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NON-COMPETE CLAUSES, JOB MOBILITY AND WAGES IN AUSTRALIA

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Summary

Has the increased use of non-compete clauses (NCCs) by Australian firms reduced workers' ability to switch jobs and bargain for higher wages? We examine these questions using a detailed ABS survey of the use of NCCs and other post-employment restraints, linked for the first time to employer-employee microdata. We find that:

- Increased use of NCCs is associated with a subsequent decline in job mobility, including for job switches to firms within the same industry. By contrast, increased use of non-disclosure agreements (NDAs) – an alternative method for firms to protect trade secrets – is not associated with a significant decline in job mobility.
- Workers at firms that use NCCs extensively are paid 4 per cent less on average than similar workers at similar firms that only use NDAs. Workers at these two groups of firms start out with similar wages, but workers at NCC using firms experience slower wages growth over the first few years of their employment.
- NCCs have different associations for high- and low-skill workers. Low-skill workers see larger declines in job mobility and wages, while high-skill workers spend more time in between jobs when leaving an NCC using firm.

Some caution is warranted with these results. We confront methodological challenges and cannot completely disentangle correlation from causation. Neither do we provide a full cost-benefit analysis of NCC use. Nonetheless, our results are consistent with the view that the rising prevalence of NCC use has been a factor contributing to lower rates of job mobility and wages growth in Australia

Could non-compete clauses be suppressing job mobility and wage growth?

Job mobility has fallen back to a record low level in 2024, despite a labour market that is still in solid shape by many measures (ABS, 2024). This raises questions about structural barriers to job mobility in Australia and what policymakers can do about them. Job mobility matters because it is an important mechanism for productivity and wages growth.

One factor that could be contributing to the decline in job mobility and weak wages growth is the increasing use of non-compete clauses (NCCs). NCCs are clauses in employment contracts where an employee agrees not to compete with their employer – in a similar industry or area for a period of time – after their job ends. Surveys conducted by the e61 Institute and the ABS indicate that around 1 in 5 Australian workers are subject to a NCC (Andrews & Jarvis, 2023; Andrews et al., 2024). This includes many low-wage workers, which is difficult to reconcile with the traditional view that NCCs are being used to protect legitimate business interests. Firms' use of NCCs has also increased over the past 5 years and is expected to increase further absent policy intervention (Andrews et al., 2024).

The Federal Government is concerned about the growing use of NCCs and the potential for them to be deployed in an anti-competitive fashion (Leigh, 2024). The Government's Competition Review is considering the case for reform to NCCs and other post-employment restraints (Treasury, 2024). Experts have explored the case for a variety of policy responses (e.g. Ross, 2024). But until now, the discussion has lacked Australian evidence on the economic impacts of NCCs.

We present the first empirical evidence on the relationship between rising NCC use, job mobility and wages in Australia. While we cannot identify the causal effect of NCC use, our results are consistent with a view that the proliferation of NCCs has contributed to the recent low levels of job mobility and wages growth.

The economics of non-compete clauses

The traditional arguments in favour of NCCs are that they **protect firms' trade secrets and client relationships**, and that they **encourage firms to invest**. Without NCCs, firms could face an investment 'hold-up' problem: they may invest less in worker training because they fear the worker will leave. There may also be an equivalent 'hold-up' problem with innovation: firms may be hesitant to engage in collaborative innovative activities for fear that workers will leave and share information with other firms.

A growing body of US research suggests that NCCs do increase worker training, but have an overall negative effect on innovation and wages (Box 1). This casts doubt on the idea that NCCs help to unleash significant productivity gains and seems consistent with a view that the dominant effect of NCCs is to reduce workers' outside employment options and **lower worker bargaining power over wages**. Workers can also miss out on the wage gains that typically accompany a change in jobs, since NCCs explicitly limit their options. A lower rate of reallocation between jobs suggests that **on average workers are a worse match for their jobs** as a result of NCCs.

There are also questions of fairness in the use of NCCs. Firms may impose NCCs on workers after initial wages have already been agreed. This can put **workers at a disadvantage in striking a fair bargain**, given the transition costs of quitting and finding a new job. In addition, some employers may have undesirable monopsony power to dictate terms to workers. Since wages and many other employment conditions are subject to minimum standards by law, firms may see NCCs as an alternative means to lower effective labour costs. Given the potential for both positive and negative influences, it is ultimately an empirical question whether NCCs lead to higher or lower wages for the workers directly covered.

