geq \$585.50$ & $0.8 * preearns < \$585.50)$; (iv) $Close80 = (0.8 * preearns \geq \$585.50$ & $preearns < 2 * \$585.50)$; and (v) $Above2FT = (preearns \geq 2 * \$585.50)$. The omitted baseline range is where $preearns < \$350$.

active periods: in column (4), 5.1 ppt during the March 2020 period, 2.3 ppt during the Extension period, 1.6 ppt in the Resurgence period, and a small and insignificant 0.1 ppt in March 2021. The coefficients on the pre-earnings location indicators imply most of the spiking at 80% occurs for higher-earnings jobs – i.e. with pre-earnings above 1.25*$585.50. The incidence associated with these two higher-earnings bands are about the same (4.4 ppt and 4.6 ppt).

As well as these separate analyses for each focal spike point, we have attempted to provide a combined analysis using a multinomial logit (MNL) model. For this, we constructed a multinomial outcome variable for whether job earnings were at only the part-time rate, full-time rate, or 80% of pre-earnings, or at both the part-time rate and 80%, or the full-time rate and 80%. Attempts to estimate the model with control variables was unsuccessful, but we present coefficient estimates for a simple MNL model specification with no controls in Table A5. In this table, each column presents estimates for a separate multinomial outcome relative to the base outcome of job-earnings not being at either subsidy-rate or 80% of prior earnings. The coefficient estimates suggest stronger spiking effects at the full-time subsidy rate, at 80% of pre-earnings, or both of these; the estimates also suggest there was a greater incidence of earnings adjustment to these levels associated with the Original March 2020 WSS-wave.

Finally, we consider how the earnings adjustment varied across various identifiable population subgroups. A summary of results are presented in Table 26 (by sex), Table 27 (by age), Table 28 (by ethnicity), Table 29 (by region), and Table 30 (select industries). For this we present linear probability regressions results for the combined part-time and full-time outcomes, and separately for the at-80% of pre-earnings, for specifications corresponding to those in column (5) of Table 23 (i.e. with covariate controls and proximity to the focal spike-points). The patterns across these subgroups are complicated to summarise, but appear broadly similar, and we discuss some apparent differences.

The results by sex in Table 26, suggest males' earnings were slightly more likely to be adjusted than females' earnings. For example, the incidence of spiking at the wage subsidy rates during the March 2020 WSS-period was 6.6 ppt for men and 4.8 ppt for women; similarly, the incidence at-80% of the pre-earnings rate was 1.7 ppt for men and 1.6 ppt for women. Table 27 suggests the effects were slightly greater for prime age (25-39 and 40-54) workers than either younger or older workers. Ethnic differences in Table 28 show generally greater incidence of spiking during the March 2020 WSS-period at either the part-time or full-time subsidy rate for Māori, Pacific peoples' and Asian, than European, workers; but a greater incidence for European workers to be paid at 80% of their pre-earnings.

The patterns across regions (Table 29) are broadly similar. Table 30 shows some differences across industries. For example, during the March 2020 WSS-period, there were relatively high degrees of spiking at the wage-subsidy rates for Construction workers (10 ppt), and those in Accommodation and Food industries (11 ppt). It also appears Manufacturing workers (2.3 ppt), and Construction workers (3.6 ppt) were more likely to have earnings paid at 80% of their pre-earnings level. These differences likely reflect in part the typical earnings levels of workers in the various industries.

In summary, from the basic patterns of earnings spikes discussed in this section, we conclude there was a tendency of WSS-recipient firms to reduce earnings to either the subsidy rate or to 80%. The decision of firms as to which level to reduce earnings to appears to be determined by which was the closest (lower) focal point. The patterns shown in the kernel density graphs support the hypothesis that earnings reduction was largely confined to the active WSS-periods associated with lockdowns, with earnings returning to previous levels following these periods.

6.3 Analysis of worker outcomes

Our main analysis of worker outcomes focuses on three primary outcomes of interest: first, the job-retention for subsidy versus non-subsidy receiving workers – i.e. workers who were or were-not listed on successful subsidy applications by their employers; second, the broader subsequent employment continuity of subsidy-receiving workers; and third, the subsequent earnings of workers across their jobs held. For each of these analyses, we use monthly employment and earnings data from IR's Employer Monthly Schedule (EMS) tables in the IDI. The EMS tables identify on a calendar month basis employer-employee "job" level employment and earnings to facilitate the job-retention analysis. In addition, the monthly job-level data can be aggregated for each worker to identify workers' monthly employment and earnings to facilitate the second and third analyses.

As in the analysis of firm outcomes, we follow the Hirano and Imbens (2001) doubly-robust method of estimating the effects of the COVID-19 Wage Subsidy on job retention and workers' outcomes. This involves estimating regressions for the outcome of interest, both controlling for covariates that may affect either the outcome or the propensity to receive wage subsidy support, and also using weights based on the inverse of the estimated propensity score associated with wage subsidy receipt.

More specifically, if $\hat{P}_{iw}$ is the estimated propensity score that observation-*i* (either job or worker) was listed in a successful wave-*w* wage subsidy application, we assign weight $\omega_{iw} = 1$

for WSS-recipient observations, and $\omega_{iw} = \hat{P}_{iw}/(1-\hat{P}_{iw})$ for WSS non-recipient observations. To estimate the average WSS treatment effect for WSS recipient (ATT), we then use these weights to estimate regressions of the form:

$$y_{it} = \beta_t CWS_{iw} + X'_{it}\gamma + \epsilon_{it} \quad (8)$$

where CWS_{iw} is an indicator variable for whether observation-*i* was listed on a successful wave-*w* WSS application, and X'_{it} is a vector of control variables, and ϵ_{it}. In equation (8), the coefficient β_t captures the WSS treatment effect of interest. The vector X_{it} includes the following covariates: a quadratic in the worker's age (measured in March 2020), indicator variables for the worker's sex, ethnicity,[54] location (regional council) and 1-digit industry. In addition, for the WSS-waves beyond the original (March 2020) wave, we also control for receipt of previous WSS-wave subsidies.

In the following sections we report results from equation (8) regression analyses for each of the WSS-waves for job-retention, worker employment, and worker earnings. For each of these outcomes, we condition on jobs that existed (or workers employed) in the baseline month associated with each WSS-wave, and then focus on the effects on these samples in month-*t* following the baseline month, for up to *t*=12 months from the baseline month. The baseline months are as follows: March 2020 for the Original (March 2020) wave; May 2020 for the Extension wave; July 2020 for the Resurgence wave; and February 2020 for the March 2021 wave.

6.3.1 Job retention

We begin by analysing the job retention effects of the wage subsidies, and focus first on the effects of the March 2020 WSS-wave. Table 31 summarises regression results for the effects of the March 2020 WSS on subsequent job retention for the sample of jobs that existed in March 2020 baseline month. We report results for five different regression specifications (discussed below) over five different time horizons (1, 3, 6, 9, and 12 months). The actual job retention rate for subsidised workers is shown in brackets in the heading of each panel. After 1 month, 98.2% of subsidised workers were still in the same job – 15.5 ppt higher than for unsubsidised workers. After 12 months, this had dropped to 71.1% – 2.8 ppt higher than for unsubsidised workers.

In order to provide a sense of the relative selectivity of the WSS across jobs, we first compare estimates from simple unweighted (column (1)) and weighted by the estimated inverse propensity scores (column (2)) regressions with no covariate controls. The weighted estimates in

[54] We control for the following mutually exclusive ethnic group responses: European only, Māori only, European and Māori, Pacific peoples only, Asian only, other miscellaneous single ethnicity responses, other miscellaneous two ethnicity responses, and all other ethnicity responses

column (2) are substantially larger and more positive than the unweighted estimates, implying the March 2020 WSS tended to support less stable jobs on average. In particular, the weighted estimates imply the March 2020 wage subsidy wave had substantial positive effects on the rate of job retention over the following year: these estimates ranged from 27 ppt after 1-month down to 19.7 ppt after 6-months, 15.7 ppt after 9-months, and 13.2 ppt after 12-months. In contrast, the unweighted estimates of job retention effects were typically 10 ppt lower, ranging from about 15 ppt after 1- and 3-months, down to 8.8 ppt after 6-months, 5 ppt after 9-months and 2.8 ppt after 1- months.

One potentially important issue is the non-random selectivity of jobs that are listed or not by a WSS-receiving firm. In particular, a relatively large fraction of non-listed jobs in WSS-receiving firms end in March, and there is concern that any causality between WSS-listing and job-retention may be due to jobs that are ending (for secular reasons) are not listed on WSS-applications by the firms. Given the monthly granularity of the month in the EMS tables, it is not possible to identify whether a job was ongoing at the time a firm made a wage subsidy application. However, to examine the possible effect of this factor on the estimates here, we have restricted the sample of March 2020 jobs to those that appear sometime in the following 12-months. The results, reported in column (3), show substantially lower (about 15 ppt on average) job-retention effects, ranging from 11 ppt after 1- and 3-months down to 5.4 ppt and 1.9 ppt after 6- and 9-months, and a small and statistically insignificant 0.1 ppt after 12-months.

The final two columns report the results when additional controls are included in the regression (column (4)) and the sample is again restricted to jobs that appear sometime in the following 12-months (column (5)). Comparing the estimates in columns (4) and (5) again shows the estimated retention rates are 12-15 ppt lower when jobs that are not observed in the 12-months following the March 2020 baseline are excluded, but the estimated job retention rates are somewhat larger than those in columns (2) and (3): ranging from 31.3 ppt and 15.6 ppt after 1-month down to 16.3 ppt and 4.4 ppt after 12-months.

For the reasons discussed above, we expect the estimated job retention effects based on the full sample of jobs that existed in March 2020 to overstate the true retention effects, given the high rate of job ends in March of jobs not-listed on firms' WSS-applications. However, to the extent that jobs are not observed after March <u>because</u> of the effects of not receiving a wage subsidy, the estimates based on excluding such jobs from the analysis will understate the true retention effects. For these reasons, we interpret the estimates in columns (4) and (5) as providing upper and lower bounds on the true effects of the WSS on job retention.

We next consider results from similar analyses of the effects of the subsequent WSS-waves on job retention. Table 32 summarises the regression results for 3-, 6- and 12-month job retention effects of the Extension, Resurgence and March 2021 WSS waves for jobs that existed in the baseline month for each of these waves (i.e. May 2020, July 2020, and February 2021 respectively). For each wave, we present analogous results to those in columns (4) and (5) of Table 31, that Control for observable characteristics and are based on the full sample of baseline month jobs and excluding jobs that are not observed over the subsequent 12-months. In addition, we also include indicator variables for whether the job had been listed in a successful application in a previous WSS-wave.

We again estimate (mostly) positive effects of the wage subsidies on job retention, although the effect magnitudes are generally smaller than for the original March 2020 WSS-wave. For example, the estimated 3-month retention effects are 15.0–18.4 ppt for the Extension wave, 0.2–3.9 ppt for the Resurgence wave, and 5.3–13.2 ppt for the March 2021 wave; and the estimated 12-month retention effects are 4.1–6.6 ppt for the Extension wave, 1.2–1.3 ppt for the Resurgence wave, and 1.2–6.1 ppt for the March 2021 wave. The differences between the full sample ('upper bound') estimates and those based on excluding jobs that are not observed after the baseline month ('lower bound') are usually smaller (typically on the order of 5pp). In addition, we estimate generally positive coefficients on the previous-wave indicators (particularly the March 2020 wave indicator), which are suggestive of both extended effects of wage subsidy receipt during earlier waves and further non-random selection into subsequent WSS-receipt.

We summarise the patterns of job-retention effects for each WSS-wave across population subgroups by sex of workers (in Figure 16), age (Figure 17), ethnicity (Figure 18), region of residence (Figure 19), and for select 1-digit industries (Figure 20). The subgroup patterns generally reflect the aggregate effects for each wave, and the relative patterns often vary across waves. For instance, the estimated job-retention effects associated with the March 2020 and March 2021 waves appear stronger for male workers after 3-4 months; while the effects for the Extension and Resurgence waves are stronger for female workers.

The job-retention effects appear to be substantially stronger for young workers (aged less than 25) than older workers, with the effects for workers aged 25-39 also relatively better than older ages (Figure 17). The ethnic subgroup effects in Figure 18 show higher job-retention effects for Asian (especially) workers, and also for Pacific peoples in all waves. The effects for Māori and European were generally more similar, except the effects for Māori were somewhat higher for the Extension wave.

Job-retention effects across regional areas (Auckland, Wellington, rest of the North Island, and the South Island) in Figure 19 suggest that the Extension wave effects for workers in the South Island were relatively stronger than elsewhere; and the March 2021 wave effects were relatively worse for workers in Wellington. Finally, the patterns across selective industries in Figure 20 show the wage subsidy was particularly effective in maintaining job-attachment for workers in the hospitality sector (Accommodation and Food industries) across each wave. While Construction workers' job-retention rates were also somewhat higher following the March 2020 wave, this does not appear to be the case for the other waves.

6.3.2 Employment

Next, we consider the broader impacts that the WSS had on workers' employment (in any job) and earnings. In this subsection, we focus on the employment effects over the 12-months from the baseline month for each WSS-wave. The sample for this analysis for each wave consists of all workers employed in the baseline month, and we aggregate job observations for workers who hold multiple jobs in a month. Our analysis follows a similar structure to that for the job-retention effects of each wage subsidy wave above.

Table 33 summarises regression results for the effects of the March 2020 WSS on workers subsequent employment (in any job) for workers that were employed in March 2020. We report results for alternative regression specifications (in columns), and various time-horizons (1, 3, 6, 9, and 12-months) following the baseline period of March 2020 in panels (A) – (E). Column (1) contains results for a simple unweighted regression specification with no controls: this shows positive employment effects associated with the March 2020 wage subsidy of 6.7-6.8 ppt 1-3 months from the baseline, declining to 2.9 ppt after 6 months, and 1.4 ppt after 12 months. The inverse propensity-score weighted simple regression estimates are shown in column (2):[55] these decline from over 15 ppt after 1-month and 13.8 ppt after 3 months, down to 9.1 ppt after 6 months, 6.6 ppt after 9 months, and 6.0 ppt after 12 months. As in the case of job-retention, that the weighted effects are substantially larger than the unweighted estimates in column (1) implies that workers who were listed on WSS-applications had less stable employment patterns on average.

By analogy with the 12-month observation restriction we applied to job-retention analysis, in column (3) we restrict the sample to workers who are observed in EMS sometime during the following 12 months. This restriction has a much smaller effect on the sample used here, with less than 1% of observations lost compared to over 5% in the job-retention analysis above.

[55] For workers who have multiple jobs in the baseline month, we have calculated their inverse propensity score weight as the inverse of the earnings-weighted average propensity scores across each of the jobs held in that month.

Nonetheless, the estimated effects are uniformly about 2.5 ppt lower than the corresponding estimates in column (2).

In columns (4) and (5), we control for worker demographic and employment characteristics for each of the full and restricted samples respectively. The estimated effects are higher than the corresponding estimates for the simple specifications in columns (2) and (3). The full sample estimates range from over 20 ppt after 1-month and 17.7 ppt after 3 months, down to 11.9pp, 8.6 ppt and 7.8 ppt after 6-, 9- and 12-months; and the restricted sample estimates are again about 2.5 ppt lower.

Table 34 summarises regression results for the effects of the Extension, Resurgence and March 2021 WSS waves on workers subsequent employment (in any job) for workers that were employed in the baseline month for each of these waves (i.e. May 2020, July 2020, and February 2021 respectively). We report results based on regressions with controls for both the full sample and excluding workers not seen in the EMS in the following 12 months, for 3-, 6- and 12-months following baseline for each WSS-wave. The restricted estimates are again lower, but the gap is smaller (typically 0.5–1pp) than for the March 2020 estimates in Table 33.

We estimated generally positive effects of the Extension and March 2021 WSS waves on workers' subsequent employment. For the Extension wave, the effects are 9.4–9.8 ppt after 3 months, 3.4–3.7 ppt after 6 months, and 1.3–1.6 ppt after 12 months; while the March 2021 effects are smaller: 3.8–4.7 ppt after 3 months, 2.0–3.0 ppt after 6 months, and -0.3–0.6 ppt after 12-months. The effects of the Resurgence wave are smaller, and by 6 months marginally negative: ranging from 0.7–1.1 ppt after 3 months, to -0.8– -0.3 ppt after 6 months, and -0.5– -0.1 ppt after 12 months.

We summarise the effects of each of the WSS-waves on workers' employment outcomes across population subgroups by sex of workers (in Table 35), age (Table 36), ethnicity (Table 37), region of residence (Table 38), and for select 1-digit industries (Table 39). The effects are broadly similar across the various subgroups. For example, the estimated employment effects of the March 2020 wave are slightly better for men than women (Table 35); but the later wave effects are generally more positive (or less negative) for women. The employment effects for young workers were more positive across each of the waves than for prime-aged and older workers (Table 36). The differences between ethnic groups are fairly complex, although the effects for Māori workers are stronger than other groups for three of the four subsidy waves – e.g. the employment rates of Māori recipients were 16.1 ppt and 9.9 ppt higher than non-recipients 6- and 12-months after the March 2020 subsidy, compared to 8.6 ppt and 4.9 ppt effects for European workers. Finally, the results for select industries (Table 39) shows particularly strong

employment continuity effects of the March 2020 wage subsidy for workers in Construction and Accommodation and Food industries: the 6-month effects are 20.8 ppt and 28.4 ppt respectively, and the 12-month effects are 14.5 ppt for each of these industries.

6.3.3 Earnings

We next discuss results for the effects of each WSS-wave on workers' subsequent earnings. To do this, for workers employed in each wave's baseline month, we analyse the effects of being listed on any successful wage subsidy application for that wave on their subsequent total monthly earnings from all jobs, conditional on being employed. For this analysis, we adopt two outcome earnings measures: first, the log(total earnings) of workers in month-*s* following the baseline month-t – i.e. $\log(earnings_{t+s})$; and second, in order to control for possible constant unobserved person-effects, we analyse the change in their log(earnings) from the baseline month – i.e. $Dlog(earnings_s) = \log(earnings_{t+s}) - \log(earnings_t)$. In addition, as our earnings measures condition on being employed, by focusing on earnings growth should adequately control for any bias associated with relative changes in the compositional makeup of the employment samples of subsidised and non-subsidised workers over time.

Table 40 summarises regression results for the effects of the March 2020 WSS on workers subsequent earnings (in any job) for workers that were employed in March 2020. We present results at 1-, 3-, 6-, 9- and 12-months following the March 2020 baseline, and for three specifications for each of the $\log(earnings_{t+s})$ and $\mathrm{D}\log(earnings_s)$ outcomes: unweighted and inverse propensity score weighted simple regressions, and weighted regressions including covariates. The generally negative estimates imply workers supported by the wage subsidy generally earned lower earnings, or had lower earnings growth over the year following its introduction; however there is substantial variation between the estimates for the $\log(earnings_{t+s})$ and $\mathrm{D}\log(earnings_s)$ outcome measures, as well as across both time and specification.

The weighted and unweighted simple estimates for $\log(earnings_{t+s})$ in columns (1) and (2) again imply workers listed on WSS-applications were negatively selected in terms of their earnings. Controlling for observable covariates (column (3)) further reduces the estimated effects: the estimated 6-12 months effects in columns are small (less than 1% in magnitude), suggesting the wage subsidy had little lasting effect on conditional earnings. When we use the growth in individual workers' earnings from the baseline month as the outcome measure, the patterns are somewhat different. First, the weighted and unweighted simple estimates are more similar, implying that differencing corrects for much of the bias: in fact the 6-12 month weighted effects are now larger than the unweighted. Second, including covariates has less effect on the

estimates in this case, with similar 3-12 month estimates in columns (5) and (6). Given the importance of controlling for observable characteristics of workers and unobserved fixed effects in earnings differences, our preferred specification is the differenced outcome $\mathrm{Dlog}(earnings_s)$ in column (6).

The column (6) estimates suggest that, among workers employed at each point in time, earnings growth among wage subsidy recipients was about 5% (4.9%) lower than for non-recipients after 1-month, 1% higher after 3-months, and then 6.5%, 9.6% and 10.2% lower after 6-, 9- and 12 months. Thus, while the employment effects in Table 33 suggest the March 2020 wage subsidy had a declining positive effect on recipients' employment over time (from about 15 ppt after 3 months down to about 5 ppt after 12 months), it had increasing negative effects on recipients' earnings growth, among those who retained employment.

Table 41 summarises regression results for the effects of the Extension, Resurgence and March 2021 WSS waves on workers subsequent earnings (from all jobs held in a month) for workers who were employed in the baseline month for each of these waves (i.e. May 2020, July 2020, and February 2021 respectively). For each wave, we present 3-, 6- and 12-month estimates for both the earnings level ($\log(earnings_{t+s})$) and growth ($\mathrm{Dlog}(earnings_s)$) specifications controlling for covariates, corresponding to the specifications in columns (3) and (6) in Table 40.

First, the earnings growth estimates for the Extension-wave, in column (2), imply recipients of this wave experienced 3-4% stronger earnings growth after 3-12 months than non-recipient workers, controlling for receipt of the March 2020 subsidy. Thus, both positive employment and earnings effects were achieved for this wave. The earnings growth effects for recipients of the Resurgence-wave subsidy were much smaller, ranging between -1.0% and +1.1% after 3-12 months. The earnings growth effects for recipients of the March 2021 wave subsidy were slightly positive (0.6%) after 3-months, before turning negative (-2.6– -2.9%) after 6- and 12-months. The estimates for the earnings level specifications are all relatively strongly negative for each wave, ranging from -5.1– -8.4% but, as discussed above, we consider the growth specifications more reliably robust.

We summarise the effects of each of the WSS-waves on workers' monthly earnings conditional on being employed across population subgroup by sex of workers (in Table 42), age (Table 43), ethnicity (Table 44), region of residence (Table 45), and for select 1-digit industries (Table 46). Again, results are generally similar across subgroups, although the patterns of results are not straightforward to interpret. Table 43 shows substantially slower earnings growth for young subsidy recipients than non-recipients in the 6- (-15.9%) and 12-months (-31.1%), than for

other age groups following the March 2020 baseline. The adverse wage growth effects following the March 2020 baseline were weaker for Māori (-3.1%) than other workers after 6-months (e.g. European relative wage growth was -6.0%), but stronger (-10.0%) after 12-months (e.g. -9.0% for Europeans; although Asian workers fared worse at -11.1% relative earnings growth). Finally, in contrast to the stronger employment effects for subsidy receiving workers in the Construction and Accommodation and Food industries, Table 46 shows the earnings growth of such workers was particularly weak – e.g. the earnings growth of March 2020 subsidy recipients in Accommodation and Food was 42.6 log points (about 35%) slower than non-recipient workers after 12-months; similarly the 12-month earnings effect for Construction workers was -23.6 log points (about -21%). The patterns across industries are more similar for the other waves, although the March 2021 effects are weaker for workers in Accommodation and Food than the other industries reported in (except Retail).

7 Unintended consequences

The findings summarised in the previous section are focused on the intended outcomes for firms and workers of the WSS. This section discusses whether WSS support had unintended consequences in the form of supporting non-viable firms, reducing job reallocation, poor targeting, and macroeconomic spillover effects.

7.1 Did WSS support non-viable firms?

The WSS aimed to help viable firms survive the temporary hardships arising from COVID-19 lockdowns. However, in practice there was no mechanism to prevent firms that would otherwise have ceased operating even in the absence of lockdowns from accessing support. There is an inherent difficulty in identifying whether firms remained alive only because of the wage subsidy. The lack of a concrete definition or reliable measure of viability makes this analysis challenging. To the extent that support went to non-viable firms, we would expect to see a raised level of firm deaths at the end of subsidy periods. We found that the survival rate for subsidised firms was higher than for unsubsidised firms (section 5.4); in addition, survival was sustained well beyond the initial subsidy period, pointing to the ongoing viability of subsidised firms. Although there was a relatively high firm death rate in March 2021, this is evident for both subsidised and unsubsidised firms, suggesting that the closures are a result of changing business conditions rather than due to the WSS per se. The lower firm birth rates in April 2020 and April 2021 are also consistent with that pattern, and the contribution of changes in firm birth and death rates to employment change was relatively small, as shown in Figure 4.

Employment in subsidised firms did grow more slowly than employment in comparable unsubsidised firms, as discussed in section 5.5, but this was primarily due to employment changes within continuing firms, rather than to deaths of non-viable firms. The combination of higher survival rates and lower employment growth rates in subsidised firms suggests that the WSS may have kept firms in operation that had poorer growth prospects (surviving but not thriving), since there is no obvious reason why the WSS would have lowered employment growth. Our estimation approach (using reweighting and regression-based adjustment) compares the outcomes of subsidised firms with outcomes of firms that have the same mix of characteristics and growth prospects. We cannot distinguish whether the estimated lower employment growth of subsidised firms is due to the correlation between survival and growth, or to the failure of the characteristics we are able to include in our analysis to fully control for differences in growth prospects.

Overall, there is not compelling evidence that WSS had the unintended effect of supporting non-viable firms.

7.2 Did WSS reduce job reallocation?

The success of WSS in increasing job retention rates for subsidised workers may have had the unintended consequence of locking workers into less productive jobs, and impeding the reallocation of jobs to more productive firms. A difficulty in judging whether this occurred is that we cannot tell what level of reallocation was needed following the onset of COVID-19. The evidence in Figure 5 indicates that, after an initial (March 2020) increase in job exit rates in both subsidised and unsubsidised firms, rates of job starts and job ends declined in both subsidised and unsubsidised firms during lockdown periods, returning to more-or-less historical levels otherwise. The slightly higher exit rates and lower entry rates for subsidised firms had the effect of reallocating jobs from subsidised to unsubsidised firms – reflected in their relative employment growth rates.

Although the effects of WSS and lockdowns are evident in worker and job flows, there is not strong evidence that the process of reallocation was greatly disrupted or suppressed by WSS support.

7.3 How did worker earnings compare with pre-WSS levels?

During periods when wage subsidies were paid to firms, firms were required to pass on the subsidy amount to workers and were encouraged to pay at least 80% of prior earnings. One potential consequence of how the WSS was delivered was that workers may have been paid less

than the subsidy amount or, for low-hours part-time workers, they may have received more than their prior earnings if the full subsidy amount was passed on by the firm.

Our analysis of weekly earnings in section 6.2 shows there was a tendency for subsidised workers to be paid either the WSS part-time or full-time subsidy rate, or at 80% of their pre-WSS earnings during active subsidy periods. The analysis finds that workers' earnings are predominantly "rounded down" to each of these points.[56] This implies there is no evidence of either widespread underpayment of subsidy amounts, or of subsidised workers being paid above what they had previously earned. We conclude that firms appear to have largely complied with their obligations to pass on the subsidy payments to their workers and to pay them at least 80% of their previous earnings when possible.

7.4 Was subsidy support targeted effectively?

Two potential unintended consequences of the way that WSS was delivered were that support may have been paid to firms that were not the intended recipients of support, or that not all eligible workers received subsidy support. Because our analysis used confidentialised data, our ability to pinpoint potential misallocation is limited. It is also outside the scope of the outcome evaluation. We observed three patterns in the data that could be investigated further to determine the extent to which they represent misallocation:

7.4.1 WSS-only firms:

In the March 2020 wave, we observed 99,200 subsidised sole trader firms and 13,200 subsidised employing firms that could not be linked to other administrative business data sources. The majority of these firms were associated with individuals who had declared business income in personal tax returns. Although we cannot rule out fraudulent claims as an explanation for some of these WSS-only firms; we expect some of these firms were legitimately entitled to subsidy support or were small businesses outside the scope of the administrative data. In addition, imperfect administrative linking may contribute to some of WSS-only observed firms.

7.4.2 Unsubsidised workers in subsidised firms:

Not all workers in subsidised firms were listed on the subsidy applications. This may reflect a failure of employers to extend support to all workers, or it may reflect the ineffectiveness of wage subsidy policies more generally to reach groups of workers with high job turnover rates.

[56] That is, workers paid at the part-time subsidy rate during an active WSS-period predominantly had earnings above and close-to the part-time rate previously; similarly, workers paid at the full-time rate were predominantly those with prior earnings above and close-to the full-time rate; and workers paid at 80% of their prior earnings were predominantly those who earned more than the full-time rate.

For the March 2020 wave, about 15% of employees in subsidised firms were not listed on paid WSS applications. Of these, one third were not employed after March 2020, which is high compared with a 4% separation rate among unsubsidised firms, and 0.6% among listed employees. Section 6.1.1 shows that the unlisted employees were disproportionately workers in less stable employment, with lower job tenure, multiple jobs, and lower pay. Unlisted employees could also include employees in firms where some employees were able to work during lockdown. We cannot tell with any certainty whether the unlisted employees were eligible employees who were inappropriately denied subsidy support or were ineligible because they were no longer employed by the subsidised firm.

7.5 Did unsubsidised firms and workers benefit from the subsidy?

Our analysis of the impact of wage subsidy support is based on a comparison of outcomes for subsidised firms and their workers with outcomes of unsubsidised firms and workers. To the extent that the impact of wage subsidy support was due to the stimulatory macroeconomic effect of increased fiscal transfers, outcome differences between subsidised and unsubsidised forms and workers will understate the total policy impact.

An alternative explanation may be that the main positive impacts of the wage subsidy support were macroeconomic, operating largely as a large fiscal stimulus, delivered as payments to subsidised firms. This is not necessarily an unintended consequence of the WSS policy – but it is a feature of the policy that does not require it to be structured as a wage subsidy. The positive stimulus would have positively affected both subsidised and unsubsidised firms. As our analysis identifies only the direct effects of the payments to subsidised versus unsubsidised firms, such direct effects may have been relatively smaller than the broader macroeconomic stimulus which benefited all firms. The fact that our estimated employment effects are similar or larger than the macroeconomic estimates by Graham and Ozbilgin (2021) lessens the concern that our estimates understate the true employment impact.

8 Comparison with international evaluation findings

We have found relatively few international evaluations of COVID-19-related job retention or wage subsidy programmes that have estimated policy impacts as we have, using microdata to create credible counterfactual outcomes, or have examined the broad range of outcomes (take-up for firms and for workers, firm survival, firm employment, job retention, worker employment probability, equity of take-up and outcomes for firms and for workers).

It is difficult to make direct comparisons of our findings for the New Zealand WSS with findings from other countries. Job retention schemes differed across countries in terms of coverage, eligibility and targeting, generosity, duration, and delivery. Furthermore, there was variation in the range of COVID-19-related policies, as well as in the institutional context for labour market regulation and income protection, which would have affected the nature of wage subsidy impacts. Finally, differences in data availability, analytical methods, and the range and definition of outcome measures makes direct comparisons of findings challenging. Nevertheless, in this section, we summarise and compare selected findings from the WSS outcome evaluation with those from selected international estimates of related job retention schemes.

The take-up rates for New Zealand's WSS were high by international standards. For example, the OECD (2020, Fig 1) show that New Zealand had the highest worker take-up rate among 22 OECD countries examined. Almost all job retention schemes internationally were targeted towards jobs and firms that were the hardest hit by the pandemic. By design, STW (short time work) schemes target support to workers whose hours have been reduced. Wage subsidy schemes were often targeted towards smaller firms, who were less able to absorb the impact of COVID-19, and to firms with expected COVID-19-related declines in revenue or employment (Eichhorst et al. 2022). Support was more often conditional on maintaining employment or pay levels than on anticipated revenue loss, as in New Zealand. Even where revenue loss was not an eligibility condition, take-up was found to be higher among firms with such declines (HM Treasury and HM Revenue and Customs 2022). Similarly, vulnerable workers are generally found to be most adversely affected by pandemic-related job losses, and thus have relatively high take-up rates.

There was concern in many countries about take-up by non-viable firms, and the risk that job retention policies would suppress the ongoing process of reallocation of jobs to more productive firms (Barrero, Bloom, and Davis 2020; Santarelli and Vivarelli 2002). The existing evidence suggests, however, that this was not a widespread occurrence – similar to our findings for New Zealand (Andrews, Charlton, and Moore 2021; Cros, Epaulard, and Martin 2021; Groenewegen, Hardeman, and Stam 2021).

The average generosity of the WSS in New Zealand was lower than in other OECD countries, paying a fixed amount that was less than half the average wage (OECD 2020, p13). However, the fact that NZ provided flat-rate subsidies had the effect of providing greater relative support for lower-wage workers.

Comparing estimates of the effect of WSS-like schemes on employment, job retention, and firm survival is challenging because of the wide variation in evaluation methods and approaches,

as well as the inherent 'negative selection' resulting from the fact that assisted firms, jobs, and workers were generally those that faced a stronger adverse impact of the pandemic. Their outcomes are therefore expected to be poorer than non-recipients, and may be poorer even if the support has a positive impact.

