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The JobKeeper Payment: How Good Are Wage Subsidies?

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Keywords

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JEL Classification

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1 Introduction

"It goes without saying that JobKeeper has been one of the most significant, and successful, Government programs in our nations history."

The Hon Josh Frydenberg MP, Treasurer, 24 February, 2021.

The COVID-19 pandemic resulted in the sharpest contraction in economic activity in Australia in recorded history, with Gross Domestic Product (GDP) falling by 7 per cent in the June Quarter of 2020 (ABS, 2022). Ahead of COVID-19 related lockdowns, on 23 March 2020 large queues of people were observed lining up outside Centrelink offices, Australia's social security agency, in scenes reminiscent of the Great Depression (Henriques-Gomes and McGowan, 2020). During March and April 2020 almost 500,000 additional people became unemployment benefit recipients (DSS, 2022). At peak in August 2020 there were over 700,000 additional people on unemployment benefits compared to February 2020. Employment fell by over 850,000 people between March and May 2020 (ABS, 2022a), and there were over 1.3 million Australians who lost employment or were employed on zero hours of work in April 2020 (Treasury, 2020).

In response to the escalating employment crisis surrounding the pandemic, the JobKeeper Payment was announced on 30 March 2020. At the time, it was the largest individual labour market program and economic security measure ever implemented in Australian history. Overall, around 4.0 million workers in almost 1.1 million businesses received the wage subsidy with program disbursements totalling around 4.6 per cent of 2019 GDP. This made the JobKeeper Payment alone equivalent to roughly 80 per cent of the size of all of the discretionary fiscal stimulus measures implemented in Australian between 2008 and 2012 in response to the GFC (Charlton, 2019), and the largest individual economic security measure ever implemented in Australia.

The JobKeeper Payment followed the release of two preceding COVID-19 economic support packages by the Australian Government. Unlike Australia's stimulus package in response to the GFC, which consisted overwhelmingly of a combination of cash transfers to households and public investment; the COVID-19 economic support packages focused on transfers as well as business subsidies and loan guarantees, with the focus more on economic security and business hibernation as opposed to economic stimulus. The first stimulus package totalling $17.6 billion was announced on 12 March 2020. This included $700 million to increase the instant asset write-off limit from $30,000 to $150,000 for businesses with turnover of less than $500 million (up from $50 million), and $3 billion in accelerated depreciation for businesses with turnover of less than $500 million also. A $6.7 billion cash flow boost for businesses, and $1.3 billion in wage subsidies for apprentices and trainees. Pensioners, social security, veteran and other income support recipients, and concession card holders, were entitled to a one off $750 stimulus payment, with around $4.8 billion in payments expected to be made in April and May 2020. Finally, a $1 billion fund was established to support regions expected to be disproportionately effected by the pandemic (Australian Government, 2020).

A second $66.1 billion stimulus package was then announced on 22 March 2020. This included a Coronavirus supplement of $550 per fortnight to (normally) activity tested welfare payments totalling $14.1 billion over six months. There was also an additional $750 stimulus payment to non-activity tested welfare recipients scheduled for July 2020 totalling $4.1 billion. Individuals in financial distress due to the pandemic were allowed to access $10,000 in preserved superannuation savings in 2019-20 and a further $10,000 in 2020-21. This turned out to be a major form of economic support - with $37.0 billion withdrawn from the scheme as at 13 December 2020- just before the scheme expired on 1 January 2021 (Mizen, 2021). Social security deeming rates were reduced by 25 basis points, with expected benefits to income support recipients of $876 million over the forward estimates (Australian Government, 2020a). Of particular relevance to employers, the Small and Medium Enterprise (SME) Casflow Boost Program was expanded to a $35.5 billion scheme, and the Government agreed to provide up to $20 billion to guarantee $40 billion in SME borrowing under the Coronavirus SME Loan Guarantee program.

Despite these already significant fiscal policy commitments, almost as soon as the second stimulus package was announced, there was a sense that the combined stimulus packages would be insufficient to shield the economy from an anticipated protracted COVID-related lock-down, and up to 150,000 deaths from COVID-19 (McCauley et al., 2020). There were particular concerns about retaining employee-employer connections in a rapidly deteriorating labour market. Against this backdrop, demands for a wide-scale wage subsidy scheme began to emerge driven by the adoption of a number of novel COVID-19 wage subsidy schemes overseas, particularly in jurisdictions with institutional similarities to Australia. On 14 March 2020 Denmark announced its 'temporary salary compensation scheme'- the first COVID-19 specific wage subsidy scheme, shortly followed by New Zealand's 2020 COVID-19 Wage Subsidy scheme on 17 March 2020. On 20 March 2020 the United Kingdom (UK) Government announced the Coronavirus Job Retention Scheme (CJRS, House of Commons Library, 2021), while on the 27 March Canada introduced the Canada Emergency Wage Subsidy (CEWS, Canadian Government, 2020) and the Paycheck Protection Program (PPP) in the United States (US) was signed into law on the same date (SBA, 2022). Like the JobKeeper Payment, all schemes were subsequently extended as the extent of the economic dislocation caused by the pandemic became more apparent.

On 23 March, the Finance Minister Mathias Cormann indicated that the Australian Government was considering a range of measures to help businesses survive through the lock down period (Kehoe, 2020). He also indicated the Government would be looking at adapting existing features of the Australian tax and transfer system to achieve these ends, rather than devising a new system of support. During the following days articles voicing support for a wage subsidy scheme from economists began appearing (see Kehoe, 2020; and Hamilton and Vueger, 2020).

The Australian Council of Trade Unions called for a wage subsidy scheme on 25 March 2020, a move that was formally supported by the opposition Labor Party on 26 March 2020 (Burke and O'Connor, 2020). At the same time, the call for a wage subsidy scheme was broadly supported by business groups (see Strong, 2020; and Bonyhady, 2020), and commentators from the conservative side of politics normally reluctant to endorse fiscal stimulus measures (Credlin, 2020). So there was a broad coalition of support emerging around the idea that a wage subsidy scheme should be implemented. This culminated in reports on 28 March 2020 that the Government was planning to introduce a wage subsidy scheme in the coming days (Kehoe, 2020a).

At the time the JobKeeper Payment was announced and entering effect, there were also a significant number of other economic security measures being introduced at the Commonwealth level in addition to those announced in the initial two 2020 stimulus packages. On 2 April 2020 the Commonwealth Government announced that childcare would be made effectively free between 6 April and 13 July 2020, and between March and December 2020 a range of expanded social security measures were introduced in addition to what was announced in the initial two stimulus packages (see Parliamentary Library, 2020). By the time of the 2021-22 Budget in early-May 2021, $291 billion in economic support had been provided by the Commonwealth Government alone (Australian Government, 2021).

State and territory governments also introduced a range of business support payments, as well as various tax rebates, subsidies, payroll tax and other tax and regulatory holidays. These were mainly targeted at small and medium sized enterprises. Indeed, so many stimulus and economic security measures were introduced at the Commonwealth, state and territory level that a dedicated online resource was created to track all of the available support for individuals, families and businesses (see Australian Government, 2020b; and Parliamentary Library, 2020a).

New rules were also introduced into Australia's primary industrial relations legislation the Fair Work Act (2009) which intended to provide greater flexibility for employers receiving JobKeeper to respond to the pandemic. Changes to insolvency law resulted in 40 per cent fewer companies going into external administration in 2020 compared to 2019 (Australian Government, 2021). A range of state government and private sector initiatives around rent and debtor relief were also initiated to help support business and household balance sheets, with a 'business hibernation' strategy being expressly pursued during the early stages of the pandemic (see Norman, 2020). At the same time a complex web of state and territory public health and border restrictions imposed differential economic effects based on employer location and industry.

Finally, the Reserve Bank of Australia (RBA) undertook a range of unprecedented actions to ease monetary and financial conditions (see Debelle, 2021; and Lowe, 2022) for further details). Most notably, the cash rate was reduced to the effective lower bound; a yield target was introduced for Commonwealth Government 3 year bonds; the Term Funding Facility (TFF) was introduced to lower funding costs for banks; and quantity targets for the purchase of $100 billion in Commonwealth, state and territory government bonds in the secondary market were announced in November 2020 and again in February 2021.

With copious amounts of stimulus and numerous other support measures in the system, and Australia's relative success at suppressing COVID-19 from a public health perspective, the Australian economy roared back to life after the June quarter of 2020. Workers who lost employment during 2020 were 17 per cent more likely to have regained employment after six months in comparison to those who lost employment following the early 90s recession (Australian Government, 2021). By June 2021 more Australians were employed than was the case in December 2019 before the pandemic had begun (Lowe, 2021). GDP growth experienced a 'V shaped' recovery, with quarterly growth of 3.4 and 3.2 per cent respectively in the September and December quarters of 2020 (ABS, 2022). As the most prominent economic security measure introduced at the time, the JobKeeper Payment has in popular opinion taken a lot of the credit for these successes. Although how much credit can JobKeeper take for this success, and do wage subsidies represent a cost effective economic security measure?

This paper seeks to quantify the impact of JobKeeper on payroll jobs and wages at the firm-level over the entire program period, controlling for the influence of other policy measures, most specifically the qualitatively similar SME Cashflow Boost program, and different location and industry specific trends in economic activity driven by the pandemic and location specific variation in public health policy. In order to do this, our analysis focuses on employers experiencing actual declines in turnover of between 30 and 50 per cent in the first three quarters of the program period relative to the same quarters in the preceding year, and with turnover below $1 billion. These employers represent the primary intended beneficiaries under the program. For employers with these characteristics, access to JobKeeper depended on elements of the program rules unrelated to underlying economic conditions, or firm performance, helping mitigate concerns of endogeneity bias.

In order to estimate the effects of JobKeeper we use novel Australian Government population level administrative datasets related to the JobKeeper Payment and SME Cashflow Boost measures, as well as payroll jobs and wages from the Australian Taxation Office's (ATO's) Single Touch Payroll (STP) system. Using this data we are able to directly estimate the impact of the JobKeeper Payment on payroll jobs and wages covering the entire JobKeeper Payment period between 30 March 2020 and 28 March 2021. We also develop a medium scale New Keynesian business cycle model featuring heterogeneous households and learning-by-doing in the production technology that can help relate estimates of costs per job-year saved to plausible closed economy, no monetary response output multipliers.

We find that the JobKeeper Payment achieved a cost per job-year saved of $112,819 ($US80,959) during the 12 month program period, implying that around 812,000 jobs were saved during this time. Weekly payroll wages for employers were estimated to be almost $1.1 billion ($US761 million) higher throughout the program period as a result of the JobKeeper Payment. This implies wage effects equivalent to around 60 per cent of program spending during the program period. Results for payroll jobs and wages are mutually reinforcing with 0.89 job-years saved per $100,000 of program spending being consistent with effects on weekly payroll wages equivalent to around 0.90 of average weekly total earnings in Australia per $100,000 of program spending during the program period (ABS, 2021). The impact of the JobKeeper Payment is relatively persistent, implying that over time estimates of the *cumulative* costs per-job year saved will be lower, and the cumulative impact of the program on payroll wages will be higher, both potentially in a very significant way. Therefore, these estimates are best regarded as preliminary assessments of the overall benefits that the program is likely to deliver in terms of payroll jobs and wages. On the other hand, results with respect to payroll jobs may overstate program employment benefits as defined in traditional labour force statistics which treat multiple job holders as singularly employed. We find that estimates for the costs per job-year saved are consistent with approximate closed economy, no monetary policy response cumulative output multiplier of around 1.3 at the one year time horizon.

Of the academic studies conducted with respect to COVID-era wage subsidy schemes that all relate to the US PPP, our results suggest that the JobKeeper Payment was most likely more cost effective from an employment protection perspective, and workers were more likely to benefit in the form of higher wages. This was potentially because it was better targetted towards liquidity constrained households and businesses. However, we generally find that estimates for costs per job-year saved due to JobKeeper were towards the middle-to-upper end of those found in relation to studies related to previous fiscal policy episodes studied internationally when assessed on a comparable basis, and output multipliers are towards the middle-to-lower end of previous studies. This may in part be due to circumstances related to the COVID-19 pandemic, but equally it may relate to challenges in designing wage subsidy schemes to ensure that they generate *additional* employment, and related risks concerning high deadweight, substitution and displacement costs associated with similar schemes.

Our theoretical results suggest that liquidity constrained households and hysteresis effects can help generate plausible closed economy, no monetary response output multipliers without the need to resort to Greenwood, Hercowitz, and Huffman (1988) (GHH) preferences, and reinforce the case for ensuring economic security measures are tightly targeted towards financially vulnerable workers.

Section 2 provides further background information concerning the design and implementation of the JobKeeper Payment. Section 3 includes a detailed literature review covering evidence regarding the efficacy of private sector wage subsidy schemes as Active Labour Market Policies (ALMPs); similar Short-time Work (STW) schemes; and other preliminary evidence regarding the efficacy of COVID-19 related wage subsidy programs including the US PPP and the JobKeeper Payment itself. Section 4 details the approach to achieving statistical identification, and the econometric model used to generate empirical results. Section 5 provides a detailed data description, while section 6 provides a discussion of the empirical results. Section 7 covers the derivation and simulation of the theoretical model, and a discussion of our theoretical results; while section 8 concludes. In what follows references to dollar amounts relate to Australian dollars, unless otherwise specified.

2 Program Design and Implementation

This section provides a high level overview of JobKeeper Payment program design features and implementation. Further detailed program design information can be found at ATO (2021). First, concerning rules related to employer eligibility, Australia's major banks, Australian government agencies, local government bodies, entities owned by the Australian government or local government bodies, foreign government entities or entities owned by foreign governments, companies in liquidation or bankrupt individuals were excluded from receiving the JobKeeper Payment. Additionally, a range of minor amendments to the Job-

Keeper rules were made with the apparent purpose of excluding Australia's major public universities from eligibility for JobKeeper (Karp, 2020).

Second, for the initial JobKeeper Payment announced on 30 March 2020 and running up until 27 September 2020, employer eligibility was based on a threshold decline in *actual* or *projected* turnover relative to the corresponding month or quarter in the preceding year. The decline in turnover required was 15 per cent for charitable businesses, 30 per cent or more for other eligible employers provided their GST turnover was less than $1 billion in the 2018-19 income year, or *anticipated* to be less than $1 billion in the 2019-20 income year, and 50 per cent or larger for businesses with turnover of greater than $1 billion. For the first and second JobKeeper extensions running between 28 September 2020 and 3 January 2021, and 4 January 2021 to 28 March 2021 respectively, the decline in turnover test was subsequently restricted to the *actual* decline in turnover relative to the corresponding quarter in the preceding year, while the $1 billion threshold related to the 2019-20 income year, or *anticipated* turnover in the 2020-21 income year.

Once employer eligibility was established, entitlement to JobKeeper Payments then depended on an employer's 'eligible employees'. Eligible employees included all full, part-time, and casual employees who were employed on a regular basis for at least 12 months. Eligible employees had to be either Australian residents or New Zealand citizens on Special Category 444 visas, and generally aged at least 18.[1] Initially eligible employees had to have been on the payroll as at 1 March 2020 for full and part-time employees, and either 1 March 2020 or 1 July 2020 for JobKeeper fortnights after 3 August 2020. Initially casual employees had to be employed for at least 12 months as at 1 March 2020, and 1 July 2020 for JobKeeper fortnights after 3 August 2020. These rules meant that around 950,000 short-tenured casual workers (Cassells and Duncan, 2020) and 900,000 temporary migrants with working rights were excluded from receiving the JobKeeper Payment (Tham, 2020). There were no requirements for employees to be 'furloughed' as in the UK Coronavirus Job Retention Scheme (CJRS) or the Danish scheme, or for employers to maintain employment levels as in the US PPP (where loan relief depended in part on maintaining employment levels)

[1] From 11 May 2020 the JobKeeper Rules were amended such that 16 and 17 year olds only qualified if they had been employed as at 1 March 2020, were deemed to be financially independent, and not in full-time study.

The JobKeeper Payment was initially paid at a single rate of $1,500 per fortnight, irrespective of hours worked or wages. This made the payment more or less equivalent to the minimum wage for full-time employees in Australia at the start of the program ($1,481.60). Workers had to be paid at least this amount, and employers 'should' make 'top up' payments to bring worker pay in line with their regular incomes (ATO, 2021a). In effect this meant that many low-wage employees and workers that had been stood down without pay received a pay boost due to JobKeeper. As such, Treasury (2020) estimated that around 75 per cent of payments operated as a wage subsidy, and 25 per cent as an effective transfer payment to zero income and low wage workers. Mitigating this transfer effect was the fact that JobKeeper Payments could only be received in relation to one job, so income replacement may have been imperfect for some workers who may have lost multiple jobs.

Under the initial scheme, the JobKeeper Payment was available between 30 March 2020 and 27 September 2020, once employers had met the eligibility requirements they were entitled to receive the wage subsidy for the entirety of the remainder of the program period to 27 September 2020, and the same was true with respect to the following two extension periods. The JobKeeper Payment was made to employers on a monthly basis in arrears via the ATO. Employers were responsible for paying workers for each fortnight they planned to claim the JobKeeper Payment. Employers could express interest in accessing payments from 30 March 2020, employer enrolment commenced on 20 April 2020, and from 4 May businesses lodged declarations concerning JobKeeper Payments made to eligible workers. JobKeeper Payments were then received from the 4th of May with respect to JobKeeper fortnights in the month of April. Each employer was then required to report on a monthly basis with respect to JobKeeper payments that had been made to eligible employees in the previous month.

On 21 July 2020 it was announced that the JobKeeper Payment would be extended from 28 September 2020 to 28 March 2021 with two important important changes. First, as discussed, there was the introduction of a revised *actual* turnover test for program eligibility. Second, along the lines of the New Zealand 2020 Wage Subsidy, a two-tier payment system was introduced for full and part-time workers which reduced payments to $1200 per fortnight for eligible employees working 80 hours or more in the four weeks before either 1 March 2020 or 1 July 2020 (tier 1), and other eligible employees to $750 (tier 2) in the December quarter of 2020 (Extension 1); declining to $1000 (tier 1) and $650 (tier 2) respectively in the March quarter of 2021 (Extension 2). More detailed descriptions of the JobKeeper Payment rules, and how these changed over time can be found in ATO (2021b).

In many ways the program can be viewed as an extension of the earlier SME Cashflow Boost measure. Under this measure the Australian Government delivered tax-free cash payments of between $20,000 and $100,000 to eligible businesses automatically upon lodgement of their regular Goods and Services Tax (GST) Business Activity Statements (BAS). Eligible businesses under this program were required to hold an Australian Business Number (ABN) on 12 March 2020; have an aggregate annual turnover under $50 million (generally based on prior year turnover); and have generally made eligible payments where they were required to withhold income tax (predominantly in the form of employee salary and wages, and similar staff-related payments).

Eligible businesses had to have derived income in the 2018-19 income year and lodged their 2019 tax returns, as well as made GST taxable, GST-free or input-taxed sales in a previous tax period (since 1 July 2018), and lodged the relevant BAS on or before 12 March 2020. Registered charities and not-for-profits were also eligible for the payment. Eligible businesses that withheld tax on their employees salary and wages then received a tax free payment equal to 100 per cent of the amount withheld, up to a maximum of $50,000. Eligible businesses could receive a minimum credit of $10,000, even if they had withheld no amounts with respect to employees. Payments were available in two waves, the initial payment available with respect to the March or June quarter 2020 BAS lodgement periods (or monthly equivalents for monthly lodgers), and potentially an additional payment corresponding to the June and September quarter 2020 BAS Lodgement periods respectively (see Treasury, 2020a for further details). Given the size of the measure totalling $35.5 billion, and its co-occurrence with the JobKeeper Payment, controlling for direct receipt of the SME Cashflow Boost is likely to be important to help identify JobKeeper payroll jobs and wages effects.