Even if workers and firms do strike mutually beneficial agreements on NCCs, there can be **negative externalities for those not covered by a NCC**. Other firms face greater challenges in securing suitable labour as NCCs grow in prevalence and lock workers into their existing roles. **Fewer new firms** may be created as a result (Andrews et al., 2024 presents suggestive Australian evidence that firm entry rates are lower in industries where restraint clauses are more prevalent.)[1] **Other workers' wages can suffer from a less competitive labour market overall**, an effect that is not estimated in this analysis and would be in addition to the consequences of NCCs for the wages of the workers directly covered. Fewer new firms may lead incumbents to face **less competitive pressure**, reducing incentives to invest and innovate (in an offsetting force to the potential positive impacts on investment and innovation described above). Fewer new firms may also lead to **higher prices and less consumer choice**.

Box 1: US research on the effects of non-competes

Evidence on the effects of NCCs is dominated by US research that considers the result of changes in the legal enforceability of NCCs in one US state relative to others. The balance of this evidence suggests that NCCs:

- **Reduce job mobility** (Johnson et al., 2023b; Shi, 2023).
- **Diminish workers' earnings by reducing their outside options** (Johnson et al., 2023b; Starr, 2019), with particularly strong effects for workers with low bargaining power (Balasubramanian et al., 2024).
- Increase firm level **investment in worker training** (Starr, 2019) and intangible capital (Shi, 2023).
- But have an overall **negative effect on innovation** and lead to the misallocation of talent across firms (Johnson et al., 2023a; Reinmuth & Rockall, 2023; Starr, 2023).
- **Decrease the formation of new, innovative firms** (Starr et al., 2017).

For a more detailed review of the US literature see Starr (2023). The literature outside the US is more limited, but highlights the potential for different-sized effects of NCCs depending on country context. For example, evidence from Austria suggests that NCCs had smaller impacts on job mobility and wages than in the US (Young, 2024).

Challenges in identifying the economic impacts of non-competes

To identify the economic consequences of NCC use, we have to separate them from the factors that cause firms and workers to use NCCs (see Box 2). For instance, firms may be more likely to deploy NCCs if they have something valuable to protect, potentially biasing a comparison of wages paid by firms that use NCCs and those that do not use NCCs.

To tackle this challenge, we first restrict our sample to firms that use NDAs. We compare workers at firms that use both NCCs and NDAs to workers at firms that use only NDAs. We argue that this comparison helps to control for *unobservable* differences between workers and firms. All of these NDA using firms have revealed a tendency to have something to protect (e.g. trade secrets) and thus ought to be different to firms that do not use NDAs (Balasubramanian et al., 2024). By focusing within this group, we avoid some of the selection bias that causes a positive correlation between NCC use, job mobility and wages in the

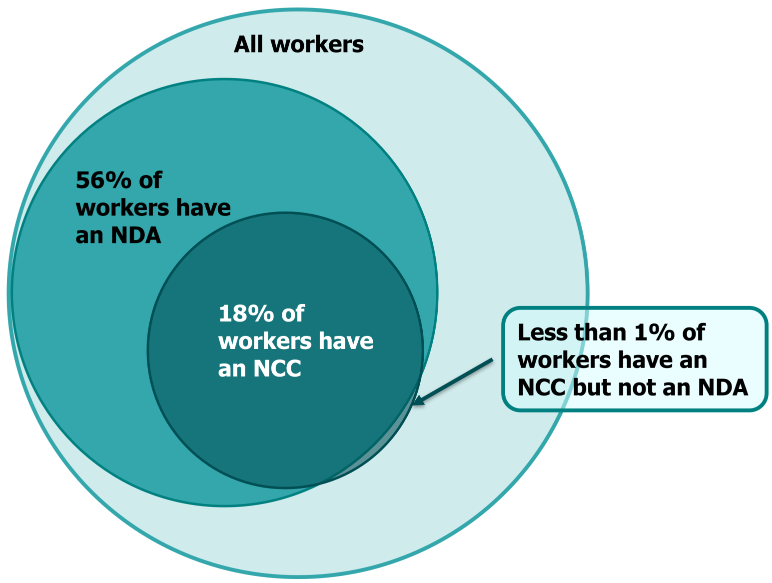
1 The analysis in Andrews et al., 2024 was completed before the ABS survey was linked to the longitudinal employer-employee microdata. Nonetheless, it would be difficult to improve on this industry-level correlation evidence as firm entry dynamics are likely to operate at something like the industry level.