Studies that estimate wage subsidy scheme impacts can be classified under three headings. First, 'prospective' studies rely on structural models of the economy or labour market to provide a baseline projection of what would happen in the absence of a subsidy. The assumptions of the model are then adjusted to reflect the supposed impact of the subsidy scheme. An alternative projection is created and the difference in outcomes between the original and adjusted projection is interpreted as the impact of the wage subsidy scheme. Second, 'observational' studies rely on observed changes in outcomes over time, or changes in relative outcomes between subsidised and unsubsidised workers or firms. Changes that occur after the period of the subsidy are attributed to the subsidy. A third approach relies on estimates of what outcomes would have been for subsidised workers or firms in the absence of the subsidy, allowing for the fact that recipients are a non-random sample of all workers or firms. We refer to this approach as a 'causal' approach because it attempts to provide an estimate of the causal effect of subsidies on outcomes.

Many of the evaluations of COVID-19-related policies do not aim to provide estimates of programme impacts. The OECD (2022a) summarises insights from 67 evaluations produced in OECD countries in the first 15 months of the pandemic. Many of the evaluation studies focus on issues of pandemic preparedness and crisis management. The evaluation of job retention schemes is classified under the heading of response and recovery – a grouping that also includes evaluations of health measures, of lockdowns and restrictions, and social policy, as well as other economic and financial support policies.[57] The focus of many of the government evaluations reported on by OECD (2022a) was on policy design, development, coordination, and delivery, with a strong emphasis on process issues, rather than on the impact of wage subsidy support. With reference to wage subsidy programmes, OECD (2022a) reports that evaluators found that it was too early to assess the total impact of schemes such as the New Zealand WSS (Key Insight 10). They also report that "while [evaluators] noted strong evidence that unemployment, output losses and business bankruptcies were contained, they also suggested that support schemes might have sustained non-viable firms or firms that were not in need of support or exploited the rules perhaps due to the speed of implementation and low conditionality" (Box 13). The OECD (2022a) also note that job retention schemes (which they characterise as being aimed at

[57] The only included New Zealand evaluation directly related to the WSS is Controller and Auditor General (2021).

households) were seen as effective in limiting income losses incurred by households, particularly households with more vulnerable workers, during the crisis (Box 12).

Graham and Ozbilgin (2021) report a prospective evaluation of WSS employment effects in New Zealand, using a structural macroeconomic model. They calibrate their model by incorporating sectoral productivity declines (to capture lockdowns), and a wage subsidy that reduces the cost of labour. They estimate that a wage subsidy similar in size and coverage to the actual WSS preserved 6.5% of employment, which is slightly smaller than our central estimate. They show stronger effects (17.2%) for workers under the age of 30, and just 2.6% of jobs for those over 50. In a prospective modelling of the UK Job Retention scheme, which had lower take-up than the NZ WSS, (Martin and Okolo 2020) report even larger impacts, amounting to 14%-17% of employment.

Observational studies generally show that employment levels recovered after periods of subsidy support, and interpret that pattern as an indicator of subsidy success. For an illustrative example from the UK, see Pope et al (2020). In New Zealand, Maani (2021) observes the modest changes in the unemployment rate and concludes that "the policy was successful in preventing possible mass unemployment across the country and a domino-effect of losses of output, which would have been further caused by lost earnings and spending."

Some observational studies compare outcomes of subsidised and unsubsidised claimants to provide estimates that are more closely related to the subsidy support. An evaluation by Government of Canada (2022) takes this approach and interprets post-subsidy differences as an indicator of policy effectiveness, while acknowledging that "claimants are expected to fare worse on these indicators, since by definition, they were more likely to have been deeply affected by the pandemic than non-claimants" (s 4.1). Claimant firms' share of employment declined and remained low for at least 12 months, and average employment in surviving claimant firms remained lower than for non-claimant firms. The study also found that claimant firms had a consistently lower closure rate than non-claimant firms.

Causal studies provide the most credible estimates of the impacts of subsidy support. They identify the effect of wage subsidy support on supported firms or workers either by matching recipients to similar non-recipients (as in our approach), or by focusing on eligibility rules that result in some firms receiving support while other (arguably otherwise similar) firms are excluded. For example, for the US Paycheck Protection Program (PPP), only firms with fewer than 500 employees were eligible for support. A number of studies compare within industry differences in outcomes between (eligible) firms with fewer than 500 employees and larger (ineligible) firms (Autor et al. 2022a; Chetty et al. 2020; Hubbard and Strain 2020). Similarly,

Kuchakov and Skouravskiy (2021) base their impact estimates on eligibility conditions related to firm employment and revenue. The UK (CJRS) and Australian (Jobkeeper) schemes used job tenure as an eligibility criterion. Bishop and Day (2020) compare outcomes for casual workers with less than 12 months of tenure (ineligible) with outcomes for casual workers with longer tenure, who were eligible for Jobkeeper support. A UK study by HM Treasury and HM Revenue and Customs (2022) compares outcomes for newly hired employees who started just before versus just after a cut-off date that defined eligibility (jobs that started after 19 March 2020 were ineligible).

A further subset of causal studies use spatial or timing variation to identify policy impacts. Holzer et al (2021) compare the timing of employment changes in US states that terminated COVID-19-related extensions of unemployment insurance eligibility at different times. Afridi et al. (2021) uses variation across Indian states in the historical capacity to provide public employment as a measure of exposure to assistance under the Mahatma Gandhi National Rural Employment Guarantee Act (MG-NREGA).

For any of these causal studies identified from eligibility or spatial variation, impact estimates for the programmes are based on the impacts for firms or workers affected by the eligibility cut-offs or spatial differences. Where overall impacts are reported, they generally rely on the assumption that overall impacts are the same as the 'local' impacts.

Our focal estimate of the employment impact of New Zealand's WSS is that the March 2020 WSS increased the employment rate of workers by 15.4 ppt three months after the subsidy started.[58] Subsidised workers accounted for about 60% of overall employment, implying a 9% boost to aggregate employment. We also find that employment in subsidised firms grew more slowly than employment in unsubsidised firms, and that firm survival was higher after WSS support.

Causal studies of job retention and subsidy schemes from other countries generally find positive employment effects, whether based on firm employment levels or total employment. We found a somewhat stronger employment effect than is found in most international studies. Where other studies have examined firm survival, they have generally found, as we have, that WSS-like support improves firm survival probabilities. The following bullet points provide some examples of relevant international comparisons.

[58] From column 5 of Table 33, which is described as a lower bound. The upper bound of 17.7 ppt (in column 4) is not substantially higher. The upper bound implies a 10.6 rather than a 9.2 boost to aggregate employment.

- Afridi et al. (2021) find that the Indian MG-NREGA Act reduced job losses in rural areas from April to August 2020 by 3.1 percentage points, or 7% over the baseline employment rate. Further, rural women's employment rose by 8.6 percentage points or 74%.
- Autor et al (2022a) conclude that the US PPP boosted employment in eligible firms by 2 to 4.5 percent 2-3 months after support was given. This implies an increase in aggregate US employment of 1.4 – 3.2 million jobs.
- Autor et al. (2022b) investigate outcomes for individuals from the COVID-19 relief Paycheck Protection Programme (PPP) in America. They determine that, having spent approximately $800 billion in uncollateralised, low-interest loans, almost all which were forgiven, the PPP had nearly saturated the market in its first two months with 93% of SMEs receiving one or more loans. Thus, estimating that the programme preserved somewhere between 2-3 million job-years of employment over a 14-month period, they deduce that it cost the US government between $170K-$257K per job-year retained. This implies only 23-34% of the PPP budget was allocated directly to workers who would have otherwise lost jobs. The remaining amount went to business owners and shareholders.
- Hubbard and Strain (2020) investigate the financial health, employment rate, and viability of US firms following PPP support. They assess what happens to businesses who *apply* for (but may not receive) a PPP loan of greater than $150,000 in the US. They find that applying for a PPP loan substantially increases employment, financial health, and survival of these small businesses.
- Chetty et al. (2020) found that in firms eligible for PPP support employment was raised by 2 percent of pre- COVID-19 employment levels compared with employment in ineligible firms, before settling back to similar trajectories in August 2020. They calculate that the PPP had a cost of USD377,000 per job saved, more than 8 times the USD45,000 mean annual earnings for employees in supported firms.
- Holzer at al. (2021) finds that early termination of COVID-19-related UI support increased movement of unemployed workers to employment by approximately 2/3, suggesting that support had a negative effect on employment. Among unemployed workers aged 25-54, early termination is associated with a 14-percentage point increase in the flow. They also estimated that if UI support had been terminated in July, the national unemployment rate would have been around 0.3 percentage points lower and the aggregate employment rate would have been 0.2 percentage points higher.
- In Australia, Bishop & Day (2020) estimated the impact of the *Jobkeeper Payment*, by comparing outcomes of 377 eligible and 274 ineligible casual workers. They find that, in May

2020, 74% of those potentially eligible for JobKeeper were still in employment compared to only 67% of those who were ineligible. Because only a fraction of causal workers took up the subsidy, the implied effect on actual recipients is larger – about 20 percentage points. By applying this impact estimate to the 3.5m people who received *Jobkeeper* payments, they estimate that the scheme saved around 700,000 jobs, which is around 5.4% of jobs overall. Based on this, they calculate that the cost of Jobseeker was about AUD100,000 per job saved for six months.

- Kuchakov and Skougarevskiy (2021) assess the short-run effect of Russia's COVID-19 job retention schemes on SME performance. They estimate a small and barely significant 5.1 ppt increase in the short-run probability of survival for supported firms, but no effect on employment or profits in the short-run.

9 Implications for key evaluation questions

This section brings together insights that are relevant for answering the first and third of the key evaluation questions (the second question, on value for money, is addressed in a separate paper (Reference to be provided). For each question, the findings are linked to the evaluation rubrics identified in section 4.1.[59]

1. To what extent did the WSS reach the intended people and businesses?
a) To what extent did the WSS support employment attachment, business survival/resilience, employee income and other key outcomes in the short and medium term?
b) How were these outcomes distributed across different population groups, firms, sectors, industries, and regions?
2. What was the value for money of the WSS?
3. What (if any) were the unintended outcomes/consequences/risks of the WSS? eg unfair or illegal treatment of employees by employers, support for non-viable firms, potential misuse of funds

9.1 EQ1: To what extent did the WSS reach the intended people and businesses?

The first evaluation question includes a broad range of sub-questions, covering most of the outcomes that have been examined in this paper, and implicitly applied to each wave and to multiple types of firms and workers. The following table summarises the main implications of the analysis for the evaluation questions.

[59] As noted in section 4.1, a subset of the components for consideration that were identified in the Rubrics table have not been analysed.

Outcome	Rubric	Expected short-term and medium-term effects	Response
Reaching businesses	None identified	Not stated	• Take-up rates were 72% for employing firms in the March 2020 wave. Subsequent waves affected fewer firms. Over 90% of firms receiving later subsidy support had received March 2020 support. • Take-up rates were lower for sole trader firms (27% in March 2020) Distribution of outcomes • Subsidised firms generally experienced larger revenue declines • Substantial variation across industry and, to a lesser extent, across regions • Generally lower take-up by Māori firms, European/other firms; large firms
Reaching workers	None identified	Not stated	• Almost half (47%) of all workers employed at some point in Jan 2019-March 2022 were listed on at least one paid WSS application. Subsequent waves affected fewer workers Distribution of outcomes • Lower take-up by Pacific peoples, Māori, Young people, high earners; increased Asian take-up over successive waves • More precarious workers/ jobs less likely to be listed by subsidised firms
Firm survival	Firm survival	Short term reduction in closures; medium term return to baseline rates of firm birth and closure	• Subsidised firms had increased survival rates in the short term (6 months) and medium term (12 months) following the March 2020 and March 2021 waves. Effects were small following the Extension and Resurgence Distribution of outcomes • Increases in survival generally strongest for groups of firms with high take-up rates; apart from by firm size, with smaller increases for small firms.

Outcome	Rubric	Expected short-term and medium-term effects	Response
Firm employment	Business confidence	Medium term: Firm expansion and hiring rises to baseline levels	• Subsidised firms had lower employment growth than comparable unsubsidised firms, with the difference in employment levels growing over time. Hiring (job entry) rates into subsidised firms remained below those for unsubsidised firms Distribution of outcomes • Slowest relative growth for subsidised small firms, young firms, Pacific firms, Māori firms
Job retention	Employment continuity/ Job attachment	Short term increase in tenure; medium term return to baseline rates of job turnover	• Job retention higher for subsidised workers than for unsubsidised workers. This difference persisted for at least 12 months, but became smaller Distribution of outcomes • Stronger increases in job retention for young workers, Asian workers, Pacific workers, and in industries where take-up was high
Worker employment	Labour market attachment	Proportion of workers employed remains high in short and medium term.	• Subsidised workers were more likely to be employed than comparable unsubsidised workers. The difference became smaller over time, but persisted for at least 12 months for March 2020 and Extension waves, 3 months for Resurgence, and 6 months for March 2021 waves Distribution of outcomes • Strongest employment effects for young workers and Māori workers
Worker earnings	Earnings/ employee income	Short term earnings remain above 80% of prior earnings; Limited and evenly experienced increase in hardship	• During subsidy waves, subsidised workers had a higher probability of being at the higher of the subsidy rate; or 80% of their prior earnings. • Following the subsidy period, earnings growth was slower for subsidised workers than for unsubsidised workers Distribution of outcomes • The reduction in earnings growth was greater for young workers, and after the March 2020 wave, for Construction and Accommodation & Food industry workers

9.2 EQ3: What (if any) were the unintended outcomes/consequences/risks of the WSS?

The following table summarises the discussion in section 7.

Unintended Outcome	Rubric	Response
Supporting non-viable firms	Firm Survival	• There is not compelling evidence that WSS had the unintended effect of supporting non-viable firms
Reducing job reallocation	Labour market attachment	• There is not strong evidence that the process of reallocation was greatly disrupted or suppressed by WSS support
Over-paying/ under-paying subsidised workers	Earnings/ employee income	• There is no evidence of widespread underpayment of subsidy amounts, or of subsidised workers being more likely to be paid mor than they had previously earned
Subsidising ineligible firms	Not stated	• We cannot rule out imperfect administrative linking or fraudulent claims as explanations for some of the WSS-only firms
Failing to pay eligible workers	Earnings/ employee income	• Unlisted employees in subsidised firms could have been inappropriately denied subsidy support or could have been ineligible because they were no longer employed by the subsidised firm
Benefits go to unsubsidised firms	Not stated	• Macroeconomic spillover of WSS expenditure is likely to have improved outcomes also for unsubsidised firms.

10 Summary discussion

This paper provides a micro-econometric analysis of the effects of the COVID-19 wage subsidy waves on various firm and worker labour market outcomes of interest. The methodology adopted uses a combination of propensity score matching subsidised and unsubsidised firms, together with reweighting the unsubsidised firms' outcomes of interest, to conduct counterfactual analyses for the outcomes of subsidised firms and workers. Separate analyses are conducted for each WSS wave, controlling for subsidy receipt from previous waves. The March 2020 wave of WSS support was by far the largest of the four waves that we examine, accounting for over 77% of payments in the first 4 iterations covered by the outcome evaluation; in addition, most (over 90%) firms that received WSS support in later waves had also received support in the March 2020 wave. For these reasons, our summary focuses primarily on the impact of the March 2020 wave.

The results point to moderately strong positive effects of wage subsidy support on firm survival, job-retention and workers' employment, especially for the largest wave of support in March 2020. However, the estimated impacts on firm employment are weakly negative, implying that employment in subsidised firms did not grow as fast as employment in unsubsidised firms after the COVID-19 lockdowns. We also find predominantly negative impacts on workers' subsequent monthly earnings, particularly following the March 2020 subsidy, implying weaker earnings growth for workers that received subsidy payments. There are two possible contributing explanations for the higher firm survival and job retention, but lower firm employment growth and workers' earnings. First, a relatively large number of jobs that were non-subsidised were shed by subsidised firms in March 2020. Second, although we find no evidence that it supported non-viable "zombie" firms, it is possible that the wage subsidy scheme may have supported firms with poorer growth prospects on average, cushioning the impact of lockdowns and the pandemic by keeping afloat firms that were surviving but not thriving. Associated with this assessment is the evidence that, during active periods of firm subsidy receipt, subsidised workers were more likely to earn reduced wages consistent with firms' receipt-of-subsidy obligations.

We have gone to some lengths to control for the possibility that differences in outcomes for subsidised and unsubsidised firms and their employees could be due to factors other than subsidy receipt. We implemented a 'doubly robust' estimation approach to increase our confidence that findings do more closely reflect causal effects of subsidy receipt. Our findings may nevertheless still be biased by unobserved differences between the two groups, although we cannot be certain of the direction of those biases. Subsidy receipt was targeted towards firms that were expecting revenue losses. To the extent that our method (based on observed differences in characteristics) does not perfectly capture these expectations of loss, we might expect that subsidy recipients would have experienced even worse outcomes. In that case, our estimates would understate the positive impacts of the subsidy. In contrast, if firms that were anticipating failure (for reasons not strongly correlated with observed characteristics) did not apply for subsidy support, differences between subsidised and unsubsidised firms' outcomes would overestimate the subsidy impacts. Given these potentially offsetting effects that are not observable, we consider that our estimates provide a reasonable picture of the impacts of COVID-19 wage subsidy support.

In line with the revenue-loss criteria for WSS eligibility, we find that supported firms experienced disproportionately larger revenue losses than unsupported firms. Furthermore, the variation in WSS take-up, with higher take-up by businesses in tangibly affected industries

(construction, manufacturing, hospitality and administrative support services), together with lower take-up by larger firms, suggests that the WSS was relatively successful in supporting the intended businesses. However, that support was relatively low for female, Māori, Pacific peoples and young workers, it appears that the WSS was relatively less likely to support more precarious jobs and workers, which is consistent with it being more difficult to deliver support to workers in such jobs using a firm-based subsidy scheme.

Finally, we conclude that supported firms largely complied with their obligations to pass on the subsidy amount to their employees, and endeavour to pay them at least 80% of their prior earnings. Although WSS-listed employees had a higher probability of receiving less than their previous earnings than unsubsidised workers, they were typically being paid at the higher of the WSS subsidy rate or at 80% of their prior earnings, while firms were receiving subsidy support.

Despite the scale of support provided by the WSS, it could not prevent the negative employment and earnings impacts arising from the pandemic and associated public health measures. Nevertheless, it appears to have been effective at offsetting the impacts on subsidised firms and workers, and did not result in widespread adverse consequences.

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Table 1: Allocating subsidy applications to firm type

	Firm type		
	Employing	Sole Trader	Total
Application type			
• Employer	358,900	14,000	372,900
	96%	4%	(47% of total)
• Sole Trader	66,900	348,100	415,000
	16%	84%	(53% of total)
Total	425,700	362,200	787,900
	54%	46%	

Table 2: Wage Subsidy waves

	Start date	End date	Days of support	Part-time weekly rate	Full-time weekly rate
March 2020	17/03/2020	09/06/2020	84	$350.00	$585.80
Extension	10/06/2020	04/08/2020	56	$350.00	$585.80
Resurgence	21/08/2020	03/09/2020	14*	$350.00	$585.80
March 2021	08/03/2022	21/03/2021	14	$350.00	$585.80
August 2021	18/08/2022	09/12/2022	14*	$359.00	$600.00

Notes: * Resurgence Support could not start before Extension support had ended. Firms could apply for sequential 14-day periods of support.

Table 3: Wage Subsidy support

	March 2020	Extension	Resurgence	March 2021	Total
Firms with at least one employer application					
• Number of firms (000)	205.0	98.5	44.8	29.7	
• Number of people (000)	1417.1	486.1	281.8	141.3	
• Value of paid apps ($m)	9,681	2,340	270	153	12,444
• Repayments[1] ($m)	659	31	6	n/a	696
Firms with only Sole-trader applications					
• Number of firms (000)	191.1	89.9	36.5	21.3	
• Number of people (000)	191.1	89.9	36.5	21.3	
• Value of paid apps ($m)	1,258	396	40	24	1,719
• Repayments[1] ($m)	18	2	0	n/a	20
Total					
• Number of firms (000)	396.1	188.3	81.4	51.1	
• Number of people (000)	1608.1	575.9	318.3	162.6	
• Value of paid apps ($m)	10,939	2,736	311	177	14,163
• Repayments[1] ($m)	677	33	6	n/a	716

Notes: [1]Repayment information is incomplete - available only until 21 September 2021. Classification of firms is based on application type, rather than the 'employing v sole trader' classification used in later tables.

Table 4: Subsidy participation sequences

Mar-20 (N=396,200)		Extension (N=183,500)		Resurgence (81,195)		Mar-21 (51,600)	
1000	52%	1100	53%	1110	50%	1111	45%
1100	25%	1110	22%	1111	29%	1101	26%
1110	10%	1111	13%	1010	10%	0001	9%
1111	6%	1101	7%	1011	5%	1011	8%
1101	3%	0100	3%	0110	2%	1001	7%
1010	2%	0110	1%	0010	2%	0101	2%
1011	1%	0111	0%	0111	1%	0111	1%
1001	1%	0101	0%	0011	1%	0011	1%

Note: The 4-digit codes summarise the sequence of subsidy receipt across 4 waves. Eg: 1000 indicates receipt only in the first wave; 1100 indicates receipt only in the first and the second wave. The observation count is for the number of distinct firms in each cohort, as defined in Table 2

Table 5: Conditional participation probabilities

	Number of firms (000)	Mar-20	Extension	Resurgence	Mar-21
March 2020 recipients	396.2	100%	44%	19%	11%
Extension recipients	183.5	95%	100%	36%	21%
Resurgence recipients	81.2	94%	82%	100%	35%
March 2021 recipients	51.6	87%	75%	56%	100%

Note: For each row, the table shows the proportion of subsidy recipient firms that received subsidies in each of the four waves.

Table 6: Defining subsidy cohorts

	March 2020	Extension	Resurgence	March 2021
Definition				
• Reference month	March 2020	June 2020	August 2020	March 2021
• Last month of wave	June 2020	Oct 2020	Sept 2020	April 2020
Employing firms				
• Unsubsidised	78,800	184,100	234,300	250,400
• Subsidised	202,500	94,600	42,900	27,900
• Subsidised - WSS-only	13,200	7,400	3,600	2,800
Sole Trader firms				
• Unsubsidised	220,500	252,500	273,600	288,200
• Subsidised	81,200	39,200	16,400	8,400
• Subsidised - WSS-only	99,200	42,300	18,300	12,500
Total count	695,400	620,100	589,100	590,200

Note: Firm counts are for distinct firms observed in the reference month that are active (GST, employment, or WSS receipt) at some point from the reference month to the last month of the wave. WSS-only firms do not appear in administrative datasets other than WSS data.

Table 7: Do subsidised firms appear in other IDI data?

	Share of firms	In Business Register	In GST data	In EMS data	On IDI spine as a person	In Pers. tax (IR3) data	In WSS data only
				Employing Firms			
Unsubsidised	11%	87.0%	84.0%	74.0%	22.7%	5.2%	0.0%
Subsidised	29%	97.1%	96.7%	74.5%	17.6%	7.3%	0.0%
Subsidised WSS-only	2%	22.4%	5.7%	0.5%	71.9%	43.7%	13.0%
				Sole Traders			
Unsubsidised	32%	90.4%	96.9%	1.2%	25.9%	10.8%	0.0%
Subsidised	12%	94.4%	97.5%	1.7%	71.9%	51.0%	0.0%
Subsidised WSS-only	14%	13.6%	1.9%	0.1%	96.5%	63.7%	1.8%
Total	100%	80.2%	80.1%	30.7%	39.4%	22.0%	0.5%

Note: Data are for the March 2020 cohort, as defined in Table 2. Presence in other datasets is based on data from January 2017 on.

Table 8: Group Sizes

	Mar-20	Extension	Resurgence	Mar-21
		Employing Firms		
Number of firms (000)				
• Unsubsidised	79	184	234	250
• Subsidised: ever active	202	95	43	28
• Subsidised: WSS_only	13	7	4	3
GST Sales ($bn)				
• Unsubsidised	31.8	57.0	59.0	74.3
• Subsidised: ever active	34.7	7.3	3.5	2.1
• Subsidised: WSS_only	0.0	0.0	0.0	0.0
Number of employees (000)				
• Unsubsidised	1087	1981	2161	2380
• Subsidised: ever active	1435	457	259	142
• Subsidised: WSS_only	0	0	0	0
Total Subsidies received ($m)				
• Unsubsidised	0.0	0	0	0
• Subsidised: ever active	9623	2314	268	151
• Subsidised: WSS_only	133	40	4	3
		Sole Trader Firms		
Number of firms (000)				
• Unsubsidised	221	252	274	288
• Subsidised: ever active	81	39	16	8
• Subsidised: WSS_only	99	42	18	12
GST Sales ($bn)				
• Unsubsidised	9.8	11.6	10.0	13.6
• Subsidised: ever active	0.8	0.2	0.1	0.1
• Subsidised: WSS_only	0.0	0.0	0.0	0.0
Total Subsidies received ($m)				
• Unsubsidised	0.0	0	0	0
• Subsidised: ever active	558	178	19	9
• Subsidised: WSS_only	625	181	20	13

Table 9: Revenue change

	Mar-20		Extension		Resurgence		Mar-21	
	Unsub	Sub	Unsub	Sub	Unsub	Sub	Unsub	Sub
Short-term change								
• Mean revenue change	-18%	-34%	10%	24%	11%	20%	1%	8%
• Median	-22%	-42%	3%	17%	7%	12%	-1%	6%
• Q25	-55%	-67%	-9%	-3%	-9%	-5%	-26%	-18%
• Q75	2%	-13%	27%	49%	28%	42%	24%	32%
12-month change								
• Mean revenue change	-19%	-43%	5%	-15%	5%	-20%	42%	68%
• Median	-19%	-55%	0%	-21%	5%	-26%	55%	100%
• Q25	-64%	-78%	-27%	-55%	-29%	-58%	2%	52%
• Q75	10%	-22%	37%	16%	37%	8%	100%	100%

Note: Revenue change is measured as the percentage change of GST sales, either as a short-term change over a two month period (the two months following the reference month, compared with sales in the reference month and the preceding month); or a 12-month revenue change, measured as the percentage change in GST sales in the month following the reference month, relative to the same month a year earlier.

Table 10: Selectivity: take-up rates for Employing firms

	Mar 2020		Extension		Resurgence		March 2021	
	Raw	Adj	Raw	Adj	Raw	Adj	Raw	Adj
Overall take-up rate	72.0%		33.9%		15.5%		10.0%	
Southland	-18.0	-0.5	-16.1	-5.3	-10.8	-4.7	-8.7	-4.3
Gisborne	-11.6	-3.1	-15.0	-8.3	-10.4	-5.1	-8.7	-4.3
Taranaki	-13.9	-6.1	-13.2	-5.1	-9.5	-4.3	-7.8	-3.8
Manawatu-Wanganui	-8.5	-2.8	-13.1	-7.6	-9.5	-4.9	-8.3	-4.7
Hawke's Bay	-4.0	-0.5	-10.5	-7.1	-9.2	-5.7	-7.8	-4.2
West Coast	-8.7	-3.5	-8.2	-3.7	-6.1	-3.9	-5.7	-3.5
Marlborough	-4.5	0.3	-7.2	-3.6	-8.2	-5.7	-7.9	-4.7
Tasman	-0.4	2.9	-8.8	-7.6	-8.8	-6.0	-8.0	-4.3
Waikato	-5.4	-1.3	-8.2	-5.0	-6.1	-3.2	-5.8	-3.5
Bay of Plenty	-2.2	-1.4	-5.1	-3.9	-5.9	-4.0	-6.6	-4.4
Northland	-3.3	0.9	-5.5	-3.8	-6.0	-3.8	-5.2	-2.7
Wellington	-0.5	-3.2	-3.1	-3.0	-4.8	-3.7	-6.3	-4.7
Canterbury	0.6	0.7	-1.0	-0.5	-4.8	-4.3	-6.5	-5.0
Otago	-1.4	-0.2	-2.1	-1.3	-3.6	-3.0	-4.9	-3.6
Nelson	6.2	0.8	-3.3	-4.8	-6.2	-5.1	-7.0	-4.6
Auckland	6.7	2.1	11.3	7.0	11.7	7.6	12.2	7.8
Agriculture	-51.4	-51.4	-28.3	-0.3	-14.1	1.5	-9.3	1.8
Other primary	-12.6	-12.7	-15.3	-4.8	-10.9	-2.1	-8.6	-0.5
Finance&Ins	-13.9	-8.6	-7.8	-3.1	-4.7	-3.3	-4.2	-3.5
Mining	-9.2	-5.7	-14.4	-6.9	-10.7	-3.3	-7.6	0.1
Elec,Gas,Water	0.4	-1.7	-11.4	-7.2	-8.6	-4.7	-5.3	-1.8
Rental, Real Estate	-8.1	-5.5	-3.3	-2.0	-2.8	-1.4	-4.7	-3.3
Education	-25.0	-22.4	-10.6	-2.1	-3.2	1.8	-1.9	0.8
Public Admin	-9.6	-10.1	-6.0	2.5	-0.6	2.1	-1.4	0.5
Prof. Serv	1.5	1.0	2.4	2.4	-1.0	-2.7	-3.0	-3.9
Wholesale	5.8	4.3	1.3	-1.5	1.2	-1.9	0.4	-2.1
Health	-0.1	-1.3	-9.2	-8.3	-0.5	1.9	3.1	3.4
Retail	9.3	7.3	-3.2	-7.1	1.8	1.1	3.3	1.7
Manufacturing	13.1	11.5	6.1	2.7	0.6	-2.0	-1.3	-2.1
Other Serv	6.5	6.7	0.2	-1.9	0.9	0.6	4.2	3.6
Industry unknown	-14.6	-0.1	0.3	3.5	0.9	3.4	1.3	3.6
Info Media&Comms	-2.8	-3.0	13.1	11.3	8.6	2.8	2.5	-2.7
Construction	16.0	14.8	5.2	-1.8	-2.9	-5.1	-3.3	-3.2
Arts&Rec	6.0	5.8	14.7	10.0	8.6	5.0	3.9	1.7
Accom,food	14.2	10.4	17.4	6.7	16.0	8.4	15.6	7.9
Admin Support Serv	10.6	9.6	14.6	6.9	11.8	4.7	5.6	-0.3
Transport, Post,Wareh	6.8	5.2	5.8	2.3	5.5	2.4	4.0	1.1

(Table continues)

Table 10 (continued):

	Mar 2020		Extension		Resurgence		March 2021	
	Raw	Adj	Raw	Adj	Raw	Adj	Raw	Adj
European/other firm	-1.4	-0.1	-3.3	-1.7	-2.9	-0.9	-3.4	-1.2
Māori firm	-3.8	-1.4	-2.3	0.0	-3.5	-1.0	-4.0	-1.7
Pacific firm	4.8	0.7	10.7	3.7	5.5	-0.6	6.2	0.3
Asian Firm	6.7	1.2	11.8	5.6	12.2	3.9	14.4	5.7
MELAA firm	4.8	0.6	15.1	8.4	10.1	2.7	9.9	2.6
>50 employees	-13.0	-9.2	-15.6	-5.4	-3.8	2.0	-5.4	-0.9
11-50 employees	5.4	4.0	-0.7	-1.0	1.7	2.1	1.2	0.6
6-10 employees	5.5	-0.7	0.1	1.3	1.6	-1.3	1.7	-0.1
0-5 employees	-1.9	-0.8	0.6	0.2	-0.6	-0.5	-0.5	-0.1

Table 11: Selectivity: take-up rates for Sole trader firms

	Mar 2020		Extension		Resurgence		March 2021	
	Raw	Adj	Raw	Adj	Raw	Adj	Raw	Adj
Overall take-up rate	26.9%		13.4%		5.7%		2.8%	
Southland	-10.7	-2.8	-7.4	-1.0	-4.2	-0.8	-2.4	-0.4
Gisborne	-8.7	-3.6	-6.7	-1.7	-4.1	-1.2	-2.2	-0.5
Taranaki	-8.0	-1.9	-6.0	-0.9	-4.0	-1.1	-2.2	-0.4
Manawatu-Wanganui	-7.8	-2.9	-6.0	-1.4	-3.4	-0.8	-2.1	-0.6
Hawke's Bay	-5.0	-1.7	-4.5	-1.6	-3.1	-1.2	-1.9	-0.5
West Coast	-5.2	-2.6	-5.7	-2.2	-3.0	-0.6	-2.1	-0.6
Marlborough	-7.1	-2.5	-4.8	-0.7	-3.8	-1.7	-2.1	-0.6
Tasman	-4.4	-3.1	-4.7	-2.1	-3.4	-1.3	-1.9	-0.4
Waikato	-4.6	-1.6	-3.8	-0.9	-2.6	-0.9	-1.6	-0.5
Bay of Plenty	-1.4	0.4	-2.4	-1.1	-2.1	-1.0	-1.9	-0.9
Northland	-1.5	-0.2	-2.6	-1.3	-2.6	-1.4	-1.9	-0.8
Wellington	0.5	-3.2	-0.3	-1.1	-0.9	-1.1	-0.8	-0.6
Canterbury	-0.5	0.4	-0.6	-0.1	-1.7	-1.3	-1.6	-1.0
Otago	-1.3	0.3	-0.8	-0.1	-1.5	-1.1	-1.5	-0.9
Nelson	3.6	1.5	0.5	-0.6	-1.6	-1.4	-1.8	-1.0
Auckland	5.8	2.6	5.5	1.8	4.8	2.3	3.4	1.5
Agriculture (Base)	-21.1	-23.6	-11.6	-1.1	-5.3	0.1	-2.7	0.2
Other primary	-9.6	-10.7	-7.5	-3.2	-4.2	-1.2	-2.4	-0.4
Finance&Ins	-19.4	-12.4	-9.3	-0.1	-3.9	-0.2	-2.2	-0.5
Mining	-7.7	-4.8	-4.2	0.0	-3.8	-1.8	-2.8	-1.2
Elec,Gas,Water	-8.9	-6.1	-7.3	-3.0	-2.9	0.1	-2.8	-0.3
Rental, Real Estate	-15.8	-7.6	-7.1	0.6	-3.5	-0.3	-2.2	-0.5
Education	10.7	6.7	6.6	1.0	3.6	0.6	1.5	0.1
Public Admin	0.2	0.5	2.1	2.2	1.8	1.0	0.0	-0.5
Prof. Serv	1.3	-6.0	1.5	0.5	0.0	-1.1	-1.0	-1.6
Wholesale	-4.7	-1.8	-2.9	-1.4	-1.9	-1.4	-0.9	-0.5
Health	13.2	1.8	-0.5	-7.5	1.8	0.3	2.3	0.8
Retail	5.6	5.1	1.2	-2.5	0.4	-1.2	0.4	-0.4
Manufacturing	12.5	9.7	4.2	-2.1	-0.1	-2.2	-0.5	-0.9
Other Serv	11.5	11.9	3.3	-2.3	1.8	0.1	2.0	1.3
Industry unknown	14.1	16.5	9.9	6.0	5.5	4.2	4.4	3.4
Info Media&Comms	9.5	9.0	10.8	5.8	6.1	2.0	1.4	-0.9
Construction	29.7	23.0	9.9	-5.4	1.4	-3.6	0.4	-1.0
Arts&Rec	4.8	5.4	5.7	2.5	3.1	1.1	1.0	-0.1
Accom,food	-1.2	-0.4	0.9	-0.4	0.3	-0.5	0.3	0.0
Admin Support Serv	20.6	14.7	12.0	0.7	7.1	1.4	2.4	-0.7
Transport, Post,Wareh	33.6	22.8	29.5	9.4	24.7	10.8	19.0	8.4
European/other firm	-3.1	-1.6	-3.2	-1.3	-2.5	-1.0	-2.2	-1.1
Māori firm	10.0	5.7	4.6	0.3	0.2	-1.3	-1.0	-1.4
Pacific firm	21.0	9.0	19.0	7.9	9.1	1.1	7.5	2.5
Asian Firm	12.3	6.0	16.0	7.4	15.7	7.2	14.9	8.1
MELAA firm	17.6	10.0	20.4	9.0	16.0	6.3	12.5	5.5

Table 12: Firm exit Hazard regressions

	Mar20	Extension	Resurg	Mar21
		Employing		
Raw	0.558***	1.125***	1.125***	0.688***
	(0.00765)	(0.0166)	(0.0227)	(0.0397)
Full specification	0.596***	1.049*	0.947*	0.674***
	(0.0117)	(0.0221)	(0.0243)	(0.0533)
		Sole Trader		
Raw	0.864***	1.033*	1.046*	0.447***
	(0.00942)	(0.0146)	(0.0223)	(0.0358)
Full specification	0.796***	0.980	0.883***	0.805*
	(0.0117)	(0.0234)	(0.0293)	(0.0822)

Note: Estimates are (exponentiated) coefficients on subsidy receipt from proportional hazard regressions. The 'Full' specification includes covariates as described in the text, and is weighted to control for selection, using the weights derived from equation (3). Standard errors have not been adjusted for the first-stage propensity score estimation.