2.1 Implementation, Take-up and Coverage

In its first month of operation in April 2020 the JobKeeper payment supported approximately 3.4 million workers in 860,000 organisations increasing to around 3.6 million individuals in May 2020, and over 900,000 organisations (Treasury, 2021). Indeed, 3.2 million employees received JobKeeper payments in the very first JobKeeper fortnight- indicating that the majority of payment recipients received the full six months of JobKeeper income support under the initial program. At peak there were over 3.7 million individuals on JobKeeper in July 2020, and 964,000 employers in August 2020. At the conclusion of the first JobKeeper extension in December 2020 there were around 1.6 million individuals remaining on JobKeeper Payments, employed by almost 500,000 employers. At the end of the Program in March 2021 there were still around 1.0 million individuals remaining on JobKeeper Payments, employed by around 355,000 employers. Overall, around 4.0 million individuals and 1.1 million employers received support at various stages throughout the program's duration.[2]

Under the initial JobKeeper program $35.1 and $35.8 billion were spent in the June and September quarters of 2020 respectively (Figure 1). Under the first JobKeeper extension in September 2020 payments declined by almost one third to $12.0 billion in the December quarter of 2020, and then to $6.4 billion in the March quarter of 2021 as economic conditions dramatically improved. The decline in payments was driven by the introduction of the actual turnover test for employer eligibility in the September and December quarters of 2020, and progressively declining payment rates under the revised two-tiered payment system. At the same time SME Cashflow Boost payments of $14.6 billion, $13.5 billion, $6.7 billion and $0.7 billion respectively were made in each quarter between June 2020 and March 2021, that is during the JobKeeper Payment program period. Aggregate Commonwealth COVID-19 specific business support totalled $136.9 billion according to the ABS (2021c), of which the $35.5 billion Cashflow Boost program amounted to almost 26 per cent, and $89.3 billion in JobKeeper Payments (at the time of publication) around 65 per cent.

There were initially anecdotal reports that many businesses did not apply for JobKeeper due to the four to five week wait between making JobKeeper payments to workers and being reimbursed by the ATO (see Hamilton, 2020). However, the ABS (2020) found that

[2] Authors' calculations based on ATO program data supplied

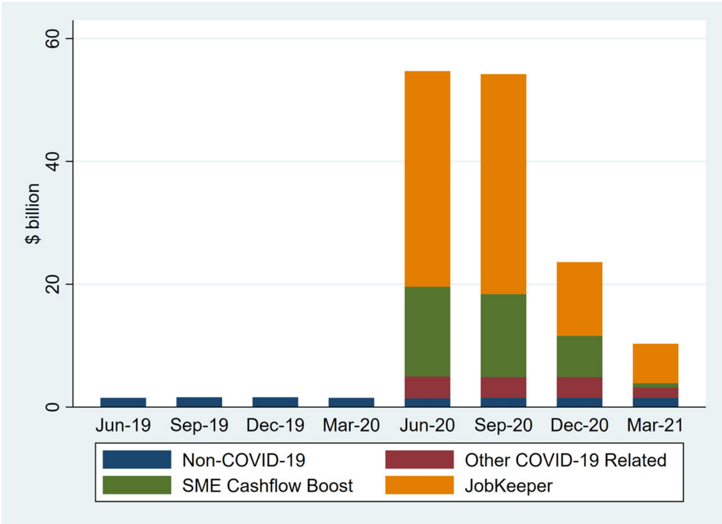


Figure 1: Government Support for Business in Response to COVID-19, source: ABS (2021)

business or employee ineligibility were responsible for 82 per cent of businesses not registering for JobKeeper (Figure 2). Small businesses were more likely to report ineligible employees than medium and large businesses, whereas medium and large businesses were more likely to fail the business eligibility requirement. Bishop and Day (2020) exploit variation in employee eligibility to identify JobKeeper Program employment effects at the individual-level, whereas this papers seeks to exploit both employee and business eligibility rules to identify employment and wage effects at the firm-level.

Based on STP Payroll data linked to JobKeeper status, on 1 March 2020 (the testing point for employee eligibility), 60 per cent of jobs were held by ineligible organisations, or organisations that chose not to apply for JobKeeper. Within the remaining 40 per cent of organisations, two thirds of employees were eligible at the testing point (Treasury, 2020). Using this data, Treasury was also able to ascertain that the overwhelming majority of job losses between the announcement of JobKeeper and the end of May 2020 were experienced by ineligible employees in JobKeeper eligible businesses.

Using private Xero payroll data that pertains predominantly to small businesses, Treasury (2020) found that employment fell by 13 per cent between the beginning of March and the end of April 2020. Most of this fall took place in the second-half of March, before the announcement of JobKeeper, with job losses largest amongst casuals (25 per cent), followed

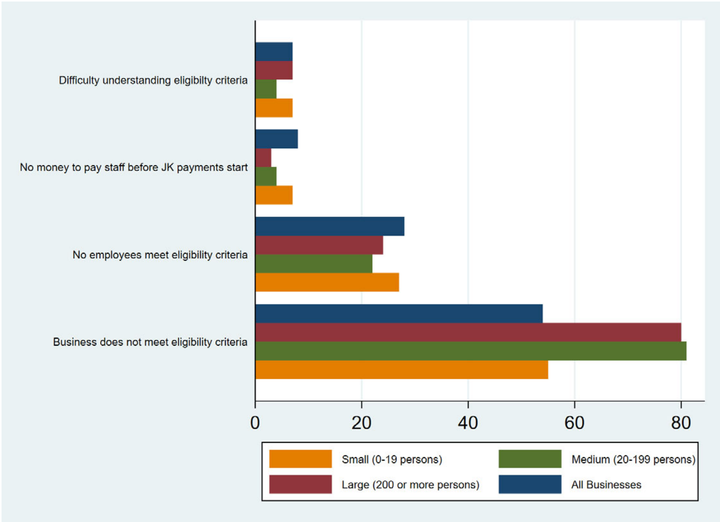


Figure 2: Reasons for not Registering for the JobKeeper Payment (Percentage), source: ABS (2020)

by part-time (5 per cent), and then full-time employees (2 per cent). According to Treasury (2020) 29 per cent of total job losses between mid-March and the end of May 2020 were the secondary jobs of multiple job holders- reinforcing the concentrated impact of the recession on casuals, something that was most likely intensified by the JobKeeper eligibility rules that excluded short-tenured casuals.

Treasury (2020) also observed that around a quarter of the total value of JobKeeper payments in the first quarter of the program's operation constituted income transfers to individuals, consistent with one of the three underlying objectives of JobKeeper to be an alternative means of income support while retaining the connection between employees and their employer. By corollary, around three quarters of JobKeeper payments constituted a pure wage subsidy to the employer. As labour market conditions improved during the pandemic, and payments became less generous for part-time workers in particular, the share of the JobKeeper Payment that represented a transfer payment declined over the life of the Program (see Figure 12 in Section 7.6 below).

The fact that the decline in turnover test associated with the initial JobKeeper Payment could be satisfied on an anticipated basis resulted in a situation where $11.4 billion and $15.6 billion in the June and September quarters of 2020 respectively was provided to employers whose turnover did not *actually* decline by 30 per cent (or 50 per cent) compared to the corresponding period in the previous year (Treasury, 2021). $4.6 billion and $9.2 billion respectively went to employers that actually saw turnover increases compared to the corresponding quarter a year earlier.

Another dimension of program implementation that may be relevant in evaluating its effectiveness and efficiency relates to program integrity and compliance. As of August 2021 the ATO had identified $284 million in JobKeeper overpayments in relation to businesses (Sharples, 2021), increasing to $497 million as at 28 February 2022. While as at the end of April 2021, Services Australia had raised 11,771 welfare debts related to JobKeeper Payments totalling $32.8 million (Coughlan, 2021). By June 2020 the Fair Work Commission had received 496 dispute applications concerning the JobKeeper Payment, of which 474 had been resolved (Treasury, 2020).

Overall, given the significant size of the JobKeeper Program, the number and size of integrity issues identified was relatively small, and their consequences for the overall efficiency of the Program is likely to be minor. This justified the conclusion of ANAO (2020) that the ATO had effectively managed risks related to the rapid implementation of the initial JobKeeper Payment. Eligibility rules based on projected rather than actual turnover, and a lack of income contingency or clawback provisions, were leading causes of potential inefficiency under the Program, rather than fraudulent behaviour on the part of businesses and individuals.

3 Literature Review

3.1 Private Sector Wage Subsidies as Active Labour Market Policies (ALMPs)

There is now a voluminous literature assessing the efficacy of private sector wage subsidy schemes as ALMPs, and this short survey focuses mainly on meta-analyses of the literature. Most schemes tend to focus on assisting unemployed workers back into the labour force, and particularly relatively more disadvantaged cohorts such as the long-term unemployed. For instance, Card et al. (2010) in a meta-analysis covering 199 active labour market programs between 1995 and 2007 found that, in the Anglosphere, 60 per cent are targetted towards the long-term unemployed, and 20 per cent people experiencing other forms of disadvantage. Their results point to statistically significant positive effects of private sector wage subsidies in terms of reducing unemployment duration, with program benefits increasing over-time. The average benefits of private sector wage subsidies are in line with those for job-search assistance and training schemes, and greater than direct public sector employment. However, Escudero (2018) and Yeyati et al. (2019) also demonstrate that private sector job subsidies are typically more expensive than other ALMPs, suggesting that they while employment benefits can appear impressive, wage subsidies may not beat other ALMPs on a cost-benefit basis. These studies also typically do not assess the cost effectiveness of private sector wage subsidies compared to other employment generation programs- such as various forms of fiscal stimulus. Card et al. (2018) revisit their earlier analysis based on 200 programs between 1980 and 2012. Overall, the updated study reinforces the findings of the original paper, finding that private sector wage subsidies can be particularly helpful for reintegrating women and the long-term unemployed into the workforce.

On the question of cost effectiveness, Brown and Koetl (2015) observe that all employment subsidy schemes have been associated with significant deadweight, substitution, and displacement costs. Deadweight costs are costs associated with taxpayers subsidising employment that would have happened anyway; substitution costs are where employers substitute towards subsidised workers and away from non-subsidised workers; and displacement costs relate to situations where employment maintained by the program crowds out employment that otherwise would have occurred elsewhere in the economy if the retention incentives were not available. Alternatively, displacement costs can be thought of as costs associated with preserving jobs that would not be viable without the employment subsidy.

For example, Dar and Tzannatos (1999) report deadweight costs for wage subsidies in 12 studies covering advanced economies between the mid-70s and early-90s ranging between 20 and 70 per cent, as well as substitution and displacement costs ranging between 10 and 55 per cent. They also present additionality estimates ranging from 4 to 35 per cent. They conclude that the substantial costs, and low levels of additionality, suggest that wage subsidies are generally unlikely to be cost-effective. Further, Calmfors (1994) found deadweight and substitution costs of between 70-90 per cent in Australia and the Netherlands. Marx

(2001) found deadweight costs ranging between 42 and 93 per cent, and substitution costs ranging between 21 and 85 per cent for a range of wage subsidies relating to the long-term unemployed, unemployed youth and low paid workers. Martin and Grubb (2001) found deadweight and substitution costs of around 90 per cent relative to scheme benefits in Australia, Belgium and the Netherlands. In exceptional cases, deadweight and substitution costs could be reduced to as low as 20 per cent with tight additionality requirements (Calmfors, 1994), or targetting towards particularly disadvantaged cohorts amongst the unemployed (Martin and Grubb, 2001). Martin (2000) also finds deadweight costs amounting to around 90 per cent in wage subsidy programs in Australia, Belgium, Ireland and the Netherlands suggesting low net benefits. In an evaluation of a Michigan hiring credit program, Bartik and Erickcek (2010) found that 92 per cent of credits were paid for jobs that would have existed without the scheme. Boockmann et al. (2012) and Huttunen et al. (2013) study wage subsidy schemes where there were no employment benefits, and the wage subsidy schemes were a complete waste of money. However, on a more positive note, Bartik (2001) found a cost per job created under the US New Job Tax Credit (NJTC) of as low as $US17,765 (2009) based on an upper bound estimate of the number of jobs created under the program, and Bartik and Bishop (2009) suggest a new hiring credit of 15 per cent of *additions* to payroll would have a cost per job saved of about $US28,000. These results are consistent with other evidence suggesting that wage subsidy programs that focus sharply on additionality in hiring are likely to be the most cost effective.

In summary, the evidence from research regarding the efficacy of private sector wage subsidies as ALMPs suggests that they have been generally effective at increasing employment and incomes in the medium to long-term, particularly for some disadvantaged cohorts such as the long-term unemployed, while having more muted short-term effects. Evidence suggests that *hiring* subsidies are more cost effective than *employment* subsidies, and that tight targeting towards relatively disadvantaged groups and strict additionality requirements can increase cost-effectiveness (Neumark, 2013; and Bernhard et al., 2008). Even where these conditions are in place, deadweight, substitution and displacement costs can be high, even to the point of effectively cancelling out employment and wage benefits in net terms.

3.2 Short-time Work and Employee Retention Schemes

So-called 'Short-time Work' (STW) schemes such as the Chômage Partiel in France, Kurzarbeit in Germany, and Cassa Integrazione in Italy, provide subsidies to employers who reduce hours rather than laying-off workers in response to negative shocks. As such, these schemes provide a closer policy analogue to the novel COVID-19 wage subsidy schemes introduced in the Anglosphere such as the PPP in the US, the Canada Emergency Wage Subsidy (CEWS) in Canada, the CJRS in the UK, and the 2020 COVID-19 Wage Subsidy in New Zealand. These schemes typically subsidise 60-80 per cent of employees salaries for 'furloughed' workers, which is to say those the have been 'stood down'. In this respect, the similarities to the UK's CJRS and Canada's CEWS are particularly obvious and direct. Schemes of this nature have a long institutional history in many European countries (see Brenke et al., 2013).

Boeri et al. (2011), Hijzen and Martin (2013), Efstathiou et al. (2019), Lydon, Math and Millard (2018), Kopp and Siegenthaler (2019), and Balleer et al. (2016) all find that STW schemes had positive employment effects during the GFC in a range of mainly European advanced economies. Boeri et al. (2011) observe that significant numbers of workers were covered by these schemes during the GFC, with between 2.5 and 5 per cent of workers covered by such schemes in Germany, Japan and Italy at the height of the recession. They find that STW schemes reduced employment loses during the GFC, however the number of jobs saved is typically low relative to the number of program participants- suggesting substantial deadweight losses. For instance, they find that although around 1.5 million workers were covered by the STW program in Germany in 2008-2009, only around 300,000 jobs were effectively 'saved'. Things were possibly even worse in Japan with 2.5 million STW participants, and under 34,000 jobs saved. Boeri et al. (2011) and Hijzen and Martin (2013) also present evidence suggesting that STW programs are only effective during the most severe recessions, and can actually increase job losses at other times.

A range of papers also indicate that STW schemes are typically more effective in preventing unemployment amongst permanent rather than temporary employees. This is often by direct design as, like under the JobKeeper Payment, short-tenured casual or part-time employees are often excluded from program coverage. For example, Cahuc and Carcillo (2011), OECD (2010), Hijzen and Venn (2011) and Lydon, Math and Millard (2018) found firms using STW schemes were less likely to lay off permanent employees following a negative shock; however, observed no effects in relation to temporary workers.

Brenke et al. (2013) and OECD (2010), Hijzen and Venn (2011) point out that like other wage subsidy programs, STW schemes can be subject to significant deadweight costs, exploitation, and can be politically difficult to withdraw once a shock has passed. Brown and Koetl (2015) also note that STW schemes can inhibit entry into the labour force and labour reallocation, and lock workers into lower-productivity and lower-paying jobs- with negative consequences for aggregate employment, productivity and wages. Policies that inhibit labour force realloaction have been found to be costly. For instance, Hopenhayn and Rogerson (1993) examined the impact of government policies that make it more costly for firms to adjust their employment levels, finding that firing costs equal to a year's wages result in a productivity loss of around 2 per cent. These considerations can substantially reduce the net benefits of STW schemes, although tight targeting and time limitations can help mitigate these costs (see Bartelsman, Haltiwanger, and Scarpetta, 2009). In summary, while STW schemes have proven successful in limiting employment losses during recessions, particularly amongst permanent employees, Brown and Koetl (2015) conclude that 'wage subsidies have proven very cost-ineffective and undesirable to incentivize the retention of workers.'

3.3 COVID-era Wage Subsidy Schemes

A number of countries that do not usually operate wage subsidy or STW schemes introduced specific schemes to address the COVID-19 pandemic, with Hubbard and Strain (2020) Appendix A1 providing a comprehensive summary. To date there have been relatively few attempts to evaluate the efficacy of these schemes. Of the formal evaluations that do exist, almost all relate to the $US953 billion PPP that was introduced in the US as part of the $US2.2 trillion Coronavirus Aid, Relief and Economy Security (CARES) Act signed into law on 27 March 2020. Like JobKeeper in Australia, it was the largest component of the US Government's COVID-19 economic security response, with the amount of fiscal support provided exceeding the entirety of that provided in response to the GFC.

Under this program small businesses were provided funds equivalent to 8 weeks worth of payroll obligations including worker benefits, subsequently expanded to 24 weeks on 3 June 2021. In addition to workers salaries, the loans could be used to repay mortgage interest, rent and utility bills (US Treasury, 2021). The PPP was predominantly available for small, active businesses, with payroll relief provided equal to 2.5 times average monthly payroll over the 1 Jaunary 2019 to 31 December 2019 period, capped at $US10 million per employer.

PPP Loans were provided by private financial institutions, with a maturity of two years and an interest rate of 1 per cent. The rationale for providing PPP loans via private lending institutions was that existing systems and customer relationships could be used to distribute funds quickly. A similar rationale was used to justify a preference for wage subsidies in the context of the Australian JobKeeper scheme, in that existing employer payroll systems could be used to facilitate a large volume of payments quickly.

The United States Small Business Administration (SBA) could then forgive loans on the condition that: (1) Loans were used to cover payroll costs, mortgage interest, rent and utilities over the 8 weeks (extended to 24 weeks on 3 June 2021), with no more than 25 per cent of the loan being allocated to non-payroll expenses (increased to 40 per cent on 3 June 2021); and (2) full-time equivalent (FTE) employee numbers, hours and remuneration were maintained over the loan period. Given the fungibility of funding provided, it would be effectively impossible to enforce the first requirement. As of late-2021, 94 per cent of PPP loan recipients had applied for forgiveness, and virtually all applications had been approved by the SBA (Autor et al., 2022).

Applications for a 'first draw' under the scheme had to be made by 30 June 2020, later extended to 8 August 2020; while some firms were entiteld to a 'second draw' with applications due by 31 March 2021 (subequently extended to 31 May 2021). Generally speaking, to be eligibile for a 'first draw' the small business only need certify that, in good faith, 'current economic uncertainty makes this loan request necessary to support the ongoing operations of the applicant' Rothenberg et al. (2020). Second draws were provided with additional restrictions, most notably with respect to firm size (fewer than 300 employees for second draw entitlements, down from 500 or less under the first draw), and there was a requirement for an actual decline in quarterly turnover of 25 per cent or less relative to the corresponding quarter in 2019.

Demand for PPP loans was initially extremely high, and between 3 and 16 April all of the initial $US349 billion budget allocation was disbursed, forcing the program to halt for a short period. A Bill passed Congress allocating a further $US320 billion to the Program on 23 April, and PPP applications were accepted once again from the 27 April. 60 per cent of the additional funding was allocated within two weeks; however $US144 billion of 'first draw' loans remained unallocated by the end of the program period on 8 August, suggesting demand had more or less dried up at this stage.

Starting with the most comprehensive studies, Autor et al. (2022) extend the analysis of Autor et al. (2020) by linking SBA data on loan take up to their ADP private sector payroll dataset, and undertaking an event study along the lines of Granja et al. (2020). Incorporating estimates of the influence of the PPP on employment in smaller firms to their earlier results they find a cost per job-year of $US170-257,000, and that roughly 23-34 per cent of PPP loan funding went to workers incomes. They find that the PPP reduced the rate of temporary business closures due to the pandemic, but it is unclear that it reduced permanent closures. They attribute the high costs per job-saved and low flow through to wages to the fact that PPP loans were largely untargeted. However, the authors acknowledge that better targetting would have potentially delayed program rollout, which would have also had consequences for efficacy. Further, Dalton (2021) uses administrative data and a doubly robust dynamic DiD approach to find a cost per job-month saved of between $US20-34,000, with around 24 per cent of PPP funding contributing towards wage retention. While superficially lower than other estimates, the costs per job saved are actually quite high considering that the median *annual* income in the United States was $42,065 in 2019 according to the US Census Bureau (2021)- the cost to save a single job for two months was at best close to the pre-pandemic annual median wage.