raw data (see Figure A.1). Moreover, almost all workers with an NCC also have an NDA so there is little loss of generality at this step (Figure 1). That said, we do apply some further restrictions on firms and workers in our analysis, as discussed below. These restrictions increase the accuracy of our results, but mean that our findings are specific to the (still large) sample of firms and workers that remain in our analysis.

Second, our regression analysis controls for *observable* differences in the characteristics of workers and firms, including the average effects of worker age, tenure, gender and occupation, as well as firm size, industry, state and remoteness area location. Our job mobility analysis also includes firm fixed effects, which control for time-invariant firm characteristics.

One remaining empirical challenge is that almost all firms that use NCCs and NDAs also use another type of post-employment restraint: non-solicitation clauses that prevent workers from taking clients or coworkers with them when they leave a firm. Because of this joint use, it is difficult to assess the relationship between NCCs, job mobility and wages in the absence of non-solicitation clauses. As a result, a portion of the results that we attribute to the presence of NCCs may reflect the effect of non-solicitation clauses, but we do not test this directly.[2]

Figure 1: The overlap between workers subject to NDAs and NCCs



Sources: ABS, e61 Institute

Box 2: The empirical challenge: selection bias in the comparison of NCC-using firms and workers to others

To identify the *consequences* of NCCs use we have to separate them from the factors that *cause* firms and workers to use NCCs. Perhaps some firms are deploying NCCs in a near-random fashion due to the use of boilerplate employment contracts. But most firms are likely choosing to use NCCs for a reason, creating a 'selection bias' in the types of firms who use NCCs.

- **There may be reverse causality between NCC use and job mobility**. Some firms increase their use of NCCs because of high worker turnover. In the raw data we see that, on average, worker turnover is higher at firms that use NCCs than at other firms (Appendix A.2). But like umbrellas don't cause rain, this doesnt mean that NCC use causes higher turnover. In fact, our more careful analysis suggests the opposite: increased NCCs is associated with lower worker turnover.
- **NCC use and wages may be jointly determined by other firm characteristics**. Firms that have something to protect - valuable firm-specific knowledge (e.g. trade secrets) or workers that are particularly scarce in the labour market - are likely to pay higher wages. These same firms have a greater incentive to use NCCs. Higher-skilled workers may also select to work at the type of firm that has something to protect with an NCC (Starr, 2023). These factors suggest a positive correlation between NCC use and wages. Consistent with this, in the raw data we see firms with higher paid workers make more widespread use of NCCs (Appendix A.2). But our analysis suggests that NCCs are actually associated with lower wages once we abstract from other business characteristics that jointly affect worker wages and NCC use.

2 In forthcoming work, Treasury's Competition Taskforce compare firms with NDAs and non-solicitation clauses to firms with NDAs, NCCs and non-solicitation clauses. Their work is in a similar spirit to our analysis, seeking to control for unobservable differences between firms and to isolate the effects of non-competes from other post-employment restraints, but has a number of differences including that it restricts the comparison group of non-NCC using firms to a much smaller group than in our analysis.