Table 13: Implied Impact on firm survival

	Mar20	Extension	Resurg	Mar21
		Employing		
N subsidised firms	215700	102000	46500	30700
Impacts on survival				
• 3 months	674	-79	47	176
	[612 ; 741]	[-142 ; -10]	[5 ; 95]	[104 ; 275]
• 6 months	2715	-121	69	463
	[2467 ; 2983]	[-219 ; -15]	[7 ; 138]	[273 ; 722]
• 9 months	3735	-288	170	n/a
	[3395 ; 4102]	[-522 ; -35]	[16 ; 339]	n/a
• 12 months	7664	-357	221	n/a
	[6971 ; 8411]	[-646 ; -43]	[21 ; 440]	n/a
		Sole trader		
N subsidised firms	180400	81500	34700	20900
Impacts on survival				
• 3 months	393	66	190	104
	[339 ; 450]	[-81 ; 227]	[91 ; 303]	[15 ; 238]
• 6 months	2005	85	247	0
	[1730 ; 2295]	[-105 ; 293]	[118 ; 393]	[0 ; 0]
• 9 months	2474	207	620	n/a
	[2136 ; 2831]	[-257 ; 715]	[298 ; 985]	n/a
• 12 months	5326	238	740	n/a
	[4602 ; 6090]	[-295 ; 821]	[356 ; 1173]	n/a
		Total		
• N subsidised firms	396100	183500	81200	51600
Impacts on survival				
• 3 months	1067	-13	237	280
• 6 months	4720	-36	316	463
• 9 months	6209	-81	790	n/a
• 12 months	12990	-119	961	n/a

Note: The implied impacts are calculated by deriving survival rates from the integrated hazards for the regressions in Table 12, and applying them to the number of subsidised firms (including WSS-only firms) active in the reference month for each wave.

Table 14: Firm exit Hazard regression coefficients: for subgroups of employing firms

	Mar-20	Extension	Resurgence	Mar-21
All	0.596***	1.052*	0.947*	0.674***
Age: <12 months	1.003	1.399***	0.948	0.926
Age: 12-35 mths	0.692***	1.083	0.988	0.612***
Age: 36+ mths	0.519***	0.973	0.950	0.704***
Ethn: Europ/other	0.484***	0.986	0.926*	0.816
Ethn: Māori	0.580***	1.157*	0.952	0.918
Ethn: Pacific	0.508***	1.218	0.974	1.079
Ethn: Asian	0.635***	1.157**	0.993	0.590***
Ethn: MELAA	0.549***	1.095	1.097	0.612
Ind: Primary	0.695***	1.314**	0.659*	0.368
Ind: Mfrg	0.491***	1.063	1.079	0.555
Ind: Ret/Acc/Food	0.440***	1.129**	1.100	0.990
Ind: Constr	0.413***	0.842**	0.947	0.595**
Ind: Network	0.601***	1.081	0.895	0.719
Ind: FIRE	0.812***	1.216*	0.953	1.233
Ind: Soc/Publ	0.698***	1.082	0.904	0.437*
Ind: ProfServ	0.667***	0.977	0.893	1.048
Ind: OthServ	0.644***	1.012	0.962	0.582**
Reg: Akld	0.676***	0.998	0.956	0.564***
Reg: Wgtn	0.513***	1.100	0.938	2.694***
Reg: Cant	0.445***	0.982	1.136	1.027
Reg: Waik	0.561***	1.298***	1.113	0.768
Reg: Upper NI	0.527***	0.981	0.804*	1.595
Reg: Lower NI	0.485***	1.227*	1.035	1.084
Reg: Lower SI	0.468***	0.944	1.052	2.078*
Reg: Other SI	0.478***	0.827	0.988	0.406
Size: 1-5 ee	0.668***	1.099***	0.951	0.707***
Size: 6-10 ee	0.221***	0.861	1.031	0.601
Size: 11-50 ee	0.250***	0.759***	0.986	0.621
Size: 51+ ee	0.356**	1.046	1.068	n/a

Notes:

Table 15: Firm exit Hazard regression coefficients: for subgroups of sole trader firms

	Mar-20	Extension	Resurgence	Mar-21
All	0.796***	0.977	0.883***	0.805*
Age: <12 months	0.871***	0.856**	0.815**	0.835
Age: 12-35 mths	0.914**	1.115*	0.915	0.775
Age: 36+ mths	0.788***	0.917**	0.896*	0.858
Ethn: Europ/other	0.711***	0.997	0.952	0.727*
Ethn: Māori	0.783***	1.090	1.008	0.545
Ethn: Pacific	0.807	1.286	0.757	0.218**
Ethn: Asian	0.838**	0.793**	0.721***	0.586
Ethn: MELAA	0.797	1.045	0.887	1.291
Ind: Primary	0.798***	1.353**	1.460	1.165
Ind: Mfrg	0.795**	0.938	0.850	1.252
Ind: Ret/Acc/Food	0.712***	1.240***	1.240**	1.034
Ind: Constr	0.864**	0.940	0.903	0.815
Ind: Network	0.886*	0.881	0.829*	0.669
Ind: FIRE	0.859***	0.967	1.007	1.685
Ind: Soc/Publ	0.862**	1.076	0.878	0.935
Ind: ProfServ	0.800***	0.922	0.927	1.026
Ind: OthServ	0.717***	0.996	0.892	0.832
Reg: Akld	0.798***	0.975	0.916	0.926
Reg: Wgtn	0.821***	0.968	0.723*	0.311
Reg: Cant	0.723***	1.014	0.988	0.746
Reg: Waik	0.736***	0.871	0.882	0.617
Reg: Upper NI	0.782***	1.190*	1.082	0.657
Reg: Lower NI	0.653***	1.255*	1.336	1.043
Reg: Lower SI	0.763***	0.891	0.939	3.246*
Reg: Other SI	0.843	1.200	0.944	0.990

Notes:

Table 16: Impact on Employment – regression coefficients

	Mar20	Extension	Resurg	Mar21
• O months	-0.0111**	-0.00997**	-0.0105**	-0.0769***
	(0.00349)	(0.00341)	(0.00397)	(0.0126)
• 3 months	-0.0109**	-0.0179***	-0.0551***	-0.0254***
	(0.00330)	(0.00321)	(0.00331)	(0.00341)
• 6 months	-0.0277***	-0.0478***	-0.0660***	
	(0.00324)	(0.00322)	(0.00334)	
• 9 months	-0.0426***	-0.0483***	-0.0601***	
	(0.00324)	(0.00330)	(0.00335)	
• 12 months	-0.0512***	-0.0562***		
	(0.00328)	(0.00325)		

Note: Based on ln(emp) regression (equation (6)). All coefficients are relative to the month prior to the reference month.

Table 17: Implied Impact on Employment (cumulative FTE annual employment in thousands)

	Mar20	Extension	Resurg	Mar21
		Employing		
Initial employment in subsidised firms	1435	457	259	142
Employment Impact				
• 0 months	0.0	0.0	0.0	0.0
• 3 months	-3.0	-0.1	-2.3	2.4
• 6 months	-6.9	-3.7	-5.5	
• 9 months	-16.6	-8.0	-8.6	
• 12 months	-28.4	-12.5		

Note: Based on ln(emp) regression. Estimates are based on cumulative differences between actual and counterfactual employment, normalised to the difference in the reference month.

Table 18: Employment regression 9-month coefficients – for subgroups of employing firms

	Mar-20	Extension	Resurgence	Mar-21 (3 months)
All	-0.0426***	-0.0483***	-0.0601***	-0.0254***
Age: <12 months	-0.154***	-0.0745**	-0.0799***	0.0115
Age: 12-35 mths	-0.0512***	-0.0584***	-0.0690***	-0.0487***
Age: 36+ mths	-0.0248***	-0.0406***	-0.0539***	-0.0270***
Ethn: Europ/other	-0.0320***	-0.0457***	-0.0518***	-0.0221***
Ethn: Māori	-0.0508***	-0.0241*	-0.0645***	-0.0241*
Ethn: Pacific	-0.0975***	-0.0635**	-0.0139	-0.0521*
Ethn: Asian	-0.0809***	-0.0395***	-0.0756***	-0.0294***
Ethn: MELAA	0.0141	-0.0925**	-0.0820**	-0.0278
Ind: Primary	0.0586***	-0.0170	-0.0358**	0.0397***
Ind: Mfrg	-0.0322**	-0.0562***	-0.0603***	-0.0108
Ind: Ret/Acc/Food	-0.139***	-0.0110	-0.0473***	-0.0376***
Ind: Constr	-0.0684***	-0.153***	-0.0585***	-0.0173*
Ind: Network	-0.0395***	-0.0794***	-0.0728***	-0.0396***
Ind: FIRE	-0.0373**	-0.0297*	-0.0418**	-0.0370*
Ind: Soc/Publ	-0.0320**	-0.0161	-0.0132	-0.0235*
Ind: ProfServ	-0.0411***	-0.0418***	-0.0732***	-0.0558***
Ind: OthServ	-0.0154	-0.0513***	-0.0697***	-0.0289**
Reg: Akld	-0.0564***	-0.0551***	-0.0510***	-0.0294***
Reg: Wgtn	-0.0163	-0.0447***	-0.0110	0.00343
Reg: Cant	-0.0462***	-0.0589***	-0.0693***	0.00635
Reg: Waik	0.0101	-0.0259*	-0.0258*	-0.0149
Reg: Upper NI	-0.00865	-0.0404***	-0.0642***	-0.0269**
Reg: Lower NI	0.0157	-0.0108	-0.00716	0.00350
Reg: Lower SI	-0.0230	-0.0632***	-0.0732***	0.00848
Reg: Other SI	-0.0591***	-0.0344*	-0.130***	-0.0217
Size: 1-5 ee	-0.0480***	-0.0215***	-0.0475***	-0.0327***
Size: 6-10 ee	-0.0478***	-0.111***	-0.0842***	-0.0140*
Size: 11-50 ee	-0.0390***	-0.140***	-0.0849***	-0.0186***
Size: 51+ ee	-0.0180	0.0790***	0.0828***	0.00537

Notes:

Table 19: Any-WSS take-up

	Raw take-up	Regression-estimates		
		All workers	Employees	Non-Employees
	(1)	(2)	(3)	(4)
Male	0.530***			
	(0.0004)			
Female	0.415***	-0.030***	-0.028***	-0.036***
	(0.0004)	(0.001)	(0.001)	(0.003)
Sex missing	0.523***	0.069***	0.094***	-0.013
	(0.002)	(0.008)	(0.008)	(0.058)
Ethnicity:				
European	0.489***			
	(0.0004)			
Maori	0.381***	-0.069***	-0.072***	0.011
	(0.001)	(0.001)	(0.001)	(0.008)
Pacific peoples	0.419***	-0.089***	-0.091***	0.0060
	(0.001)	(0.001)	(0.001)	(0.011)
Asian	0.538***	-0.002***	-0.008***	0.070***
	(0.001)	(0.001)	(0.001)	(0.004)
Misc single ethnic	0.483***	-0.011***	-0.013***	0.053***
	(0.002)	(0.002)	(0.002)	(0.009)
Euro & Maori	0.454***	-0.022***	-0.026***	0.043***
	(0.001)	(0.001)	(0.001)	(0.006)
Misc two ethnic	0.455***	-0.027***	-0.030***	0.016**
	(0.001)	(0.001)	(0.001)	(0.007)
Misc 3 ethnic	0.425***	-0.035***	-0.038***	0.040***
	(0.003)	(0.002)	(0.002)	(0.015)
Ethnicity missing	0.294***	-0.097***	-0.089***	-0.044***
	(0.001)	(0.002)	(0.002)	(0.017)
Age:				
Aged < 25	0.374***	-0.096***	-0.095***	-0.111***
	(0.001)	(0.001)	(0.001)	(0.009)
Aged 25-39	0.494***			
	(0.0005)			
Aged 40-54	0.525***	0.011***	0.005***	0.034***
	(0.001)	(0.001)	(0.001)	(0.004)
Aged 55+	0.470***	-0.022***	-0.015***	-0.096***
	(0.001)	(0.001)	(0.001)	(0.004)
Age missing	0.535***	-0.086***	0.059***	0.011
	(0.002)	(0.008)	(0.008)	(0.059)
Earnings:				
Quartile 1	0.318***			
	(0.001)			
Quartile 2	0.512***	0.021***	0.020***	
	(0.001)	(0.001)	(0.001)	
Quartile 3	0.553***	-0.032***	-0.033***	
	(0.001)	(0.001)	(0.001)	
Quartile 4	0.442***	-0.136***	-0.136***	
	(0.001)	(0.001)	(0.001)	
Earnings missing	0.671***	0.308***	0.502***	
	(0.001)	(0.002)	(0.004)	

(Table continues)

Table 19 (continued)

Industry:	Raw take-up	Regression-estimates: All workers	Employees	Non-Employees
A: Agriculture	0.181***	-0.275***	-0.274***	-0.130***
	(0.001)	(0.001)	(0.001)	(0.023)
B: Mining	0.547***	-0.021***	-0.020***	-0.406***
	(0.006)	(0.006)	(0.005)	(0.081)
C: Manufacturing	0.610***			
	(0.001)			
D: Elec,Gas,Water	0.362***	-0.260***	-0.261***	-0.392***
	(0.003)	(0.003)	(0.003)	(0.077)
E: Construction	0.825***	0.205***	0.209***	0.096***
	(0.001)	(0.001)	(0.001)	(0.019)
F: Wholesale	0.655***	0.026***	0.026***	-0.054**
	(0.001)	(0.001)	(0.001)	(0.027)
G: Retail	0.539***	-0.047***	-0.047***	0.070***
	(0.001)	(0.001)	(0.001)	(0.025)
H: Acc & Food	0.597***	0.115***	0.116***	0.048
	(0.001)	(0.001)	(0.001)	(0.031)
I: Trans,Post,Wareh	0.647***	0.038***	0.039***	-0.166***
	(0.001)	(0.002)	(0.002)	(0.025)
J: Info Meda	0.446***	-0.181***	-0.197***	0.014
&Comms	(0.002)	(0.002)	(0.002)	(0.020)
K: Finance&Ins	0.177***	-0.451***	-0.454***	-0.176***
	(0.002)	(0.002)	(0.002)	(0.025)
L: Rental &	0.679***	0.041***	0.036***	0.162***
Real Estate	(0.002)	(0.002)	(0.002)	(0.019)
M: Prof Servs	0.507***	-0.101***	-0.101***	-0.185***
	(0.001)	(0.001)	(0.001)	(0.021)
N: Admin,Support	0.413***	-0.048***	-0.037***	-0.222***
Services	(0.001)	(0.001)	(0.001)	(0.019)
O: Public Admin	0.084***	-0.521***	-0.528***	-0.182***
	(0.001)	(0.001)	(0.001)	(0.022)
P: Education	0.185***	-0.421***	-0.424***	-0.198***
	(0.001)	(0.001)	(0.001)	(0.024)
Q: Health	0.237***	-0.382***	-0.385***	-0.086***
	(0.001)	(0.00119)	(0.001)	(0.023)
R: Arts & Rec	0.617***	0.071***	0.071***	-0.033
	(0.002)	(0.002)	(0.002)	(0.024)
S: Other Servs	0.609***	-0.004**	-0.005***	-0.107***
	(0.001)	(0.002)	(0.002)	(0.025)
Ind_NEC	0.653***	0.138***	0.165***	-0.129***
	(0.001)	(0.002)	(0.004)	(0.019)
Ind-Miss	0.180***	-0.288***	-0.294***	-0.124***
	(0.005)	(0.004)	(0.004)	(0.036)
Fraction of period		0.577***	0.586***	0.177***
observed		(0.001)	(0.001)	(0.007)

(Table continues)

Table 19 (continued)

	Raw	Regression-estimates		
	take-up	All workers	Employees	Non-Employees
Regional Council:	(1)	(2)	(3)	(4)
Northland	0.454***	-0.040***	-0.036***	-0.054***
	(0.001)	(0.001)	(0.001)	(0.005)
Auckland	0.526***			
	(0.0005)			
Waikato	0.465***	-0.053***	-0.052***	-0.068***
	(0.001)	(0.001)	(0.001)	(0.004)
Bay of Plenty	0.460***	-0.048***	-0.048***	-0.056***
	(0.001)	(0.001)	(0.001)	(0.005)
Gisborne	0.377***	-0.075***	-0.074***	-0.065***
	(0.003)	(0.002)	(0.002)	(0.013)
Hawke's Bay	0.419***	-0.065***	-0.067***	-0.024***
	(0.001)	(0.001)	(0.001)	(0.007)
Taranaki	0.450***	-0.082***	-0.080***	-0.091***
	(0.002)	(0.002)	(0.002)	(0.008)
Manawatu/	0.417***	-0.084***	-0.082***	-0.087***
Whanganui	(0.001)	(0.001)	(0.001)	(0.006)
Wellington	0.409***	-0.077***	-0.074***	-0.071***
	(0.001)	(0.001)	(0.001)	(0.004)
West Coast	0.471***	-0.078***	-0.079***	-0.067***
	(0.003)	(0.003)	(0.003)	(0.014)
Canterbury	0.502***	-0.044***	-0.045***	-0.023***
	(0.001)	(0.001)	(0.001)	(0.004)
Otago	0.482***	-0.040***	-0.041***	-0.039***
	(0.001)	(0.001)	(0.001)	(0.005)
Southland	0.464***	-0.064***	-0.061***	-0.114***
	(0.002)	(0.002)	(0.002)	(0.009)
Tasman	0.479***	-0.047***	-0.046***	-0.044***
	(0.003)	(0.002)	(0.002)	(0.009)
Nelson	0.485***	-0.047***	-0.048***	-0.012
	(0.003)	(0.002)	(0.002)	(0.010)
Marlborough	0.444***	-0.076***	-0.077***	-0.059***
	(0.003)	(0.002)	(0.002)	(0.011)
RC missing	0.193***	-0.073***	-0.104***	-0.388***
	(0.002)	(0.002)	(0.002)	(0.012)
Constant		0.288***	0.282***	0.704***
		(0.001)	(0.001)	(0.019)
R-squared	***	0.306	0.334	0.080

Notes: The first column documents the raw take-up rates for each sub-group. The other columns contain multivariate regression take-up rates, conditional on other characteristics; and estimated relative to the omitted sub-group. The number of observations are 3,409,671 for all workers; 3,222,702 employees; and 186,969 non-employees.

*** p<0.01, ** p<0.05, * p<0.1

Table 20: WSS take-up by Wave

	Any WSS	March 2020	Extension	Resurgence	March 2021
	(1)	(2)	(3)	(4)	(5)
Overall rate	47.4%	45.4%	16.0%	8.3%	4.8%
Sex: base=Male					
Female	-0.030***	-0.030***	-0.021***	-0.004***	-0.007***
	(0.0005)	(0.001)	(0.0004)	(0.0003)	(0.0003)
Sex missing	0.069***	0.039***	0.053***	0.036***	0.047***
	(0.008)	(0.008)	(0.006)	(0.005)	(0.004)
Ethnicity: base=European					
Māori	-0.069***	-0.068***	-0.032***	-0.017***	-0.007***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.0005)
Pacific peoples	-0.089***	-0.084***	-0.051***	-0.029***	-0.021***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Asian	-0.002***	-0.013***	0.038***	0.032***	0.043***
	(0.001)	(0.001)	(0.001)	(0.0004)	(0.0003)
Misc single ethnic	-0.011***	-0.017***	0.028***	0.023***	0.020***
	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)
Euro & Maori	-0.022***	-0.021***	-0.008***	-0.006***	-0.002***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.0004)
Misc two ethnic	-0.027***	-0.028***	-0.007***	-0.003***	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Misc 3 ethnic	-0.035***	-0.035***	-0.010***	-0.005***	-0.001
	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)
Ethnicity missing	-0.097***	-0.065***	-0.066***	-0.038***	-0.035***
	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)
Age: base=Aged 25-39					
Aged < 25	-0.096***	-0.100***	-0.061***	-0.030***	-0.014***
	(0.001)	(0.001)	(0.001)	(0.0005)	(0.0004)
Aged 40-54	0.011***	0.013***	0.006***	0.003***	0.002***
	(0.001)	(0.001)	(0.001)	(0.0004)	(0.0003)
Aged 55+	-0.022***	-0.016***	-0.020***	-0.006***	-0.007***
	(0.001)	(0.001)	(0.001)	(0.0004)	(0.0003)
Age missing	-0.086***	-0.065***	-0.037***	-0.013***	-0.028***
	(0.008)	(0.008)	(0.007)	(0.005)	(0.004)
Earnings: base=Quartile 1					
Quartile 2	0.021***	0.023***	0.010***	0.005***	0.0004
	(0.001)	(0.001)	(0.001)	(0.0005)	(0.0004)
Quartile 3	-0.032***	-0.019***	-0.039***	-0.028***	-0.025***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.0004)
Quartile 4	-0.136***	-0.119***	-0.094***	-0.057***	-0.049***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.0004)
Earnings missing	0.308***	0.301***	0.187***	0.066***	0.033***
	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)

(Table continues)

Table 20 (continued)

	Any WSS (1)	March 2020 (2)	Extension (3)	Resurgence (4)	March 2021 (5)
Industry: base=C: Manufacturing					
A: Agriculture	-0.275***	-0.273***	-0.077***	-0.020***	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
B: Mining	-0.021***	-0.029***	0.048***	0.057***	0.006**
	(0.006)	(0.006)	(0.005)	(0.004)	(0.003)
D: Elec, Gas	-0.260***	-0.258***	-0.127***	-0.057***	-0.015***
& Water	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)
E: Construction	0.205***	0.198***	0.102***	0.016***	0.010***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
F: Wholesale	0.026***	0.027***	-0.035***	-0.010***	-0.003***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
G: Retail	-0.047***	-0.045***	-0.073***	0.018***	0.008***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
H: Acc & Food	0.115***	0.085***	0.151***	0.137***	0.124***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
I: Trans,Post,Wareh	0.038***	0.036***	0.051***	0.076***	0.0003
	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)
J: Info Meda&Comms	-0.181***	-0.181***	-0.008***	0.010***	0.002*
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
K: Finance&Ins	-0.451***	-0.448***	-0.132***	-0.065***	-0.028***
	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)
L: Rental&RealEstate	0.041***	0.039***	0.108***	0.047***	0.012***
	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)
M: Prof Servs	-0.101***	-0.104***	-0.025***	-0.014***	-0.005***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
N: Admin,Support Servs	-0.048***	-0.054***	0.024***	0.047***	0.010***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
O: Public Admin	-0.521***	-0.520***	-0.134***	-0.054***	-0.013***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
P: Education	-0.421***	-0.418***	-0.120***	-0.048***	-0.014***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Q: Health	-0.382***	-0.378***	-0.127***	-0.049***	-0.011***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.006)
R: Arts & Rec	0.071***	0.042***	0.231***	0.123***	0.091***
	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)
S: Other Servs	-0.004**	-0.013***	0.030***	0.029***	0.049***
	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)
Ind_NEI	0.138***	0.129***	0.017***	0.042***	0.036***
	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)
Ind-Miss	-0.288***	-0.283***	-0.069***	-0.019***	-0.0004
	(0.004)	(0.004)	(0.004)	(0.003)	(0.002)
Fraction of	0.577***	0.585***	0.184***	0.120***	0.066***
period obs'd	(0.001)	(0.001)	(0.001)	(0.001)	(0.0004)

(Table continues)

Table 20 (continued)

	Any WSS (1)	March 2020 (2)	Extension (3)	Resurgence (4)	March 2021 (5)
Regional Council: base=Auckland					
Northland	-0.040***	-0.027***	-0.064***	-0.080***	-0.070***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Waikato	-0.053***	-0.040***	-0.076***	-0.077***	-0.071***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.0004)
Bay of Plenty	-0.048***	-0.034***	-0.070***	-0.083***	-0.076***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.0005)
Gisborne	-0.075***	-0.057***	-0.092***	-0.100***	-0.083***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
Hawke's Bay	-0.065***	-0.049***	-0.092***	-0.096***	-0.080***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Taranaki	-0.082***	-0.066***	-0.089***	-0.096***	-0.079***
	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)
Manawatu/	-0.084***	-0.066***	-0.099***	-0.098***	-0.084***
Whanganui	(0.001)	(0.001)	(0.001)	(0.001)	(0.0001)
Wellington	-0.077***	-0.061***	-0.069***	-0.076***	-0.075***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.0004)
West Coast	-0.078***	-0.059***	-0.087***	-0.089***	-0.080***
	(0.003)	(0.003)	(0.002)	(0.002)	(0.001)
Canterbury	-0.044***	-0.028***	-0.053***	-0.079***	-0.079***
	(0.001)	(0.001)	(0.001)	(0.0005)	(0.0004)
Otago	-0.040***	-0.025***	-0.052***	-0.068***	-0.070***
	(0.001)	(0.001)	(0.001)	(0.007)	(0.001)
Southland	-0.064***	-0.044***	-0.110***	-0.108***	-0.085***
	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)
Tasman	-0.047***	-0.029***	-0.090***	-0.100***	-0.084***
	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)
Nelson	-0.047***	-0.030***	-0.081***	-0.094***	-0.084***
	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)
Marlborough	-0.076***	-0.059***	-0.088***	-0.102***	-0.082***
	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)
RC missing	-0.073***	-0.101***	-0.050***	-0.044***	-0.032***
	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)
Constant	0.288***	0.248***	0.137***	0.065***	0.057***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
R-squared	0.306	0.303	0.110	0.079	0.081

Notes: Each column reports multivariate regression take-up rates, conditional on other characteristics; and estimated relative to the omitted 'base' category for each characteristic. The 'Any WSS' results in the first column are repeated from the second column in Table 19. All regressions based on 3,409,671 observations.
*** p<0.01, ** p<0.05, * p<0.1

Table 21: March 2020 jobs, stratified by WSS-status of Firm and Job

	All Jobs	Non-WSS Firm-jobs	WSS-firm Jobs		
			All	Non-WSS	WSS-Jobs
Age	41.2	43.3	39.7	37.9	40.1
Female	0.491	0.564	0.441	0.493	0.431
European	0.570	0.585	0.560	0.538	0.563
Māori	0.058	0.062	0.055	0.069	0.052
Euro & Māori	0.074	0.073	0.075	0.079	0.074
Pacific peoples	0.054	0.053	0.054	0.065	0.052
Asian	0.169	0.152	0.180	0.163	0.183
Misc. ethnicities	0.076	0.073	0.077	0.086	0.075
Northland	0.029	0.030	0.029	0.032	0.029
Auckland	0.345	0.310	0.368	0.356	0.370
Waikato	0.091	0.093	0.089	0.087	0.090
Bay of Plenty	0.063	0.065	0.062	0.066	0.062
Gisborne/Hawke's Bay	0.045	0.050	0.042	0.055	0.040
Tara/Mana/Whanganui	0.070	0.079	0.064	0.060	0.064
Wellington	0.113	0.139	0.095	0.105	0.093
Canterbury/W.Coast	0.138	0.131	0.143	0.130	0.146
Otago/Southland	0.073	0.071	0.074	0.074	0.074
Nelson/Tasman/Marl	0.032	0.031	0.033	0.036	0.032
A: Agriculture	0.049	0.072	0.033	0.073	0.025
B: Mining	0.002	0.002	0.002	0.001	0.003
C: Manufacturing	0.097	0.064	0.119	0.073	0.128
D: Elec, Gas & Water	0.008	0.012	0.006	0.004	0.006
E: Construction	0.081	0.013	0.128	0.074	0.138
F: Wholesale	0.049	0.027	0.063	0.044	0.067
G: Retail	0.091	0.067	0.108	0.070	0.115
H: Accomm & Food	0.071	0.004	0.117	0.110	0.118
I: Trans, Post &Warehousing	0.042	0.019	0.057	0.050	0.058
J: Info Media & Comms	0.016	0.022	0.012	0.022	0.010
K: Finance & Ins	0.029	0.058	0.009	0.007	0.009
L: Rental & Real Estate	0.020	0.011	0.026	0.049	0.021
M: Prof Services	0.080	0.080	0.079	0.069	0.081
N: Admin, Support Services	0.059	0.031	0.079	0.144	0.066
O: Public Admin	0.062	0.141	0.010	0.023	0.007
P: Education	0.085	0.170	0.028	0.028	0.028
Q: Health	0.102	0.175	0.054	0.092	0.047
R: Arts & Rec	0.019	0.008	0.026	0.038	0.024
S: Other Services	0.037	0.026	0.045	0.030	0.047
Job tenure (since Jan'19)	11.5	12.0	11.2	8.5	11.6
No. jobs in March'20	1.16	1.16	1.15	1.41	1.11
No. jobs (Apr'19--Mar'20)	1.56	1.54	1.58	2.14	1.47
No. mths emp (Apr'19--Mar'20)	10.8	11.0	10.7	9.5	10.9
Rec. Bft (March'20)	0.094	0.097	0.092	0.160	0.079
No. mths Bft (Apr'19--Mar'20)	1.16	1.19	1.15	1.77	1.03
Job earns (March'20)	$5,046	$5,692	$4,608	$3,744	$4,771
Job earns (Apr'19--Mar'20)	$4,833	$5,472	$4,399	$3,551	$4,560
Total earns (March'20)	$5,408	$6,086	$4,947	$4,659	$4,994
Total earns (Apr'19--Mar'20)	$5,453	$6,191	$4,953	$4,703	$5,000
Nobs	2,456,541	992,778	1,463,763	233,010	1,230,750