Of the earlier papers looking at preliminary program effects, Autor et al. (2020) use a DiD strategy exploiting the 500 employee cut-off for program eligibility to find a cost per job saved of $US224,000 over the first two months of the Program, albeit with a relatively wide confidence interval spanning $US162,000 and $US381,000. Using a similar identification approach Chetty et al. (2020) found a cost per job saved of $US377,000 over the first 3.5 months of the program. They find that the PPP had such modest employment effects because the vast majority of loans went to infra-marginal firms that were not planning to retrench a significant number of workers. Using Dunn & Bradstreet data Hubbard et al. (2020) also investigate differences in employment either side of the 500 employee cut off under, finding the PPP program increased employment by between 0.5 to 1.25 per cent. Using a Bartik-style shift-share measure of program exposure which identifies regions where lenders over or under-performed relative to the their national share of PPP lending relative to their national share of small business lending as a whole, Granja et al. (2020) found a cost per job saved of somewhere between $US109-164,000 over the first three months of the Program. Although confidence intervals are broad, they argue that their results rule out the possibility of large employment effects.

Some preliminary studies find the PPP was more cost effective, for instance Doniger and Kay (2021) identify PPP employment effects using a ten day period where loans were delayed due to funding exhaustion in April using a generalised dynamic DiD approach. These authors suggest that the PPP had a cost per job saved of around $US43,000 over the six month period between April and September 2020. Using a banking instrument to help control for heterogenous treatment effects Bartik et al. (2020b) find that the first round of PPP loans increased business survival rates by between 9 and 22 percentage points, with a cost per job saved ranging from $US32,000 in IV estimates to $US67,000 in the OLS estimates (albeit with very wide confidence intervals). Bartik et al. (2020a) found higher volumes of PPP loans in states are associated with fewer layoffs and and higher hours worked. Although these results a potentially subject to concerns of reverse causality because firms in less adversely effected states might have found it more beneficial to obtain a PPP loan because they were planning to keep on employees in any event.

Overall, estimates of the employment effects of the PPP program have been highly variable reflecting differences in identification techniques and related challenges, potentially heterogenous treatment effects, different time periods, and generally the use of sub-population level datasets. Concerning the preliminary evaluations, Angrist and Pischke (2009) point out that it is necessary to have data covering the before, during and after program period to establish the parallel trends equivalent in the generalised DiD framework. Questions have also been raised about the respresentativeness of the priate sector datasets used in papers such as Autor et al. (2020), Autor et al. (2022), and Hubbard et al. (2020); and only Dalton (2021) directly estimates program effects using comprehensive administrative data spanning the entire program period as we do for the JobKeeper Payment. Loans earlier in the program period predominantly went to large businesses, which also raises issues regarding the representatives of the earlier studies. Further, Granja et al. (2020) provide no provide no formal evidence suggesting that the Bartik-instrument used is valid, with Faulkender et al. (2020) arguing that there may not be enough variation in their Bartik-style instrument to identify program effects, because in practice PPP penetration was very high in all regions. Only Autor et al. (2022) and Dalton (2021) formally control for potentially heterogenous treatment effects. Autor et al. (2020), Chetty et al. (2020) and Granja et al. (2020) all rely on assumptions regarding PPP take-up to map estimated intent-to-treat effects to calculated program employment effects. Confidence intervals around estimates of employment effects in Granja et al. (2020) and Bartik et al. (2020b) are considerable. For studies such as Autor et al. (2020), Chetty et al. (2020), and Hubbard et al. (2020) that rely on the 500 employee cut off, there are questions concerning the external validity of these results, as the results become significantly less precise as window size around the cut-off point is expanded. Similiarly, for Doniger and Kay (2021) there are questions about the external validity of results with respect to entire program effects that are derived from their 10 day event window in April 2020 when program funding was exhausted.

3.4 Preliminary Evaluations of the JobKeeper Payment

This section provides an overview of preliminary assessments of the efficacy of the JobKeeper Payment. The most relevant evaluation to this study is Bishop and Day (2020). They find that 20 per cent of employees who received JobKeeper wage subsidies would not have remained employed during the first four months of the Program without the subsidy, which equates to around 700,000 jobs saved, and a cost per job saved of around $100,000. The authors find that JobKeeper increased average hours worked by around 1 hour per week between April and June, or the equivalent of about 5 per cent of total hours worked relative to February 2020. This is broadly equivalent to the scale of the employment effect, implying that JobKeeper primarily operated via the extensive (employment) rather than intensive (hours) margin, as would be expected for a scheme of this type. To identify causal effects, Bishop and Day (2020) make use of program eligibility criteria that excluded casual employees who had less than 12 months tenure with their employer to compare employment outcomes for casual employees who had a little more and less than 12 months employment tenure with their current employer. Some limitations of the paper include the fact that it doesn't cover the entire program period; program effects are narrowly identified with respect to short-tenured casuals, and therefore might not be generalisable; it utilises ABS Longitudinal Labour Force survey data which is survey rather than administrative data, and cannot be used to directly estimate the impact of JobKeeper on employment at the employer level; and finally, the lack of availability of program data means that the estimates provided are intent-to-treat effects for the particular employees studied, and assumptions regarding 'take-up-rates' are required to translate these into broader employment effects associated with receiving wage subsidies under the Program.

Borland and Hunt (2021) estimate that temporary lay-offs related to JobKeeper may have saved between 292,000 and 375,000 jobs. They find that as few as 100,000 workers were recalled from temporary lay-off in the first month of the program, and very few in the subsequent three months, with most workers during this time period being hired directly from out of the labour force during this time. Based partly on the estimates of Bishop and Day (2020), they suggest a cost per job saved of $107,900 (about $US80,500) over the first four months of the program.

Using STP payroll and tax administrative data, Andrews et al. (2021a) found that aggregate labour productivity would have been around 4.5 and 5.5 per cent lower if there had been a decoupling between reallocation and productivity, and that JobKeeper can account for around half of this. Higher productivity firms were more likely to take up the initial JobKeeper Payment compared to the two extensions, probably reflecting the influence of the forecast vs. actual turnover tests between the two schemes. The authors also note that the degree of labour misallocation due to JobKeeper appeared to increase over time, with the two program extensions being more tightly targeted towards more marginal and less productive employers.

Andrews et al. (2021b) confirm the finding that JobKeeper disproportionately shielded higher productivity, infra-marginal firms from the pandemic shock using Xero private sector payroll data. Xero data is broadly representative of the small business community by size and industry; however, being a cloud based platform it is more likely to be subject to selection issues, as these firms tend to be more technologically proficient. At the start of the pandemic job destruction was higher in Australia compared to both NZ and the UK, a fact that the authors attribute to the NZ wage subsidy scheme commencing on 17 March, and the UK scheme on 20 March, 13 and 10 days before JobKeeper was announced. They observe that in Australia job destruction was disproportionately concentrated amongst employees that were ineligible for JobKeeper, and productivity enhancing reallocation was also greatest amongst JobKeeper ineligible workers.

4 Evaluating the Effect of JobKeeper on Payroll Jobs and Wages

4.1 Statistical Identification and Econometric Model

In order to identify the impact of JobKeeper on payroll jobs and wages at the employer-level we seek to isolate a subset of employers where variation in employment is driven overwhelmingly by eligibility or ineligibility for the JobKeeper Payment based on elements of the program rules that were plausibly unrelated to contemporaneous variation in economic conditions or employer performance.

To this end, the dataset is first constrained to only include firms that experienced *actual* declines in turnover of greater than or equal to 30 per cent, and less than 50 per cent, in each quarter between June 2020 and December 2020 relative to the same quarter in the preceding year. That is, the sample is confined to firms that demonstrably satisfied the actual turnover test on a quarterly basis during the relevant testing periods for program eligibility. In the first phase of the JobKeeper Payment firms that lodged Business Activity Statements (BASs) on a monthly basis were allowed to qualify based on a monthly decline in turnover. Monthly turnover data was not made available to us in the context of this research project to enable a more granular assessment of employer eligibility for the first phase of the Program. This restriction constrains the sample to employers that were significantly negatively affected by the COVID-19 pandemic, and excludes employers that may have qualified for the first phase of the program based on *anticipated* declines in turnover that did not eventuate. It also excludes eligible employers that satisfied the smaller decline in turnover threshold for charities and not-for-profits. Collectively, these restrictions help mitigate potential endogeneity issues associated with firm employment and wage outcomes varying based on underlying economic conditions or employer performance unrelated to receiving the JobKeeper Payment. Second, the dataset was constrained to firms with less than $1 billion in GST turnover in each of the 2018-19, 2019-20 and 2020-21 income years. The intention here is again to limit the extent to which employer (in)eligibility was related to changes in contemporaneous income.

Within the constrained sample used for estimation, a firm's eligibility for JobKeeper was determined by the type of business undertaken (i.e. banks and public universities excluded), business ownership (Australian, state, local and foreign government owned and controlled businesses were excluded), and additionally the number and type of eligible employees determined at dates before the announcement of the program as a whole, and the two extension periods respectively. These factors are all plausibly exogenous to contemporaneous economic conditions, reducing the scope for endogeneity bias. However, even in the constrained sample there is still potentially some scope for bias- even although all employers in the sample experienced similar persistent declines in turnover related to the pandemic, some employers still would have done better than others, or had particular characteristics that may have affected their ability to ride out the pandemic. Although in Table 2 below we demonstrate that treated and untreated employers experienced statistically indistinguishable declines in turnover during the relevant testing periods. Further, with the availability of pre-program data we can check whether jobs and wages growth in treated and untreated firms were statistically indistinguishable prior to the implementation of the program. In practice it will be demonstrated that within the constrained sample, and subject to relevant controls, the parallel trends assumption is satisfied. We also control for state, industry and employer size specific time trends which help support internal and external validity; and interacting time dummies with all right hand side variables helps control for other confounding factors determined above the state level such as monetary policy, exchange rates, and national fiscal policy.

Advantages of the identification strategy proposed include that it allows for the direct estimation of program effects covering the entire program period utilising broadly representative administrative data. Unlike Bishop and Day (2020), it allows for the evaluation of JobKeeper employment effects at the employer as opposed to the employee level. The program data allows for the precise identification of which firms received JobKeeper, and how much each individual firm received, helping to address concerns regarding heterogeneous treatment effects in other similar studies. STP data for payroll jobs and wages also provides a more comprehensive and accurate data source than is available in labour force and business surveys, and private payroll data. We further address matters regarding the representativeness of the restricted sample and external validity in the detailed data description below.

We then estimate the following generalised dynamic Difference-in-Differences (DiD) model on the constrained sample:

$$y_{i,t} = \alpha + \delta_{s,j,t} + \lambda_{JK} + \kappa_{CFB} + \sum_{t \in T \neq 2020m3} \beta_t (JK_i \times \delta_t) + \sum_{t \in T \neq 2020m3} \gamma_t (CFB_i \times \delta_t) + \epsilon_{i,t} \quad (1)$$

where $y_{i,t}$ is either average weekly jobs or total wages for firm i at month t indexed to unity in the March quarter of 2020. $\delta_{s,j,t}$ is a set of state-by-industry-by-month fixed effects. These controls are important to capture the differential effects the COVID pandemic had by industry, taking into account geographic differences related to the different border and infection control policies adopted by different Australian states and territories. In Appendix Tables A1 and A2 we expand the control set to include additional controls related to firm size as a robustness exercise. As discussed, the interaction of all right hand side variables with time dummies also controls for factors that may have influenced employer payroll employment and wages determined above the state level such as monetary policy, exchange rate movements, and the average effects of other Commonwealth economic security measures.

λ_{JK} and κ_{CFB} are indicator variables for firms receiving the JobKeeper Payment, and SME Cashflow Boost payments respectively. JK_i and CFB_i reflect the total JobKeeper and Cashflow Boost payments received by firm i normalised by average weekly jobs or total wages in the March quarter of 2020. By interacting the time dummies δ_t with JK_i the β_t can be interpreted as time-varying treatment multipliers associated with both past and future expected receipt of JobKeeper payments with respect to average weekly payroll jobs or wages during each month t. $\delta_{s,j,t}$ and δ_t are all set equal to zero in March 2020, such that JobKeeper employment or wage multipliers are assessed relative to the last month before the program was implemented. Similarly, the γ_t are time-varying controls for past and future expected SME Cashflow Boost receipts during the JobKeeper program period. Given the substantial size of SME Cashflow Boost subsidies, contemporaneous program timing, and the significant crossover in entitlement under each program, JobKeeper Payment effects are likely to be overstated if the direct benefits of SME Cashflow Boost payments are not controlled for.

Estimation is analytically weighted by average weekly payroll wages or jobs for the March quarter of 2020 respectively depending on the relevant dependent variable. Results can therefore more precisely be interpreted as reflecting the effect of JobKeeper spending on average weekly payroll jobs or wages for the entire sample in month t, rather than the impact of JobKeeper receipt on average weekly payroll jobs or wages for the average employer in the sample during month t. Following Autor et al. (2020), standard errors are clustered by industry based on the Australia and New Zealand Industry Classification 2006 (ANZSIC).

5 Detailed Data Description

Payroll jobs and wages data is obtained from the ATOs Single Touch Payroll (STP) dataset for the 18 months between January 2020 and June 2021. This period includes three months before and after the 12 month JobKeeper program period which ran between 30 March 2020 and 28 March 2021. Employers with more than 20 employees started transitioning to the STP system on 1 July 2018, and by the start of the JobKeeper Program period in April 2020 99 per cent of these employers reported via the STP system. Employers with less than 20 employees were required to transition from 1 July 2019, and by April 2020 70 per cent of these employers were reporting via the STP system (Gruen, 2020). As a consequence, during the JobKeeper program period many small businesses in particular were still in the process of transitioning to the STP system. Therefore, to control for the possibility of reporting related effects, the empirical strategy in this paper focuses on a balanced panel of firms that are represented in the data throughout the entire 18 month period. STP data also does not cover self-employed individuals and unincorporated enterprises, who account for around 9 per cent of employees covered by the labour force survey (ABS, 2020c). Despite these limitations, the STP data provides very comprehensive coverage of wage and payroll employment in Australian businesses.

An important point to observe is that the payroll jobs data reflects individual jobs in the economy rather than employment as measured in traditional labour force surveys which treat multiple job holders as singularly employed. About 6 per cent of Australian employees have multiple jobs (ABS, 2020c). This is important to take into account when comparing the results to other studies that focus mainly on employment, such as those in the literature related to geographic cross-sectional multipliers (see Chodorow-Reich, 2019 for a comprehensive survey). Given multiple job holders, the results most likely *overstate* the effect of the Program on employment as traditionally defined (ABS, 2020c). Data is available at a weekly frequency, although given the significant size of the dataset, total weekly jobs and payroll wages data for each employer was aggregated on a monthly average basis. Monthly average weekly payroll jobs and wage data was then indexed to equal 1 based off the March quarter of 2020.

JobKeeper and SME Cashflow Boost spending data at the employer level was constructed based on administrative datasets for the respective programs obtained from the ATO and accessed via the ABS DataLab environment. This is the first academic study to be undertaken using these unique program datasets. For both programs payments were aggregated at the employer (ABN) level, and normalised based on average weekly payroll jobs or wages for the March quarter of 2020, depending on the relevant dependent variable used in estimation. Thus the primary explanatory variables can be interpreted as total program spending per worker or as a proportion of total wages based on the March quarter of 2020.

For the initial phase of the JobKeeper Payment, administrative data identifies when a firm became eligible for JobKeeper, and when it stopped receiving JobKeeper, as well as firm Australian Business Number (ABN) identifier, as well as the number of employees eligible for JobKeeper per firm. Given the fixed rate of the payment, and the fortnightly payment schedule, this enables the calculation of aggregate entitlements at the firm level. For the first and second JobKeeper Payment extensions the administrative data identifies payment fortnights, whether the employee was a tier 1 (full-time) or tier 2 (part-time) employee, as well as the total JobKeeper Payment amount received by each firm. The identification of payment fortnights in the data makes it possible to identify firms that received payments under each extension period. From the data provided, $91.7 billion in total program disbursements were identified as at August 2021. Differences related to different data vintages, ATO compliance activity, and voluntary repayments that grew over time, meant that net cash payments under the program were eventually smaller at around $88.2 billion according to the ATO based on more recently available data (as at April 2022).

The SME Cashflow Boost data are derived from ATO BAS data between March and September 2020 received from the ATO in May 2021, and accessed via the ABS Datalab environment. The data identifies the first (March-June 2020) and second (June-September 2020) cashflow boost instalments received by eligible businesses, and the times these payments were received. The data is aggregated at the employer (ABN) level for the purposes of estimation. The dataset includes $35.5 billion in SME Cashflow Boost Payments, which reflects all spending undertaken under this program.

Location and industry fixed effects are constructed based on data contained in the Business Longitudinal Analysis Data Environment (BLADE) ABN Level Database which can be linked to STP and Program administrative data on an ABN basis. The ABS translates business location data obtained from multiple administrative datasets to Australian Statistical Geography Standard 2016 geographic locations. Data is sourced from the Australian Business Register, ATO client data, webscraping from company websites, and state workers compensation insurance agency datasets. Locations data in BLADE is regarded as experimental in nature, it is not comprehensive, and there can be issues regarding the treatment of firms with multiple trading locations. Although in practice issues concerning multiple locations are more problematic when data is used at levels of aggregation below the state and territory level utilised in this paper.

In practice public health restrictions during the pandemic varied at the state and territory government level, because within the Federation this level of government is largely responsible for health policy. It is also the case that different states and territories had different degrees of success in managing the pandemic, which is also expected to influence firm payroll employment and wage levels. Therefore, utilising state-level location data to construct state-by-industry-by-month fixed effects is the most pertinent for controlling for trends related to differential public health measures and pandemic effects. Industry classifications are determined based on the primary 19 Australian and New Zealand Standard Industrial Classification (ANZSIC) 2006 industry classifications, which are linked to ABNs in the BLADE ABN Level Database available to researchers via the ABS DataLab. For the purposes of determining eligibility based on the quarterly actual turnover test and the $1 billion turnover threshold, firm turnover data at the quarterly and annual frequencies is obtained from the BLADE ABN Level Database also.

As an additional robustness exercise models are also estimated with the inclusion of employer size-by-month fixed effects (Appendix Table A1) and employer size-by-industry-by-month and size-by-month in place of state-by-industry-by-month and state-by-month fixed effects respectively (Appendix Table A2). Here employer size is defined in relation to ABS business size classifications, with micro-businesses regarded as employing less than 5 people; a small business employing between 5 and less than 20 employees; a medium business between 20 and less than 200 employees; and a large business 200 or more employees.

Table 1 provides an overview of key descriptive statistics. In particular the table sets out descriptive statistics for the constrained sample used in estimation, and compares these to those for all employers receiving JobKeeper, and the entire employer population over the period of investigation. The first point to observe is that of the 94,428 observations in the sample, there are 91,116 observations for JobKeeper treated employers, and 3,312 for untreated employers. Employers within the sample tend to have more employees than those receiving JobKeeper and the broader population, while average weekly wages tend to be higher than the JobKeeper population on average, and slightly below the population average. Within the group of employers receiving JobKeeper roughly half received SME Cashflow Boost. Likewise, within the group of employers receiving the SME Cashflow Boost, roughly half received JobKeeper. However, within the constrained sample of 5,246 employers used for estimation, 5,081 received the SME Cashflow Boost, and 5,062 received JobKeeper. Employers within the sample were more likely to receive both payments than JobKeeper and Cashflow Boost recipients more broadly, highlighting the importance of controlling for Cashflow Boost receipt within the sample used for estimation to help isolate the employment and wage effects of the JobKeeper Program. Employers within the sample received higher JobKeeper amounts than average for JobKeeper recipients more broadly, owing to the fact that in practice treated firms within the sample received JobKeeper in all three parts of the program period, but also due to the fact that they have more employees on average compared to the JobKeeper and population averages. They tended also to have higher levels of SME Cashflow Boost receipts on average, which is primarily a function of them having more employees on average than JobKeeper employers more broadly, and the overall employer population.