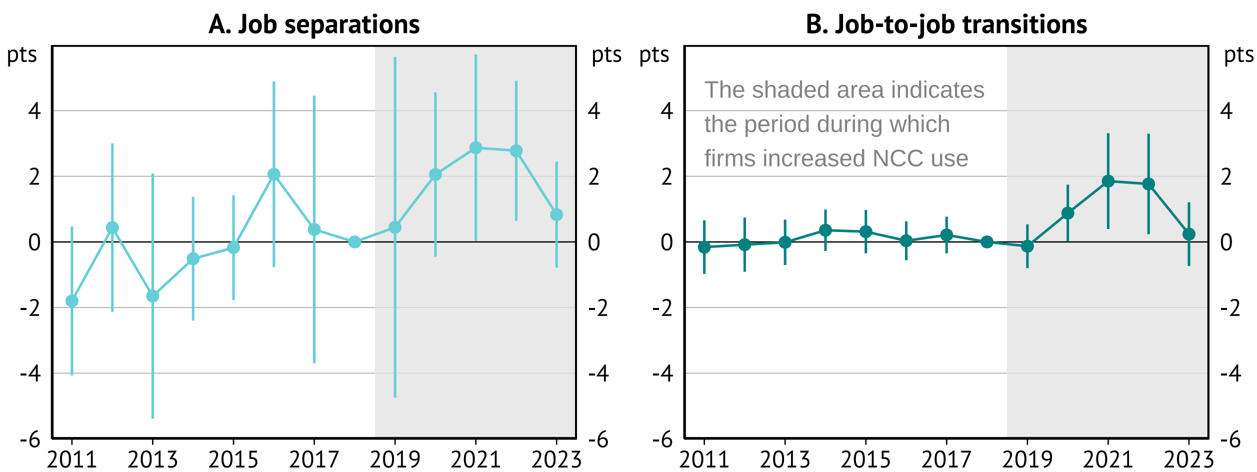
Worker turnover falls after non-compete usage rises

Firms use NCCs to prevent workers from leaving to join or start a competing firm. However, the degree to which NCCs actually reduce worker mobility is disputed, especially for lower paid workers where NCCs are generally not legally enforceable.[3] Systematic evidence on the relationship between NCC use and job mobility has not previously been available in Australia.

In this section, we investigate whether firms that increased their use of NCCs saw a subsequent decline in the rate of workers leaving the firm.[4] However, even when we focus on changes within individual firms over time, we still have to contend with the possibility of reverse causality: increased worker turnover may lead firms to increase their use of NCCs (as discussed in Box 2).[5] Indeed, we find that firms that increased their use of NCCs over the 5 years to 2023 appear to have done so in response to an increase in worker turnover between 2018 and 2021 (Figure 2).

Figure 2: Trends in job mobility

Firms that increased their use of NCCs at some stage between 2018 and 2023, relative to firms that did not change their use



* The figure plots OLS estimates of Equation 1 with 90% confidence intervals. The base year in the regressions is set to the 2018 financial year. Two different datasets are used to produce the final dataset used in this analysis: STP and PAYG. Values are multiplied by 100 for readability. Standard errors are corrected for two-way clustering at the firm and worker level. Regressions include occupation, state, remoteness area and firm fixed effects as well as a binned control for firm size, a second degree polynomial for worker age and tenure, and a control for worker gender. For more information on the data and empirical approach see Appendix B.
Sources: ABS; e61

Importantly, these firms that increased their use of NCCs experienced a decline in job mobility over the latter part of the 5 year period, between 2021 and 2023. Our intepretation is that firms successfully deployed NCCs to stem job departures.

For our headline results, we believe that the difference in job mobility between the peak of worker turnover in 2021 and 2023 provides the best possible view of the *consequences* of increasing use of NCCs. The ABS survey data does not indicate exactly when within the 5-year period firms increased their use, but it stands to reason that most increases in use would have happened by the latter years. By contrast, we are casting aside the earlier period from 2018 to 2021, on the basis that the trends in job mobility then are dominated by the *causes* of the increased use of NCCs. Appendix B.1 and B.3 provide further details on our data and empirical approach.