Notes:

Table 22: March 2020 jobs, stratified by WSS-receipt and March- vs Post-March end

	Non-WSS Firm-jobs:		Non-WSS Jobs:		WSS-jobs:	
	March	April+	March	April+	March	April+
Age	37.1	43.6	33.2	40.4	35.1	40.1
Female	0.514	0.567	0.483	0.498	0.480	0.431
European	0.534	0.588	0.500	0.557	0.489	0.564
Māori	0.087	0.061	0.075	0.066	0.058	0.052
Euro & Māori	0.087	0.072	0.085	0.077	0.080	0.074
Pacific peoples	0.053	0.053	0.062	0.066	0.055	0.052
Asian	0.154	0.152	0.177	0.156	0.225	0.182
Misc ethnicities	0.085	0.073	0.100	0.079	0.093	0.075
Northland	0.030	0.030	0.029	0.033	0.030	0.029
Auckland	0.319	0.310	0.384	0.342	0.418	0.370
Waikato	0.090	0.093	0.084	0.089	0.082	0.090
Bay of Plenty	0.091	0.064	0.068	0.065	0.064	0.062
Gisborne/Hawke's Bay	0.062	0.050	0.046	0.059	0.039	0.040
Tara/Mana/Whanganui	0.067	0.079	0.055	0.062	0.047	0.064
Wellington	0.108	0.141	0.096	0.109	0.091	0.093
Canterbury/W.Coast	0.120	0.131	0.125	0.132	0.125	0.146
Otago/Southland	0.082	0.071	0.079	0.071	0.072	0.075
Nelson/Tasman/Marl	0.030	0.031	0.033	0.038	0.031	0.032
A: Agriculture	0.196	0.067	0.078	0.070	0.040	0.025
B: Mining	0.001	0.002	0.001	0.001	0.000	0.003
C: Manufacturing	0.066	0.064	0.074	0.072	0.066	0.128
D: Elec, Gas & Water	0.005	0.012	0.004	0.004	0.003	0.006
E: Construction	0.032	0.012	0.081	0.071	0.106	0.138
F: Wholesale	0.026	0.027	0.041	0.045	0.065	0.067
G: Retail	0.086	0.066	0.087	0.061	0.096	0.115
H: Accomm & Food	0.034	0.002	0.195	0.067	0.271	0.117
I: Trans, Post & Warehousing	0.021	0.019	0.040	0.056	0.029	0.059
J: Info Media & Comms	0.066	0.021	0.019	0.024	0.005	0.010
K: Finance & Ins	0.025	0.059	0.007	0.007	0.013	0.009
L: Rental & Real Estate	0.017	0.010	0.024	0.062	0.020	0.021
M: Prof Services	0.067	0.080	0.054	0.077	0.051	0.082
N: Admin, Support Services	0.071	0.029	0.165	0.133	0.113	0.066
O: Public Admin	0.083	0.143	0.012	0.028	0.008	0.007
P: Education	0.067	0.175	0.021	0.031	0.025	0.028
Q: Health	0.088	0.178	0.033	0.121	0.027	0.047
R: Arts & Rec	0.020	0.007	0.037	0.038	0.024	0.024
S: Other Services	0.027	0.026	0.028	0.030	0.040	0.047
Job tenure (since Jan'19)	6.9	12.2	6.8	9.5	7.9	11.7
No. jobs in March'20	1.69	1.14	1.56	1.33	1.34	1.11
No. jobs (Apr'19--Mar'20)	2.85	1.48	2.50	1.95	2.02	1.47
No. mths emp (Apr'19--Mar'20)	9.0	11.1	8.8	9.9	9.1	11.0
Rec. Bft (March'20)	0.171	0.094	0.160	0.161	0.131	0.078
No. mths Bft (Apr'19--Mar'20)	1.87	1.16	1.68	1.81	1.44	1.03
Job earns (March'20)	$3,473	$5,790	$2,423	$4,421	$3,572	$4,779
Job earns (Apr'19--Mar'20)	$3,044	$5,580	$2,357	$4,163	$3,023	$4,569
Tot earns (March'20)	$4,917	$6,127	$3,358	$5,240	$4,187	$4,999
Tot earns (Apr'19--Mar'20)	$4,527	$6,264	$3,450	$5,346	$3,676	$5,009
No. observations	42,111	950,664	78,984	154,026	7,689	1,223,064

Notes:

Table 23 Weekly earnings at part-time wage-subsidy rate

	Simple DiD (1)	WSS-waves (2)	Pre-March 2020 jobs (3)	Covariates (4)	Relative to spikes (5)
WSS*Post	0.301***	0.301***	0.0510*	-0.0745***	-0.097***
	(0.036)	(0.036)	(0.027)	(0.027)	(0.026)
WSS-active period	0.823***				
	(0.043)				
March 2020 Period		0.693***	0.925***	0.850***	2.770***
		(0.028)	(0.023)	(0.023)	(0.138)
Extension Period		1.104***	0.851***	0.590***	2.360***
		(0.085)	(0.063)	(0.063)	(0.163)
Resurgence Period		0.611***	0.487***	0.093	1.744***
		(0.144)	(0.120)	(0.120)	(0.190)
March 2021 Period		0.539***	0.266**	-0.339***	1.210***
		(0.142)	(0.129)	(0.131)	(0.187)
Close to PT-rate					1.590***
					(0.374)
Between PT & FT-rate					-0.481**
					(0.204)
Close to FT-rate					-2.113***
					(0.169)
Close to 80% pre-earns					-2.489***
					(0.147)
Above 2*FT-rate					-2.513***
					(0.147)
Observations	19,146,432	19,146,432	14,761,221	14,757,018	14,757,018
R-squared	0.000	0.000	0.001	0.008	0.025

Notes: Coefficients have been multiplied by 100 to represent percentage point effects. All regressions are estimated using a 25% random sample of workers. Standard errors in parentheses, clustered at the job-level. The dependent variable is an indicator for whether measured weekly earnings are ("at") within +/-$10 of the part-time ($350) wage subsidy rate.
*** p<0.01, ** p<0.05, * p<0.1

Table 24 Weekly earnings at full-time wage-subsidy rate

	Simple DiD (1)	WSS-waves (2)	Pre-March 2020 jobs (3)	Covariates (4)	Relative to spikes (5)
WSS*Post	0.271***	0.271***	0.098***	-0.009	-0.017
	(0.027)	(0.027)	(0.023)	(0.023)	(0.023)
WSS-active Period	4.407***				
	(0.054)				
March 2020 Period		6.996***	6.999***	6.948***	2.688***
		(0.047)	(0.044)	(0.044)	(0.115)
Extension Period		3.451***	3.817***	3.618***	-0.661***
		(0.111)	(0.098)	(0.098)	(0.152)
Resurgence Period		0.857***	0.953***	0.664***	-3.620***
		(0.141)	(0.098)	(0.098)	(0.145)
March 2021 Period		0.459***	0.506***	0.039	-4.351***
		(0.145)	(0.130)	(0.132)	(0.185)
Close to PT-rate					1.761***
					(0.244)
Between PT & FT-rate					5.411***
					(0.266)
Close to FT-rate					13.20***
					(0.286)
Close to 80% pre-earns					6.732***
					(0.146)
Above 2*FT-rate					1.216***
					(0.125)
Observations	19,146,432	19,146,432	14,761,221	14,757,018	14,757,018
R-squared	0.005	0.006	0.012	0.016	0.035

Notes: Coefficients have been multiplied by 100 to represent percentage point effects. All regressions are estimated using a 25% random sample of workers. Standard errors in parentheses, clustered at the job-level. The dependent variable is an indicator for whether measured weekly earnings are ("at") within +/-$10 of the full-time ($585.50) wage subsidy rate.

*** p<0.01, ** p<0.05, * p<0.1

Table 25 Weekly earnings at 80% of pre-WSS earnings

	Simple DiD (1)	WSS-waves (2)	Pre-March 2020 jobs (3)	Covariates (4)	Relative to spikes (5)
WSS*Post	-0.091***	-0.091***	0.147***	0.143***	0.141***
	(0.017)	(0.017)	(0.026)	(0.026)	(0.026)
WSS-active period	2.596***				
	(0.025)				
March 2020 Period		5.184***	5.068***	5.066***	1.682***
		(0.036)	(0.039)	(0.039)	(0.070)
Extension Period		1.126***	2.289***	2.299***	-0.847***
		(0.039)	(0.069)	(0.068)	(0.078)
Resurgence Period		0.268***	1.577***	1.602***	-1.397***
		(0.046)	(0.119)	(0.119)	(0.125)
March 2021 Period		-0.611***	0.045	0.107	-2.693***
		(0.016)	(0.092)	(0.093)	(0.118)
Close to PT-rate					0.332**
					(0.133)
Between PT & FT-rate					0.380***
					(0.119)
Close to FT-rate					1.200***
					(0.125)
Close to 80% pre-earns					4.410***
					(0.0912)
Above 2*FT-rate					4.629***
					(0.094)
Observations	19,146,432	19,146,432	14,761,221	14,757,018	14,757,018
R-squared	0.003	0.005	0.004	0.004	0.005

Notes: Coefficients have been multiplied by 100 to represent percentage point effects. All regressions are estimated using a 25% random sample of workers. Standard errors in parentheses, clustered at the job-level. Pre-WSS earnings are estimated as the average weekly earnings in February and March 2020. The dependent variable is an indicator for whether measured relative weekly earnings (Weekly earnings/Pre-WSS weekly earnings) are ("at") within +/-1 percentage point of 80%.

*** p<0.01, ** p<0.05, * p<0.1

Table 26 Weekly earnings at WSS-rates or 80% spikes, by sex

	Earnings @PT or FT WSS-rate:		Earnings @ 80% Pre-earnings level:	
	Males	Females	Males	Females
WSS*Post	-0.131***	-0.105*	0.152***	0.122***
	(0.0397)	(0.0606)	(0.0364)	(0.0368)
March 2020	6.627***	4.765***	1.703***	1.607***
period	(0.357)	(0.194)	(0.115)	(0.0903)
Extension	2.510***	1.489***	-1.015***	-0.630***
Period	(0.407)	(0.259)	(0.123)	(0.102)
Resurgence	-0.534	-2.743***	-1.579***	-1.284***
Period	(0.434)	(0.273)	(0.169)	(0.188)
March 2021	-1.790***	-3.983***	-2.825***	-2.670***
Period	(0.412)	(0.330)	(0.159)	(0.177)
Close to	2.738***	3.671***	0.297	0.369**
PT-rate	(0.775)	(0.533)	(0.252)	(0.154)
Between	4.856***	4.994***	0.348	0.440***
PT & FT-rate	(0.622)	(0.383)	(0.226)	(0.136)
Close to	9.500***	12.06***	1.095***	1.327***
FT-rate	(0.594)	(0.373)	(0.230)	(0.140)
Close to	3.033***	4.955***	3.972***	5.020***
80% pre-earns	(0.386)	(0.241)	(0.136)	(0.130)
Above	-2.311***	-0.899***	4.360***	5.521***
2*FT-rate	(0.374)	(0.213)	(0.130)	(0.180)
Observations	7,534,248	7,217,910	7,534,248	7,217,910
R-squared	0.041	0.038	0.006	0.005

Notes: Coefficients have been multiplied by 100 to represent percentage point effects. All regressions are estimated using a 25% random sample of workers. Standard errors in parentheses, clustered at the job-level. Pre-WSS earnings are estimated as the average weekly earnings in February and March 2020. The dependent variable for first four columns is an indicator for whether measured weekly earnings are within $10 of either the part-time or full-time WSS-rate; and for final four columns is an indicator for whether measured relative weekly earnings (Weekly earnings/Pre-WSS weekly earnings) are ("at") within +/-1 percentage point of 80%.
*** p<0.01, ** p<0.05, * p<0.1

Table 27 Weekly earnings at WSS-rates or 80% spikes, by age

	Earnings @PT or FT WSS-rate:				Earnings @ 80% Pre-earnings level:			
	<25	25-39	40-54	55+	<25	25-39	40-54	55+
WSS*Post	-0.356***	-0.188***	-0.0251	0.0133	-0.267***	0.153***	0.224***	0.285***
	(0.124)	(0.0551)	(0.0653)	(0.0591)	(0.0823)	(0.0455)	(0.0438)	(0.0498)
March 2020	4.925***	8.204***	5.486***	4.534***	1.269***	1.891***	1.920***	1.680***
Period	(0.250)	(0.467)	(0.393)	(0.334)	(0.112)	(0.155)	(0.185)	(0.176)
Extension	0.0443	2.957***	2.777***	2.879***	-0.178	-0.860***	-0.909***	-0.837***
Period	(0.431)	(0.527)	(0.421)	(0.380)	(0.126)	(0.168)	(0.203)	(0.205)
Resurgence	-3.941***	-1.249**	0.231	-0.241	-0.220	-1.752***	-1.286***	-1.512***
Period	(0.360)	(0.581)	(0.499)	(0.388)	(0.198)	(0.193)	(0.357)	(0.266)
March 2021	-3.590***	-2.772***	-1.797***	-1.226**	-0.890***	-2.950***	-2.924***	-3.270***
Period	(0.725)	(0.490)	(0.526)	(0.495)	(0.278)	(0.216)	(0.239)	(0.273)
Close to	3.926***	2.449***	2.911***	2.551***	0.425	0.418*	0.102	0.195
PT-rate	(0.668)	(0.882)	(1.128)	(0.854)	(0.283)	(0.234)	(0.268)	(0.298)
Between	6.312***	4.655***	3.639***	3.098***	0.0682	0.213	0.603**	0.731*
PT & FT-rate	(0.558)	(0.723)	(0.685)	(0.692)	(0.174)	(0.206)	(0.271)	(0.386)
Close to	13.02***	10.38***	10.70***	7.715***	1.202***	1.290***	1.347***	0.880***
FT-rate	(0.584)	(0.650)	(0.752)	(0.628)	(0.198)	(0.260)	(0.276)	(0.265)
Close to	5.933***	3.156***	2.693***	2.734***	3.752***	4.284***	4.367***	5.044***
80% pre-earns	(0.336)	(0.502)	(0.434)	(0.387)	(0.209)	(0.175)	(0.219)	(0.240)
Above	1.996	-3.043***	-2.088***	-1.540***	1.523***	4.191***	4.817***	4.823***
2*FT-rate	(1.387)	(0.483)	(0.402)	(0.346)	(0.457)	(0.190)	(0.209)	(0.215)
Observations	1,456,029	4,832,742	4,737,501	3,730,743	1,456,029	4,832,742	4,737,501	3,730,743
R-squared	0.025	0.047	0.045	0.037	0.004	0.006	0.006	0.005

Notes: Coefficients have been multiplied by 100 to represent percentage point effects. All regressions are estimated using a 25% random sample of workers. Standard errors in parentheses, clustered at the job-level. Pre-WSS earnings are estimated as the average weekly earnings in February and March 2020. The dependent variable for first four columns is an indicator for whether measured weekly earnings are within $10 of either the part-time or full-time WSS-rate; and for final four columns is an indicator for whether measured relative weekly earnings (Weekly earnings/Pre-WSS weekly earnings) are ("at") within +/-1 percentage point of 80%.
*** p<0.01, ** p<0.05, * p<0.1

Table 28 Weekly earnings at WSS-rates or 80% spikes, by ethnicity

	Earnings @PT or FT WSS-rate:				Earnings @ 80% Pre-earnings level:			
	European	Māori	Pacific peoples	Asian	European	Māori	Pacific peoples	Asian
WSS*Post	-0.0158	-0.0966	-0.0260	-0.343***	0.102***	-0.0674	0.113	0.293***
	(0.0367)	(0.152)	(0.124)	(0.124)	(0.0325)	(0.0998)	(0.123)	(0.0687)
March 2020	4.343***	6.801***	6.866***	8.004***	1.896***	0.611**	1.314***	1.500***
Period	(0.212)	(0.787)	(0.856)	(0.447)	(0.0924)	(0.243)	(0.335)	(0.176)
Extension	1.669***	2.611***	3.636***	1.055**	-1.077***	-1.051***	0.223	-0.532***
Period	(0.265)	(0.810)	(0.979)	(0.505)	(0.103)	(0.276)	(0.386)	(0.184)
Resurgence	-1.159***	-1.646	-0.132	-3.707***	-1.464***	-1.599***	-0.890**	-1.489***
Period	(0.244)	(1.251)	(1.337)	(0.641)	(0.213)	(0.374)	(0.403)	(0.204)
March 2021	-2.175***	-1.859	-1.837*	-5.489***	-3.038***	-1.723***	-1.202**	-2.572***
Period	(0.271)	(1.151)	(0.969)	(0.558)	(0.206)	(0.529)	(0.502)	(0.209)
Close to	3.632***	7.267***	0.420	1.962*	0.267	0.264	-0.0189	0.101
PT-rate	(0.529)	(2.201)	(1.983)	(1.135)	(0.173)	(0.458)	(0.452)	(0.307)
Between	4.976***	4.795***	4.709***	5.595***	0.460**	0.663*	-0.649*	0.240
PT & FT-rate	(0.414)	(1.218)	(1.690)	(0.819)	(0.187)	(0.377)	(0.380)	(0.240)
Close to	10.35***	10.32***	9.559***	12.95***	1.093***	1.451***	0.558	1.523***
FT-rate	(0.416)	(1.344)	(1.209)	(0.819)	(0.156)	(0.394)	(0.423)	(0.349)
Close to	3.413***	2.104***	1.850**	6.856***	4.967***	3.096***	2.922***	4.176***
80% pre-earns	(0.252)	(0.785)	(0.905)	(0.507)	(0.127)	(0.293)	(0.417)	(0.207)
Above	-1.219***	-2.405***	-2.459***	-0.0958	5.312***	3.192***	1.236***	3.360***
2*FT-rate	(0.222)	(0.758)	(0.884)	(0.518)	(0.120)	(0.384)	(0.431)	(0.243)
Observations	8,683,527	782,127	751,362	2,388,894	8,683,527	782,127	751,362	2,388,894
R-squared	0.038	0.034	0.035	0.056	0.006	0.003	0.005	0.005

Notes: Coefficients have been multiplied by 100 to represent percentage point effects. All regressions are estimated using a 25% random sample of workers. Standard errors in parentheses, clustered at the job-level. Pre-WSS earnings are estimated as the average weekly earnings in February and March 2020. The dependent variable for first four columns is an indicator for whether measured weekly earnings are within $10 of either the part-time or full-time WSS-rate; and for final four columns is an indicator for whether measured relative weekly earnings (Weekly earnings/Pre-WSS weekly earnings) are ("at") within +/-1 percentage point of 80%.
*** p<0.01, ** p<0.05, * p<0.1

Table 29 Weekly earnings at WSS-rates or 80% spikes, by region

	Earnings @PT or FT WSS-rate:				Earnings @ 80% Pre-earnings level:			
	Auckland	Wellington	Rest of NI	South Island	Auckland	Wellington	Rest of NI	South Island
WSS*Post	-0.109*	-0.389***	-0.109**	-0.0441	0.267***	0.214***	-0.0658	0.155***
	(0.0655)	(0.0903)	(0.0525)	(0.0679)	(0.0490)	(0.0681)	(0.0415)	(0.0486)
March 2020	6.085***	4.595***	5.204***	4.848***	1.850***	1.455***	1.752***	1.480***
Period	(0.312)	(0.399)	(0.283)	(0.341)	(0.123)	(0.248)	(0.123)	(0.127)
Extension	2.110***	0.353	1.364***	1.290***	-0.675***	-0.697**	-1.116***	-0.992***
Period	(0.365)	(0.471)	(0.352)	(0.433)	(0.125)	(0.310)	(0.144)	(0.144)
Resurgence	-1.872***	-3.044***	-1.148**	-2.352***	-1.567***	-0.784*	-1.361***	-1.156***
Period	(0.382)	(0.493)	(0.479)	(0.454)	(0.161)	(0.435)	(0.221)	(0.419)
March 2021	-3.222***	-4.703***	-1.969**	-3.623***	-2.848***	-2.881***	-1.992***	-2.323***
Period	(0.352)	(0.904)	(0.771)	(0.489)	(0.142)	(0.437)	(0.406)	(0.526)
Close to	2.398***	3.887***	3.648***	4.541***	0.368	0.341	0.197	0.500*
PT-rate	(0.811)	(1.055)	(0.706)	(0.885)	(0.227)	(0.400)	(0.223)	(0.269)
Between	4.484***	5.034***	6.077***	5.081***	0.189	0.384	0.299	0.747***
PT & FT-rate	(0.596)	(0.912)	(0.541)	(0.623)	(0.183)	(0.365)	(0.201)	(0.285)
Close to	10.09***	11.84***	10.21***	13.86***	1.111***	1.585***	0.842***	1.541***
FT-rate	(0.545)	(1.097)	(0.512)	(0.664)	(0.236)	(0.397)	(0.202)	(0.214)
Close to	4.569***	4.945***	2.836***	5.231***	4.020***	4.670***	4.388***	4.980***
80% pre-earns	(0.357)	(0.474)	(0.322)	(0.398)	(0.150)	(0.322)	(0.165)	(0.181)
Above	-1.407***	-0.772*	-1.583***	-0.840**	4.655***	5.686***	4.000***	4.695***
2*FT-rate	(0.337)	(0.414)	(0.302)	(0.361)	(0.153)	(0.382)	(0.165)	(0.176)
Observations	5,030,652	1,689,945	4,358,223	3,661,530	5,030,652	1,689,945	4,358,223	3,661,530
R-squared	0.045	0.041	0.035	0.040	0.006	0.006	0.004	0.005

Notes: Coefficients have been multiplied by 100 to represent percentage point effects. All regressions are estimated using a 25% random sample of workers. Standard errors in parentheses, clustered at the job-level. Pre-WSS earnings are estimated as the average weekly earnings in February and March 2020. The dependent variable for first four columns is an indicator for whether measured weekly earnings are within $10 of either the part-time or full-time WSS-rate; and for final four columns is an indicator for whether measured relative weekly earnings (Weekly earnings/Pre-WSS weekly earnings) are ("at") within +/-1 percentage point of 80%.
*** p<0.01, ** p<0.05, * p<0.1

Table 30 Weekly earnings at WSS-rates or 80% spikes, by industry

	Earnings @PT or FT WSS-rate:					Earnings @ 80% Pre-earnings level:				
	Manufacturing	Construction	Retail	Accommodation & Food	Health	Manufacturing	Construction	Retail	Accommodation & Food	Health
WSS*Post	-0.185***	-0.563***	-0.0245	0.115	-0.0502	-0.00229	0.0752	0.222***	0.0176	0.0863
	(0.0481)	(0.177)	(0.130)	(0.324)	(0.0827)	(0.0719)	(0.149)	(0.0769)	(0.157)	(0.0725)
March 2020	6.933***	10.14***	2.542***	11.20***	3.488***	2.324***	3.585***	1.419***	1.776***	1.516***
Period	(0.714)	(1.630)	(0.272)	(0.407)	(0.480)	(0.340)	(0.290)	(0.161)	(0.161)	(0.242)
Extension	3.049***	5.391***	0.378	-1.727***	0.621	-0.00705	-0.656**	-1.192***	-0.678***	-1.539***
Period	(0.750)	(1.729)	(0.412)	(0.557)	(0.565)	(0.346)	(0.274)	(0.257)	(0.163)	(0.304)
Resurgence	1.751**	3.670**	-4.050***	-7.066***	-0.0688	-1.132**	-1.723***	-2.351***	-0.816**	-2.700***
Period	(0.744)	(1.646)	(0.332)	(0.677)	(0.711)	(0.444)	(0.330)	(0.292)	(0.348)	(0.444)
March 2021	1.119	2.280	-3.036***	-8.624***	0.00572	-2.219***	-2.166***	-3.148***	-2.554***	-3.080***
Period	(0.737)	(1.613)	(0.826)	(0.588)	(0.917)	(0.854)	(0.369)	(0.291)	(0.210)	(0.416)
Close to	2.568*	1.436	2.982***	2.963***	2.818**	-0.350	-0.791**	0.406	0.319	0.678
PT-rate	(1.471)	(2.623)	(0.790)	(1.060)	(1.166)	(0.516)	(0.391)	(0.281)	(0.291)	(0.508)
Between	5.143***	1.179	5.326***	6.333***	2.776***	0.662	1.060	0.0739	0.221	0.495
PT & FT-rate	(1.146)	(2.221)	(0.680)	(0.750)	(0.958)	(0.521)	(0.783)	(0.220)	(0.239)	(0.388)
Close to	5.931***	2.582	12.56***	16.50***	6.466***	0.812*	-0.169	1.158***	1.921***	1.160***
FT-rate	(0.992)	(2.072)	(0.623)	(0.847)	(0.889)	(0.426)	(0.445)	(0.253)	(0.344)	(0.405)
Close to	-0.947	-1.727	5.455***	12.07***	1.724***	4.335***	1.724***	5.368***	4.389***	5.129***
80% pre-earns	(0.736)	(1.693)	(0.340)	(0.538)	(0.548)	(0.379)	(0.315)	(0.221)	(0.245)	(0.364)
Above	-4.007***	-5.722***	1.508***	5.140***	-0.345	3.900***	2.544***	6.840***	4.924***	4.897***
2*FT-rate	(0.727)	(1.663)	(0.325)	(0.636)	(0.528)	(0.382)	(0.297)	(0.425)	(0.399)	(0.407)
Observations	1,537,851	1,170,972	1,345,866	777,204	1,711,845	1,537,851	1,170,972	1,345,866	777,204	1,711,845
R-squared	0.026	0.039	0.029	0.046	0.032	0.005	0.008	0.007	0.008	0.002

Notes: Coefficients have been multiplied by 100 to represent percentage point effects. All regressions are estimated using a 25% random sample of workers. Standard errors in parentheses, clustered at the job-level. Pre-WSS earnings are estimated as the average weekly earnings in February and March 2020. The dependent variable for first four columns is an indicator for whether measured weekly earnings are within $10 of either the part-time or full-time WSS-rate; and for final four columns is an indicator for whether measured relative weekly earnings (Weekly earnings/Pre-WSS weekly earnings) are ("at") within +/-1 percentage point of 80%.

*** p<0.01, ** p<0.05, * p<0.1

Table 31: Effects of March 2020 WSS on Job retention

	Simple	IP-weighted	12-month	Covariates	12-month
	(1)	(2)	(3)	(4)	(5)
		(A) 1-month job retention (Actual=98.18)			
March 2020	15.35***	27.02***	11.04***	31.34***	15.61***
WSS	(0.072)	(0.134)	(0.093)	(0.120)	(0.089)
R-squared	0.069	0.062	0.024	0.286	0.149
		(B) 3-month job retention (Actual=94.84)			
March 2020	15.57***	26.23***	10.83***	29.96***	14.62***
WSS	(0.083)	(0.140)	(0.105)	(0.128)	(0.104)
R-squared	0.054	0.054	0.018	0.250	0.107
		(C) 6-month job retention (Actual=83.58)			
March 2020	8.85***	19.73***	5.36***	23.03***	9.34***
WSS	(0.103)	(0.151)	(0.126)	(0.137)	(0.123)
R-squared	0.012	0.027	0.003	0.227	0.114
		(D) 9-month job retention (Actual=76.88)			
March 2020	5.01***	15.67***	1.87***	18.74***	5.65***
WSS	(0.111)	(0.155)	(0.135)	(0.143)	(0.132)
R-squared	0.003	0.016	0.000	0.204	0.107
		(E) 12-month job retention (Actual=71.05)			
March 2020	2.82***	13.19***	0.13	16.29***	4.38***
WSS	(0.117)	(0.159)	(0.143)	(0.147)	(0.140)
R-squared	0.001	0.011	0.000	0.193	0.105
Observations	614,979	611,655	578,826	608,247	575,748

Notes: Coefficients have been multiplied by 100 to represent percentage point effects. All regressions are estimated using a 25% random sample of workers. Standard errors in parentheses. All samples are based on jobs that existed in the baseline month (March 2020). The column (1) specification is the simple unweighted regression; columns (2)–(5) are weighted using the inverse propensity score (IP): column (2) is the weighted simple regression; column (4) includes covariate controls; and columns (3) and (5) repeat the specifications in columns (2) and (4) on the sample restricted to jobs that are observed sometime over the following 12 months. See text for more details.

*** p<0.01, ** p<0.05, * p<0.1

Table 32: Effects on Job retention – Extension, Resurgence, March 2021 waves

	WSS-Extension		WSS-Resurgence		WSS-March 2021	
	Covariates	12-month	Covariates	12-month	Covariates	12-month
WSS-receipt	(1)	(2)	(3)	(4)	(5)	(6)
			(A) 3-months			
March 2020	7.437***	2.930***	8.660***	7.885***	13.76***	9.473***
	(0.149)	(0.146)	(0.135)	(0.125)	(0.152)	(0.138)
Extension	18.40***	14.99***	7.099***	-0.037	-1.321***	-0.853***
	(0.101)	(0.095)	(0.116)	(0.107)	(0.157)	(0.141)
Resurgence			3.865***	0.203**	1.191***	1.211***
			(0.108)	(0.097)	(0.138)	(0.125)
March 2021					13.24***	5.287***
					(0.116)	(0.104)
R-squared	0.129	0.101	0.095	0.058	0.119	0.064
Actual	95.07		84.48		87.03	
			(B) 6-months			
March 2020	8.381***	4.645***	15.47***	15.86***	15.48***	12.20***
	(0.175)	(0.178)	(0.154)	(0.155)	(0.169)	(0.165)
Extension	11.01***	7.980***	3.395***	-2.797***	-0.193	0.333**
	(0.119)	(0.116)	(0.133)	(0.132)	(0.175)	(0.169)
Resurgence			1.702***	-1.401***	1.206***	1.361***
			(0.124)	(0.120)	(0.154)	(0.150)
March 2021					10.08***	3.557***
					(0.129)	(0.125)
R-squared	0.104	0.084	0.116	0.094	0.126	0.086
Actual	81.57		72.02		74.74	
			(C) 12-months			
March 2020	8.864***	6.090***	13.50***	13.98***	16.82***	14.59***
	(0.193)	(0.200)	(0.164)	(0.170)	(0.178)	(0.181)
Extension	6.581***	4.136***	0.921***	-4.236***	0.173	0.655***
	(0.131)	(0.131)	(0.141)	(0.144)	(0.184)	(0.185)
Resurgence			1.343***	-1.144***	-0.201	-0.024
			(0.132)	(0.131)	(0.162)	(0.163)
March 2021					6.129***	1.173***
					(0.136)	(0.136)
R-squared	0.110	0.098	0.124	0.111	0.131	0.103
Actual	66.72		59.42		59.59	
Observations	577,962	561,225	585,984	562,704	596,013	569,061

Notes: Coefficients have been multiplied by 100 to represent percentage point effects. All regressions are estimated using a 25% random sample of workers. Standard errors in parentheses. All samples are based on jobs that existed in the baseline month (May 2020, July 2020, or February 2021). All specifications include covariate controls (including indicators for receipt of wage subsidy in previous WSS waves), and are weighted using the inverse propensity score (IP): columns (1), (3) and (5) are based on all observations; and columns (2), (4) and (6) are based on samples restricted to jobs that are observed sometime over the following 12 months. See text for more details.