Table 1: Descriptive Statistics

	Obs.	Mean	Std. Dev.	Med.	Min	25^{th} Cent.	75^{th} Cent.	99th Cent.
Firm Weekly Payroll Jobs (Month Average)								
Sample	94,428	20	409	5	1	3	11	127
JK rec.	7,459,870	10	78	3	1	2	8	101
Pop.	11,827,318	17	385	3	1	2	8	166
Firm Weekly Total Payroll Wages (Month Average)								
Sample	94,428	22,781	420,276	5,023	0	2,120	12,357	177,763
JK rec.	7,459,870	11,945	101,897	3,208	0	1,360	7,950	136,781
Pop.	11,827,318	22,934	532,307	3,147	0	1,276	8,096	242,459
Total SME Cashflow Boost Receipts								
Sample	5,081	59,409	34,744	54,856	20,000	20,000	100,000	100,000
JK rec.	423,193	47,792	32,927	29,184	10,000	20,000	83,744	100,000
CFB rec.	815,173	43,480	31,824	21,320	10,000	20,000	67,704	100,000
Total JobKeeper Payment Receipts								
Sample	5,062	308,629	704,180	142,950	12,200	71,700	299,850	2,626,632
JK rec.	1,071,098	85,431	920,548	30,900	1,500	19,500	55,800	834,902
CFB rec.	502,318	115,243	385,745	55,800	1,500	30,000	102,300	1,026,658

Notes: For weekly payroll jobs and total payroll wages, the Sample is restricted based on satisfying (or failing) the actual turnover test during all three JobKeeper quarters, as well as turnover of less than $1 billion in all income years between 2018-19 and 2020-21. This reflects the sample used in estimation below. For total SME Cashflow Boost and JobKeeper receipts Sample refers to firms within the sample used for estimation that received the SME Cashflow Boost and JobKeeper Payments respectively. JK rec. and CFB rec. constrains the population to only include firms that received JobKeeper and the SME Cash Flow Boost respectively. Population is all available data. Total SME Cash Flow Boost and JobKeeper is cross-sectional data summing total firm receipts under each program. Source: ATO and ABS.

Figure 3 indicates the proportion of average weekly payroll employment and wages in the March quarter of 2020 attributable to each ANZSIC 2006 industry category. Employers from all industry categories are represented in the sample used for identification purposes, although there are clearly industries that are over and under represented with respect to payroll jobs or wages in the sample relative to JobKeeper Payment recipients more broadly, and the general firm population. Employers in the education and training and healthcare sectors are generally overrepresented in the sample relative to JobKeeper recipients more broadly, and accommodation and food services providers, administrative and support services, and arts and recreation services under-represented relative to overall JobKeeper recipients, both with respect to payroll employment and wages. However, employment and wages for accommodation and food services, administrative and support services, health care and social services businesses in the sample are closer to the population average. That leaves education and training providers as being overrepresented in employment and wages terms both relative to JobKeeper participants, and the broader business population. This is not unexpected on the basis that education providers are likely to be amongst the most significantly impacted by COVID-related health and travel restrictions- whilst also being more institutionally resilient to shocks in comparison to hospitality, arts and recreation businesses that are dominated by smaller, less resilient enterprises. Controls for industry-by-month and state-by-industry-by-month fixed effects are used in estimation to account for potentially divergent trends related to industry grouping.

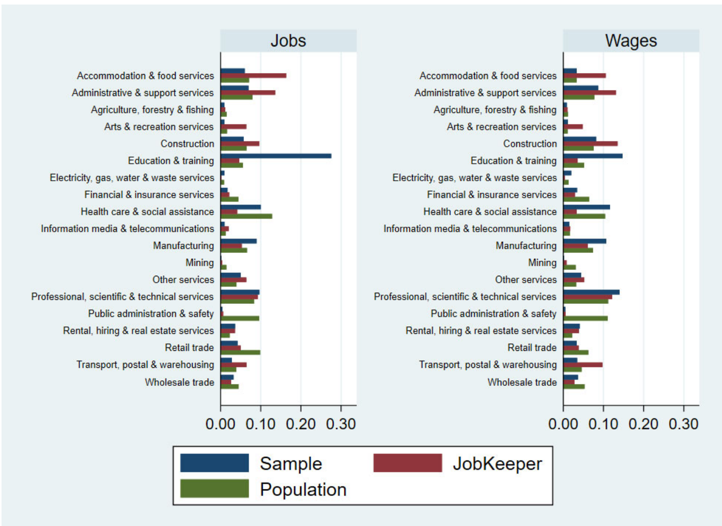


Figure 3: Proportion of Payroll Jobs and Wages by Industry, Source: ATO and ABS

Figure 4 provides indications of the representativeness of the sample used for identification purposes relative to firm state and territory location. It indicates the proportion of average weekly payroll jobs and wages in the March quarter of 2020 attributable to each particular Australian state and territory. All states and territories are represented in the constrained sample used for identification purposes, although South Australian employers in particular are overrepresented in the sample, particularly with respect to payroll jobs. This again reinforces the importance of including state-by-month fixed effects to control for particular trends affecting Australian states and territories differentially.

Figure 4: Proportion of Payroll Jobs and Wages by State, Source: ATO and ABS

Figure 5 indicates the representativeness of the sample with respect to employer size. Size representativeness with respect to payroll jobs and wages is as expected given how the sample is constructed. Notably there are relatively fewer micro, small and medium businesses in the sample compared to all firms receiving JobKeeper, and relatively more large businesses. This is expected given that a balanced sample is employed in estimation, and firms are required to have experienced a decline in turnover of between 30 to 50 per cent. Clearly larger employers are more likely to be able to absorb such significant declines in turnover relative to smaller employers as indicated by Andrews et al. (2021a) and Andrews et al. (2021b). Further, there are fewer large employers in the sample relative to the overall population on account of the requirement that employers within the sample had turnover of less than $1 billion in each income year between 2018-19 and 2020-21. Nonetheless, firms of all size categories are represented in the sample used for estimation.

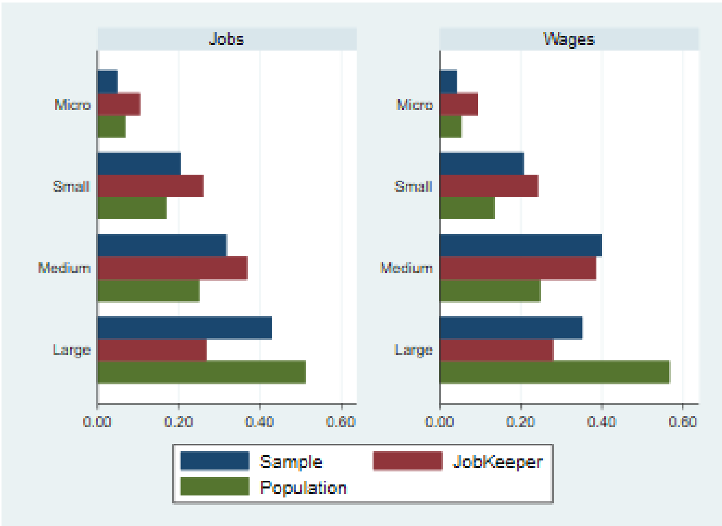


Figure 5: Proportion of Payroll Jobs and Wages by Size, Source: ATO and ABS

Zooming in on treated and untreated groups within the sample used for estimation, Figure 6 indicates that all industries are represented in the treated and untreated groups, with the exception of the Electricity, gas, water and waste water industry that is not represented in the untreated group. The education and training and health care and social assistance industries are also overrepresented in the untreated group. This is unsurprising given rule changes to the JobKeeper program to exclude public universities, and also the fact that state government owned entities including many public hospitals for instance, were excluded from JobKeeper. Including state-by-industry-by-month and industry-by-month fixed effects helps control for differences in performance between treated and untreated firms that may be related to different industry representativeness.

Further, looking at geographic representativeness between the treated and untreated groups, all states and territories are represented in each group (Figure 7). There is a preponderance of untreated employers in South Australia, and again the inclusion of state-by-industry-by-month and state-by-month fixed effects in estimation helps control for different trends in payroll jobs and wages between different geographic locations.

Finally, large businesses are proportionally overrepresented in the untreated group compared to the treated group (Figure 8); and conversely micro, small and medium employers are proportionally overrepresented in the treated group as would be expected. As dis-

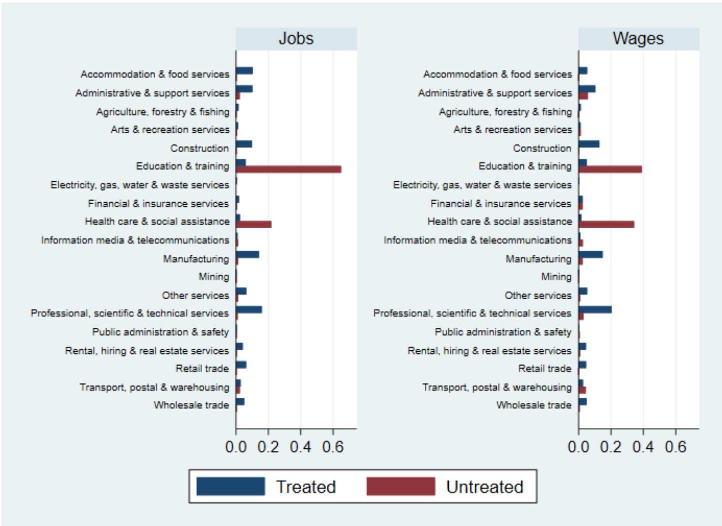


Figure 6: Treated and Untreated Groups Proportion of Payroll Jobs and Wages by Industry, Source: ATO and ABS

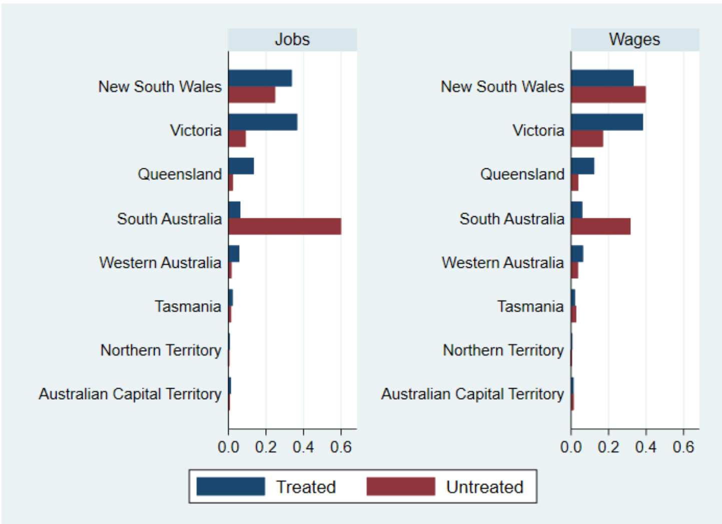


Figure 7: Treated and Untreated Groups Proportion of Payroll Jobs and Wages by State, Source: ATO and ABS

cussed, additional robustness exercises are performed with empirical specifications adding size-by-month fixed effects to each equation 1 (Appendix Table A1); and replacing state-by-industry-by-month and state-by-month fixed effects with size-by-industry-by-month and size-by-month fixed effects respectively (Appendix Table A2). As will be demonstrated, estimates where additional controls for size are incorporated into the empirical specification do not yield statistically significantly different results to those that do not include these controls. This appears to be mainly due to significant overlaps between location, industry and size characteristics in the untreated cohort.

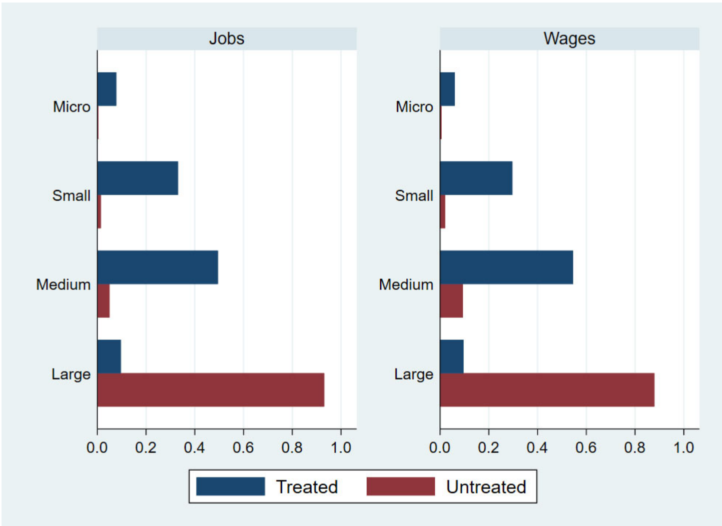


Figure 8: Treated and Untreated Groups Proportion of Payroll Jobs and Wages by Size, Source: ATO and ABS

Table 2 indicates that overall, turnover declined by a similar percentage in the treated and untreated groups. Turnover declines are virtually identical in the initial reference quarter for JobKeeper Payment eligibility, and are also similar in the second two reference periods. The first reference quarter (June 2020) relates to eligibility for the initial JobKeeper program, the second reference quarter (September 2020) relates to eligibility for the initial JobKeeper Payment, and the first extension period; and the third reference quarter (December 2020) relates to eligibility for the second extension period. In the second two reference quarters, turnover declines slightly more in the untreated group compared to the treated group. This is to be expected, given that we would expect employers in receipt of JobKeeper would have a trading advantage over those that did not. However, these differences are small in practice, amounting to well within one standard deviation in all cases, and the turnover differences between treated and untreated employers are statistically indistinguishable overall.

Table 2: Decline in Turnover Relative to Corresponding Quarter in Preceding Year (Per Cent)

	June 2020			**September 2020**			**December 2020**			**Overall**	
	Mean	Med.	Std. Dev.	Mean	Med.	Std. Dev.	Mean	Med.	Std. Dev.	Mean	Std. Dev.
Treated	-39.03	-38.52	5.79	-37.41	-36.19	5.62	-36.76	-35.29	5.50	-37.73	3.65
Untreated	-39.11	-38.31	5.46	-39.41	-39.47	5.67	-39.57	-39.20	5.32	-39.36	3.55

Notes: Source: ATO and ABS.

Finally, an additional question concerns the external validity of estimates of JobKeeper Program effects based on the constrained sample used for identification purposes. Overall, during the duration of the JobKeeper program roughly 26 per cent of employers experienced declines in turnover equivalent to those experienced by the employers in the sample used for estimation, compared to 45 per cent having turnover changes above -30 per cent compared; and 28 per cent experiencing declines in turnover of below -50 per cent respectively. During the initial JobKeeper period around 20 per cent of employers experienced turnover declines equivalent to employers in the sample used for identification purposes; however, this increased to around 44 per cent during the two extension periods with the introduction of the *actual* decline in turnover requirement. This reflects the fact that employers in the sample used for estimation are representative of for-profit firms that were *intended* to be the most numerous beneficiaries under the program if the turnover rule applied on an actual rather than projected basis during the initial phase of JobKeeper.

Interestingly, during the initial JobKeeper program period, on average 58 per cent of employers had turnover changes greater than -30 per cent compared to the same quarter in the preceding year, which declined to only 12 per cent on average in the two extension periods. Remembering that not-for-profit entities had a lower decline in turnover threshold than for-profit employers, and in the first phase of the JobKeeper program employers could satisfy the turnover test on an anticipated basis, and with respect to monthly declines in turnover compared to the same month in the preceding year and not only on a quarterly basis. This comparison reinforces Treasury (2021) analysis that many firms participating in the initial program period experienced more transient declines in turnover, or anticipated declines in turnover that did not materialise.

To the extent that individual firm performance within and outside of the restricted sample relates to common factors influencing firms at the macroeconomic level, or in their particular industry and/or location- the interaction of all right hand side variables with time dummies and controls for state-by-industry-by-month fixed effects provides added confidence concerning the external validity of the results from estimation.

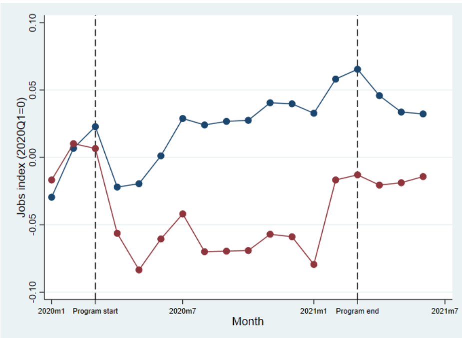
Within the constrained sample, our estimation strategy approximately holds income constant between employers, helping address potential endogeneity bias. While observed program effects are effectively independent of turnover within sample, it may still be the case that firms experiencing different declines in turnover were differently affected by the program. For example, one argument may be that employers experiencing changes in turnover above -30 per cent would be less likely to change employment decisions due to receiving the wage subsidy. While this is possible, it is instructive to compare our results below to those of Bishop and Day (2020), who's identification strategy would not be subject to the same question. Given that we focus on the firm level, and they on the individual employee level, we would generally expect to find costs per job-saved lower than they do over the corresponding period due to employment spillovers at the firm level. Given that firms experiencing changes in turnover above -30 per cent on a quarterly basis were predominant in the initial program period, if our results were biased in this way, we would expect our estimates would deliver significantly lower estimated costs-per-job-year saved again during this period. However, below we actually find costs per job-year saved of around $130,000 in our preferred empirical model over the first four months of the program compared to $100,000 in their model, inconsistent with the notion that we may be over-estimating program effects due to non-random selection.

Another question concerning external validity relates to the fact that the way that the sample is selected makes it more likely that employers within the sample will receive JobKeeper persistently throughout the three separate phases of the program, and that this might contribute to the observation of persistent employment and wage effects related to the program. However, in practice, 99.2 per cent of employers receiving JobKeeper in the first extension period participated in the initial JobKeeper program. Further, 84.2 per cent of employers participating in the second extension also participated in the first extension, and 83.6 per cent of employers in the second extension participated in both the initial program and first extension. So the persistence of JobKeeper receipt within the sample is not unusual or unrepresentative of how the program operated as a whole.

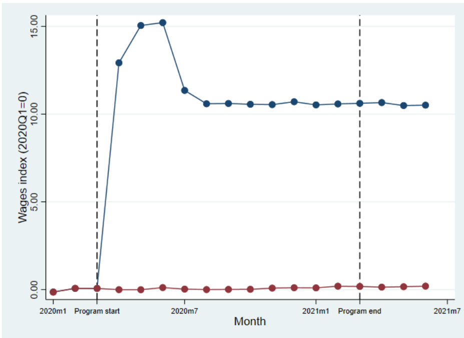
Overall, employers within the sample are broadly representative of a significant number of employers participating in the program as a whole; while controlling for turnover within sample, and common factors operating at the macroeconomic, sub-national and industry level provides added confidence regarding external validity. Nonetheless, different results cannot be ruled out using different data or identification approaches. Regardless, there is significant policy and empirical merit in assessing internally valid employment and wage effects within a sample of employers that have characteristics consistent with a significant proportion of program participants, and who are highly representative of employers the program was primarily aiming to support.

6 Empirical Results

A preliminary means to get a sense of the effects of JobKeeper on payroll jobs and wages is to graph the average jobs and wage indexes for the treated and untreated employers in the estimation sample. Figure 9 indicates a clear difference in the dynamics of payroll jobs and wages between the treated and untreated employers during the program period. It is also immediately apparent that JobKeeper had a much clearer and more immediate effect on employers average weekly wage payments than on firm employment. This is to be expected given that the program constituted a wage subsidy to employers, while employees with certain characteristics- most notably casuals employed with less than 12 months employment tenure, and temporary migrants- were not covered by JobKeeper. Further, employers were not required to maintain employment at any specified level in order to qualify for the Payment. Employers in the treatment and control groups both saw average jobs decline substantially between March and April 2020, although for the untreated group this decline was more significant. Treasury (2020) report similar employment trends in JobKeeper recipient and non-recipient firms more broadly. A sustained jobs recovery commenced in treated employers from May 2020, whereas in untreated employers there was a brief recovery period in June and July 2020. In untreated employers average jobs then returned to a lower level until towards the very end of the program period in February and March of 2021, with average jobs converging slowly towards their March quarter 2020 levels in the post-JobKeeper period. Overall, there appears to be slightly more convergence between treated and untreated groups in the jobs data compared to the wages data. Wage effects in treated employers appear more pronounced in the first three months of the program period, before levelling out. When combined with the evidence that it took four months for average job levels to return to pre-pandemic levels in the treated firms- there is an implication that in its initial phase the JobKeeper Payment benefited relatively more higher wage/ productivity workers than was the case during the extension periods. The wages data does suggest that more lower-wage employees began to benefit from the support provided by JoKeeper around the middle of 2020, once the public health situation had come under control in Australia. This inference is due to the fact that the impact of JobKeeper on wages began to decline before payment rates were reduced in the December quarter of 2020, and again in the March quarter of 2021. While Andrews et al. (2021a) and Andrews et al. (2021b) suggest that higher productivity firms benefited more under the initial phase of the JobKeeper Payment, the evidence presented here suggests that it may also have been higher productivity workers *within* treated firms that benefited the most under the initial JobKeeper Payment.