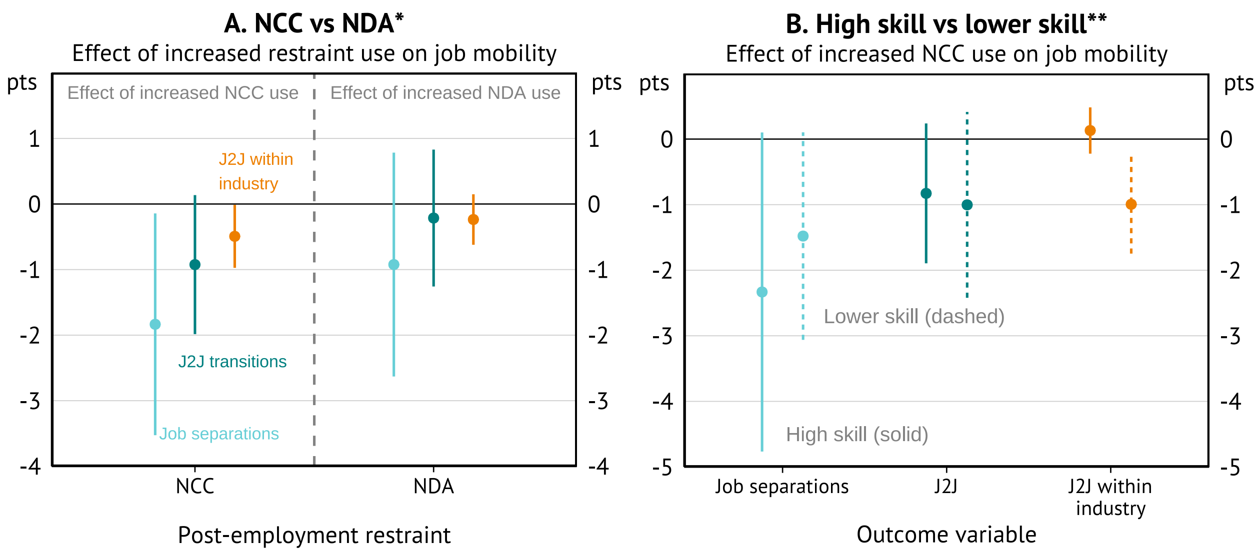
Over the period from 2021 to 2023, the relative decline in job mobility for workers at firms that increased their use of NCCs was economically and statistically significant (Figure 3 panel A; Table A.2).[6] For job separations, we estimate that workers at

3 For instance, DeBoos (2023) argues that a court will only uphold a non-compete if it is to protect a reasonable business interest and this is most common in highly remunerated professions. By contrast, overseas research suggests that NCCs can have a 'chilling' effect on worker mobility even when not enforceable.
4 We also include in our analysis firms that decreased their use of NCCs over this period. Given the very small number of such firms, we focus on the effect of increased NCC use in our headline job mobility findings. However, the estimates of the effect of decreased NCC use are consistent with our main findings that NCCs decrease worker mobility. See Appendix A.3.1 for more details.
5 By looking at changes over time within firms, our regression analysis incorporates firm fixed effects. We also include a range of worker and firm level controls and restrict our sample to firms that use either NDAs alone or both NCCs and NDAs (as described in the section above).
6 A focus on this period from 2021 also has the benefit of coinciding with access to high-frequency Single-Touch-Payroll (STP) data that allows us to precisely identify when a worker leaves their job and whether they moved to another firm after doing so. We show in Appendix A.5 that our results are broadly similar when we compare 2022 and 2023 instead.

firms that increased their use of NCCs saw a decrease in job-separation probability of about 1.8 percentage points, relative to workers at firms that did not change their use (equivalent to a roughly 11 percent decline in separation probability compared to the average rate for workers at these firms). Workers also saw a decline in job-to-job transition (J2J) probability of about 0.9 percentage points (a 10 percent decline), driven by a large decline in within industry (ANZSIC sub-division) J2J probability of about 0.5 percentage points (a 29 percent fall).[7]

We conduct a similar exercise looking at firms that increased their use of NDAs (as opposed to NCCs), but do not find any statistically significant changes in job mobility. The point estimate for the effect of increased NDA use on job separations is a material 1 percentage point, but the estimate is imprecise and an effect of zero is well within the confidence interval.[8]

Figure 3: Estimated change in job mobility between 2021 and 2023



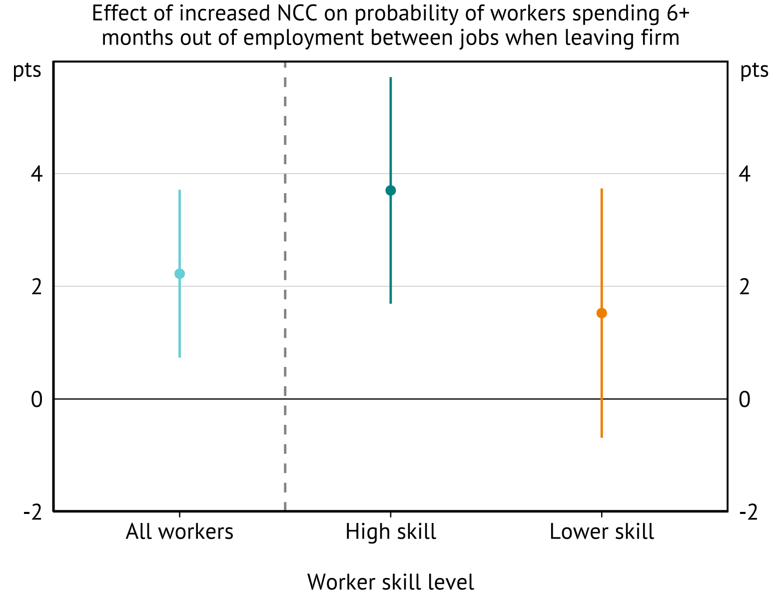
* The figure plots OLS estimates of Equation 1 with 90% confidence intervals. Values are multiplied by 100 for readability. Standard errors are corrected for two-way clustering at the firm and worker level. Regressions include occupation, state, remoteness area and firm fixed effects as well as a binned control for firm size, a second degree polynomial for worker age and tenure, and a control for worker gender.
** High skill workers are defined as those in occupations with ANZSCO skill level 1, which have skills commensurate with a bachelor degree or higher qualification. All other workers are defined as lower skill.
Sources: ABS; e61