*** p<0.01, ** p<0.05, * p<0.1

Table 33: Effects of March 2020 WSS on workers employment

WSS-receipt	Simple (1)	IP-weighted (2)	12-month (3)	Covariates (4)	12-month (5)
		(A) 1-month (April 2020) (Actual=98.19)			
March 2020	6.853***	15.18***	12.61***	20.19***	17.89***
	(0.057)	(0.103)	(0.095)	(0.096)	(0.090)
R-squared	0.025	0.038	0.031	0.241	0.207
		(B) 3-months (June 2020) (Actual=96.44)			
March 2020	6.735***	13.75***	11.19***	17.73***	15.38***
	(0.066)	(0.105)	(0.098)	(0.101)	(0.096)
R-squared	0.018	0.030	0.023	0.190	0.144
		(C) 6-months (September 2020) (Actual=91.24)			
March 2020	2.900***	9.056***	6.502***	11.86***	9.485***
	(0.081)	(0.111)	(0.104)	(0.108)	(0.104)
R-squared	0.002	0.012	0.007	0.156	0.112
		(D) 9-months (December 2020) (Actual=89.99)			
March 2020	1.556***	6.607***	4.010***	8.585***	6.097***
	(0.083)	(0.110)	(0.103)	(0.108)	(0.103)
R-squared	0.001	0.006	0.003	0.132	0.088
		(E) 12-months (March 2021) (Actual=91.31)			
March 2020	1.354***	6.022***	3.458***	7.777***	5.330***
	(0.089)	(0.114)	(0.107)	(0.112)	(0.108)
R-squared	0.000	0.005	0.002	0.125	0.084
Observations	557,856	557,850	553,431	557,772	553,368

Notes: Coefficients have been multiplied by 100 to represent percentage point effects. All regressions are estimated using a 25% random sample of workers. Standard errors in parentheses. All samples are based on workers employed in the baseline month (March 2020). The column (1) specification is the simple unweighted regression; columns (2)–(5) are weighted using the inverse propensity score (IP): column (2) is the weighted simple regression; column (4) includes covariate controls; and columns (3) and (5) repeat the specifications in columns (2) and (4) on the sample restricted to workers that are observed in EMS sometime over the following 12 months. See text for more details.
*** p<0.01, ** p<0.05, * p<0.1

Table 34: Effects of WSS on workers employment – Extension, Resurgence, March 2021 waves

	WSS-Extension		WSS-Resurgence		WSS-March'21	
	Covariates	12-month	Covariates	12-month	Covariates	12-month
WSS-receipt	(1)	(2)	(3)	(4)	(5)	(6)
			(A) 3-months			
March 2020	3.345***	2.734***	3.969***	3.668***	7.160***	6.408***
	(0.147)	(0.145)	(0.125)	(0.121)	(0.111)	(0.107)
Extension	9.820***	9.424***	3.276***	2.165***	0.211**	0.434***
	(0.081)	(0.079)	(0.091)	(0.088)	(0.102)	(0.098)
Resurgence			1.138***	0.655***	0.969***	1.090***
			(0.083)	(0.080)	(0.094)	(0.090)
March 2021					4.746***	3.792***
					(0.0822)	(0.0790)
R-squared	0.073	0.068	0.038	0.029	0.052	0.045
Actual	96.46		91.72		94.17	
			(B) 6-months			
March 2020	3.986***	3.385***	7.319***	7.099***	7.358***	6.621***
	(0.163)	(0.162)	(0.147)	(0.145)	(0.123)	(0.120)
Extension	3.730***	3.342***	0.837***	-0.239**	0.109	0.330***
	(0.090)	(0.089)	(0.107)	(0.106)	(0.113)	(0.110)
Resurgence			-0.346***	-0.814***	1.432***	1.559***
			(0.097)	(0.095)	(0.104)	(0.101)
March 2021					2.990***	2.049***
					(0.091)	(0.089)
R-squared	0.049	0.044	0.038	0.032	0.042	0.038
Actual	90.50		86.42		90.87	
			(C) 12-months			
March 2020	3.986***	3.402***	6.579***	6.349***	7.347***	6.621***
	(0.180)	(0.179)	(0.154)	(0.152)	(0.138)	(0.136)
Extension	1.646***	1.270***	-0.190*	-1.252***	-0.0210	0.202
	(0.099)	(0.098)	(0.112)	(0.111)	(0.127)	(0.125)
Resurgence			-0.0589	-0.520***	1.085***	1.197***
			(0.102)	(0.100)	(0.117)	(0.115)
March 2021					0.610***	-0.316***
					(0.102)	(0.100)
R-squared	0.042	0.038	0.034	0.029	0.024	0.021
Actual	86.33		84.95		85.94	
Observations	541,440	539,544	541,494	538,851	550,302	548,112

Notes: Coefficients have been multiplied by 100 to represent percentage point effects. All regressions are estimated using a 25% random sample of workers. Standard errors in parentheses. All samples are based on workers employed in the baseline month (May or July 2020, or February 2021). All specifications include covariate controls (including indicators for receipt of wage subsidy in previous WSS waves), and are weighted using the inverse propensity score (IP): columns (1), (3) and (5) are based on all observations; and columns (2), (4) and (6) are based on samples restricted to workers that are observed in EMS sometime over the following 12 months. See text for more details.

*** p<0.01, ** p<0.05, * p<0.1

Table 35: WSS employment effects, by Sex

	Males		Females	
	Full sample	In EMS	Full sample	In EMS
		(A) March 2020		
6 months	12.33***	9.848***	11.28***	9.034***
	(0.145)	(0.139)	(0.162)	(0.157)
R-squared	0.150	0.110	0.163	0.116
12 months	8.632***	6.092***	6.679***	4.337***
	(0.148)	(0.142)	(0.170)	(0.165)
R-squared	0.129	0.093	0.122	0.078
No. Observations	285,954	283,776	271,524	269,325
		(B) Extension		
6 months	2.519***	2.187***	5.313***	4.859***
	(0.118)	(0.117)	(0.137)	(0.136)
R-squared	0.045	0.041	0.054	0.048
12 months	0.631***	0.307**	2.980***	2.542***
	(0.131)	(0.130)	(0.151)	(0.150)
R-squared	0.041	0.038	0.043	0.038
No. Observations	278,826	277,833	262,347	261,450
		(C) Resurgence		
6 months	-0.654***	-1.071***	0.0388	-0.482***
	(0.131)	(0.128)	(0.145)	(0.142)
R-squared	0.040	0.036	0.035	0.028
12 months	-0.407***	-0.818***	0.309**	-0.205
	(0.139)	(0.136)	(0.149)	(0.147)
R-squared	0.038	0.034	0.032	0.025
No. Observations	277,638	276,273	263,589	262,317
		(D) March 2021		
6 months	2.140***	1.234***	3.766***	2.816***
	(0.124)	(0.120)	(0.134)	(0.131)
R-squared	0.047	0.044	0.039	0.035
12 months	-0.264*	-1.156***	1.488***	0.562***
	(0.139)	(0.136)	(0.150)	(0.148)
R-squared	0.029	0.026	0.022	0.019
Observations	282,390	281,256	267,630	266,580

Notes: Coefficients have been multiplied by 100 to represent percentage point effects. All regressions are estimated using a 25% random sample of workers. Standard errors in parentheses. All samples are based on workers employed in the baseline month (March, May or July 2020, or February 2021). All specifications include covariate controls (including indicators for receipt of wage subsidy in previous WSS waves), and are weighted using the inverse propensity score (IP): the "full sample" estimates are based on all observations; and the "In EMS" are based on samples restricted to workers that are observed in EMS sometime over the following 12 months. See text for more details.

*** p<0.01, ** p<0.05, * p<0.1

Table 36: WSS employment effects, by age

	Aged:			
	< 25	25-39	40-54	55+
		(A) March 2020		
6 months	20.37***	8.754***	5.245***	8.048***
	(0.349)	(0.176)	(0.154)	(0.223)
R-squared	0.121	0.096	0.055	0.087
12 months	8.921***	5.327***	3.233***	5.872***
	(0.337)	(0.184)	(0.162)	(0.246)
R-squared	0.078	0.094	0.048	0.073
No. Observations	76,980	191,400	161,550	123,441
		(B) Extension		
6 months	5.838***	4.545***	0.476***	3.183***
	(0.273)	(0.155)	(0.138)	(0.191)
R-squared	0.056	0.066	0.017	0.040
12 months	3.340***	1.537***	-0.911***	2.185***
	(0.302)	(0.169)	(0.155)	(0.217)
R-squared	0.042	0.051	0.015	0.042
No. Observations	70,548	187,737	160,074	121,188
		(C) Resurgence		
6 months	0.387	-0.765***	-1.744***	-0.323
	(0.287)	(0.165)	(0.157)	(0.202)
R-squared	0.037	0.038	0.021	0.040
12 months	0.381	-0.906***	-1.372***	0.955***
	(0.299)	(0.174)	(0.161)	(0.215)
R-squared	0.028	0.035	0.014	0.039
No. Observations	71,046	186,804	159,474	121,527
		(D) March 2021		
6 months	7.571***	0.631***	0.279**	0.269
	(0.294)	(0.148)	(0.134)	(0.170)
R-squared	0.050	0.018	0.017	0.029
12 months	0.695**	-0.787***	-0.643***	0.00600
	(0.294)	(0.172)	(0.165)	(0.212)
R-squared	0.021	0.022	0.017	0.033
Observations	77,667	188,292	160,107	122,046

Notes: Coefficients have been multiplied by 100 to represent percentage point effects. All regressions are estimated using a 25% random sample of workers. Standard errors in parentheses. All samples are based on workers employed in the baseline month (March, May or July 2020, or February 2021). All specifications include covariate controls (including indicators for receipt of wage subsidy in previous WSS waves), are weighted using the inverse propensity score (IP), and are based on samples restricted to workers that are observed in EMS sometime over the following 12 months. See text for more details.
*** p<0.01, ** p<0.05, * p<0.1

Table 37: WSS employment effects, by ethnicity

	Ethnicity:			
	European	Māori	Pacific peoples	Asian
		(A) March 2020		
6 months	8.575***	16.14***	9.738***	8.038***
	(0.130)	(0.553)	(0.523)	(0.238)
R-squared	0.090	0.129	0.113	0.072
12 months	4.920***	9.850***	6.783***	3.669***
	(0.137)	(0.557)	(0.533)	(0.247)
R-squared	0.058	0.058	0.110	0.039
No. Observations	314,214	31,626	29,544	92,820
		(B) Extension		
6 months	3.330***	4.308***	3.861***	2.130***
	(0.111)	(0.434)	(0.420)	(0.215)
R-squared	0.030	0.036	0.051	0.024
12 months	1.723***	1.455***	1.873***	-0.288
	(0.126)	(0.471)	(0.459)	(0.233)
R-squared	0.029	0.027	0.045	0.018
No. Observations	306,678	30,240	28,914	92,016
		(C) Resurgence		
6 months	-0.249**	-0.893*	0.0496	-2.013***
	(0.122)	(0.465)	(0.446)	(0.226)
R-squared	0.028	0.056	0.037	0.019
12 months	-0.0532	-1.353***	1.609***	-1.652***
	(0.128)	(0.485)	(0.467)	(0.241)
R-squared	0.022	0.035	0.029	0.017
No. Observations	307,419	29,580	28,173	92,493
		(D) March 2021		
6 months	3.085***	3.341***	0.386	0.810***
	(0.118)	(0.457)	(0.431)	(0.198)
R-squared	0.044	0.064	0.071	0.026
12 months	-0.630***	2.164***	-0.357	-0.385*
	(0.133)	(0.524)	(0.473)	(0.228)
R-squared	0.019	0.045	0.042	0.017
Observations	310,458	30,819	28,746	94,137

Notes: Coefficients have been multiplied by 100 to represent percentage point effects. All regressions are estimated using a 25% random sample of workers. Standard errors in parentheses. All samples are based on workers employed in the baseline month (March, May or July 2020, or February 2021). All specifications include covariate controls (including indicators for receipt of wage subsidy in previous WSS waves), are weighted using the inverse propensity score (IP), and are based on samples restricted to workers that are observed in EMS sometime over the following 12 months. See text for more details.
*** p<0.01, ** p<0.05, * p<0.1

Table 38: WSS employment effects, by region

	Region:			
	Auckland	Wellington	Rest of North Island	South Island
		(A) March 2020		
6 months	9.294***	9.940***	10.37***	9.087***
	(0.175)	(0.299)	(0.198)	(0.208)
R-squared	0.110	0.125	0.105	0.113
12 months	4.792***	4.302***	6.260***	5.682***
	(0.181)	(0.318)	(0.206)	(0.215)
R-squared	0.081	0.063	0.070	0.100
No. Observations	189,303	62,070	165,405	135,108
		(B) Extension		
6 months	3.032***	2.252***	2.414***	5.225***
	(0.153)	(0.249)	(0.159)	(0.183)
R-squared	0.040	0.034	0.033	0.062
12 months	0.736***	0.893***	0.921***	2.721***
	(0.168)	(0.287)	(0.180)	(0.199)
R-squared	0.032	0.031	0.031	0.055
No. Observations	185,238	60,732	160,620	131,103
		(C) Resurgence		
6 months	-0.987***	0.628**	-0.773***	-0.474**
	(0.161)	(0.298)	(0.178)	(0.201)
R-squared	0.031	0.037	0.032	0.033
12 months	-0.198	0.254	-1.157***	-0.770***
	(0.168)	(0.305)	(0.190)	(0.210)
R-squared	0.026	0.028	0.029	0.033
No. Observations	184,989	61,080	160,236	130,113
		(D) March 2021		
6 months	1.912***	0.596**	2.407***	2.668***
	(0.146)	(0.289)	(0.180)	(0.188)
R-squared	0.034	0.060	0.049	0.048
12 months	-0.305*	-1.532***	0.275	-0.0634
	(0.167)	(0.328)	(0.202)	(0.208)
R-squared	0.020	0.030	0.030	0.021
Observations	185,562	62,004	163,926	132,792

Notes: Coefficients have been multiplied by 100 to represent percentage point effects. All regressions are estimated using a 25% random sample of workers. Standard errors in parentheses. All samples are based on workers employed in the baseline month (March, May or July 2020, or February 2021). All specifications include covariate controls (including indicators for receipt of wage subsidy in previous WSS waves), are weighted using the inverse propensity score (IP), and are based on samples restricted to workers that are observed in EMS sometime over the following 12 months. See text for more details.
*** p<0.01, ** p<0.05, * p<0.1

Table 39: WSS employment effects, by Industry

	Industry:				
	Manufacturing	Construction	Retail	Accommodation & Food	Health
			(A) March 2020		
6 months	5.297***	20.73***	8.911***	28.37***	-0.281
	(0.317)	(0.356)	(0.330)	(0.473)	(0.349)
R-squared	0.082	0.112	0.039	0.136	0.018
12 months	3.388***	14.55***	5.575***	14.51***	-1.040**
	(0.316)	(0.363)	(0.345)	(0.466)	(0.421)
R-squared	0.082	0.073	0.037	0.085	0.027
No. Obs	56,700	44,703	51,900	38,139	58,221
			(B) Extension		
6 months	1.761***	1.511***	-0.315	10.39***	0.662***
	(0.232)	(0.271)	(0.277)	(0.409)	(0.242)
R-squared	0.021	0.015	0.015	0.067	0.015
12 months	0.494*	0.0361	-1.163***	5.634***	-0.846***
	(0.270)	(0.314)	(0.320)	(0.432)	(0.294)
R-squared	0.026	0.012	0.019	0.056	0.018
No. Obs	55,194	44,403	50,889	35,178	57,792
			(C) Resurgence		
6 months	-1.134***	-2.836***	0.597**	0.573	1.913***
	(0.256)	(0.302)	(0.282)	(0.400)	(0.275)
R-squared	0.024	0.028	0.020	0.032	0.013
12 months	-1.674***	-1.541***	0.333	1.361***	0.0183
	(0.280)	(0.328)	(0.309)	(0.423)	(0.305)
R-squared	0.034	0.021	0.020	0.036	0.017
No. Obs	54,306	44,778	50,748	35,472	58,653
			(D) March 2021		
6 months	2.931***	1.166***	0.417	3.881***	2.497***
	(0.250)	(0.296)	(0.265)	(0.372)	(0.251)
R-squared	0.053	0.043	0.024	0.048	0.023
12 months	1.786***	-0.454	-1.175***	1.708***	-0.224
	(0.284)	(0.343)	(0.310)	(0.411)	(0.305)
R-squared	0.023	0.037	0.014	0.028	0.013
No. Obs	56,181	46,494	51,828	35,820	60,243

Notes: Coefficients have been multiplied by 100 to represent percentage point effects. All regressions are estimated using a 25% random sample of workers. Standard errors in parentheses. All samples are based on workers employed in the baseline month (March, May or July 2020, or February 2021). All specifications include covariate controls (including indicators for receipt of wage subsidy in previous WSS waves), are weighted using the inverse propensity score (IP), and are based on samples restricted to workers that are observed in EMS sometime over the following 12 months. See text for more details.
*** p<0.01, ** p<0.05, * p<0.1

Table 40: Effects of March 2020 WSS on workers earnings

	Log(earnings)			D.Log(earnings)		
	Simple	IP-wgts	Covariates	Simple	IP-wgts	Covariates
WSS-receipt	(1)	(2)	(3)	(4)	(5)	(6)
			(A) 1-month (April 2020)			
March 2020	-27.4***	-25.1***	-7.94***	-11.7***	-10.9***	-4.89***
	(0.225)	(0.276)	(0.254)	(0.137)	(0.173)	(0.185)
R-squared	0.027	0.015	0.290	0.013	0.007	0.037
Observations	530,970	530,970	530,910	530,970	530,970	530,910
			(B) 3-months (June 2020)			
March 2020	-13.9***	-8.59***	3.98***	0.897***	0.929***	0.999***
	(0.225)	(0.275)	(0.245)	(0.132)	(0.165)	(0.177)
R-squared	0.007	0.002	0.312	0.000	0.000	0.009
Observations	521,460	521,457	521,403	521,460	521,457	521,403
			(C) 6-months (September 2020)			
March 2020	-16.5***	-12.8***	-0.864***	-4.69***	-7.35***	-6.53***
	(0.237)	(0.287)	(0.254)	(0.150)	(0.191)	(0.202)
R-squared	0.010	0.004	0.314	0.002	0.003	0.021
Observations	501,894	501,888	501,840	501,894	501,888	501,840
			(D) 9-months (December 2020)			
March 2020	-14.5***	-6.48***	0.447*	-5.04***	-6.23***	-9.61***
	(0.225)	(0.271)	(0.245)	(0.174)	(0.224)	(0.230)
R-squared	0.008	0.001	0.276	0.002	0.002	0.068
Observations	498,195	498,189	498,144	498,195	498,189	498,144
			(E) 12-months (March 2021)			
March 2020	-11.9***	-8.25***	-0.139	-2.62***	-8.18***	-10.2***
	(0.234)	(0.285)	(0.256)	(0.170)	(0.221)	(0.229)
R-squared	0.005	0.002	0.288	0.000	0.003	0.048
Observations	489,375	489,372	489,327	489,375	489,372	489,327

Notes: Coefficients have been multiplied by 100 to represent percentage effects. All regressions are estimated using a 25% random sample of workers. Standard errors in parentheses. All samples are based on workers employed in both the baseline (March 2020) and follow-up month. The first three columns pertain to log(earnings) in the follow-up month: column (1) specification is the simple unweighted regression; columns (2) and (3) are weighted using the inverse propensity score (IP): column (2) is the weighted simple regression, and column (3) includes covariate controls. The final three columns pertain to the change in log(earnings) from the baseline (March 2020) and the follow-up month: columns (4)–(6) specifications are the same as columns (1)–(3). See text for more details.

*** p<0.01, ** p<0.05, * p<0.1

Table 41: Effects of WSS on workers earnings – Extension, Resurgence, March'21 waves

	WSS-Extension		WSS-Resurgence		WSS-March'21	
	log(earn)	D.log(earn)	log(earn)	D.log(earn)	log(earn)	D.log(earn)
	(1)	(2)	(3)	(4)	(5)	(6)
			(A) 3-months			
March 2020	15.9***	-14.3***	21.7***	-13.5***	25.3***	-3.33***
	(0.381)	(0.270)	(0.337)	(0.261)	(0.301)	(0.239)
Extension	-6.97***	3.71***	-1.92***	-1.24***	4.31***	-1.13***
	(0.202)	(0.143)	(0.241)	(0.186)	(0.269)	(0.214)
Resurgence			-6.82***	0.431***	0.577**	-0.731***
			(0.215)	(0.166)	(0.247)	(0.196)
March 2021					-5.76***	0.642***
					(0.217)	(0.172)
R-squared	0.300	0.011	0.282	0.012	0.289	0.008
Observations	506,760	506,760	509,316	509,316	517,695	517,695
			(B) 6-months			
March 2020	12.5***	-17.8***	15.4***	-18.5***	22.8***	-6.45***
	(0.378)	(0.299)	(0.339)	(0.298)	(0.302)	(0.265)
Extension	-5.29***	3.55***	-4.30***	-4.13***	1.58***	-3.69***
	(0.202)	(0.160)	(0.239)	(0.209)	(0.270)	(0.236)
Resurgence			-7.79***	-1.01***	0.0799	-1.04***
			(0.214)	(0.188)	(0.247)	(0.217)
March 2021					-8.43***	-2.62***
					(0.218)	(0.190)
R-squared	0.281	0.016	0.230	0.042	0.279	0.013
Observations	495,129	495,129	486,084	486,084	504,417	504,417
			(C) 12-months			
March 2020	11.3***	-19.8***	14.9***	-21.2***	18.4***	-9.70***
	(0.381)	(0.334)	(0.336)	(0.319)	(0.301)	(0.291)
Extension	-5.17***	3.26***	-2.50***	-2.77***	1.21***	-4.45***
	(0.203)	(0.179)	(0.237)	(0.225)	(0.269)	(0.261)
Resurgence			-5.70***	1.11***	-1.01***	-2.22***
			(0.213)	(0.202)	(0.246)	(0.239)
March 2021					-7.75***	-2.85***
					(0.217)	(0.210)
R-squared	0.248	0.033	0.233	0.048	0.224	0.048
Observations	478,416	478,416	479,802	479,802	489,264	489,264

Notes: Coefficients have been multiplied by 100 to represent percentage effects. All regressions are estimated using a 25% random sample of workers. Standard errors in parentheses. All samples are based on workers employed in both the baseline (March 2020) and follow-up month. All specifications are based on workers employed in the baseline month (May or July 2020, or February 2021), include covariate controls (including indicators for receipt of wage subsidy in previous WSS waves), and are weighted using the inverse propensity score (IP): columns (1), (3) and (5) pertain to log(earnings) in the follow-up month; and columns (2), (4) and (6) pertain to the change in log(earnings) from the baseline to the follow-up month. See text for more details.

*** p<0.01, ** p<0.05, * p<0.1

Table 42: WSS monthly earnings effects, by Sex

	Males		Females	
	Log(earnings)	Dlog(earnings)	Log(earnings)	Dlog(earnings)
		(E) March 2020		
6 months	3.91***	-6.59***	-6.54***	-6.49***
	(0.328)	(0.269)	(0.395)	(0.306)
R-squared	0.332	0.026	0.280	0.018
No. Observations	258,537	258,537	243,090	243,090
12 months	4.53***	-9.97***	-5.73***	-10.5***
	(0.329)	(0.300)	(0.398)	(0.352)
R-squared	0.297	0.056	0.253	0.042
No. Observations	253,209	253,209	235,920	235,920
		(F) Extension		
6 months	-6.04***	2.76***	-4.11***	4.80***
	(0.262)	(0.209)	(0.314)	(0.248)
R-squared	0.271	0.015	0.204	0.018
No. Observations	255,918	255,918	239,007	239,007
12 months	-5.45***	2.90***	-4.68***	3.89***
	(0.262)	(0.230)	(0.318)	(0.281)
R-squared	0.234	0.036	0.173	0.032
No. Observations	248,202	248,202	230,019	230,019
		(G) Resurgence		
6 months	-6.68***	0.917***	-8.90***	-3.23***
	(0.283)	(0.249)	(0.324)	(0.285)
R-squared	0.232	0.038	0.159	0.047
No. Observations	253,239	253,239	232,629	232,629
12 months	-6.16***	1.80***	-4.96***	0.431
	(0.281)	(0.264)	(0.324)	(0.311)
R-squared	0.225	0.051	0.163	0.047
No. Observations	247,914	247,914	231,684	231,684
		(H) March 2021		
6 months	-11.1***	-3.19***	-5.52***	-1.87***
	(0.288)	(0.247)	(0.327)	(0.292)
R-squared	0.276	0.016	0.224	0.013
No. Observations	259,947	259,947	244,239	244,239
12 months	-9.47***	-2.83***	-5.90***	-2.76***
	(0.283)	(0.269)	(0.328)	(0.324)
R-squared	0.233	0.050	0.157	0.048
Observations	253,434	253,434	235,611	235,611

Notes: Coefficients have been multiplied by 100 to represent percentage effects. All regressions are estimated using a 25% random sample of workers. Standard errors in parentheses. All samples are based on workers employed in the baseline month (March, May or July 2020, or February 2021). All specifications include covariate controls (including indicators for receipt of wage subsidy in previous WSS waves), and are weighted using the inverse propensity score (IP). See text for more details.
*** p<0.01, ** p<0.05, * p<0.1

Table 43: WSS monthly earnings effects, by age

Outcome:	Aged:			
Dlog(earnings)	< 25	25-39	40-54	55+
		(E) March 2020		
6 months	-15.9***	-6.01***	-3.73***	-3.11***
	(0.770)	(0.347)	(0.305)	(0.381)
R-squared	0.022	0.019	0.027	0.021
No. Observations	64,467	172,677	152,361	112,329
12 months	-31.1***	-9.22***	-3.36***	-1.75***
	(0.934)	(0.386)	(0.323)	(0.407)
R-squared	0.055	0.023	0.020	0.016
No. Observations	63,588	167,694	150,111	107,928
		(F) Extension		
6 months	2.29***	3.45***	3.91***	3.72***
	(0.600)	(0.262)	(0.253)	(0.312)
R-squared	0.032	0.017	0.007	0.011
No. Observations	61,950	170,142	151,548	111,495
12 months	0.982	3.98***	3.96***	2.81***
	(0.699)	(0.296)	(0.269)	(0.340)
R-squared	0.042	0.027	0.014	0.018
No. Observations	58,761	164,541	148,626	106,491
		(G) Resurgence		
6 months	-5.86***	-0.467	0.209	-0.057
	(0.711)	(0.310)	(0.281)	(0.344)
R-squared	0.066	0.029	0.015	0.017
No. Observations	61,572	167,982	148,233	108,297
12 months	-2.39***	0.650*	2.21***	2.68***
	(0.768)	(0.340)	(0.298)	(0.365)
R-squared	0.056	0.032	0.018	0.020
No. Observations	59,925	164,277	148,566	107,034
		(H) March 2021		
6 months	-5.62***	-2.40***	-1.69***	-1.22***
	(0.737)	(0.304)	(0.286)	(0.319)
R-squared	0.019	0.018	0.017	0.014
No. Observations	64,950	172,995	153,015	113,460
12 months	-5.10***	-3.38***	-1.47***	-1.88***
	(0.797)	(0.329)	(0.313)	(0.356)
R-squared	0.027	0.027	0.016	0.016
No. Observations	66,468	166,137	148,722	107,934

Notes: Dependent variable is Dlog(earnings). Coefficients have been multiplied by 100 to represent percentage effects. All regressions are estimated using a 25% random sample of workers. Standard errors in parentheses. All samples are based on workers employed in the baseline month (March, May or July 2020, or February 2021). All specifications include covariate controls (including indicators for receipt of wage subsidy in previous WSS waves), are weighted using the inverse propensity score (IP). See text for more details.

*** p<0.01, ** p<0.05, * p<0.1

Table 44: WSS monthly earnings effects, by ethnicity

Outcome:	Ethnicity:			
Dlog(earnings)	European	Māori	Pacific peoples	Asian
		(E) March 2020		
6 months	-5.66***	-3.09***	-5.72***	-8.05***
	(0.252)	(1.03)	(1.00)	(0.484)
R-squared	0.019	0.028	0.051	0.027
No. Observations	288,930	26,964	25,995	85,452
12 months	-8.95***	-9.99***	-9.29***	-11.1***
	(0.289)	(1.15)	(1.07)	(0.548)
R-squared	0.043	0.042	0.081	0.058
No. Observations	281,154	26,730	25,554	83,391
		(F) Extension		
6 months	4.23***	3.80***	2.91***	2.35***
	(0.204)	(0.756)	(0.785)	(0.379)
R-squared	0.016	0.021	0.010	0.017
No. Observations	285,126	26,541	25,683	84,666
12 months	3.99***	2.22***	3.82***	2.29***
	(0.232)	(0.850)	(0.827)	(0.415)
R-squared	0.031	0.034	0.019	0.033
No. Observations	274,947	25,815	24,984	82,161
		(G) Resurgence		
6 months	0.212	-5.01***	-3.29***	-1.59***
	(0.251)	(0.886)	(0.886)	(0.419)
R-squared	0.039	0.054	0.047	0.051
No. Observations	278,883	25,728	25,089	84,432
12 months	2.77***	-1.11	0.290	-1.24***
	(0.267)	(0.976)	(0.934)	(0.462)
R-squared	0.047	0.051	0.048	0.051
No. Observations	276,477	25,347	24,489	82,719
		(H) March 2021		
6 months	-1.40***	-0.371	-6.38***	-2.86***
	(0.257)	(0.910)	(0.937)	(0.423)
R-squared	0.010	0.029	0.040	0.014
No. Observations	287,829	27,132	25,980	87,516
12 months	-3.24***	0.700	-4.38***	-3.14***
	(0.286)	(0.976)	(0.998)	(0.460)
R-squared	0.057	0.041	0.072	0.042
No. Observations	279,216	26,238	25,239	84,918

Notes: Dependent variable is Dlog(earnings). Coefficients have been multiplied by 100 to represent percentage effects. All regressions are estimated using a 25% random sample of workers. Standard errors in parentheses. All samples are based on workers employed in the baseline month (March, May or July 2020, or February 2021). All specifications include covariate controls (including indicators for receipt of wage subsidy in previous WSS waves), are weighted using the inverse propensity score (IP). See text for more details.

*** p<0.01, ** p<0.05, * p<0.1

Table 45: WSS monthly earnings effects, by region

		Region:		
Outcome: Dlog(earnings)	Auckland	Wellington	Rest of North Island	South Island
		(E) March 2020		
6 months	-6.03***	-5.70***	-4.92***	-8.92***
	(0.332)	(0.585)	(0.388)	(0.411)
R-squared	0.018	0.026	0.025	0.024
No. Observations	171,849	57,402	149,202	122,388
12 months	-8.60***	-10.6***	-9.69***	-13.2***
	(0.376)	(0.681)	(0.439)	(0.466)
R-squared	0.042	0.058	0.049	0.051
No. Observations	167,289	55,800	145,722	119,697
		(F) Extension		
6 months	3.68***	1.80***	4.09***	3.87***
	(0.269)	(0.487)	(0.295)	(0.329)
R-squared	0.012	0.018	0.019	0.020
No. Observations	169,560	56,742	147,057	120,447
12 months	3.07***	2.36***	3.10***	4.48***
	(0.300)	(0.536)	(0.333)	(0.366)
R-squared	0.030	0.035	0.037	0.037
No. Observations	163,998	54,702	142,047	116,472
		(G) Resurgence		
6 months	-1.74***	-0.839	-0.636*	1.33***
	(0.308)	(0.587)	(0.367)	(0.412)
R-squared	0.039	0.043	0.051	0.045
No. Observations	166,602	55,824	144,090	117,747
12 months	0.444	0.283	0.750*	3.53***
	(0.334)	(0.662)	(0.390)	(0.435)
R-squared	0.048	0.050	0.046	0.046
No. Observations	164,814	55,026	142,104	116,166
		(H) March 2021		
6 months	-2.87***	-5.44***	-3.56***	0.168
	(0.315)	(0.640)	(0.385)	(0.402)
R-squared	0.015	0.018	0.010	0.014
No. Observations	171,852	57,639	149,718	121,944
12 months	-3.04***	-7.68***	-1.16***	-1.67***
	(0.347)	(0.740)	(0.411)	(0.446)
R-squared	0.047	0.062	0.039	0.056
No. Observations	166,203	55,707	145,080	119,385

Notes: Dependent variable is Dlog(earnings). Coefficients have been multiplied by 100 to represent percentage effects. All regressions are estimated using a 25% random sample of workers. Standard errors in parentheses. All samples are based on workers employed in the baseline month (March, May or July 2020, or February 2021). All specifications include covariate controls (including indicators for receipt of wage subsidy in previous WSS waves), are weighted using the inverse propensity score (IP). See text for more details.