(a) Average Jobs Index



(b) Average Wages Index

Figure 9: Jobs and Wages in JobKeeper (Blue) and Control (Red) Firms, Source: ATO and ABS

Table 3 below summarises the results from estimation. Columns (1) to (3) contain parameter estimates for selected payroll jobs equations, and (4) to (6) estimates for selected payroll wages equations. Columns (1) and (4) contain the preferred model specification for payroll jobs and wages respectively. These equations include controls for receipt of SME Cashflow Boost payments, and state-by-industry-by-month fixed effects as per equation 1. Equations (2) and (5) include state-by-industry-by-month fixed effects as controls, while equations (3) and (6) include state-by-month and industry-by-month fixed effects as controls only. As can be seen in column (1), for the payroll jobs equations including controls for the SME Cashflow Boost program is important to help satisfy the parallel trends assumption that requires parameter estimates on pre-program periods to be not significantly different from zero. Controls for state-by-industry-by-month fixed effects also help satisfy identification requirements in comparison to separate state-by-month and industry-by-month fixed effects. In the payroll jobs equations with additional controls related to employer size specific time trends in Appendix Table A1 and Table A2, the parallel trends assumption is satisfied with greater assurance in all equivalent equations.

The program period costs per job-year saved, that is the cost of saving a job for a period of one year, is estimated to be around $112,819 in the preferred model, which is equivalent to roughly $US80,959 translated at the average daily exchange rate during the program period (0.7176 Australian dollars per US dollar). The robustness exercises adding size-by-month fixed effects (Appendix Table A1) and replacing state-by-industry-by-month and state-by-month fixed effects with size-by-industry-by-month and size-by-month fixed effects respectively (Appendix Table A2) to the preferred model provide statistically indistinguishable results, also indicating costs per job-year saved of around $110,000 ($US78,936) for the program period. On a monthly basis, costs per job saved progressively declined during the first phase of the Program, while average costs per job saved were lowest during the second JobKeeper extension, followed by the first extension, and then the initial program period. Given that the jobs multipliers capture the impact of all past program spending, and future expected program spending on treated firm employment within the given month, it is not possible to distinguish average cost per job estimates for each sub-period, suffice to say that costs per job-year declined over the program period. This could be partly attributable to the fact that the two program extensions were more tightly targetted than the original program model, but equally these effects could be interpreted as relating to persistent benefits associated with early program spending, and/or small incremental benefits associated with qualifying for each extension. Indeed, the degree of persistence of employment benefits seems to be a defining characteristic of the Program with significant employment benefits extending into the post-program period. Therefore, estimates of costs per job and job-year saved under the program should be viewed as conservative and preliminary in nature, given that at the time of writing, program benefits are likely continuing to be experienced. In the three months where post-program data has been made available, jobs multipliers remain stubbornly high and statistically significant. Therefore, over time, cumulative estimates of costs per job-year saved are likely to decline further. However, based on the identification approach used a challenge will be how much of the treatment and control groups will survive in future periods to enable a more conclusive analysis. The preferred model suggests that there were over 812,000 more jobs averaging a year in duration during the program period as a consequence of the program. Again, this is likely a conservative estimate of the overall employment benefits of the program, as employment benefits continued into the post-program period. Explicitly controlling for SME Cashflow receipts reduces estimates of jobs saved during the program period by roughly 90,000 jobs, indicating the importance of controlling for the direct benefits of this program for identifying JobKeeper Payment effects.

As would be expected based on Program design and what was observed in Figure 9, the impact of the JobKeeper Payment on average weekly wages was greatest during the initial program period where the payment was a flat $1,500 for all eligible employees. However, the impact on average weekly wages appears to decline considerably after the first three months of the program, which is essentially half way through the initial program period. There then appears to be a gradual decline in the wage multiplier from the fourth month of the program and then throughout the first and second JobKeeper extensions. This would be consistent with more workers in lower paying cohorts on average such as short-tenured casuals and temporary migrants being excluded from the Program, but then regaining employment at supported firms at a faster pace than unsupported employers as the public health crisis abated. Overall, parameter estimates are more precise in the wage equations, and the wage models explain a much larger share of variation in the data for total wages than do the job equation equivalents. This is expected given that the program was a wage subsidy that directly flowed through to employer wage bills, while not all employees were eligible for the wage subsidy, and jobs losses were more concentrated amongst ineligible employees (see Treasury, 2020).

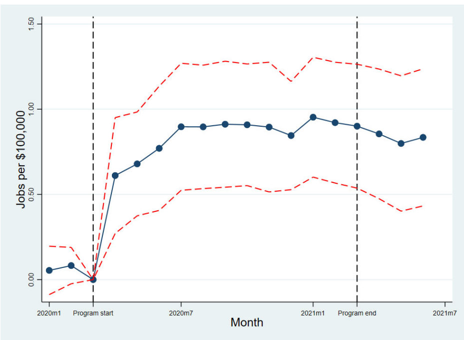
The preferred model in column (4) suggests that average weekly wages for employers were $1,160 higher per $100,000 in JobKeeper spending over the program period, and in aggregate almost $1.1 billion ($US761 million) higher throughout the program period thanks to the JobKeeper Payment. This implies wage benefits equivalent to roughly 60 per cent of program spending during the program period. Results for wages are slightly lower, although

Table 3: Effect of JobKeeper on Payroll Jobs and Wages

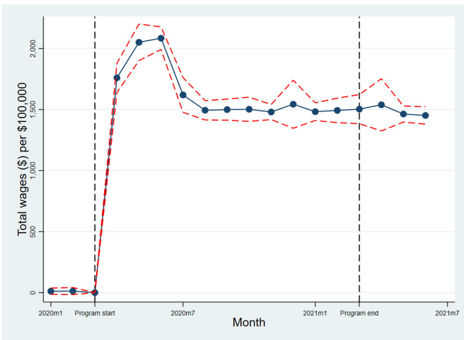
	Increase in Jobs per $100,000			Increase in Wages per $100,000		
	(1)	(2)	(3)	(4)	(5)	(6)
	JK per Worker × Month FE			JK to Wage Ratio × Month FE		
Jan 2020	0.10	0.13	0.26	-8.58	14.41	30.06
	(0.08)	(0.08)	(0.11)	(34.09)	(14.95)	(30.62)
Feb 2020	0.12	0.15	0.26	21.36	15.09	29.14
	(0.06)	(0.06)	(0.09)	(35.46)	(10.21)	(30.19)
Apr 2020	0.65	0.71	0.74	1,254.35	1,764.77	1,767.62
	(0.19)	(0.17)	(0.16)	(155.73)	(57.85)	(54.51)
May 2020	0.71	0.79	0.82	1,514.20	2,054.76	2,059.16
	(0.17)	(0.16)	(0.15)	(149.06)	(74.30)	(69.42)
Jun 2020	0.81	0.90	0.92	1,570.90	2,092.55	2,097.60
	(0.20)	(0.19)	(0.18)	(154.31)	(43.48)	(39.66)
Jul 2020	0.94	1.04	1.04	1,165.29	1,624.64	1,628.43
	(0.21)	(0.20)	(0.19)	(128.46)	(75.94)	(77.68)
Aug 2020	0.94	1.04	1.02	1,149.41	1,498.36	1,502.80
	(0.20)	(0.19)	(0.19)	(96.67)	(43.17)	(45.14)
Sep 2020	0.95	1.05	1.04	1,101.09	1,504.04	1,509.11
	(0.20)	(0.19)	(0.19)	(112.54)	(46.40)	(49.42)
Oct 2020	0.95	1.05	1.03	1,085.42	1,506.72	1,509.98
	(0.20)	(0.19)	(0.19)	(145.20)	(52.58)	(54.07)
Nov 2020	0.93	1.03	0.99	1,130.86	1,484.26	1,482.37
	(0.21)	(0.20)	(0.20)	(121.12)	(33.11)	(31.95)
Dec 2020	0.88	1.00	0.97	1,024.56	1,550.11	1,536.09
	(0.17)	(0.17)	(0.17)	(255.89)	(104.53)	(94.24)
Jan 2021	0.99	1.10	1.09	1,003.94	1,488.00	1,477.84
	(0.19)	(0.19)	(0.18)	(172.96)	(39.15)	(35.74)
Feb 2021	0.96	1.07	1.03	977.43	1,497.39	1,482.60
	(0.19)	(0.19)	(0.19)	(211.70)	(52.82)	(47.75)
Mar 2021	0.93	1.04	1.00	953.81	1,507.06	1,491.17
	(0.12)	(0.19)	(0.19)	(229.83)	(62.26)	(55.46)
Apr 2021	0.89	1.00	0.95	822.41	1,544.21	1,531.03
	(0.21)	(0.20)	(0.20)	(347.39)	(111.92)	(103.02)
May 2021	0.83	0.93	0.88	1,095.02	1,467.91	1,452.52
	(0.22)	(0.21)	(0.21)	(186.09)	(33.53)	(38.20)
Jun 2021	0.86	0.94	0.89	1,083.86	1,456.05	1,441.20
	(0.22)	(0.21)	(0.22)	(185.36)	(35.87)	(43.51)
CFB	Yes	-	-	Yes	-	-
State-by-industry-by-month FE	Yes	Yes	-	Yes	Yes	-
State-by-month & industry-by-month FE	-	-	Yes	-	-	Yes
Obs.	94,428	94,428	94,428	94,428	94,428	94,428
Adj. R^2	0.35	0.33	0.30	0.82	0.81	0.77
Pre-period = 0 (F test p value)	0.19	0.06	0.02	0.37	0.64	0.62
JK1 period cost per job saved/ increase in weekly wages per $100k	120,077.50	108,704.90	107,715.0	1,292.54	1,756.52	1,760.79
	(26,755.60)	(20,550.37)	(19,654.82)	(109.59)	(20.67)	(58.25)
JK2 period cost per job saved/ increase in weekly wages per $100k	108,662.90	97,435.23	100,034.40	1,080.28	1,513.70	1,509.48
	(22,334.33)	(17,555.25)	(18,426.87)	(164.61)	(61.77)	(58.66)
JK3 period cost per job saved/ increase in weekly wages per $100k	104,206.70	93,674.92	96,238.76	978.39	1,497.48	1,483.87
	(20,951.98)	(16,472.12)	(17,236.40)	(202.43)	(50.92)	(45.78)
Program period cost per job-year saved/ increase in average weekly wages per $100k	112,819.10	101,685.70	102,683.20	1,160.94	1,631.05	1,628.73
	(23,698.80)	(18,390.25)	(18,461.50)	(137.28)	(33.46)	(32.05)
Program period jobs saved/ increase in average weekly wages ($m)	812,377.90	901,323.20	892,567.50	1,060.00	1,490.00	1,490.00
	(170,648.30)	(163,007.70)	(160,475.40)	(126.00)	(30.70)	(29.40)
Approx. program period Chodorow-Reich (2019) output/ implied wage multiplier	1.32	1.47	1.45	1.27	1.79	1.79

Notes: Estimation performed using analytic weights based on average weekly jobs/wages in the March quarter of 2020. For parameter estimates, standard errors in parentheses are clustered at the ANSZIC 2006 classification industry level. Cost per job-year saved=$100,000/(\sum_s^T(\beta_t/(T-s)))$ where s is the start of the period of interest, and T the end, and the increase in weekly wages per $100,000=$\sum_s^T(\beta_t/(T-s))$. Jobs saved over the program period=91651716200/100,000*$(\sum_s^T(\beta_t/(T-s)))$ with T and s defined accordingly, and the aggregate increase in average weekly wages is calculated in a synonymous manner for the wage equations. Standard errors for aggregated cost per job saved, jobs saved, and wage increase figures are calculated using the delta method.The output multiplier is calculated using the method of Chodorow-Reich (2019) where $\beta_Y \approx (1-\alpha)(1+\chi)(\text{Cost per Job-Year})^{-1}Y/E$, where $\alpha \approx 0.33$ and the elasticity of hours per worker with respect to total employment for Australia is $\chi = 0.47$ based on Dixon et al. (2004), which compares to a standard assumed value in US studies of $\chi = 0.5$. Implied wage multipliers relate the estimated annual increase in total wages divided by 2019 compensation of employees from the National Accounts, to the increase in JobKeeper spending relative to 2019 GDP also taken from the national accounts.

less precisely estimated and statistically indistinguishable, in the robustness exercises adding size-by-month fixed effects (Appendix Table A1) and replacing state-by-industry-by-month fixed effects with size-by-industry-by-month fixed effects (Appendix Table A2) to the preferred model. Although already relatively large, these results most likely understate program benefits, as there is evidence of persistent benefits of the JobKeeper Payment on firm average weekly payroll wages beyond the program period. The estimate of 0.89 job-years saved per $100,000 in program spending during the program period is consistent with the average effect on weekly payroll wages during the program period in column (4) which translates to roughly 0.90 of average weekly total earnings ($1,292.87 ABS, 2022b) per $100,000 of program spending during the program period. Likewise, taking average weekly total earnings for Australia during the program period and multiplying this by estimates of jobs-saved during the program period from column (1) of Table 3 suggests wage benefits of approximately $55 billion, or almost 60 per cent of program spending- again reinforcing the direct estimates from the wage equations. In this way the results from the wage equations reinforce those from the jobs equations and vice versa, which is reassuring. Figure 10 provides a graphical depiction of the jobs and wage multipliers from the preferred jobs and wages models in column (1) and (4) of Table 3 respectively.



(a) Jobs Multipliers



(b) Wage Multipliers

Figure 10: JobKeeper Payment Multipliers, Source: Author's Estimates, ATO and ABS

Using the method of Chodorow-Reich (2019) to map cost per job-year saved estimates to approximate 'closed economy, no monetary policy response' output multipliers suggests an output multiplier of around to 1.32 in the preferred jobs model. Taking the annualised increase in wages during the program period and dividing this by compensation of employees from the national accounts in 2019, and taking this as a ratio of JobKeeper spending as a proportion of GDP in 2019, provides an 'implied wage multiplier' of around 1.27 in the preferred model also. While these approximations point to output and income multipliers exceeding unity, it should be noted that the empirical methodology employed holds things like monetary policy and the exchange rate constant, so they cannot be strictly compared to open economy macroeconomic multiplier estimates. While interest rates were constrained by the effective lower bound throughout the program period, the Australian dollar appreciated consistently over the program period relative to the US dollar, as did the real trade weighted index, a broadly accepted measure of Australia's real exchange rate. The employment and output multiplier estimates do not take into account the marginal propensity to import, or potential crowding out effects operating via the appreciation of the real exchange rate. They also do not take into account 'Ricardian' considerations, although Chodorow-Reich (2019), Fahri and Werning (2016) and Nakamurra and Steinsson (2014) suggest that these are likely to be relatively modest in practice.

So how do these jobs and wage multiplier estimates compare to other studies in the literature? Owing to data availability issues, most comparable studies have focused on cost per job-year saved estimates. First, the cost per job-year saved estimate of around $112,819 is very close to the estimates of $100,000 estimate provided by Bishop and Day (2020) for the first six months of the program. Indeed, when the SME Cashflow Boost Program is not controlled for, the cost per job-year saved estimates are essentially identical. Building on the results of Bishop and Day (2020), this paper also highlights the surprising extent to which these effects persisted into the second six months of the program extension, and indeed even beyond the program period. The implication being that *cumulative* jobs and wage multipliers would be expected to grow larger when more data becomes available in the future. Results are also very close to estimates of costs per job saved of $107,900 (about $US80,500) over the first four months of the program suggested by Borland and Hunt (2021).

Estimates in this paper do however suggest that costs per job saved were possibly higher earlier in the program period than suggested by Bishop and Day (2020) and Borland and Hunt (2021). Indeed, writing in May 2020 Breunig and Watson (2020) suggested costs per job saved could range between $140,000 and $200,000 on an annualised basis, based on publicly available data at the time. Numbers closer to the lower end of this range were based on more realistic assumptions about people leaving the labour market due to the pandemic. Estimates from the preferred model in this paper suggest costs per job saved averaging around $150,000 in April and May 2020, within the range suggested by these back-of-the-envelope calculations if the employment effects observed at the time continued for the remainder of the program period.

Differences in estimates are to be expected based on the different reference periods, identification strategies, and empirical approaches used in each paper. Bishop and Day (2020) identify program effects based on observing different *employment* outcomes for groups of ineligible employees relative to eligible employees- mainly focusing on comparing short-tenured casuals either side of the 12 month cut-off. This paper on the other hand looks at differences in *payroll jobs* numbers in eligible versus ineligible employers, where the latter could relate to the absence of eligible employees at an employer, or other employer exclusion criteria (such as state or foreign government ownership for example). Overall, we are able to identify variation in employment based on the number of eligible employees (and therefore the amount of JobKeeper received by a firm on a per worker basis), as well as whether a business was eligible or not. This arguably provides a more comprehensive assessment of the impact of receiving the JobKeeper Payment on employer-level payroll employment.

Given the way the empirical analysis is structured makes it possible to provide costs per job saved estimates that can be directly compared to estimates in the literature regarding the US PPP that have tended have been expressed over disparate time horizons. Firstly, The cost per job-year saved estimate of $112,819 ($US80,959) over the program period is significantly lower than the most comprehensive comparable estimates for the US PPP being $US170-257,000 in Autor et al. (2022) and $US240-408,000 on an annualised basis in Dalton (2021). Over the first two months of the PPP, Autor et al. (2020) suggest a cost per job saved of US$224,000, while Granja et al. (2020) suggest a cost per job saved ranging between US$109-164,000. In contrast we find JobKeeper had a cost per job saved of $147,302 ($US97,403) over this period, below the lower end of the range suggested by both papers. Chetty et al. (2020) suggest a cost per job-year saved of $US377,000 during the first three-and-a-half months of the PPP, while over the corresponding period the cost per job saved under JobKeeper is estimated to be around $128,576 ($US87,756). Of the lower estimates, Doniger and Kay (2021) suggest a cost per job saved over the first six months of the PPP of $US43,000, compared to an average of $120,077 ($US83,580) for JobKeeper.

While, Bartik et al. (2020b) suggest a cost per job saved over the first month of the PPP of $US32-67,000; although their results are not statistically significant. The comparable figure for JobKeeper is a significantly higher $154,525 ($US101,395).

The finding that JobKeeper had relatively high costs per job saved at any point in time, alongside very persistent employment benefits, most likely relates to the fact that significant numbers of short-tenured casual employees and temporary migrants (who are often casuals and low paid) were excluded from JobKeeper, such that where jobs were saved, they were more concentrated amongst full-time ongoing employees. These employees are typically more highly paid, and therefore their jobs were also more costly to 'save'. On balance, the weight of evidence probably suggests that JobKeeper was more cost-effective than the PPP in terms of saving jobs. This was possibly due to differences in program design whereby the JobKeeper rules provided a greater direct incentive for employers to retain employees relative to the PPP, and were potentially also more tightly targeted towards businesses experiencing significant declines in turnover that otherwise may have been more likely to cease operations or lay off workers. It also potentially reflects the fact that more of the JobKeeper Payment constituted a transfer payment to liquidity constrained employees than under the PPP. When additional data becomes available, the cumulative estimates of costs per job-year saved are expected to decline further for JobKeeper as well.