*** p<0.01, ** p<0.05, * p<0.1

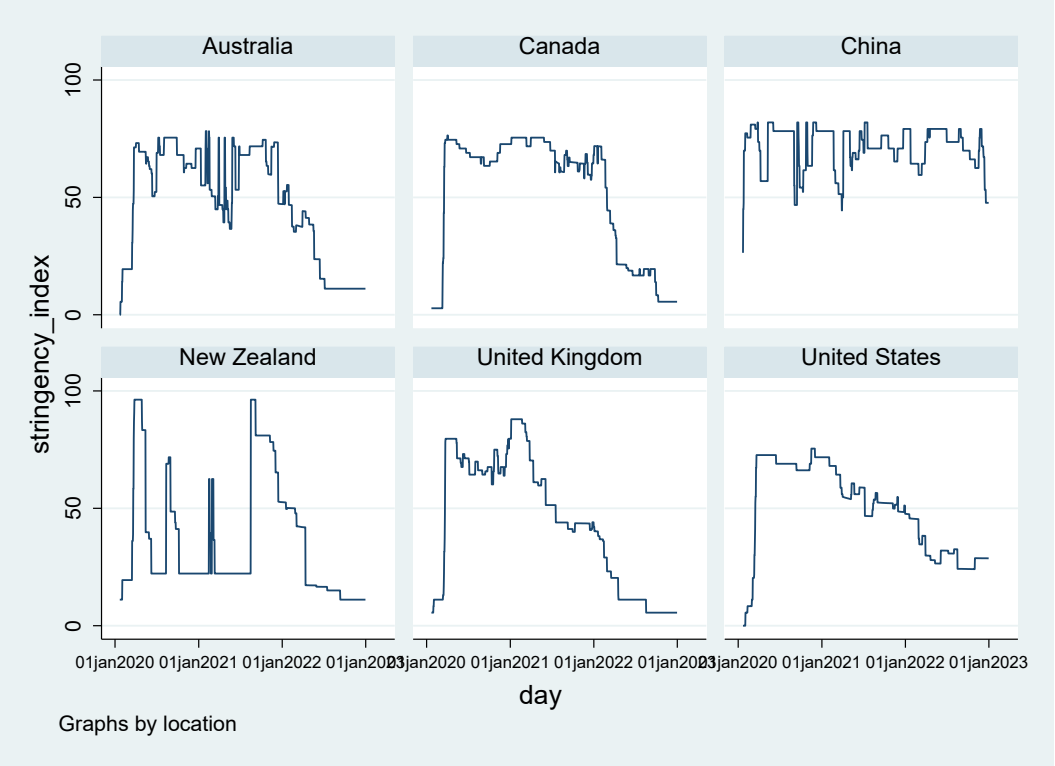
Table 46: WSS monthly earnings effects, by Industry

Outcome: Dlog(earnings)	Industry: Manufacturing	Construction	Retail	Accommod'n & Food	Health
		(E) March 2020			
6 months	3.25***	-9.52***	-3.32***	-28.8***	-10.8***
	(0.575)	(0.671)	(0.615)	(1.07)	(0.730)
R-squared	0.016	0.024	0.010	0.041	0.013
No. Obs	51,687	41,109	47,211	31,968	54,570
12 months	3.05***	-23.6***	-6.74***	-42.6***	-9.58***
	(0.589)	(0.753)	(0.741)	(1.31)	(0.833)
R-squared	0.019	0.074	0.038	0.083	0.013
No. Obs	51,138	39,825	45,519	30,741	52,794
		(F) Extension			
6 months	6.32***	2.91***	2.51***	6.84***	3.12***
	(0.425)	(0.438)	(0.537)	(0.740)	(0.498)
R-squared	0.016	0.020	0.012	0.032	0.007
No. Obs	51,237	40,872	46,734	30,147	54,441
12 months	5.08***	3.13***	4.37***	2.52***	6.55***
	(0.469)	(0.487)	(0.630)	(0.837)	(0.567)
R-squared	0.022	0.037	0.020	0.046	0.024
No. Obs	49,866	39,429	44,616	28,620	52,443
		(G) Resurgence			
6 months	1.93***	-0.294	-0.457	-4.79***	-3.64***
	(0.479)	(0.520)	(0.556)	(0.803)	(0.557)
R-squared	0.022	0.016	0.037	0.064	0.022
No. Obs	50,637	40,962	46,491	30,270	54,711
12 months	0.809	0.756	4.19***	-3.47***	0.269
	(0.531)	(0.513)	(0.645)	(0.884)	(0.626)
R-squared	0.017	0.034	0.029	0.067	0.028
No. Obs	49,143	39,915	44,856	29,154	53,133
		(H) March 2021			
6 months	-1.54***	-0.594	-5.10***	-6.96***	2.28***
	(0.463)	(0.526)	(0.616)	(0.837)	(0.609)
R-squared	0.007	0.017	0.009	0.016	0.014
No. Obs	51,783	43,026	47,406	31,362	56,439
12 months	-2.31***	-0.066	-5.14***	-5.26***	0.569
	(0.488)	(0.536)	(0.633)	(0.955)	(0.653)
R-squared	0.037	0.025	0.054	0.049	0.038
No. Obs	51,126	41,721	46,146	30,156	53,718

Notes: Dependent variable is Dlog(earnings). Coefficients have been multiplied by 100 to represent percentage effects. All regressions are estimated using a 25% random sample of workers. Standard errors in parentheses. All samples are based on workers employed in the baseline month (March, May or July 2020, or February 2021). All specifications include covariate controls (including indicators for receipt of wage subsidy in previous WSS waves), are weighted using the inverse propensity score (IP). See text for more details.

*** p<0.01, ** p<0.05, * p<0.1

Figure 1: Stringency of COVID-19 response for selected countries



Source: https://www.bsg.ox.ac.uk/research/research-projects/oxford-covid-19-government-response-tracker

Figure 2: Propensity score distribution: March 2020 Wave

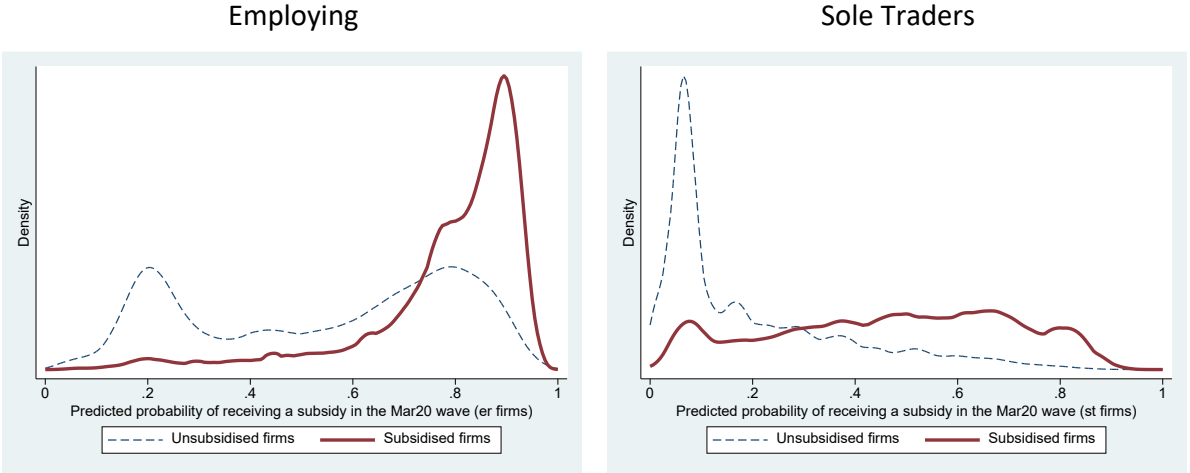
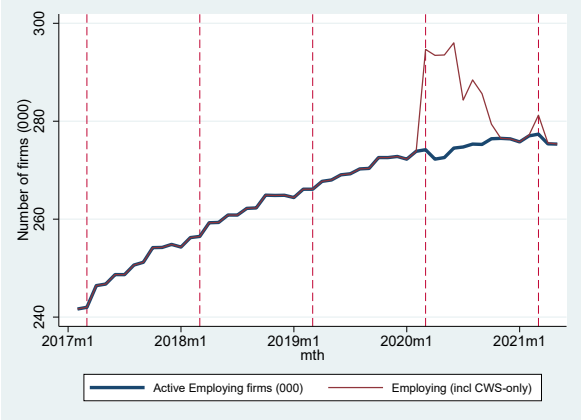
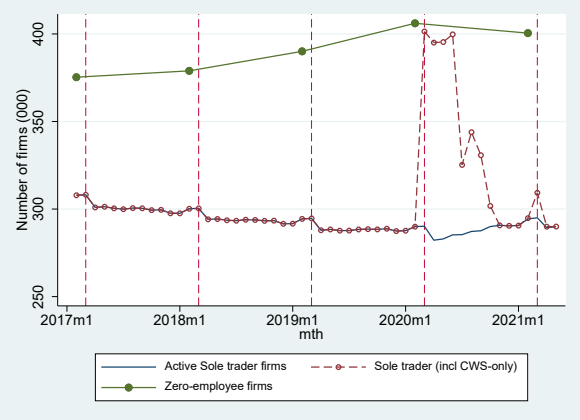


Figure 3: Firm counts, employment and sales

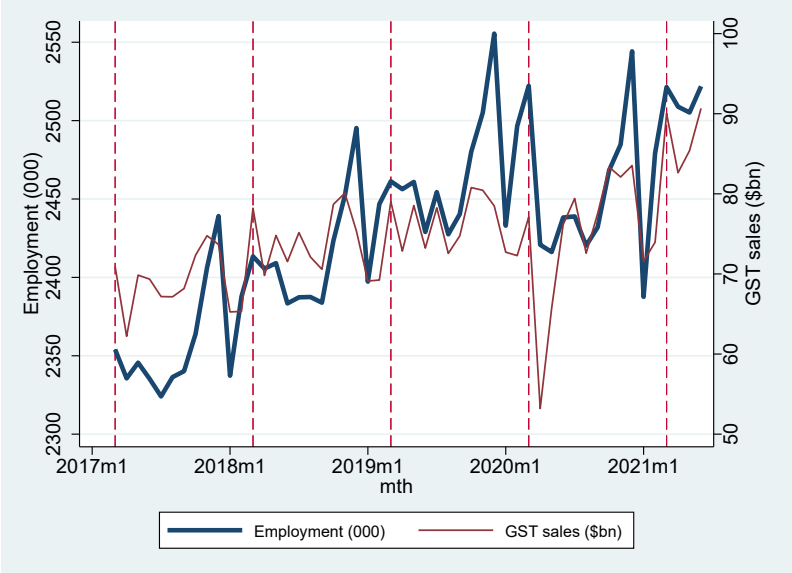
a) Firm count: Employing firms



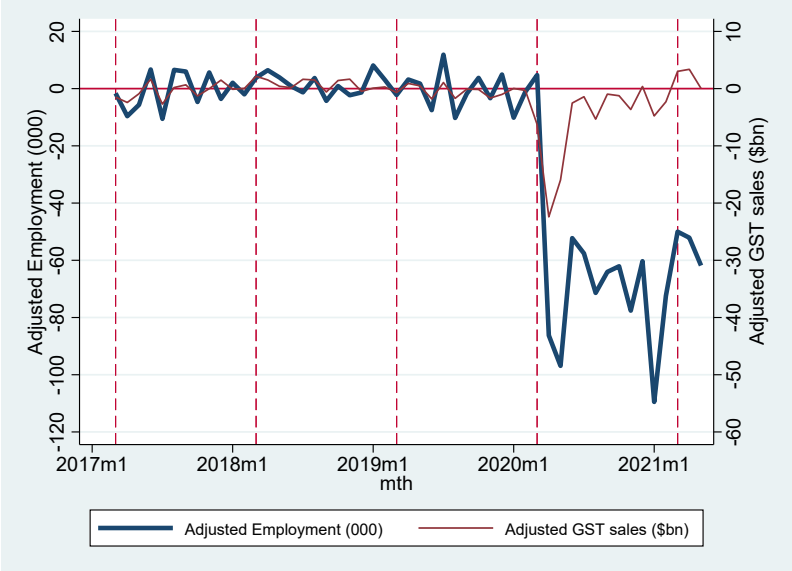
b) Firm count: Sole-trader firms



c) Employment and GST sales



d) Employment and GST sales (adjusted for seasonality and trend)

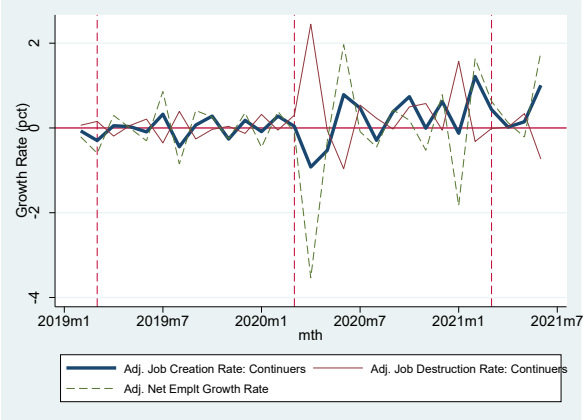


Note: In the final panel, adjustment for seasonality and trend is done by regressing levels of employment and sales on calendar month dummies and a linear time trend over the period 2017m2 – 2020m2 and retrieving the residual.

Figure 4: Contributions to employment and sales growth

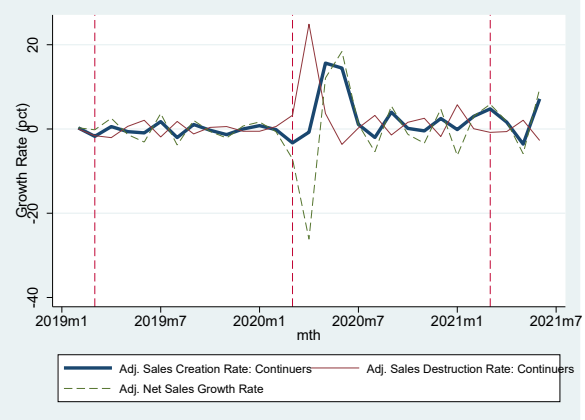
Employment growth

Job creation and destruction

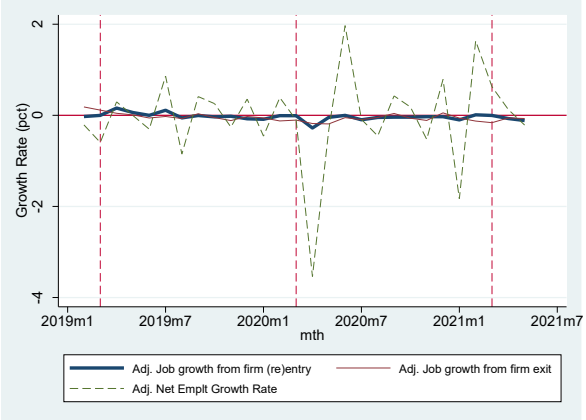


Sales growth

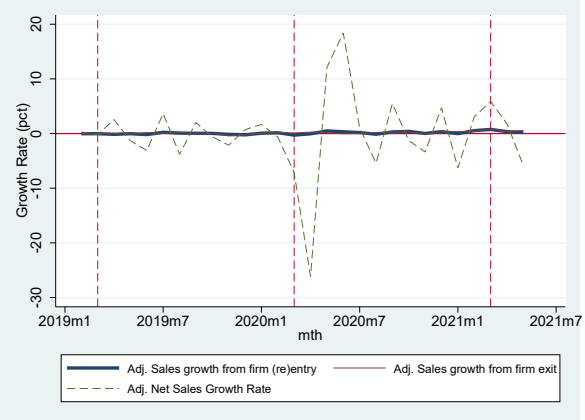
Sales creation and destruction



Firm entry and exit

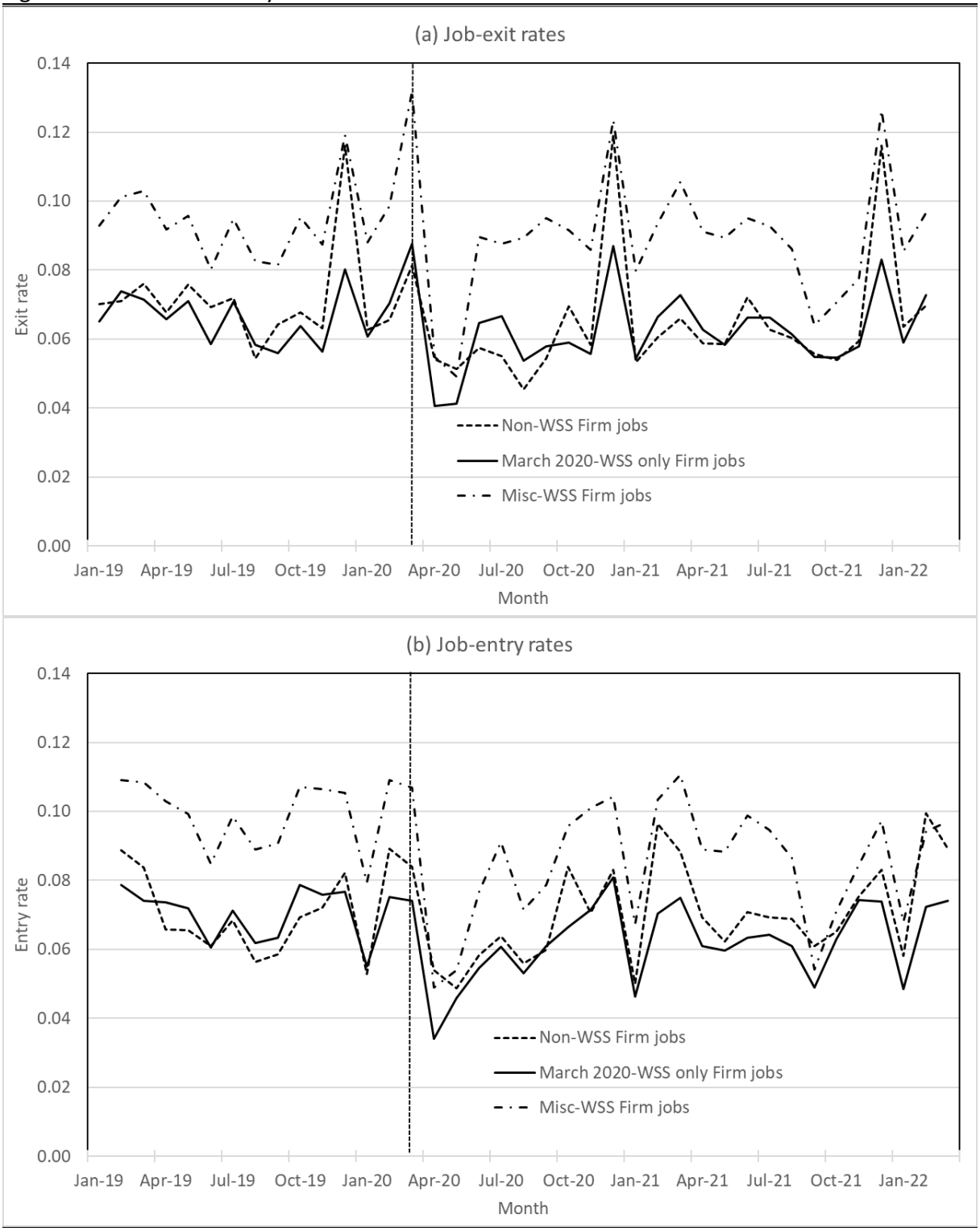


Firm entry and exit



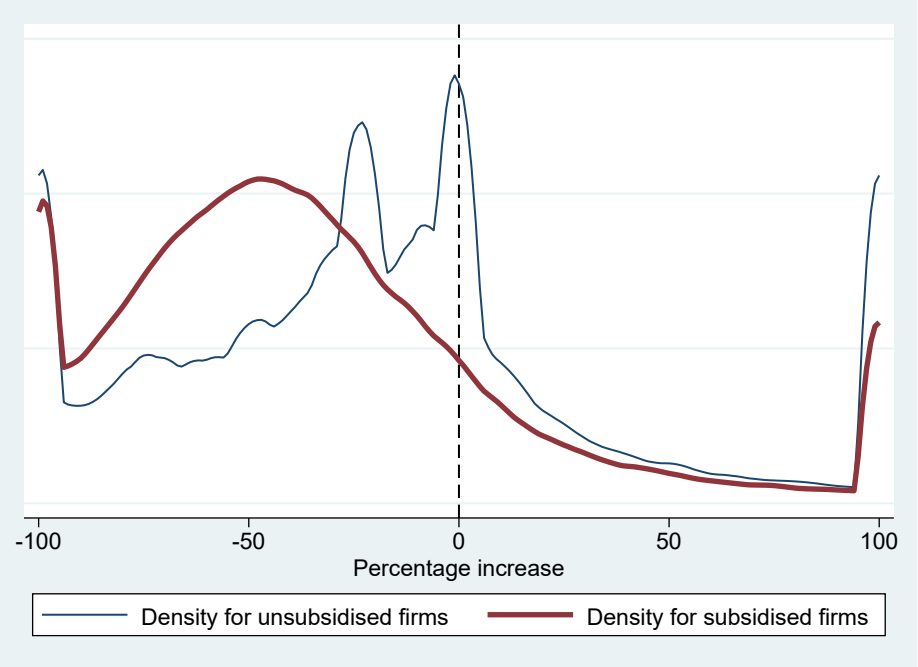
Note: vertical dashed lines indicate March months

Figure 5: Job-exit and entry rates



Notes: Based on EMS wage and salary jobs. Sample is stratified by whether the firm received no WSS-payments ("Non-WSS"), only March 2020 WSS-payments ("March 2020-WSS"), or subsequent WSS-wave payments, generally as well as March 2020 payments ("Misc-WSS"). Vertical dashed lines indicate March 2020 month.

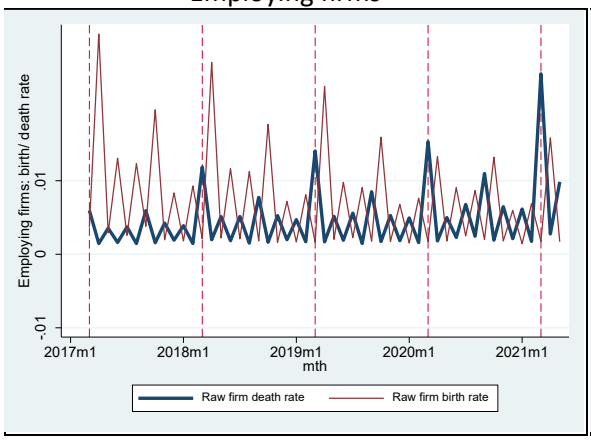
Figure 6: Revenue change distribution – March 2020



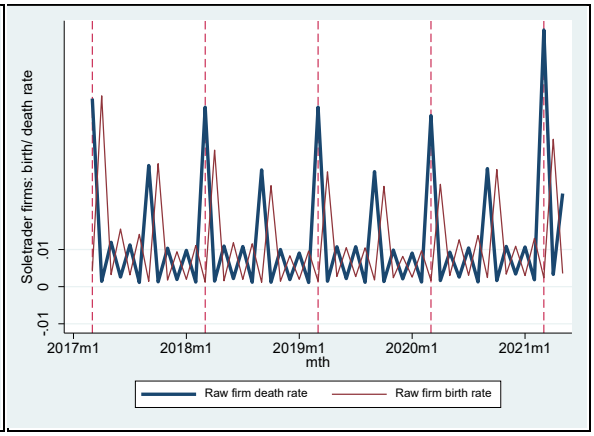
Note: Revenue change is measured as the percentage change of GST sales for a two month period (the two months following the reference month, compared with sales in the reference month and the preceding month).

Figure 7: Births, Deaths, and activity

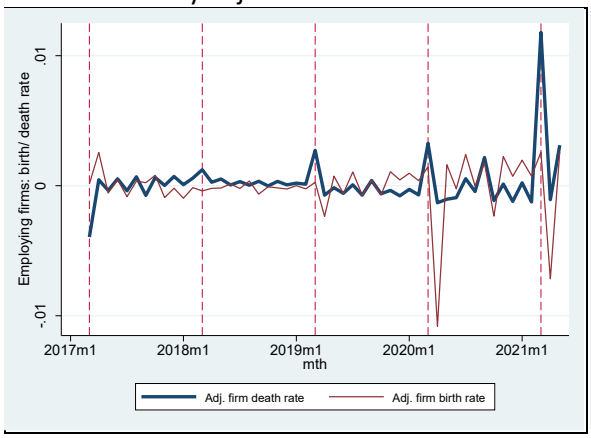
Employing firms



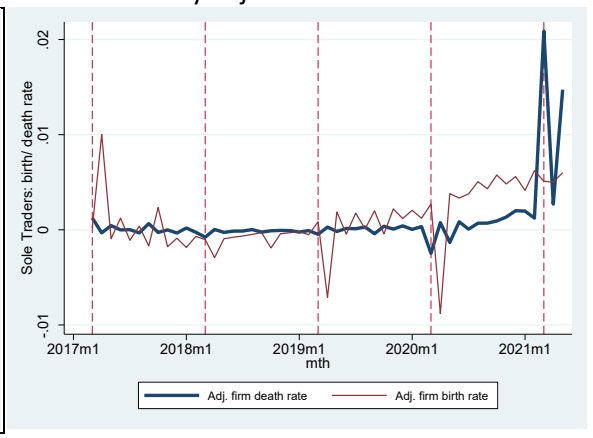
Sole trader firms



Seasonally adjusted and detrended



Seasonally adjusted and detrended



Number of active employing firms

Number of active sole trader firms

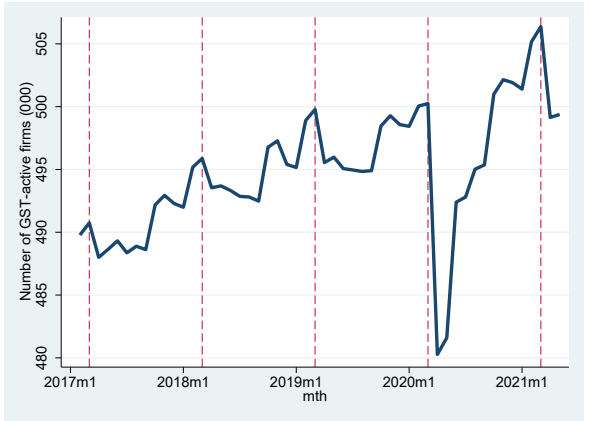


Figure 8: Cohort trajectories – March 2020 Wave

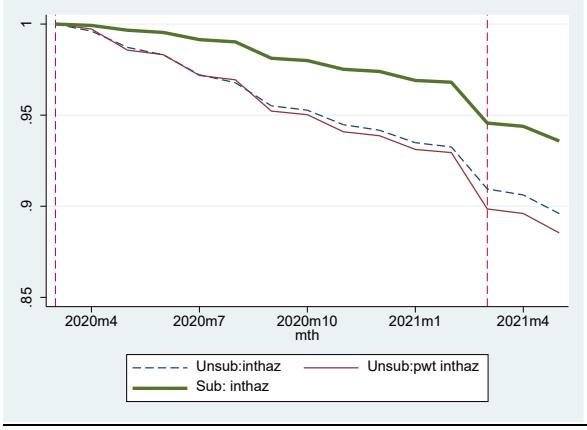
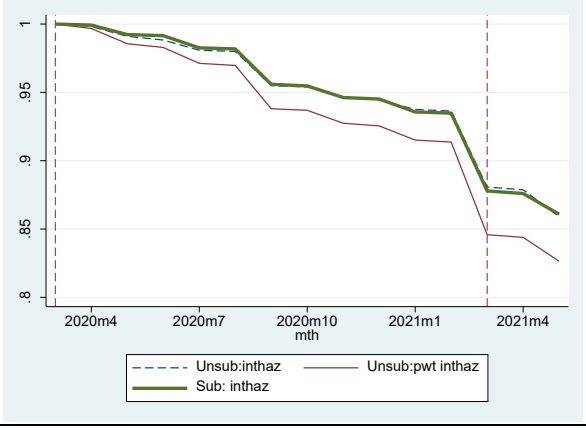
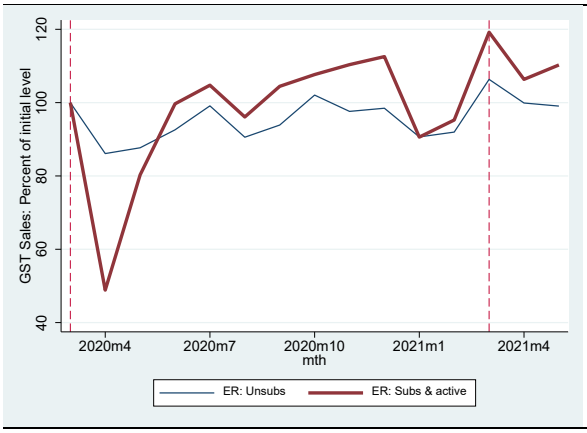
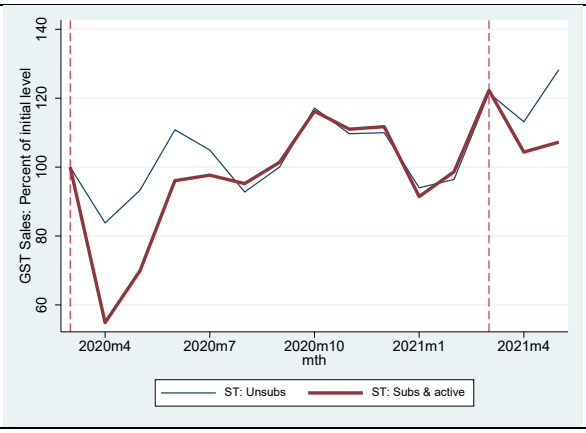
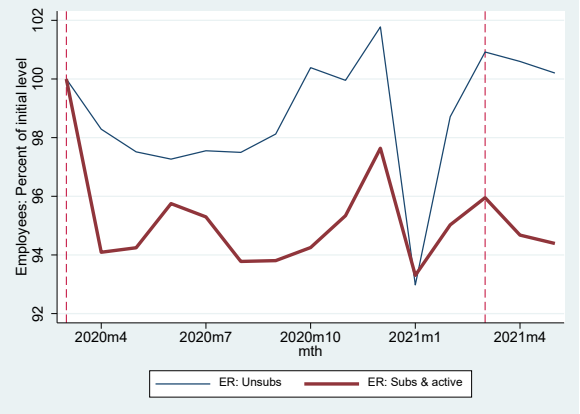
Employing firms	Sole trader firms
Number of firms (Survival)	
	
GST Sales	
	
Number of employees	
	(Not applicable)