The finding of wage benefits equivalent to 60 per cent of JobKeeper spending during the program period exceeds the findings of 23-34 per cent and 24 per cent in relation to the PPP from Autor et al. (2022) and Dalton (2021) respectively. This is perhaps to be expected given that JobKeeper was more tightly targeted towards supporting wages than the PPP, particularly of relatively higher earning permanent full-time employees. The finding of employment benefits between 2 to 5 times larger for JobKeeper compared to the PPP over 12 months Autor et al. (2022) and Dalton (2021) is also reflected in the distributional results with respect to wages, with workers benefiting roughly 2 to 3 times more as a proportion of overall JobKeeper Payments.

Drawing comparisons to non-COVID era fiscal policy episodes, Chodorow-Reich (2019) finds costs per job-year saved related to the American Reinvestment and Recovery Act (2009) ranging between $US26,316 to $US131,579, with a cross study mean of $US47,619, and a median of $US52,632. During other stimulus episodes summarised by Chodorow-Reich (2019), costs per job-year saved have typically been lower, ranging between $US19,608 and $US34,602.

In the Australian context, output multipliers calculated on the basis of Chodorow-Reich (2019) are in line with with output multipliers derived for the Australian economy during the GFC by Li and Spencer (2016) and Watson and Tervala (2021). Multipliers are slightly lower than the implied closed economy output multiplier of 1.7 for the GFC-era cash transfer program suggested by Leigh (2012), but higher than than would be implied by the negligible increase in non-durable consumption found in response to these payments by Aisbett et al. (2017). Watson (2021) found costs per job-year saved associated with the GFC-era Building the Education Revolution (BER) school infrastructure stimulus program of around $19,963 in 2009, albeit with relatively wide confidence intervals. Even where the program is assumed to have only benefited 25-34 year olds, the cost per job-year saved is a still low $31,981 for 2009, which is significant at the standard 95 per cent confidence level. These translate to closed economy, no monetary response output multipliers approximated using the method of Chodorow-Reich (2019) of about 4.1 and 2.6 respectively in 2009 alone. While these multipliers are large, they sit comfortably within the range of previous studies that focus on government infrastructure spending, notably Buchheim and Watzinger (2017) and Leduc and Wilson (2013). Chodorow-Reich (2019) output multipliers are also lower than implied output multipliers of around 4 for the Australian GFC era investment allowances found by Rodgers and Hambur (2018).

Dupor and McCrory (2016) and Dupor and Saif Mehkari (2016) find wage multipliers of 1.14 and 1 respectively for ARRA. While Buchheim and Watzinger (2017), Adelino et al. (2017) and Suárez Serrato and Wingender (2016) find wage multipliers of 1.9, 1.9 and between 1.7 and 2 respectively. The implied wage multipliers in this study sit comfortably within the range of previous estimates.

In the geographic cross-sectional multiplier literature, closed economy, no monetary response output multiplier estimates are typically larger than one (see Chodorow-Reich, 2019 for an extensive overview), although some papers find output multipliers lower than one (see Brückner and Tuladahar, 2014; Clemens and Moran, 2012; Fishback and Kachanovskaya, 2015; and Porcelli and Trezzi, 2019). Overall, the approximate closed economy, no monetary policy response output multiplier associated with the JobKeeper program period sits towards the middle-to-lower end of the estimates found in prior similar studies.

Collectively, the estimated costs per job-year saved associated with the JobKeeper program period are towards the middle-to-higher end (and conversely approximate wage and income multipliers towards the middle-to-lower end) of those found in non-COVID era studies, which it must be said did not relate specifically to wage subsidy programs. These findings appear consistent with results from the literature regarding wage subsidy and STW schemes surveyed above- to the extent that positive employment effects have been observed; however, there have been questions raised concerning the net benefits and relative cost effectiveness of these programs compared to alternative ALMPs and fiscal stimulus options. While it is not possible to conclude definitively, this may be partly related to specific circumstances surrounding the COVID-19 pandemic that may have rendered fiscal policy relatively less effective in the circumstances; and/ or it may be the case that wage subsidies are relatively less effective employment stimulus instruments relative to some other forms of government spending, such as public investment and transfer payments for instance, which Gechert and Rannenberg (2018) identify as typically having amongst the largest short-term multipliers in recessionary scenarios.

7 Theoretical Results: Relating Estimates of Costs per Job-year Saved to Approximate Output Multipliers

A question that commonly arises in the context of applied microeconometric program evaluation exercises such as the above, is how the relevant employment multipliers, or more commonly cost per job-year saved estimates, can be related to output multipliers where the lack of relevant data makes direct estimation impossible. As discussed, Chodorow-Reich (2019) demonstrates how multipliers derived from applied microeconomic empirical approaches such as the one employed in this paper can be thought of as directly relating to closed economy, no monetary response multipliers, and also suggests a method whereby costs per job-year saved estimates can be translated into approximate output multipliers.

A related challenge relates to how to generate closed economy, no monetary response multipliers in modern business cycle models that are both empirically and theoretically plausible. Leduc and Wilson (2013) and Nakamurra and Steinsson (2014) develop models that generate relatively large closed economy no monetary response multipliers utilising Greenwood, Hercowitz, and Huffman (1988) (GHH) preferences. While Cai et al. (2019) find some empirical support for GHH preferences, Auclert and Rognlie (2017) have observed that these preferences make fiscal multipliers effectively equivalent to the elasticity of substitution between intermediate goods where there is non-distortionary taxation of labour income- a parameter that would not seem to be practically relevant to the size of fiscal multipliers in the real world. Distortionary labour income taxes reduce the size of multipliers under GHH preferences- however, they often remain implausibly high with respect to empirical evidence regarding the fiscal instruments being examined. Therefore, a question remains how to generate plausible closed economy, no monetary response fiscal multipliers without resorting to GHH preferences.

In response to these challenges, and in order to provide a theoretical account of the relevant mechanisms that potentially generate approximate output multipliers derived in the preceding section, this section extends the textbook closed economy two agent New Keynesian (TANK) model to include public and private investment, deficit financed government spending, distortionary taxation, hysteresis in the production technology, and monetary policy constrained by the zero lower bound (ZLB). In this model the government levies distortionary taxes on labour, consumption and firm profits to fund government consumption, transfer payments to non-Ricardian households, public sector investment and a wage subsidy to firms. The environment features a continuum of infinitely lived workers and firms indexed by $z \in [0, 1]$, with the total population of workers and firms both normalised to unit size. It is also assumed that there is no population growth in the model.

7.1 Workers

A fraction $1 - \lambda$ of workers are assumed to have access to credit markets, and are able to smooth consumption over time. These workers are referred to as Ricardian workers. λ workers are liquidity constrained and can only consume out of current income and endowments. These are referred to as non-Ricardian workers. The utility function for Ricardian and non-Ricardian workers is identically given as follows

$$U_t(z) = E_t \sum_{s=t}^{\infty} \beta^{s-t} \epsilon_s^{TP} \left[logC_s - \frac{(N_s(z))^{1+1/\varphi}}{1+1/\varphi} \right] \tag{2}$$

Where E_t is the expectations operator, β is the worker's discount rate, ϵ_s^{TP} is a time preference shock that influences worker's inter-temporal consumption decision, C_t is an index of real consumer goods and services, $N_t(z)$ is workers' labour supply in hours, and φ is the Frisch elasticity of labour supply.

Ricardian workers have access to debt and asset markets, receive dividends from firms, and pay distortionary income and consumption taxes to Government. The nominal budget constraint for Ricardian workers is therefore given as follows

$$\begin{aligned} \frac{R_t^{-1} B_{t+1}}{1-\lambda} &= \frac{B_t}{1-\lambda} + (1-\tau_t^y) w_t N_{R,t} + \frac{(1-\tau_t^y)}{1-\lambda}(r_t^K K_t + v_t) \\ &- (1+\tau_t^c) P_t C_{R,t} - \frac{1}{1-\lambda}\left(P_t I_t + \frac{\phi}{2}\left(\frac{I_t}{K_t} - \delta\right)^2 K_t \right) \end{aligned} \tag{3}$$

Where $N_{R,t}$ and $C_{R,t}$ are the labour supply and consumption of Ricardian workers, B_t is the nominal price of government bonds with a pay off of \$1 dollar in period $t+1$, r_t is the nominal return on bonds, w_t is the nominal wage, v_t are financial returns of firms with full dividend imputation implying that these are taxed at the same rate as labour income, τ^y and τ^c are income and consumption tax rates respectively, r_t^k is the return to private capital, I_t is private investment, $\phi(\cdot) = \frac{\phi}{2}\left(\frac{I_t}{K_t} - \delta\right)^2$ are quadratic adjustment costs, and δ is the depreciation rate of private sector physical capital. Capital accumulation for Ricardian households proceeds in the usual manner

$$K_{t+1} = (1-\delta)K_t + I_t \tag{4}$$

The optimality conditions for Ricardian workers are given as follows

$$\beta R_t E_t \left(\frac{\epsilon_{t+1}^{TP}(1+\tau_t^c) P_t C_{R,t}}{\epsilon_t^{TP}(1+\tau_{t+1}^c) P_{t+1} C_{R,t+1}} \right) = 1 \tag{5}$$

$$N_{R,t}(z) = \left(\frac{(1-\tau_t^y)w_t}{C_{R,t}(1+\tau_t^c)P_t}\right)^{\varphi} \tag{6}$$

$$q_t = 1 + \phi\left(\frac{I_t}{K_t} - \delta\right) \tag{7}$$

$$q_t = E_t\left\{\Lambda_{t,t+1}\left[(1-\tau_{t+1}^y)r_{t+1}^K + q_{t+1}(1-\delta) - \phi_{t+1} + \left(\frac{I_{t+1}}{K_{t+1}}\right)\phi_{t+1}^{'}\right]\right\} \tag{8}$$

Where $\Lambda_{t,t+1} = \beta\left(\frac{C_{R,t}}{C_{R,t+1}}\right)$

Each non-Ricardian worker earns income from working for firms and transfer payments from government $\frac{G_t^T}{\lambda}$, and pays income and consumption taxes to government. However, they do not have access to credit markets, and therefore cannot smooth consumption over time. Further, they cannot borrow to invest in firms, and have no residual claims over firm profits. The non-Ricardian workers optimality conditions are therefore defined by their flow budget constraint, and their labour supply relation with the 'N' sub-script denoting 'Non-Ricardian'. The optimality conditions for non-Ricardian workers are therefore as follows

$$(1+\tau_t^c)P_tC_{N,t} = (1-\tau_t^y)w_tN_{N,t} + Pt\frac{G_t^T}{\lambda} \tag{9}$$

$$N_{N,t}(z) = \left(\frac{(1-\tau_t^y)w_t}{C_{N,t}(1+\tau_t^c)P_t}\right)^{\varphi} \tag{10}$$

Aggregate consumption and labour supply are therefore defined as follows

$$C_t = \lambda C_{N,t} + (1-\lambda)C_{R,t} \tag{11}$$

$$N_t = \lambda N_{N,t} + (1-\lambda)N_{R,t} \tag{12}$$

7.2 Firms

Following D'Alessandro et al. (2019) the production technology of firms is given as follows

$$Y_t(z) = K_t(z)^{\alpha}(N_t(z)X_t)^{1-\alpha}K_{G,t}^{\phi_g} \tag{13}$$

With $Y_t(z)$ representing the output of firm z, $K_t(z)$ and $N_t(z)$ representing physical capital and labour inputs respectively, $K_{G,t}$ is public capital, ϕ_g is the elasticity of output with respect to public capital, and X_t represents the skill level of the average worker. Therefore, according to the this production technology, worker productivity increases in the skill level of the average worker as in Chang et al. (2002). X_t is assumed to depend on the hours a worker has worked in the past reflecting learning-by-doing with a law of motion given as follows.

$$X_t = X_{t-1}^{\rho_x} N_{t-1}^{\mu_l}(z) \tag{14}$$

whereby ρ_x captures the persistence of past stock of human capital, and μ_l the impact elasticity of human capital to the level of employment in the previous period.

Under the assumption of competitive markets for factor inputs, cost minimisation implies a common capital-labour ratio

$$\frac{K_t(z)}{N_t(z)} = \frac{\alpha}{1-\alpha} \frac{(1-G_t^S)w_t}{r_t^K} \tag{15}$$

where w_t and r_t^K denote the nominal wage rate and rental cost of private sector capital respectively, and G_t^S is a government wage subsidy paid to firms that are assumed to follow an AR(1) process. Firm marginal cost can then be defined as follows.

$$MC_t(z) = \left(\frac{(1-G_t^S)w_t}{1-\alpha}\right)^{1-\alpha} \left(\frac{r_t^K}{\alpha}\right)^{\alpha} X_t^{\alpha-1} K_t^G(z)^{-\phi_g} \tag{16}$$

Firms maximise profits v_t where

$$v_t(z) = (p_t(z) - MC_t(z))y_t^d(z) \tag{17}$$

subject to the production technology (equation 13) and demand for its products from consumers and government.

$$y_t^d(z)) = C_t + I_t + \phi\left(\frac{I_t}{K_t}\right)K_t + G_t^C + G_t^I\Big) \tag{18}$$

Importantly, this specification does not assume that firms are credit constrained. This is for two reasons. First, thanks largely to the vast amounts of public and private sector support made available to businesses during the pandemic, national accounts data revealed that non-financial corporations increased their net savings significantly during the program period relative to recent historical levels (Figure 11). Further, as noted by Manea (2020), the inclusion of credit frictions into New Keynesian models like the one simulated in this paper can act to intensify or dampen demand shocks depending on the structural parameters chosen, with no strong empirical evidence to decide which parameters to chose. In theoretical terms, the effect of credit constrained firms for the size of output multipliers is best regarded as ambiguous.

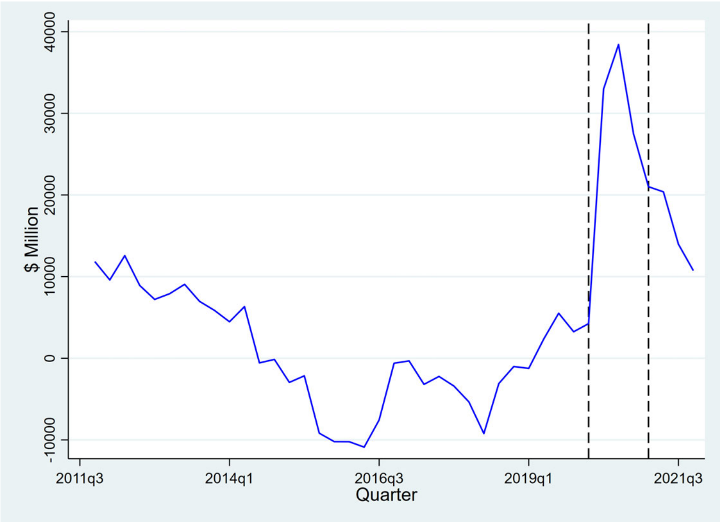


Figure 11: Non-financial Corporations Net Savings ($ Million, Seasonally Adjusted), JobKeeper Period Dashed Black. source: ABS (2022)

Under the assumption of no rigidities in price setting, profit maximisation with respect to $p_t(z)$ implies the following solution.

$$p_t(z) = \frac{\theta}{\theta - 1} MC_t(z) \tag{19}$$

Price stickiness is introduced via the familiar Calvo (1983) algorithm of stochastic price adjustment where firms can reset their price in each period with a probability $1 - \gamma$ that is independent of time. Under Calvo pricing the firm seeks to maximise the discounted present value of expected future profits

$$\max_{p_t(z)} V_t(z) = E_t \sum_{s=t}^{\infty} \gamma^{s-t} Q_{t,s} \frac{v_s(z)}{P_s} \tag{20}$$

With the stochastic discount factor between periods t and s given by $\xi_{t,s}$, the solution for $p_t(z)$ is

$$p_t(z) = \frac{\theta}{\theta - 1} \frac{E_t \sum_{s=t}^{\infty} \gamma^{s-t} \xi_{t,s} Q_s MC_s(z)}{E_t \sum_{s=t}^{\infty} \gamma^{s-t} \xi_{t,s} Q_s} \tag{21}$$

With

$$Q_s = \left(\frac{C_s + I_s + \phi(\frac{I_s}{K_s})K_s + G_s^C + G_s^I}{P_s} \right) \tag{22}$$

Log-linearising equation 22 results in a version of the familiar New Keynesian Phillips curve

$$\hat{p}_t(z) = \beta\gamma E_t(\hat{p}_{t+1}(z)) + (1 - \beta\gamma)(\hat{mc}_t(z)) + \epsilon_t^p \tag{23}$$

Where ϵ_t^p is a zero mean cost push shock. The Phillips curve implies that the firms optimal price is a weighted average of current and expected future marginal costs.

7.3 Policy

Public consumption indexes are assumed to be structurally identical to private consumption indexes, and public demand functions for domestic goods are defined in an analogous way to private demand functions. The government budget constraint is then given as follows:

$$\begin{aligned} \tau_t^y(w_t N_t + r_t^K K_t + v_t) + \tau_t^c P_t C_t = B_t - R_t^{-1} B_{t+1} + G_t^S w_t N_t \\ + P_t(G_t^C + G_t^I + G_t^T) \end{aligned} \tag{24}$$

It is also assumed that Ricardian and non-Ricardian households pay equivalent rates of income and consumption taxation. Analogous to relying on bracket creep for fiscal repair, the Government uses real increases in the income tax burden to react to changes in the public debt to GDP ratio relative to its target level in the previous period such that

$$\tau_t^y = \tau_0^y \left(\frac{B_{t-1}/Y_{t-1}}{B/Y} \right)^{\Phi} \epsilon_t^{\tau^y} \tag{25}$$

Central government spending and transfer payments evolve according to exogenous autoregressive processes of the following form

$$\hat{g}_{g,t} = \rho^g \hat{g}_{g,t-1} + \epsilon^g_{g,t} \tag{26}$$

Where g=C, I, T and S, ρ^g is between zero and one, $\hat{g}_{g,t} = (G_{g,t} - G_g)/Y$, and $\epsilon^g_{g,t}$ is an i.i.d spending shock variable with zero mean.

Central government capital evolves according to the following law of motion under the assumption of no adjustment costs as per Ratto et al. (2009), and Traum and Yang (2015)

$$K_{g,t+1} = (1 - \delta^G)K_{g,t} + I_{g,t} \tag{27}$$

To approximate a 'no monetary policy response' scenario, the central bank is assumed to be constrained to the zero lower bound on nominal interest rates for twenty quarters. In the model the interest rate peg is implemented as a 'news shock' in the policy rule. Monetary policy is unresponsive to economic conditions during this time, while at the end of the period it is assumed to revert to following a standard Henderson and McKibbin (1993) and Taylor (1993) type monetary reaction function of the following form

$$\hat{r}_t = \mu_1 \hat{r}_{t-1} + (1 - \mu_1)(\mu_2 \Delta\hat{P}_t + \mu_3 \hat{Y}_t) + \sum_{j=0}^{H-1} \epsilon^r_{t-j} \tag{28}$$

Which assumes that the monetary authority responds to deviations of aggregate inflation and output from steady state with some interest rate smoothing at the end of the pegged period. The ϵ^r_{t-j} are policy rate shocks that are known to agents in the model for $j > 0$. When the model is hit by the fiscal shocks simulated below, the model is then solved for each ϵ^r_{t-j} for $j = 0,\ldots\ H-1$, that will keep interest rates effectively unresponsive to the shock ($\hat{r}_{t+s} = \hat{r}_{t-1} = 0$) for $s = 0,\ldots\ H-1$, where H is the period during which the interest rate peg is assumed to hold.

7.4 Calibration

We set β to 0.995, implying an annualised real interest rate of 2 per cent. A wide range of values have been suggested for the Frisch elasticity of labour supply. Keane and Rogerson

(2012) suggest a range of 1 to 2 for macroeconomic applications, while Freestone (2020) suggests a range of 1.5 to 2.4 for the Australian economy, and we select a value of 1.5.