Figure 9: Firm Survival

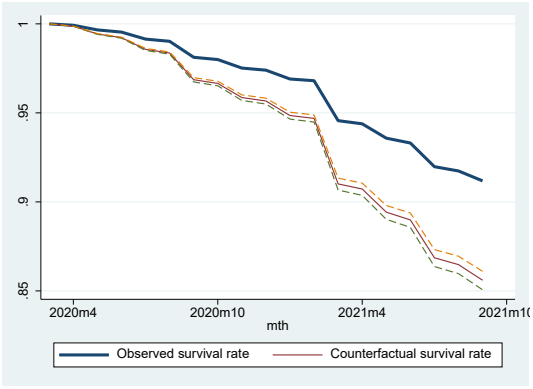
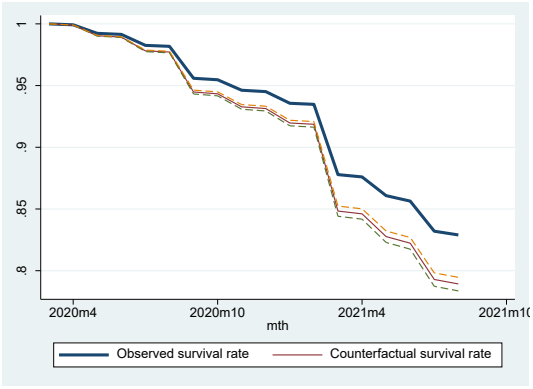
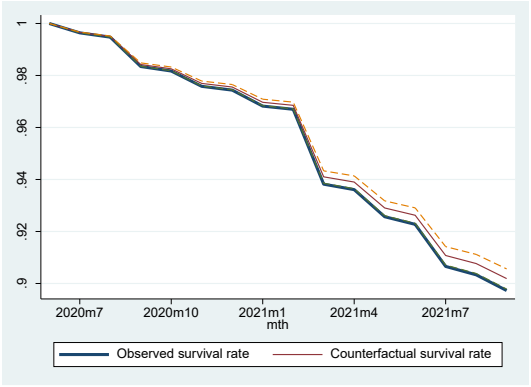
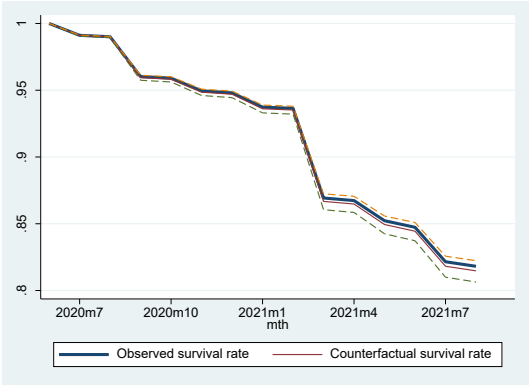
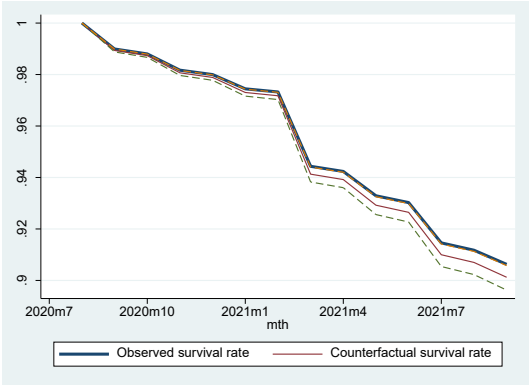
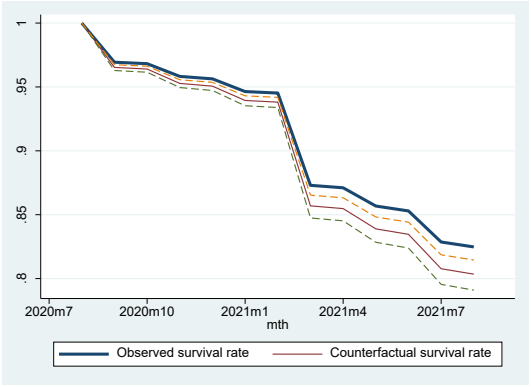
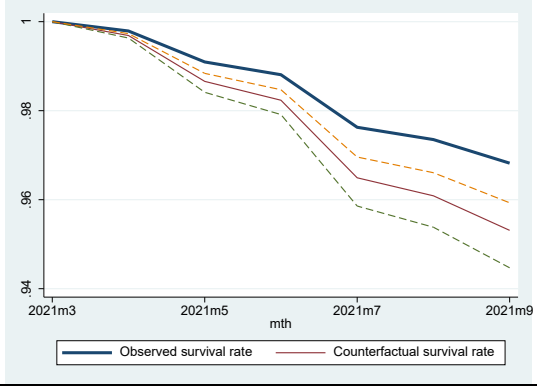
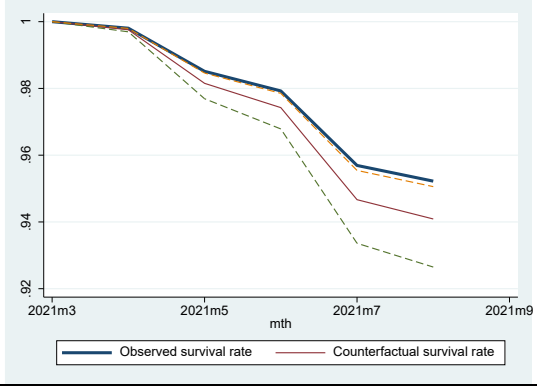
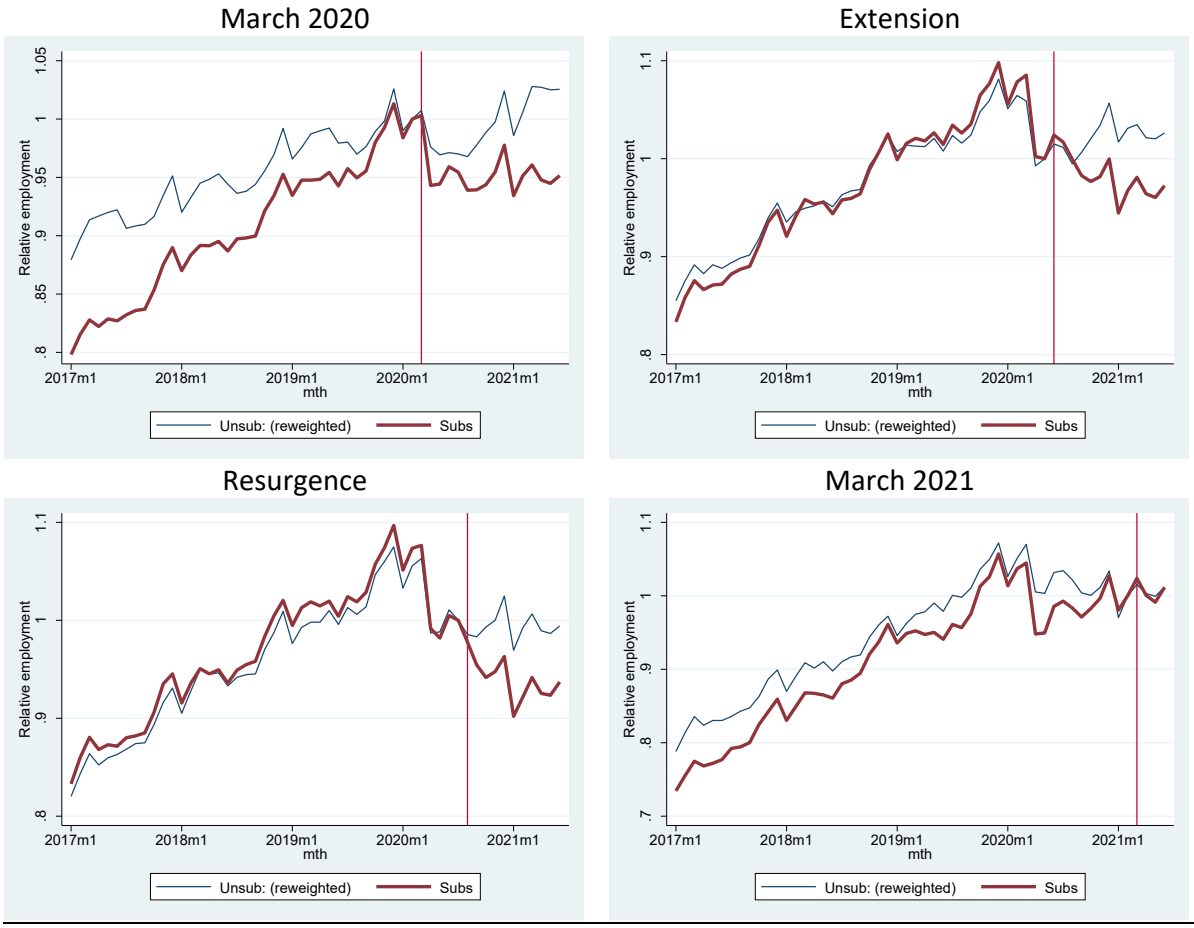
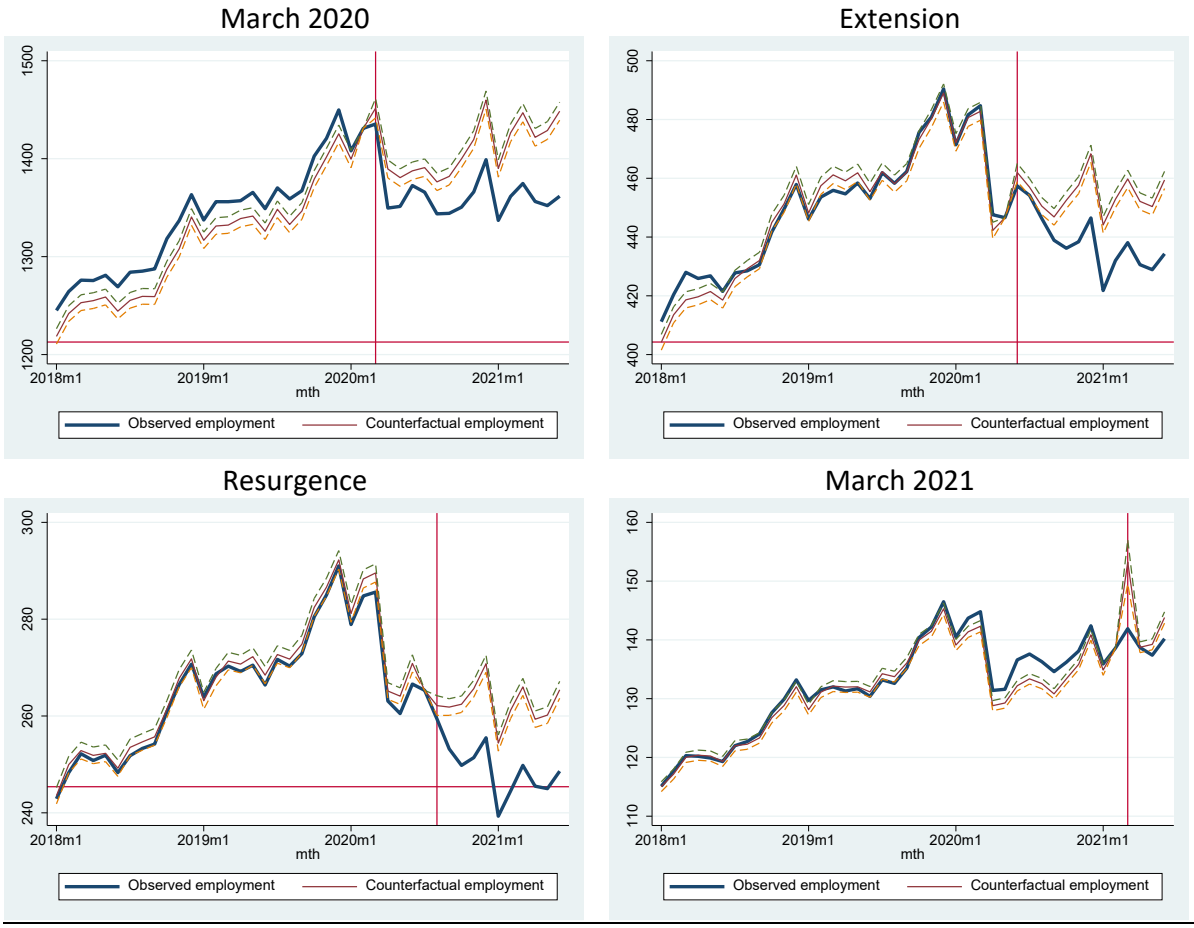
Employing firms	Sole trader firms
March 2020	
	
Extension	
	
Resurgence	
	
March 2021	
	

Figure 10: Employment levels over time: Wave-specific cohorts



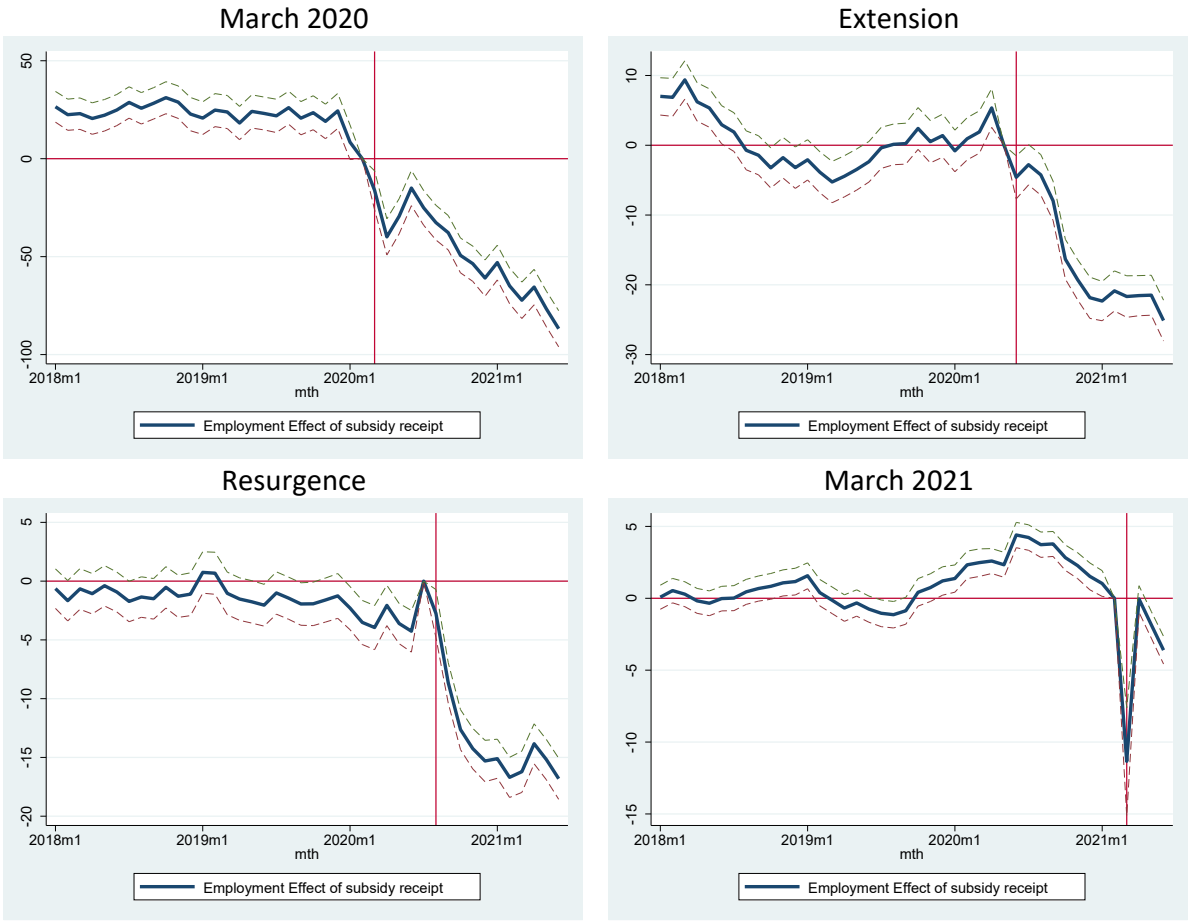
Notes: Employment is for cohorts of firms that are active (with employees) in the reference month. The vertical lines indicate the reference month for each wave. All levels are relative to the month immediately prior to the reference month. Employment in unsubsidised firms is reweighted by inverse probability weights.

Figure 11: Employment Level and counterfactual



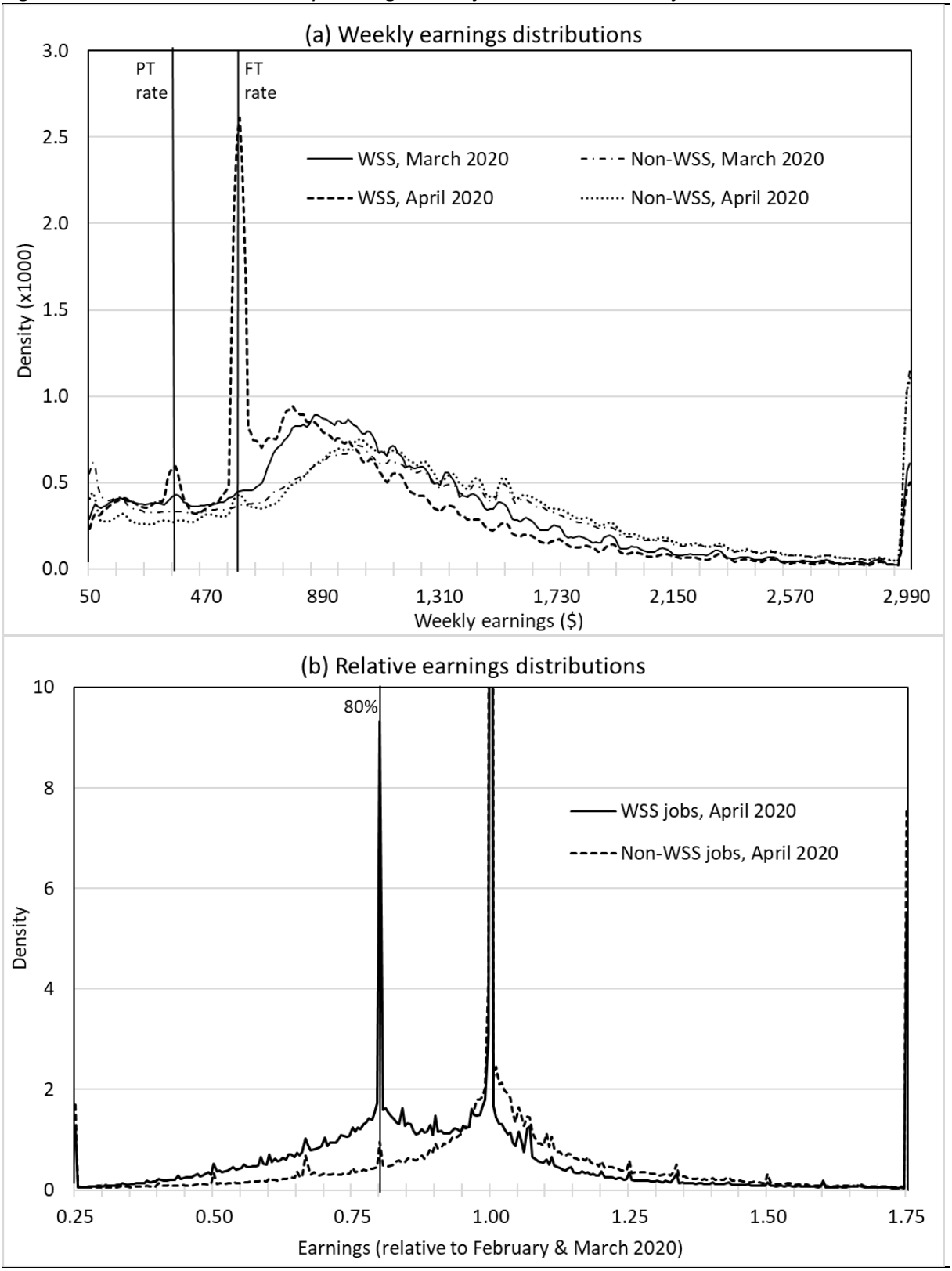
Note: Counterfactual employment is calculated base on covariate-adjusted weighted regression estimates, as documented in section 5.5.

Figure 12: Employment Impact



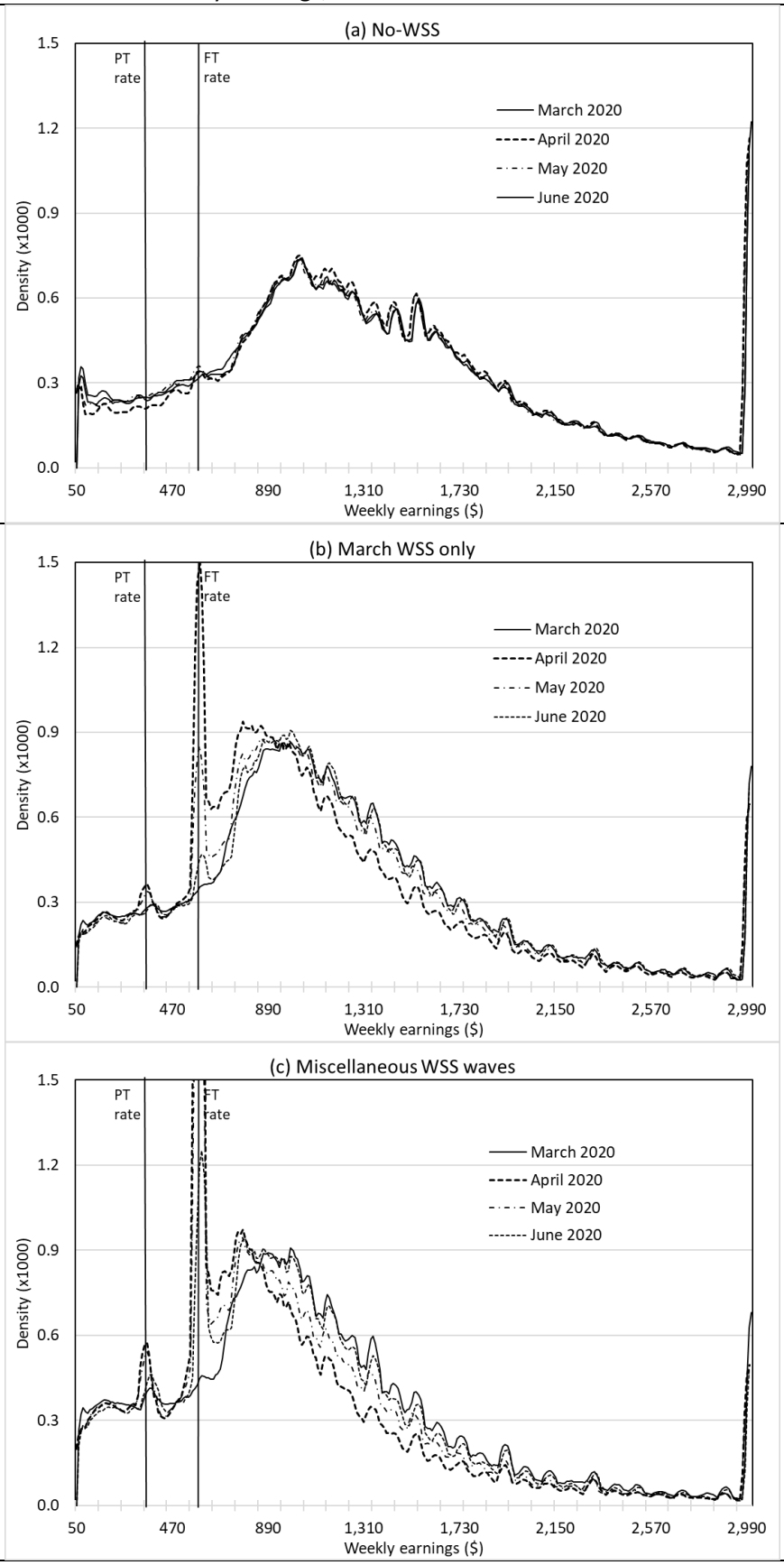
Note: Employment effects are the vertical difference between observed and counterfactual employment, as shown in Figure 11.

Figure 13: Distributions of weekly earnings – WSS jobs versus non-WSS jobs



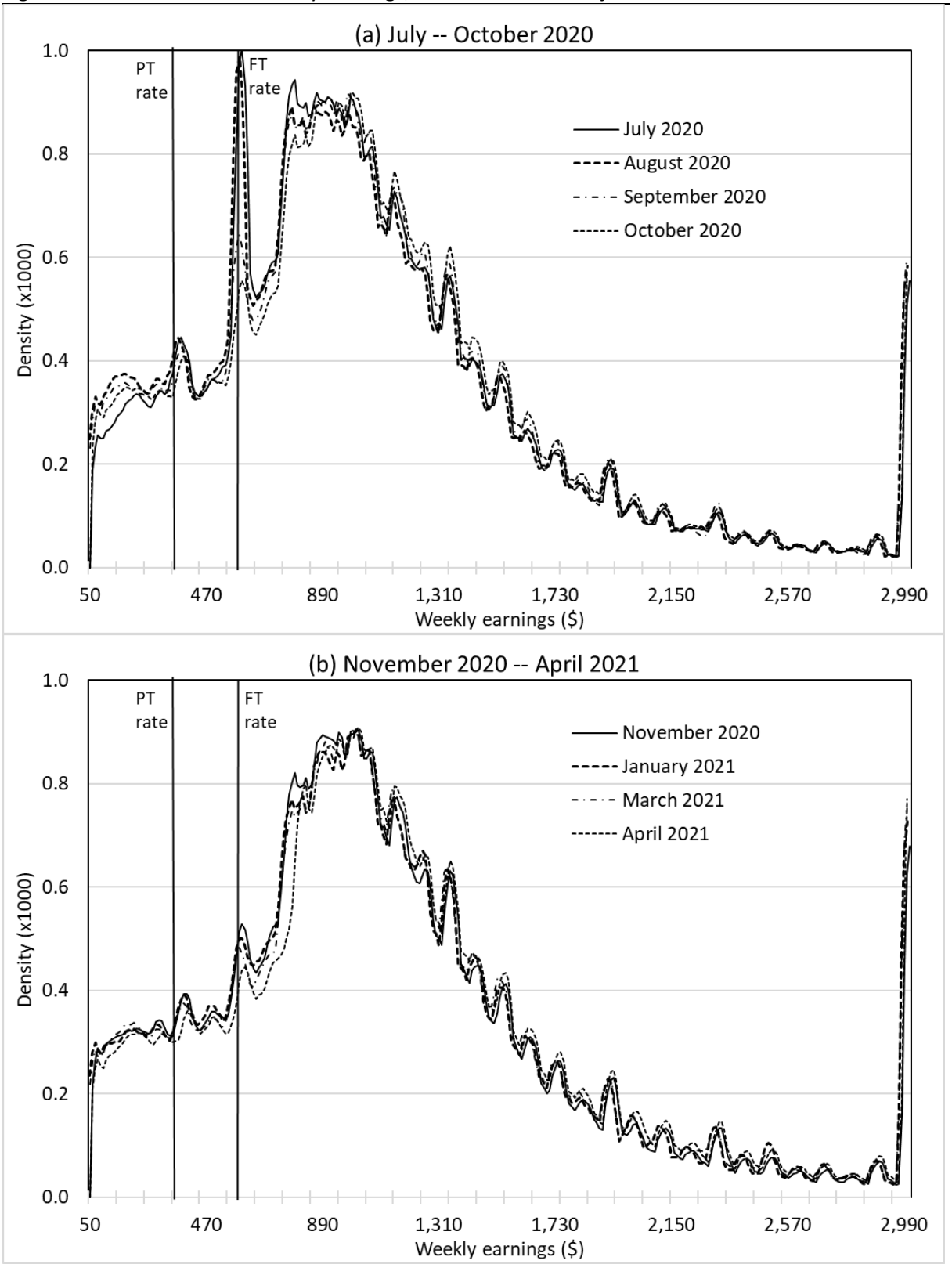
Notes: Panel (a) shows the weekly earnings distributions for WSS versus non-WSS jobs in March and April 2020. Panel (b) shows the distributions of April 2020 weekly earnings relative to pre-WSS weekly earnings (average of February and March earnings) for WSS versus non-WSS jobs.

Figure 14: Distributions of weekly earnings, March – June 2020



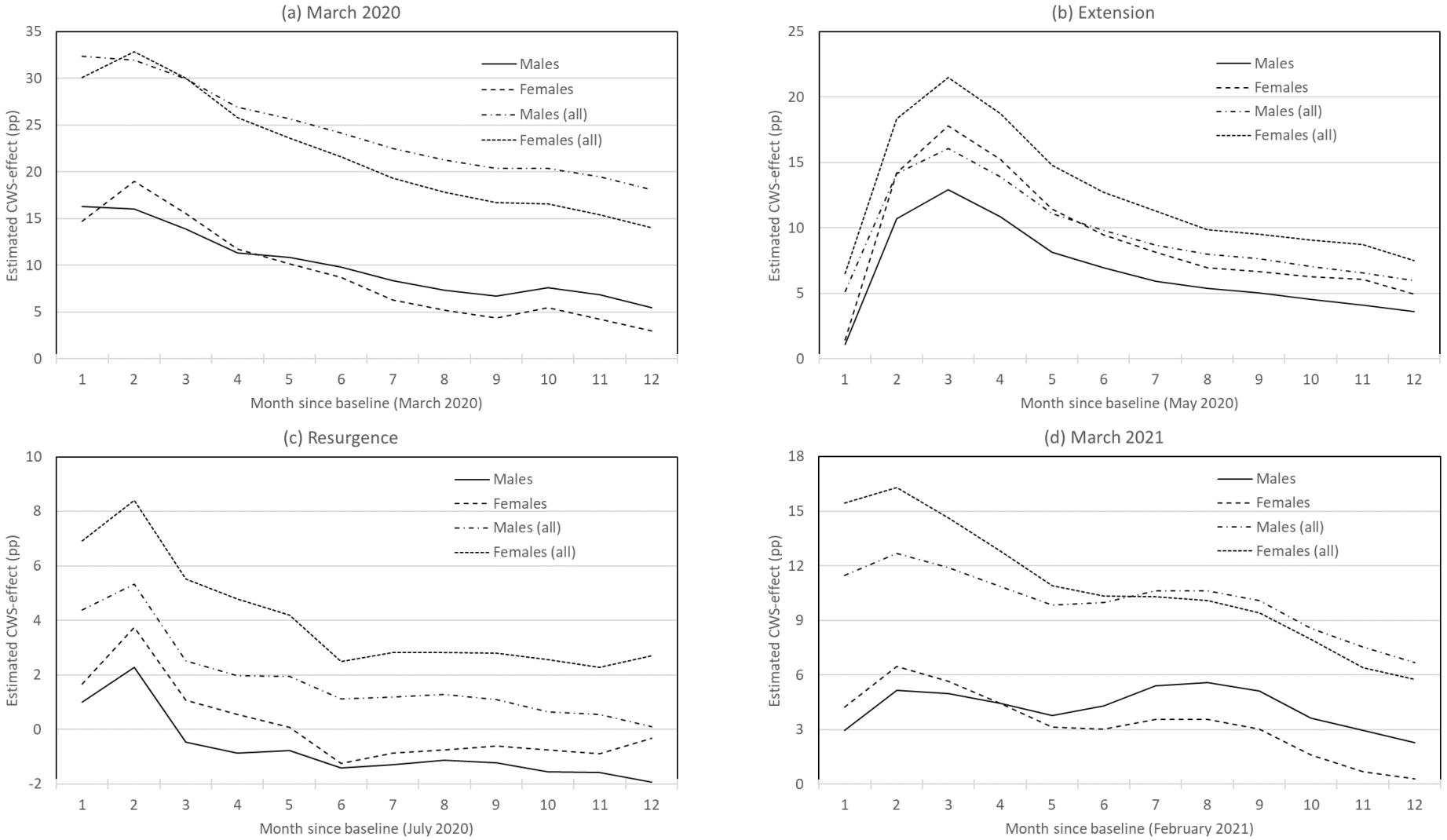
Notes: Each distribution is restricted to jobs that existed from March 2020 until June 2021. The sample in panel (a) is jobs that received no WSS; panel (b) jobs received only the March 2020 WSS; and in panel (c) jobs that received WSS over miscellaneous waves.

Figure 15: Distributions of weekly earnings, Miscellaneous WSS jobs



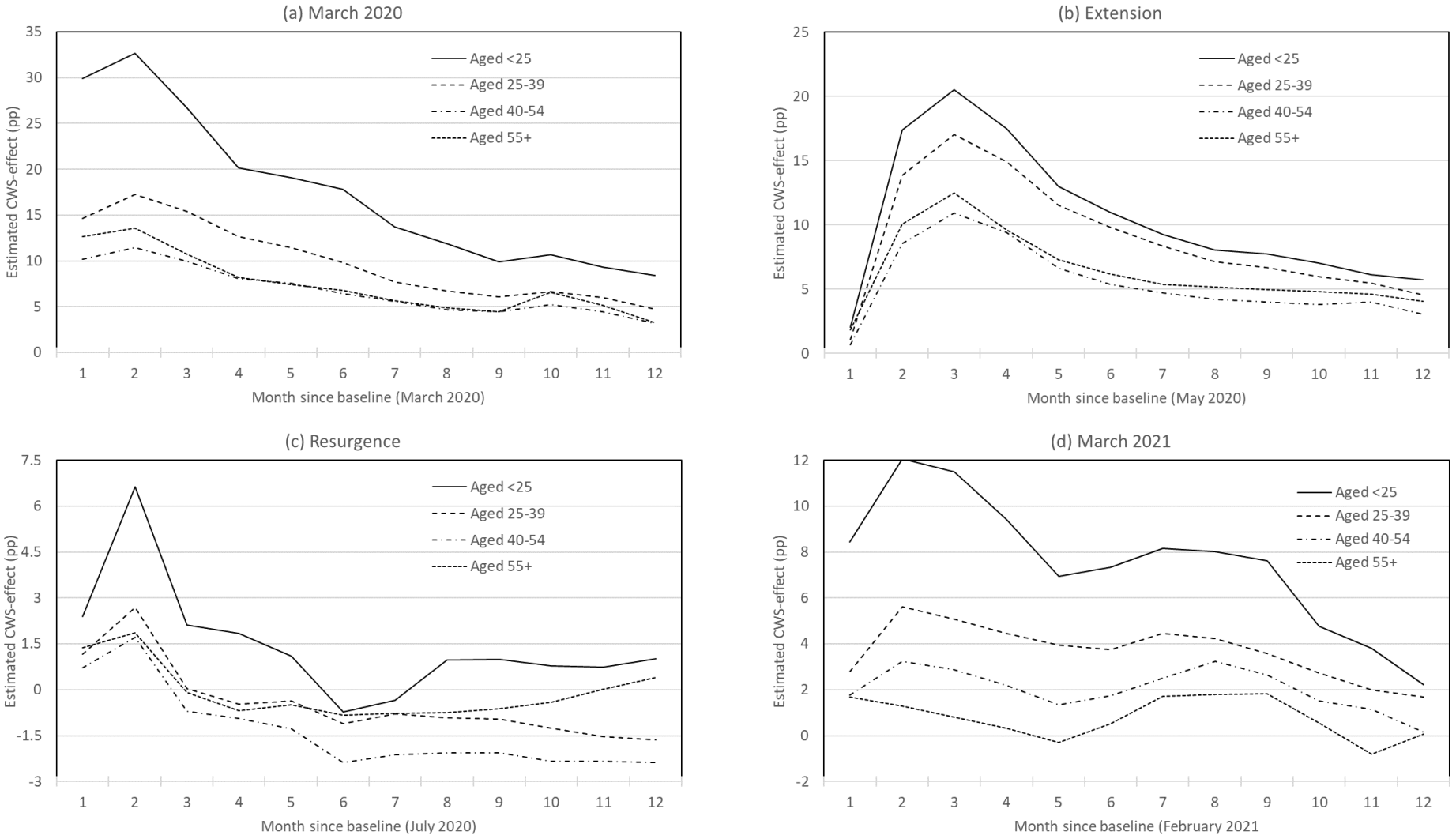
Notes: Each monthly distribution is restricted to jobs that existed from March 2020 until June 2021.

Figure 16: WSS Job-retention effects, by Sex



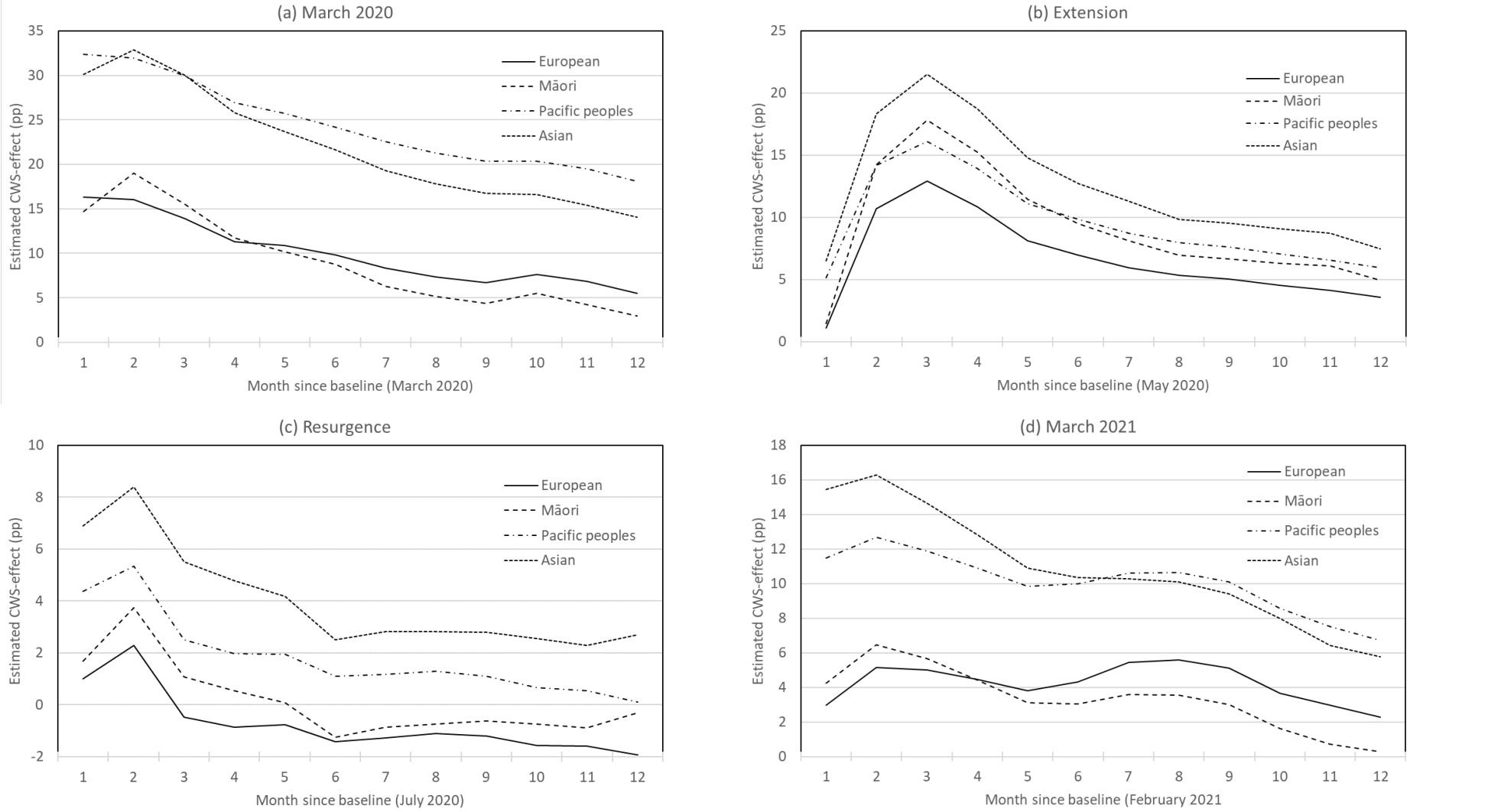
Notes: All coefficients estimated from subgroup regressions with covariate controls (see Table 32), for all baseline jobs and excluding those unobserved in the following year.

Figure 17: WSS Job-retention effects, by Age



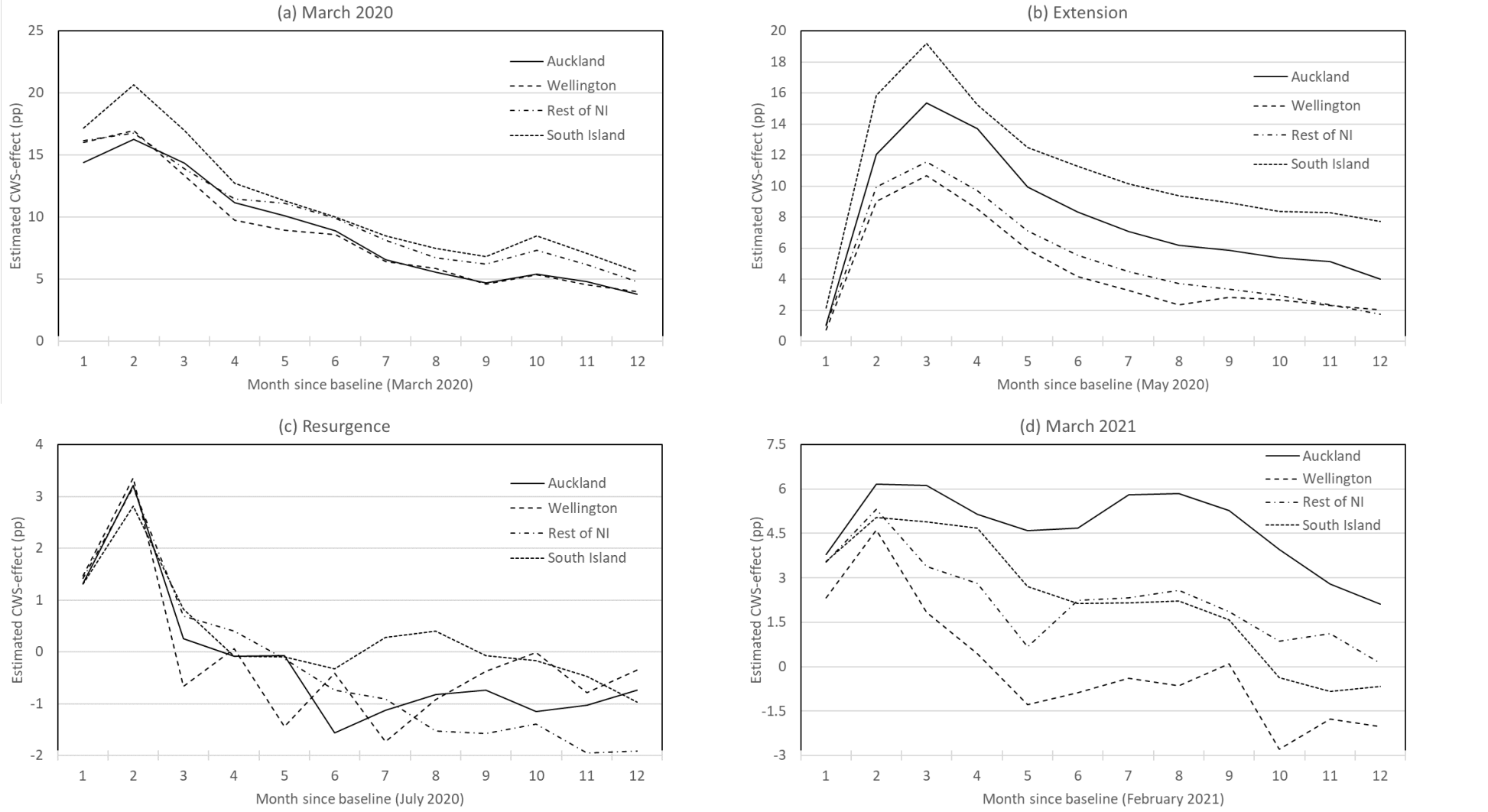
Notes: All estimated coefficients are from subgroup regressions with covariate controls (see Table 32), excluding baseline jobs not observed in the following year

Figure 18: WSS Job-retention effects, by Ethnicity



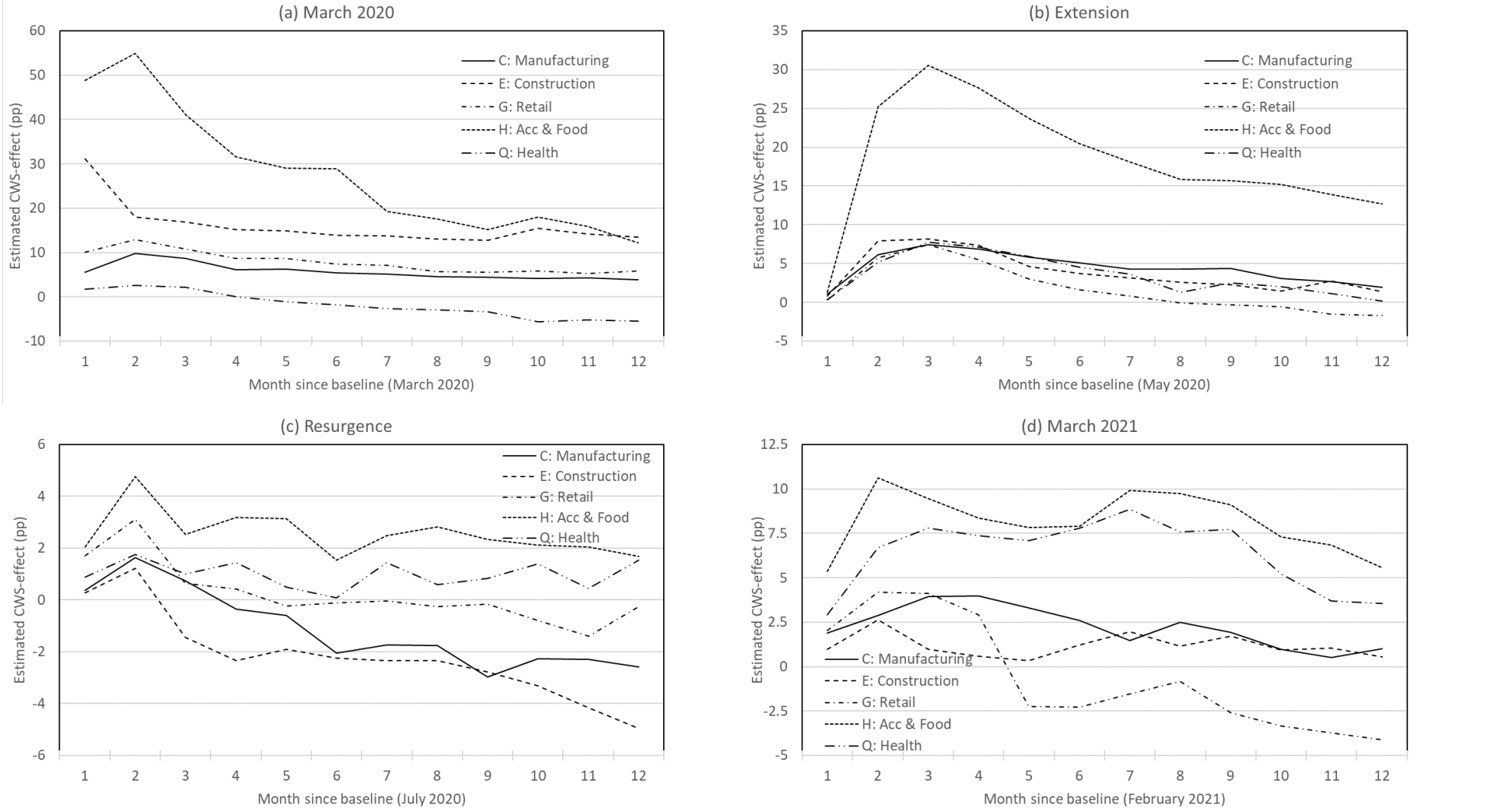
Notes: All estimated coefficients are from subgroup regressions with covariate controls (see Table 32), excluding baseline jobs not observed in the following year

Figure 19: WSS Job-retention effects, by Geographic region



Notes: All estimated coefficients are from subgroup regressions with covariate controls (see Table 32), excluding baseline jobs not observed in the following year

Figure 20: WSS Job-retention effects, by Industry



Notes: All estimated coefficients are from subgroup regressions with covariate controls (see Table 32), excluding baseline jobs not observed in the following year

Appendix 1: Firm ethnicity

Firm ethnicity is defined using definitions similar to those used by Te Puni Kōkiri & Nicholson Consulting (2020) to define Māori businesses. A firm is identified as being of a particular ethnicity on the basis of:

- Ownership: if over 50% of the owners/ shareholders of the firm identify with a particular ethnicity (including sole traders); or
- Employment: if over 75% of employees of the firm identify with a particular ethnicity.

A firm can be identified as being of more than one ethnicity because individuals can identify with more than one ethnicity, and because ownership and ethnicity may lead to different classifications. For instance, a Māori-owned firm with more than 75% of employees identifying as European will be classified as a Māori firm and also as a European firm.

The data sources used to identify firm ethnicity are shown in the box below.

IDI-Based information

Ownership

- Sole Proprietors:
  - Appear in ird_ems as payer, and receives wages and salaries or withholding payments as payee
  - Appears in IR3 with non-zero net profit
- Partners (person-firm link):
  - Appears as employer in IR20, with non-zero share of income
  - Appears in IR3 with non-zero partnership income
- Shareholder (person-firm link):
  - Link ever appears in IR4
  - Earnings are above a CPI-adjusted threshold ($15,000 in 2020)[60]

Ethnicity

- Of owners: Linked to personal_detail table
- Of employees: Employee appears in EMS; link to personal_detail table

LBD-Based information

Ethnicity

- Of owners: From Pent_year_L table
- Of employees: From pent_mth_fte and pent_year_L

Firm classification for Māori firms

- Runanga iwi: from institutional sector code in fact_LBF_enterprise_year
- StatsNZ 'Māori firm' flag in fact_LBF_enterprise_year

[60] See Fabling & Maré (2015) for the rationale for this level of threshold.

The ethnicity of firms is classified into 5 categories, based on the six categories available in the IDI personal details table. European ethnicity is grouped with the small 'other ethnicity' category, which would otherwise need to be suppressed in some output.

Ethnicity data are available for most employing firms (96%), but only for 44% of Sole trader firms.

Small firms are more likely to meet the criteria for being allocated to an ethnicity. For sole-traders, the firm ethnicity or ethnicities will match those of the sole trader. Even among employing firms, small firms are more likely to be identified with a specific ethnicity. The proportions for the March 2020 wave cohort is reproduced below. The proportion of total output or total employment in ethnic firms is uniformly lower than the proportion of firms.

Proportion of firms that are ethnic firms: March 2020 cohort

	Employing firms	Sole Trader firms
European/other firm	73.7%	43.2%
Māori firm	10.4%	3.1%
Pacific firm	2.6%	0.9%
Asian Firm	18.0%	6.1%
MELAA firm	1.6%	0.7%

Table A1: Propensity estimation – goodness of fit statistics

Subsidy wave	Block regressions Industry	Region	Firm ethnicity	Sales level and growth	Firm age	Firm size (employees) level and growth	All covariates
Number of covariates	70	16	5	3	3	5	
Employing firms							
• March 2020	19.3%	1.8%	1.2%	3.0%	0.7%	0.7%	23.9%
• Extension	7.3%	3.6%	1.6%	3.9%	0.8%	0.7%	23.1%*
• Resurgence	4.9%	5.6%	2.4%	0.8%	0.5%	0.1%	25.3%*
• March 2021	4.7%	8.7%	4.9%	0.4%	0.5%	0.2%	26.6%*
Sole-trader firms							
• March 2020	16.2%	1.2%	13.7%	0.4%	3.1%	n/a	26.0%
• Extension	8.5%	1.3%	6.9%	1.8%	0.9%	n/a	41.5%*
• Resurgence	7.4%	1.8%	5.5%	0.6%	0.4%	n/a	33.9%*
• March 2021	7.1%	1.8%	6.8%	0.2%	0.2%	n/a	27.7%*

Note: Table entries are R^2 statistics from linear probability regressions, where the dependent variable is a 0/1 variable for subsidy receipt. Block regressions include only a constant and the specified subset of covariates. The final column is from a regression that includes all covariates. * for waves after March 2020, the "all covariates" regressions also include indicator variables for subsidy receipt in prior waves.

Table A2: Data sources

Database	Schema	Table	Used for
[IDI_Clean_202206]	[acc_clean]	[claims]	(A) Firm location
	[br_clean]	[enterprise]	(E) Industry
		[ird_enterprise_xref]	(IE) Linking tables
		[pbn]	(P) Firm location
	[data]	[address_notification]	(S) Firm location
		[personal_detail]	(S) Worker age, gender ethnicity; WP ethnicity
	[ir_clean]	[ird_ems]	(SI) Earnings and employment
		[ird_rtns_keypoints_ir3]	(S) Identification of working proprietors
		[ird_attachments_ir4s]	(S) Identification of working proprietors
	[security]	[concordance]	(SO) Linking tables
		[ibuldd_to_xref_ileed_ird_uid]	(I) Linking tables
[IDI_adhoc]	[clean_read_IR]	[pent_year_L_IDI_20211020_RFabling]	(P) Employment
		[pent_IDI_20211020_RFabling]	(PE) Linking tables
		[pent_ind_IDI_20211020_RFabling]	(P) Industry
		[pent_mth_FTE_IDI_20211020_RFabling]	(P) Employment, firm ethnicity
	[clean_read_MSD_WSS]	[msd_WSS_employer_20220509]	(I) WSS firms and payments
		[msd_WSS_employee_20220509]	(I) WSS listed employees
		[msd_WSS_repayments_202109]	(I) WSS repayments
[ibuldd_clean_archive_202112]	[dbo]	[load_lbf_fact_business]	(B) Industry
		[load_gst_return]	(I) Monthly sales and purchases
[ibuldd_clean]	[ir_clean]	[ir_sbcs_disbursements]	(I) COVID-19 small business cashflow payments
		[ir_rsp_financial_report]	(I) COVID-19 Resurgence support payments
[ibuldd_research_datalab]	[STATSNZ\dl_RFabling]	[pent_pbn_month_L_IDI_20211020]	(PB) Firm location
		[pent_month_GST_IDI_20211020]	(P) Monthly sales and purchases
		[	

Note: (I) confidentialised IRD-based identifier; (P) Pent firm identifier; (E) Enterprise firm identifier (B) Permanent Business Number identifier; (S) confidentialised SNZ person identifier; (A) Confidentialised ACC-based identifier; (O) Multiple/ other identifiers

Table A3: Shares and take-up rates Employing firms

	Mar 2020		Extension		Resurgence		March 2021	
	Share	Take-up	Share	Take-up	Share	Take-up	Share	Take-up
Overall take-up rate	72.0%		33.9%		15.5%		10.0%	
Southland	2%	54%	2%	18%	2%	5%	2%	1%
Gisborne	1%	61%	1%	19%	1%	5%	1%	1%
Taranaki	3%	58%	3%	20%	3%	6%	3%	2%
Manawatu-Wanganui	4%	64%	4%	21%	4%	6%	4%	2%
Hawke's Bay	3%	68%	3%	23%	3%	6%	3%	2%
West Coast	1%	63%	1%	25%	1%	9%	1%	4%
Marlborough	1%	68%	1%	26%	1%	7%	1%	2%
Tasman	1%	72%	1%	25%	1%	6%	1%	2%
Waikato	9%	67%	10%	25%	10%	9%	10%	4%
Bay of Plenty	6%	70%	6%	28%	6%	9%	6%	3%
Northland	3%	69%	3%	28%	3%	9%	3%	5%
Wellington	8%	72%	8%	31%	9%	10%	9%	4%
Canterbury	12%	73%	12%	33%	12%	11%	12%	3%
Otago	5%	71%	5%	31%	5%	12%	5%	5%
Nelson	1%	78%	1%	30%	1%	9%	1%	3%
Auckland	33%	79%	33%	45%	33%	27%	33%	22%
Agriculture (Base)	9%	21%	9%	6%	9%	1%	9%	1%
Other primary	2%	59%	2%	19%	2%	5%	2%	1%
Finance&Ins	2%	58%	2%	26%	2%	11%	2%	6%
Mining	0%	63%	0%	20%	0%	5%	0%	2%
Elec,Gas,Water	0%	72%	0%	23%	0%	7%	0%	5%
Rental, Real Estate	5%	64%	5%	31%	5%	13%	5%	5%
Education	3%	47%	3%	23%	3%	12%	3%	8%
Public Admin	0%	62%	0%	28%	0%	15%	0%	9%
Prof. Serv	11%	73%	12%	36%	12%	14%	12%	7%
Wholesale	4%	78%	4%	35%	4%	17%	4%	10%
Health	4%	72%	4%	25%	4%	15%	4%	13%
Retail	8%	81%	8%	31%	8%	17%	8%	13%
Manufacturing	6%	85%	6%	40%	6%	16%	6%	9%
Other Serv	7%	78%	7%	34%	7%	16%	7%	14%
Industry unknown	5%	57%	4%	34%	3%	16%	3%	11%
Info Media&Comms	1%	69%	1%	47%	1%	24%	1%	13%
Construction	16%	88%	16%	39%	17%	13%	17%	7%
Arts&Rec	2%	78%	2%	49%	2%	24%	2%	14%
Accom,food	7%	86%	7%	51%	7%	31%	7%	26%
Admin Support Serv	4%	83%	4%	48%	4%	27%	4%	16%
Transport, Post,Wareh	3%	79%	3%	40%	3%	21%	3%	14%

(Table continues)

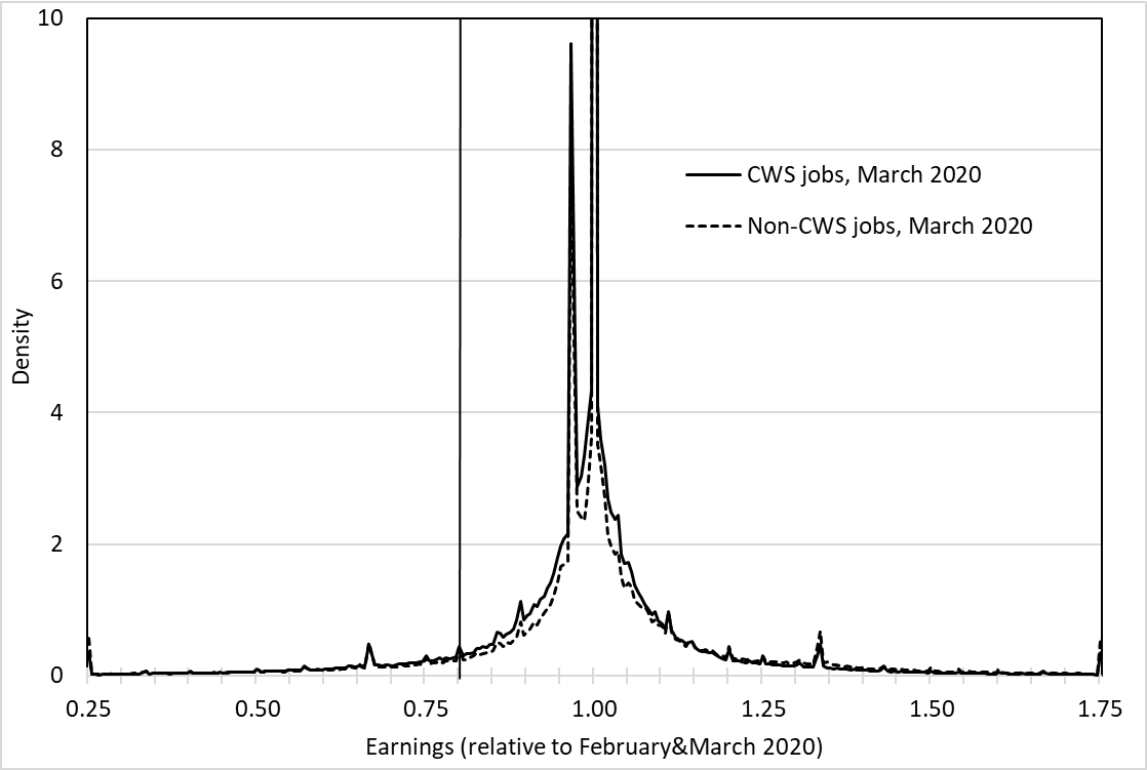
Table A2 (cont):

	Mar 2020		Extension		Resurgence		March 2021	
	Share	Take-up	Share	Take-up	Share	Take-up	Share	Take-up
European/other firm	71%	71%	70%	30%	70%	12%	70%	6%
Māori firm	9%	69%	9%	31%	9%	11%	9%	6%
Pacific firm	3%	77%	2%	44%	2%	20%	2%	16%
Asian Firm	17%	79%	17%	45%	17%	27%	17%	24%
MELAA firm	2%	77%	2%	49%	2%	25%	2%	20%
>50 employees	2%	60%	2%	18%	2%	12%	2%	5%
11-50 employees	21%	78%	21%	33%	21%	17%	22%	11%
6-10 employees	11%	78%	11%	34%	11%	17%	11%	12%
0-5 employees	77%	71%	77%	35%	77%	15%	76%	10%

Table A4: Shares and take-up rates sole trader firms

	Mar 2020		Extension		Resurgence		March 2021	
	Share	Take-up	Share	Take-up	Share	Take-up	Share	Take-up
Overall take-up rate	26.9%		13.4%		5.7%		2.8%	
Southland	2%	15%	2%	5%	2%	1%	2%	0%
Gisborne	1%	17%	1%	6%	1%	1%	1%	0%
Taranaki	2%	18%	2%	7%	2%	1%	2%	0%
Manawatu-Wanganui	4%	18%	4%	7%	4%	2%	4%	0%
Hawke's Bay	3%	21%	3%	8%	3%	2%	3%	1%
West Coast	0%	21%	0%	7%	0%	2%	0%	0%
Marlborough	1%	19%	1%	8%	1%	2%	1%	0%
Tasman	1%	21%	1%	8%	1%	2%	1%	1%
Waikato	8%	21%	8%	9%	8%	3%	8%	1%
Bay of Plenty	5%	24%	5%	10%	5%	3%	5%	1%
Northland	3%	24%	3%	10%	3%	3%	3%	1%
Wellington	9%	26%	9%	13%	9%	4%	9%	2%
Canterbury	10%	25%	10%	12%	10%	4%	10%	1%
Otago	4%	25%	4%	12%	4%	4%	4%	1%
Nelson	1%	29%	1%	13%	1%	4%	1%	1%
Auckland	26%	32%	26%	18%	26%	10%	26%	6%
Agriculture (Base)	9%	6%	9%	2%	9%	0%	9%	0%
Other primary	2%	17%	3%	6%	3%	1%	2%	0%
Finance&Ins	3%	8%	3%	4%	3%	2%	3%	1%
Mining	0%	19%	0%	9%	0%	2%	0%	0%
Elec,Gas,Water	0%	18%	0%	6%	0%	3%	0%	0%
Rental, Real Estate	28%	11%	29%	6%	29%	2%	29%	1%
Education	1%	38%	1%	20%	1%	9%	1%	4%
Public Admin	0%	27%	0%	16%	0%	7%	0%	3%
Prof. Serv	13%	28%	13%	15%	14%	6%	14%	2%
Wholesale	2%	22%	2%	11%	2%	4%	2%	2%
Health	4%	40%	4%	13%	4%	7%	4%	5%
Retail	3%	33%	3%	15%	3%	6%	3%	3%
Manufacturing	2%	39%	2%	18%	2%	6%	2%	2%
Other Serv	3%	38%	3%	17%	3%	7%	3%	5%
Industry unknown	9%	41%	8%	23%	8%	11%	8%	7%
Info Media&Comms	2%	36%	2%	24%	2%	12%	2%	4%
Construction	9%	57%	9%	23%	9%	7%	9%	3%
Arts&Rec	2%	32%	2%	19%	2%	9%	2%	4%
Accom,food	1%	26%	1%	14%	1%	6%	1%	3%
Admin Support Serv	3%	48%	3%	25%	3%	13%	3%	5%
Transport, Post,Wareh	3%	61%	3%	43%	3%	30%	3%	22%
European/other firm	41%	41%	41%	19%	40%	7%	39%	3%
Māori firm	3%	54%	3%	26%	3%	10%	3%	4%
Pacific firm	1%	65%	1%	41%	1%	18%	1%	13%
Asian Firm	5%	57%	5%	38%	5%	25%	5%	20%
MELAA firm	1%	62%	1%	42%	1%	25%	1%	18%

Figure A1: Distribution of relative earnings in March 2020



Notes:

Table A5: Multinomial Logit estimates for weekly earnings spikes

	@PT-rate	@FT-rate	@80%	@PT&80%	@FT&80%
WSS*Post	0.244**	0.028	0.169***	0.915**	0.024
	(0.106)	(0.075)	(0.049)	(0.399)	(0.340)
March'20	1.057***	2.397***	1.716***	1.848***	2.975***
Period	(0.055)	(0.036)	(0.035)	(0.257)	(0.148)
Extension	1.035***	1.856***	1.110***	0.879**	1.872***
Period	(0.134)	(0.052)	(0.050)	(0.412)	(0.226)
Resurgence	0.785***	0.925***	0.969***	1.723***	0.918*
period	(0.299)	(0.158)	(0.084)	(0.601)	(0.521)
March'21	0.935**	0.729***	0.196	-12.87***	-13.03***
period	(0.403)	(0.155)	(0.133)	(0.167)	(0.148)
Observations	2,940,342	2,940,342	2,940,342	2,940,342	2,940,342
Psuedo-Rsq	0.0242	0.0242	0.0242	0.0242	0.0242

Notes: The model is a simple DiD MNL model with no covariates and is estimated using a 5% random sample of workers. Standard errors in parentheses, clustered at the job-level. The dependent variable is a multinomial indicator for whether measured weekly earnings are within $10 of the part-time WSS-rate ("@PT-rate"), within $10 of the full-time WSS-rate ("@FT-rate"), within 1 ppt of 80% of pre-WSS earnings ("@80%"), both within $10 of the part-time WSS-rate and 1 ppt of 80% of pre-WSS earnings ("@PT&80%"), or both within $10 of the full-time WSS-rate and 1 ppt of 80% of pre-WSS earnings ("@FT&80%"). Each column presents MNL coefficient estimates for these outcomes relative to the base-outcomes of weekly earnings not at any of these spikes.

*** p<0.01, ** p<0.05, * p<0.1