Cogan et al. (2010) estimated that the share of non-Ricardian households is 0.29 in the US, while Albonico et al. (2019) assume 0.39 for Germany, 0.34 in France, 0.33 in Italy and 0.31 for Spain; while Kaplan et al. (2014) suggest 'hand-to-mouth' households constitute about 30 per cent of households in the US, Canada, UK and Germany, but 20 per cent or less in Australia, France, Italy and Spain. We set $\lambda = 0.27$ based on the latest data available from Australia on the proportion of households with no debt which has remained relatively stable since 2003-04 (ABS, 2019).

In the production technology, α is set equal to 0.33 consistent with Galí (2015), a standard value in the literature that also helps retain consistency with Chodorow-Reich (2019). $\delta = 0.0175$ which is again a standard value in the literature, and follows Rees et al. (2016). Bom and Ligthart (2014) find a short-term output elasticity of public capital of 0.083, and we therefore set $\phi_G = 0.083$. Estimates of the responsiveness of investment to Tobin's Q (ϕ) in aggregate data have varied from about 0.5 (Levin et al., 2006) to around 5 (Smets and Wouters, 2007), and we select 2.5 consistent with Christiano et al. (2005). The persistence of TFP ρ_x is set equal to 0.93, and the responsiveness of TFP to past employment μ_l is set equal to 0.2 based on the estimates for Australia presented by Watson and Tervala (2021).

Estimates of the Calvo parameter γ in recent Australian studies typically range between 0.7 and 1, and Nimark (2009) and Rees at al. (2016) estimate values of 0.89 and 0.87 respectively. Therefore, following Watson and Tervala (2021) γ is set to 0.85. We set $\theta = 6$ following Galí (2015), implying a steady state markup of 20 per cent.

Turning to policy, for the Henderson-McKibbin-Taylor rule, when the lower bound does not bind we select the following parameters $\mu_1 = 0.85$, $\mu_2 = 1.5$, and $\mu_3 = 0.2$. Values of the interest smoothing parameter in the Australian Dynamic Stochastic General Equilibrium (DSGE) modelling literature are mostly concentrated in the 0.82 to 0.87 range, indicating slightly more persistence in interest rate setting relative to other central banks. Estimates for the weight on inflation in the Taylor rule μ_2 range between 0.41 in Nimark (2009) and 1.75 in Jääskelä and Nimark (2011), with most values close to the standard 1.5 that we select. Finally, the evidence also suggests that the RBA places a relatively lower weight on output compared to other central banks, with values ranging between between 0.02 in Nimark (2009) and 0.72 in Langcake and Robinson (2013), although most estimates are between 0.1 and 0.3. As a consequence, we select $\mu_3 = 0.2$.

The autocorrelation coefficients for government spending shocks are set to 0.9 as in Galí et al. (2007), Corsetti et al. (2012), and Campbell et al. (2017). IMF (2015) estimate that the annual depreciation rate of public capital in advanced economies is around 4.6 per cent, therefore we select the quarterly depreciation rate $\delta^G = 0.0125$, implying an annualised depreciation rate of 5 per cent. Lieberknecht and Wieland (2019) suggest an elasticity of public debt with respect to GDP of 0.1 for Europe and 0.05 for the United States, and we set $\Phi = 0.075$. The income tax rate is the average tax wedge for a single worker in 2020 calculated by the OECD (2021), while the consumption rate is equal to the rate of Australia's Goods and Services Tax (see OECD, 2020).

The steady state ratio of government debt to GDP is set equivalent to the average rate of government debt for all levels of Australian government between 1993 and 2019 obtained from the IMF (2022), which is 25 per cent. The steady state value for government consumption is set equal to 0.18, which is the average share of government consumption in GDP between 1993 and 2019, and the steady state public sector capital stock is calibrated to ensure a ratio of public investment to GDP of 0.03, which is equal to the 1993 to 2019 average. Based on the above parameter values steady state consumption is equal to 0.60, and steady state private investment is equal to 0.18- values that are reasonably close to the 1993 to 2019 averages for these values in the national accounts which are 0.54 and 0.19 respectively (see ABS, 2021a). Government transfer payments and wage subsidies are assumed to be equal to zero in steady state.

Finally, a key parameter relates to the proportion of the JobKeeper Payment that can be regarded as an effective transfer payment, and the proportion that constitutes a pure wage subsidy. In their three month review of the JobKeeper Program, Treasury (2020) observed that just over 23 per cent of the JobKeeper Payment at that stage represented an effective transfer payment- in the sense that the money went to stood down workers who would otherwise have no income, or for many low paid and part-time workers in particular, the flat rate of the initial JobKeeper Payment meant that many received payments in excess of what they would normally earn in their jobs. The excess of the JobKeeper payment over their regular salary could be interpreted as an effective transfer payment to low income workers. We use the same methodology adopted by Treasury (2020) to identify the proportion of JobKeeper that can be regarded as a transfer payment. In particular, STP data includes data for total employee wages and also wages less allowances, which includes things like the JobKeeper payment and unemployment benefits for example. Because workers were not entitled to unemployment benefits while on JobKeeper, wages less allowances can be taken to constitute worker wages in the absence of the JobKeeper payment for JobKeeper recipients. Where wages less allowances were equal to zero, and the worker is regarded as employed in the STP data, they are effectively stood down or zero hours workers, and JobKeeper reflects all of their income. In these circumstances the transfer percentage is equal to 100 per cent. Otherwise the transfer percentage is equal to the excess of the JobKeeper Payment over total wages excluding allowances divided by the amount of JobKeeper Payment received by the employee. Where the JobKeeper Payment amount is less than the amount of wages excluding allowances, JobKeeper is regarded as entirely a wage subsidy. We are able to identify the transfer percentage in this manner for JobKeeper Payments equal to about 80 per cent of the total amount due to incomplete linking in the available dataset.

Figure 12 shows that in the first three months of JobKeeper the transfer percentage is around 23 per cent consistent with Treasury (2020) analysis. As workers regained hours of employment as the public health situation improved, the transfer percentage declines. The transfer percentage also continues to decline as payment rates were reduced, and distinctions were made for full and part-time employees under the first and second program extensions as would be expected. Given this finding, 20 per cent is selected as the transfer percentage for the baseline calibration, and sensitivity analysis is conducted at 5 percentage point intervals between 10 and 30 per cent.

7.5 Model Simulation and Approximate Output Multipliers

Figure 13 provides the impulse response functions for selected variables at the quarterly frequency in response to a one per cent of GDP increase in simulated JobKeeper expenditure. JobKeeper is simulated to have a positive effect on output, consumption, and productivity

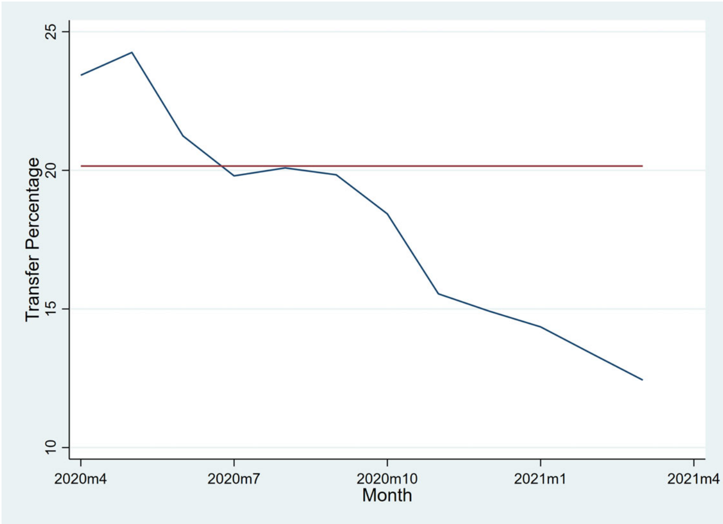


Figure 12: Monthly Transfer Proportion of JobKeeper Payment (Blue), Program Average (Red), Source: ATO and ABS

consistent with the empirical evidence for the US and the New Keynesian DSGE model featuring learning-by-doing in production simulated by D'Alessandro et al. (2019). While the model predicts no crowding out with respect to consumption as in D'Alessandro et al. (2019) and Galí et al. (2007), there is still crowding out with respect to private investment. Hours worked in the economy also respond positively to the increase in real wages- with the direct benefits of the wage subsidy for wages outweighing the reductions in the price level associated with the direct reduction in marginal costs, and the second round reductions in marginal costs driven by higher employment, and the learning-by-doing mechanism in the production technology. Inflation is persistently lower due to reductions in current and expected marginal costs due to the latter mechanisms. Further, with monetary policy stuck at the ZLB, the central bank cannot immediately contribute to counteracting the deflationary impact of the wage subsidy. However, it must be observed that the overall deflationary impulse associated with the wage subsidy is predicted to be very mild. This is because the transfer payment component actually increases the price level helping counteract the impact of the 'pure' wage subsidy component on reducing marginal cost and the price level. As would be expected the deficit financed increase in government spending increases government debt and income tax payments as a percentage of GDP. This is because the government is assumed to rely on bracket creep to stabilise public debt over the medium-term. Overall, the impulse response functions appear to provide a plausible account of the reaction of key macroeconomic variables to the simulated JobKeeper shock.

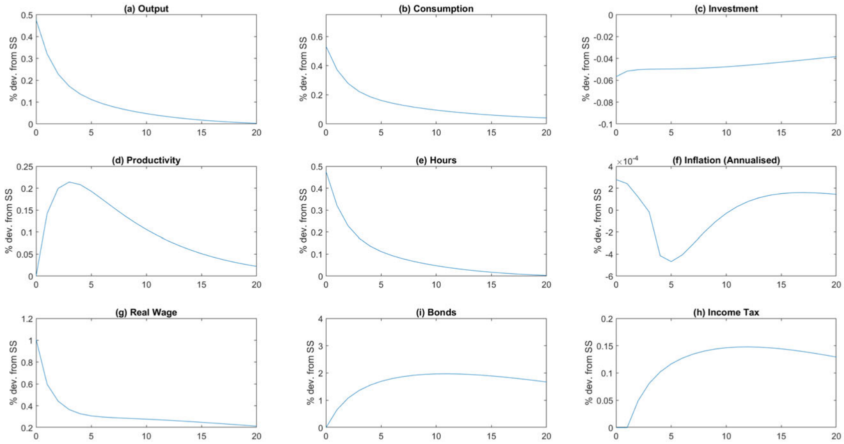


Figure 13: Quarterly Impulse Responses to a 1 per cent of GDP Increase in Simulated JobKeeper Spending

Table 4 sets out cumulative annual output multipliers under a range of different assumptions concerning the proportion of JobKeeper that constituted a transfer payment, and alternative model parametrisations. Firstly, it can be seen that the baseline model calibration with the transfer percentage assumed to by 20 per cent generates an output multiplier of 1.35 over the program period, entirely consistent with estimates presented above based on the methodology of Chodorow-Reich (2019). For the initial year, output multipliers in this paper and Chodorow-Reich (2019) are conceptually equivalent in annualised terms. At the four year time horizon the cumulative output multiplier increases to 3.05 due to hysteresis effects, reinforcing the empirical findings that program benefits are likely to be persistent. In this model the size of the multiplier is largely determined by the extent to which the JobKeeper Payment represents a transfer, and the multipliers associated with a *pure* wage subsidy are very small. This is because the modest spending multipliers are practically extinguished by the increase in distortionary income tax used to fund them in the model. Multipliers are also increasing in the proportion of payments that are assumed to flow to non-Ricardian workers- supporting the suggestion of Autor et al. (2022) that the PPP would have potentially had higher multipliers if it was more targeted, in this instance towards workers that were liquidity constrained. The key policy insight with respect to JobKeeper being that costs per job-year saved may have been lower, and output multipliers higher, if subsidies were limited to stood down workers as in the UK CJRS, or if relatively lower paid short-tenured casuals and temporary migrants (many of whom are comparatively low paid, see Attorney General's Department, 2019) were also eligible for the JobKeeper Payment.

Table 4: Simulated Cumulative Annual Output Multipliers and Sensitivity Analysis

	Transfer Percentage				
Calibration	10	15	20	25	30
Baseline	0.69	1.02	1.35	1.68	2.02
$\lambda = 0.2$ (0.27)	0.57	0.85	1.12	1.40	1.67
$\lambda = 0.3$ (0.27)	0.74	1.11	1.47	1.83	2.19
$\rho_x = 0.8$ (0.93)	0.70	1.05	1.39	1.73	2.07
$\rho_x = 0.99$ (0.93)	0.68	1.01	1.34	1.67	2.00
$\mu_l = 0.1$ (0.2)	0.78	1.16	1.54	1.92	2.30
$\mu_l = 0.3$ (0.2)	0.62	0.92	1.22	1.53	1.83
$\beta = 0.99$ (0.995)	0.61	0.91	1.21	1.51	1.82
$\beta = 0.9995$ (0.995)	0.74	1.09	1.45	1.80	2.16
$\gamma = 0.75$ (0.85)	0.67	0.99	1.31	1.63	1.95
$\theta = 9$ (6)	0.67	1.00	1.32	1.65	1.98
$\phi = 0.5$ (2.5)	0.55	0.79	1.03	1.26	1.50
$\phi = 5$ (2.5)	0.70	1.05	1.40	1.75	2.10
$\Phi = 0.05$ (0.075)	0.69	1.03	1.36	1.70	2.03
$\Phi = 0.1$ (0.075)	0.64	0.95	1.26	1.58	1.89
$\varphi = 1$ (1.5)	0.53	0.78	1.04	1.29	1.55
$\varphi = 2$ (1.5)	0.77	1.14	1.52	1.89	2.27

Notes: Cumulative annual output multipliers over the program period derived from model simulations.

Andrews et al. (2021a) and Andrews et al. (2021b) demonstrate that the JobKeeper rules were reasonably well designed in terms of allowing liquidity constrained *firms* to access the Payment. Models featuring financial frictions and credit constrained *firms* can generate relatively large multipliers in relation to *pure* wage subsidies. For instance, Barnichon et al. (2022) present a model where downward nominal wage rigidity and financial frictions at the firm level can generate large output multipliers associated with increases in wage subsidy spending relative to other increases in government spending which are generally significantly less than unity. In these models wage subsidies directly relax firm financing constraints, with positive effects for employment and output. However, theoretical results of this kind sit somewhat uncomfortably with evidence suggesting relatively large multipliers associated with increases in government consumption, investment and transfer payments during recessions documented in the meta-analysis of Gechert and Rannenberg (2018) for instance, and also the cross-country evidence concerning the typically low net benefits associated with STW and other wage subsidy programs (see Brown and Koetl, 2015). A key advantage of the model presented in this paper is that it is capable of generating output multipliers consistent with empirical evidence regarding public investment, consumption, and transfer payments; as well as a wage subsidy with design features representing the JobKeeper Payment.

Overall, the magnitude of the multipliers generated by the model under a broad range of parametrisations appear plausible relative to the empirical evidence. Increasing (decreasing) the proportion of liquidity constrained households in the model (λ) increases (decreases) multiplier size. Altering the degree of persistence of productivity shocks (ρ_x) to either 0.8 based on Chang et al. (2002) or 0.99 based on Engler and Tervala (2018) has little effect on output multiplier size. Somewhat paradoxically reducing (increasing) the elasticity of productivity with respect to employment (μ_l) increases (reduces) the size of the output multiplier. This is in contrast to Watson and Tervala (2021) and is related to the fact that monetary policy is assumed to be constrained by the ZLB in this model, whereas this is not the case in Watson and Tervala (2021). Basically output multipliers are higher when the ZLB does not bind because the increase in employment associated with fiscal stimulus increases productivity via the learning-by-doing process, and thereby reduces marginal costs and the price level. Where monetary policy is not constrained by the ZLB this would allow the central bank to lower interest rates, thereby generating a positive feedback mechanism that increases the effectiveness of fiscal policy. Where monetary policy is constrained by the ZLB the central bank is not in a position to do this, and therefore output multipliers decline in relation to the elasticity of productivity with respect to employment. Real interest rates are consequently relatively higher, resulting in lower investment and output than would be the cases in models not featuring this feedback mechanism.

Output multipliers are relatively insensitive to common alternative values chosen for the discount rate, 0.99 from Galí (2015), and 0.9995 from Rees at al. (2016). Results are also insensitive to reducing the Calvo parameter to 0.75, a common choice in the literature (see Rabanal and Tuesta, 2010), and increasing the elasticity of substitution between different varieties of consumer products (θ) which reduces the steady state markup also has very little effect on multiplier estimates. Lower (higher) values for the elasticity of Tobin's Q with respect to investment (ϕ) and the Frisch elasticity of labour supply (φ) result in lower (higher) output multipliers respectively, with multiplier estimates greater than unity under the central case assumption regarding the transfer percentage. Marginally lowering (increasing) the elasticity of income taxation to national debt (Φ) results in marginally higher (lower) output multipliers as would be expected.

Again, overall the multiplier calculations appear broadly reasonable when compared to empirical approximations based on the methodology of Chodorow-Reich (2019) under a broad range of assumptions regarding model parametrisation, and the percentage of the JobKeeper Payment that effectively constituted a transfer payment. Following the results of Tervala and Watson (2022), hysteresis and liquidity constrained households are important to help generate empirically plausible output multipliers without relying on GHH (1988) preferences, the relevance of which to fiscal policy has been contested. Of particular policy relevance, the degree to which the wage subsidy acts as a transfer payment to liquidity constrained *households* has a very significant impact on multiplier size in the closed economy context. This is not necessarily matched in open economy models featuring learning-by-doing in production, such as Tervala and Watson (2022), where transfer payments have a more deleterious effect on international competitiveness relative to government consumption or investment, especially over the medium-term.

8 Conclusion

Between 14 and 27 March 2020 a number of countries with a similar institutional environment to Australia including New Zealand, the UK and Canada all introduced novel COVID-19 related wage subsidy schemes. The US also introduced the PPP loan scheme on 27 March, a program with similar aspirations to these schemes in many respects. Likewise, over a matter of days in late March 2020 the Australian Government developed the JobKeeper Payment, which became the largest of the Government's economic security measures introduced in response to the COVID-19 pandemic, and almost as large as the entire emergency fiscal response to the GFC just over a decade earlier. What made these schemes remarkable was that none of these countries had any significant institutional history or experience of undertaking these kinds of large scale STW or wage subsidy programs, unlike many of the major European welfare states. Despite the novelty of such schemes in the Australian context, the need for a wage subsidy scheme was urged on by academic economists, business lobbyists, the opposition Labor Party and Trade Union movement, think tanks and political commentators from all sides of the political divide. This was quite remarkable given the somewhat underwhelming evidence concerning the cost effectiveness of such schemes that had emerged over the preceding two or three decades (Brown and Koetl, 2015)

Given the novelty and unprecedented nature of the program, and set against the plethora of other stimulus and economic security measures implemented at the same time, the obvious question is: How good was JobKeeper? In order to answer this question we identify employers experiencing actual declines in revenue of between 30 and 50 per cent due to the pandemic, and with revenues below $1 billion for whom access to the JobKeeper Payment depended entirely on employer and employee eligibility rules that were unrelated to underlying economic performance, and was therefore plausibly exogenous. The study relies on novel administrative datasets for the JobKeeper Payment and SME Cashflow Boost measures, as well as STP administrative data for payroll jobs and wages. These comprehensive population level datasets allow the direct estimation of JobKeeper Payment effects on payroll jobs and wages covering the entire program period between 30 March 2020 and 28 March 2021, controlling for the other major business subsidy scheme in operation during the same time period, as well as potential heterogeneous treatment effects.

We find that JobKeeper had a cost per job-year saved over the program period of around $112,819 ($US80,959), implying that the program saved around 812,000 jobs during this time period alone. We also find that weekly payroll wages for employers were on average almost $1.1 billion ($US761 million) higher throughout the program period, implying wage effects equivalent to around 60 per cent of program spending during this period alone. Results for payroll jobs and wages are mutually reinforcing with 0.89 job-years saved per $100,000 during the program period being broadly consistent with the effects on weekly payroll wages which represent roughly 0.90 of average weekly total earnings in Australia per $100,000 in program spending during the program period (ABS, 2021). It should be stressed that the payroll jobs and wage multipliers observed were very persistent, and over time *cumulative* cost per job-year saved estimates will decrease, and assessments of the cumulative impact of the program on payroll wages will increase, both potentially significantly as new data becomes available to assess these effects over time. A potential short-coming of the identification strategy employed in this paper is that it may be difficult to undertake the same analysis in the future with the likelihood of attrition from the sample set used for identification purposes over the coming years. Further, the results pertain to payroll jobs, and not employment as defined in standard labour force statistics, and given the prevalence of multiple job holders the results may overstate the benefits for employment that are typically reported in comparable studies.

The costs per job-year saved estimates are consistent with an approximate closed economy, no monetary policy response cumulative output multiplier of around 1.3 over the program period in the preferred model. Overall, the evidence presented suggests that JobKeeper had lower costs per job-year saved and greater benefits with respect to worker wages compared to the US PPP. This is most likely because JobKeeper was more targeted towards preserving employment and wages than the PPP, with our theoretical results suggesting this may have been because it was better targeted towards liquidity constrained *households*. However, compared to studies preceding the COVID-19 pandemic, approximate output and wage multipliers are towards the middle-to-lower end, and costs per job-year saved estimates are towards the middle-to-upper end. This may in part be due to certain exigencies related to the COVID-19 pandemic, but it may also be related to difficulties associated with achieving additionally in wage subsidy schemes, and risks concerning high deadweight, substitution and displacement costs, that would be expected to result in lower net benefits relative to alternative forms of fiscal stimulus including targetted transfers, public investment and consumption (see Brown and Koetl, 2015). When assessed on a comparable basis, multipliers appear lower than those for targetted transfer payments and public investment during recessions in particular (see Gechert and Rannenberg, 2018; Leigh, 2012; Watson, 2021; and Leduc and Wilson, 2013). Our theoretical results reinforce the empirical findings indicating that program effects are likely to persist beyond the program period. The theoretical results also indicate that liquidity constrained households and hysteresis effects can help generate plausible closed economy, no monetary response output multipliers without the need to resort to GHH (1988) preferences.

So how good was JobKeeper, and what are the lessons for future fiscal policy? The first point to make is that JobKeeper represented an incredible administrative achievement, with a very large and entirely novel policy in the Australian context established in a matter of days, and then implemented almost immediately with surprisingly few administrative and compliance issues. This is a major achievement for which the Australian Public Service (APS), and the Treasury and ATO in particular, should be proud. The key lesson for leaders in the APS and Government, which echoes that of the GFC, is that the APS is highly capable when it comes to rapidly implementing complex economic security measures. This is an important point to note, because concerns about capability can result in implementation delays, and decision makers opting for half-measures or lower quality policy options that may be considered lower risk and more realistically deliverable. In these ways, a lack of institutional confidence can unfortunately undermine the effectiveness of stimulus or economic security measures.

The JobKeeper experience also reinforces a number of important fiscal policy lessons such as Ken Henry's advice to the Rudd government to 'go hard, go early, go households' in the context of the GFC (Rudd, 2018), and also the benefits of 'timely, temporary and targeted' fiscal stimulus (Elmendorf and Furman, 2008). The JobKeeper Payment was the third and final major stimulus package instituted by the Australian Government in response to the COVID-19 pandemic. In many ways it reflected a larger and better targeted, version of the SME Cashflow Boost Payment that was announced only 18 days earlier. As indicated by Treasury (2020), and reflected within the sample of firms used for estimation in this study, payroll jobs declined in both JobKeeper and non-JobKeeper employers following the announcement of the policy, which may partly be attributable to the earlier stimulus packages being of insufficient size, and the JobKeeper measure itself being introduced fractionally too late in the context of how the pandemic was affecting the economy and labour market. There are obviously trade-offs between timeliness and policy quality- however, to the extent that these exist, the preference should be to announce early, provide an overwhelming level of economic support to meet the relevant economic challenges, but to also provide flexibility to calibrate and adjust the measure once it has been introduced to ensure that it is well targeted and cost effective.

Our theoretical results suggest that the cost effectiveness of the Program could have been enhanced by maximising the extent to which the JobKeeper Payment represented a transfer payment to liquidity constrained households. In this regard, limiting the payment to 'furloughed' workers as per many European STW schemes and the UK CJRS, and/or expanding program access to short-tenured casuals and low-paid temporary migrants may have enhanced the stimulus potential and cost effectiveness of the program. Limiting the payment to stood down workers would be synonymous with providing enhanced unemployment benefits to unemployed workers which would have the added benefit of being simpler and quicker to deliver. Further, the theoretical results suggest that targetted transfers should be expected to be more cost-effective and have greater stimulus potential than a wage subsidy, where it can be difficult in practice to design rules to prevent infra-marginal employers from accessing the program, and limit deadweight, substitution and displacement costs.

Expanding the program to short-tenured casuals and temporary migrants alone would have added significantly to program costs, which may partly explain why decisions were taken to exclude these workers in the first place. Aside from limiting eligibility to furloughed workers, other options could have been considered to enhance program cost effectiveness. As demonstrated in Figure 1, the move to an actual rather than projected turnover test in the first and second JobKeeper extensions contributed significantly to reducing program costs by cutting eligible employers almost in half relative to the program peak in August 2020 in the first extension period, and by over 60 per cent relative to peak in the second extension period. The adoption of a two-tiered payment structure for full and part-time employees and the tapering of payment rates also contributed to reducing costs during this time. An earlier switch to actual turnover testing as advocated by Breunig and Watson (2020), or a turnover contingent recovery scheme for firms that did not experience actual declines in turnover similar to those suggested by Chapman (2020) and Botterill et al. (2020), may have helped reduce program costs, while maintaining support for significantly liquidity constrained firms, thereby minimising job losses.

Our theoretical results suggest that a two-tier payment structure as recommended by

Breunig and Watson (2020) may have reduced the degree to which the JobKeeper Payment acted as a transfer payment, which may have reduced JobKeeper's stimulus potential and cost effectiveness in the first instance. However, our empirical results also show that the employment and wage benefits of JobKeeper were highly persistent beyond its initial three months of operation, and that the introduction of the two-tier payment structure and the tapering of payment rates did not appear to have a significantly detrimental effect on wage or employment outcomes. Treasury (2020) observed 700,000 zero hours workers in April 2020 which had declined to 360,000 by the end of May 2020; and Borland and Hunt (2021) show consistent declines in workers on zero hours (temporary lay-offs) between April 2020 and June 2021, with monthly hours worked recovering strongly from April 2020 and reaching pre-COVID levels by February 2021. This is consistent with our evidence that the the transfer percentage declined over time. In our model the declining transfer percentage results in declining output and employment multipliers, lowering the output and employment costs associated with introducing a two-tier payment structure or tapering payment rates over time. On balance, this supports a strategy of decreasing payment generosity over time as the economy recovers.

Finally, another major advance relates to the ability to monitor and evaluate program performance almost in real time using high quality administrative datasets covering payroll jobs and wages, as well as detailed program administrative data. This represents a significant improvement in the information set available to inform policymaking compared to that available during the GFC, and even more dramatically compared to during Australia's last recession in the early 1990s, where poor data availability, timeliness, and quality almost certainly contributed to suboptimal policymaking exacerbating the recession. Again, the ABS, ATO and the Treasury should be commended for their efforts to make this data available to inform policymaking in a timely manner. Likewise, the commitment to make this data available to the research community to independently evaluate the efficacy of fiscal policy within a relatively short time-period following its conclusion should be applauded. With the data that is currently available, in future work we will examine the distributional dimensions of the JobKeeper Payment in greater detail, including with respect to differential program effects by income, location, gender and demography.

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Table A1: Effect of JobKeeper on Payroll Jobs and Wages: With Size-by-month Fixed Effects

	Increase in Jobs per $100,000			Increase in Wages per $100,000		
	(1)	(2)	(3)	(4)	(5)	(6)
	JK per Worker × Month FE			JK to Wage Ratio × Month FE		
Jan 2020	0.05	0.07	0.15	53.08	7.67	16.29
	(0.08)	(0.08)	(0.11)	(43.26)	(8.36)	(17.38)
Feb 2020	0.08	0.09	0.15	82.62	9.39	15.99
	(0.06)	(0.06)	(0.07)	(42.65)	(10.21)	(17.40)
Apr 2020	0.66	0.72	0.75	975.97	1,767.55	1,769.42
	(0.21)	(0.20)	(0.20)	(161.35)	(55.85)	(52.52)
May 2020	0.73	0.79	0.82	1,166.60	2,059.59	2,062.83
	(0.18)	(0.18)	(0.18)	(148.04)	(70.67)	(67.06)
Jun 2020	0.84	0.91	0.93	1,314.65	2,095.19	2,098.39
	(0.21)	(0.21)	(0.21)	(203.23)	(41.48)	(38.90)
Jul 2020	0.98	1.06	1.07	955.74	1,625.65	1,628.42
	(0.21)	(0.21)	(0.21)	(162.11)	(77.05)	(78.18)
Aug 2020	0.97	1.05	1.05	1,012.78	1,499.05	1,502.61
	(0.20)	(0.20)	(0.20)	(94.16)	(42.89)	(44.84)
Sep 2020	0.98	1.06	1.06	941.96	1,504.79	1,508.19
	(0.21)	(0.21)	(0.20)	(135.30)	(46.90)	(48.75)
Oct 2020	0.98	1.05	1.04	880.66	1,508.27	1,510.24
	(0.20)	(0.20)	(0.20)	(164.98)	(53.84)	(54.56)
Nov 2020	0.96	1.03	1.00	935.17	1,486.73	1,484.88
	(0.21)	(0.22)	(0.22)	(128.09)	(34.24)	(33.02)
Dec 2020	0.89	0.97	0.95	763.25	1,551.94	1,541.45
	(0.17)	(0.18)	(0.17)	(328.08)	(106.25)	(98.17)
Jan 2021	1.01	1.09	1.08	748.81	1,490.62	1,482.70
	(0.20)	(0.20)	(0.20)	(199.58)	(40.41)	(35.74)
Feb 2021	0.98	1.06	1.02	667.65	1,501.38	1,490.15
	(0.20)	(0.20)	(0.20)	(252.55)	(55.09)	(50.24)
Mar 2021	0.95	1.03	0.99	637.85	1,511.63	1,500.04
	(0.20)	(0.21)	(0.21)	(286.03)	(65.35)	(59.35)
Apr 2021	0.92	0.99	0.95	401.52	1,549.39	1,539.61
	(0.21)	(0.22)	(0.22)	(494.97)	(115.60)	(108.89)
May 2021	0.87	0.94	0.91	838.88	1,473.00	1,461.63
	(0.22)	(0.24)	(0.23)	(214.15)	(34.09)	(35.60)
Jun 2021	0.88	0.95	0.90	848.53	1,459.56	1,448.02
	(0.22)	(0.22)	(0.23)	(229.12)	(34.64)	(39.78)
CFB	Yes	-	-	Yes	-	-
State-by-industry-by-month FE	Yes	Yes	-	Yes	Yes	-
State-by-month & industry-by-month FE	-	-	Yes	-	-	Yes
Size-by-month FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	94,428	94,428	94,428	94,428	94,428	94,428
Adj. R^2	0.36	0.35	0.31	0.82	0.81	0.77
Pre-period = 0 (F test p value)	0.44	0.26	0.14	0.17	0.66	0.65
JK1 period cost per job saved/ increase in weekly wages per $100k	116,245.00	107,433.70	105,612.20	1,061.28	1,758.64	1,761.65
	(26,324.42)	(22,401.81)	(21,338.36)	(123.46)	(19.95)	(19.60)
JK2 period cost per job saved/ increase in weekly wages per $100k	105,953.50	98,283.50	100,121.60	859.69	1,512.19	1,509.48
	(21,565.96)	(18,832.15)	(19,400.52)	(190.45)	(63.29)	(60.29)
JK3 period cost per job saved/ increase in weekly wages per $100k	101,842.60	94,547.46	97,097.56	684.77	1,501.21	1,490.97
	(16,472.12)	(17,862.23)	(18,928.79)	(241.43)	(53.16)	(48.40)
Program period cost per job-year saved/ increase in average weekly wages per $100k	109,702.60	101,606.70	101,978.40	916.76	1,633.53	1,631.61
	(23,220.94)	(19,987.39)	(19,947.97)	(155.93)	(34.57)	(33.28)
Program period jobs saved/ increase in average weekly wages ($m)	835,456.20	902,024.40	898,736.10	840.00	1,500.00	1,500.00
	(176,842.40)	(176,842.40)	(175,801.50)	(143.00)	(31.70)	(30.50)
Approx. program period Chodorow-Reich (2019) output/ implied wage multiplier	1.36	1.47	1.46	1.01	1.80	1.80

Notes: Estimation performed using analytic weights based on average weekly jobs/wages in the March quarter of 2020. For parameter estimates, standard errors in parentheses are clustered at the ANSZIC 2006 classification industry level. Cost per job-year saved=$100,000/($\sum_s^T(\beta_t/(T-s))$) where s is the start of the period of interest, and T the end, and the increase in weekly wages per $100,000=$\sum_s^T(\beta_t/(T-s))$. Jobs saved over the program period=91651716200/100,000*($\sum_s^T(\beta_t/(T-s))$) with T and s defined accordingly, and the aggregate increase in average weekly wages is calculated in a synonymous manner for the wage equations. Standard errors for aggregated cost per job saved, jobs saved, and wage increase figures are calculated using the delta method. The output multiplier is calculated using the method of Chodorow-Reich (2019) where $\beta_Y \approx (1-\alpha)(1+\chi)(\text{Cost per Job-Year})^{-1}Y/E$, where $\alpha \approx 0.33$ and the elasticity of hours per worker with respect to total employment for Australia is $\chi = 0.47$ based on Dixon et al. (2004), which compares to a standard assumed value in US studies of $\chi = 0.5$. Implied wage multipliers relate the estimated annual increase in total wages divided by 2019 compensation of employees from the National Accounts, to the increase in JobKeeper spending relative to 2019 GDP also taken from the national accounts.

Table A2: Effect of JobKeeper on Payroll Jobs and Wages: With Size-by-industry-by-month, or Size-by-month and Industry-by-month Fixed Effects

	Increase in Jobs per $100,000			Increase in Wages per $100,000		
	(1)	(2)	(3)	(4)	(5)	(6)
	JK per Worker × Month FE			JK to Wage Ratio × Month FE		
Jan 2020	0.16	0.17	0.18	197.42	18.31	30.06
	(0.16)	(0.15)	(0.18)	(172.69)	(22.03)	(30.62)
Feb 2020	0.17	0.17	0.18	197.69	18.10	29.14
	(0.12)	(0.11)	(0.11)	(160.97)	(21.81)	(30.19)
Apr 2020	0.72	0.76	0.78	1,066.78	1,772.14	1,767.62
	(0.20)	(0.19)	(0.21)	(195.88)	(51.74)	(54.51)
May 2020	0.77	0.83	0.84	1,279.56	2,065.78	2,059.16
	(0.18)	(0.18)	(0.18)	(197.27)	(65.24)	(69.42)
Jun 2020	0.87	0.94	0.96	1,441.00	2,100.48	2,097.60
	(0.21)	(0.21)	(0.21)	(232.00)	(38.26)	(39.66)
Jul 2020	0.99	1.07	1.08	1,061.36	1,630.81	1,628.43
	(0.21)	(0.21)	(0.20)	(186.70)	(80.61)	(77.68)
Aug 2020	0.97	1.05	1.06	1,096.61	1,504.81	1,502.80
	(0.19)	(0.20)	(0.19)	(124.58)	(46.40)	(45.14)
Sep 2020	0.98	1.05	1.07	1,027.09	1,509.93	1,509.11
	(0.20)	(0.20)	(0.20)	(112.54)	(50.81)	(49.42)
Oct 2020	0.96	1.04	1.05	951.97	1,512.54	1,509.98
	(0.20)	(0.20)	(0.20)	(174.04)	(56.59)	(54.07)
Nov 2020	0.91	0.99	1.00	953.67	1,487.10	1,482.37
	(0.22)	(0.23)	(0.23)	(119.67)	(34.19)	(31.95)
Dec 2020	0.88	0.95	0.95	649.95	1,543.77	1,536.09
	(0.17)	(0.18)	(0.18)	(386.94)	(100.87)	(94.24)
Jan 2021	1.00	1.10	1.09	670.80	1,484.13	1,477.84
	(0.19)	(0.19)	(0.20)	(248.96)	(38.94)	(35.74)
Feb 2021	0.93	1.00	1.02	548.05	1,491.61	1,482.60
	(0.21)	(0.21)	(0.21)	(328.34)	(52.48)	(47.75)
Mar 2021	0.90	0.97	0.98	504.03	1,500.98	1,491.17
	(0.22)	(0.23)	(0.22)	(362.43)	(61.44)	(55.46)
Apr 2021	0.87	0.94	0.95	316.64	1,540.77	1,531.03
	(0.23)	(0.24)	(0.23)	(513.75)	(110.45)	(103.02)
May 2021	0.81	0.89	0.90	711.85	1,463.24	1,452.52
	(0.25)	(0.25)	(0.24)	(320.72)	(38.29)	(38.20)
Jun 2021	0.82	0.90	0.90	719.59	1,449.75	1,441.20
	(0.26)	(0.26)	(0.25)	(332.78)	(42.08)	(43.51)
CFB	Yes	-	-	Yes	-	-
Size-by-industry-by-month FE	Yes	Yes	-	Yes	Yes	-
Size-by-month & industry-by-month FE	-	-	Yes	-	-	Yes
Obs.	94,428	94,428	94,428	94,428	94,428	94,428
Adj. R^2	0.32	0.31	0.30	0.76	0.75	0.74
Pre-period = 0 (F test p value)	0.30	0.22	0.21	0.46	0.71	0.71
JK1 period cost per job saved/ increase in weekly wages per $100k	113,186.60	105,275.50	103,547.90	1,162.07	1,763.99	1764.47
	(24,578.32)	(21,013.20)	(20,550.11)	(164.54)	(20.82)	(20.99)
JK2 period cost per job saved/ increase in weekly wages per $100k	108,890.70	100,733.70	100,215.10	851.86	1,514.47	1,513.09
	(23,091.07)	(20,240.64)	(19,839.74)	(187.63)	(62.08)	(60.97)
JK3 period cost per job saved/ increase in weekly wages per $100k	106,590.80	98,400.57	97,355.59	574.29	1,492.24	1,489.93
	(23,077.39)	(20,064.73)	(19,589.00)	(309.67)	(50.48)	(50.06)
Program period cost per job-year saved/ increase in average weekly wages per $100k	110,390.10	102,334.60	101,099.70	937.57	1,633.67	1,632.99
	(23,301.36)	(20,138.57)	(19,712.49)	(147.34)	(34.30)	(33.88)
Program period jobs saved/ increase in average weekly wages ($m)	830,252.90	895,608.70	906,547.60	859.00	1,500.00	1,500.00
	(175,251.40)	(176,248.20)	(176,759.30)	(135.00)	(31.40)	(31.00)
Approx. program period Chodorow-Reich (2019) output/ implied wage multiplier	1.35	1.46	1.47	1.03	1.80	1.80

Notes: Estimation performed using analytic weights based on average weekly jobs/wages in the March quarter of 2020. For parameter estimates, standard errors in parentheses are clustered at the ANSZIC 2006 classification industry level. Cost per job-year saved=$100,000/($\sum_s^T(\beta_t/(T-s))$) where s is the start of the period of interest, and T the end, and the increase in weekly wages per $100,000=$\sum_s^T(\beta_t/(T-s))$. Jobs saved over the program period=91651716200/100,000*($\sum_s^T(\beta_t/(T-s))$) with T and s defined accordingly, and the aggregate increase in average weekly wages is calculated in a synonymous manner for the wage equations. Standard errors for aggregated cost per job saved, jobs saved, and wage increase figures are calculated using the delta method. The output multiplier is calculated using the method of Chodorow-Reich (2019) where $\beta_Y \approx (1-\alpha)(1+\chi)(\text{Cost per Job-Year})^{-1}Y/E$, where $\alpha \approx 0.33$ and the elasticity of hours per worker with respect to total employment for Australia is $\chi = 0.47$ based on Dixon et al. (2004), which compares to a standard assumed value in US studies of $\chi = 0.5$. Implied wage multipliers relate the estimated annual increase in total wages divided by 2019 compensation of employees from the National Accounts, to the increase in JobKeeper spending relative to 2019 GDP also taken from the national accounts.