Comparing workers of different skill levels, the headline negative associations between NCC use and mobility are similar but there are some interesting differences under the hood (Figure 3 panel B). For lower-skill workers,[9] when NCC use increases, they experience a large decline in the probability that they will leave their job and move to another firm in the same industry (ANZSIC sub-division). High-skill workers, by comparison, experience virtually no decline in switches within industries. This is surprising given we would expect NCCs to bind most for switches to close competitors. If instead we group workers by income level rather than skill level, the results are similar (Table A.9, A.10 and A.11).

Not only did workers become less likely to change jobs when NCC use increased, they also became more likely to spend a lengthy period out of employment when they did change jobs. We find that the relative probability of workers spending at least 6 months out of employment between jobs increased by just over 2 percentage points for workers leaving firms that increased their use of NCCs (Figure 4). This effect was primarily driven by high-skill workers, whose probability of spending at least 6 months out of employment in between jobs increased by almost 4 percentage points. This raises a concern that non-competes may distort participation of high-skill workers.

7 Unfortunately, the survey data we use only qualitatively describes changes in NCC use (e.g. increased, decreased, remained the same). This means that we cannot produce a more precise quantification of the association of NCC use and worker mobility. However, given that almost 70% of firms that use NCCs apply them to 76-100% of their workers, it seems likely that most firms that said they increased their use went from no use at all to blanket use. If instead most firms steadily increased their use, we would expect to see more of a distribution of values in the data, unless we happen to be at the very late stages of NCC adoption. Our estimates are still substantial in scale even if they represent the effect of an increase in NCC coverage from 0% to 100%.
8 Given the imprecision of our estimates, we are also unable to statistically rule out the possibility that the effect of increased NCC use is different from the effect of increased NDA use for all three mobility measures.
9 Lower-skill workers are defined as those in occupations with ANZSCO skill levels 2-5. High-skill workers are those in occupations with ANZSCO skill level 1, which have skills commensurate with a bachelor degree or higher qualification. This includes most managers and professional.

Figure 4: Estimated change in likelihood of spending at least 6-months out of the workforce between jobs



* The figure plots OLS estimates of Equation 1 with 90% confidence intervals. Values are multiplied by 100 for readability. Standard errors are corrected for two-way clustering at the firm and worker level. Regressions include occupation, state, remoteness area and firm fixed effects as well as a binned control for firm size, a second degree polynomial for worker age and tenure, and a control for worker gender. High skill workers are defined as those in occupations with ANZSCO skill level 1, which have skills commensurate with a bachelor degree or higher qualification. All other workers are defined as lower skill.
Sources: ABS; e61

While we control for many observable characteristics of workers and firms, and look at changes overtime within firms, we still cannot rule out the possibility that these declines in relative job mobility from 2021 to 2023 would have occurred regardless of the increased use of NCCs. Perhaps temporary factors raised job mobility at these firms in 2021 and 2022 and then subsided. However, we think this is unlikely to be the case.[10] [11]

Also relevant to interpreting these results is that we focus on firms that increased their NCC use recently. These firms make up roughly 10% of NCC using firms. It is possible that these relatively recent adopters of NCCs differ to prior adopters in the responsiveness of job mobility to NCC use, but we cannot prove this either way.

Non-competes may suppress wages growth

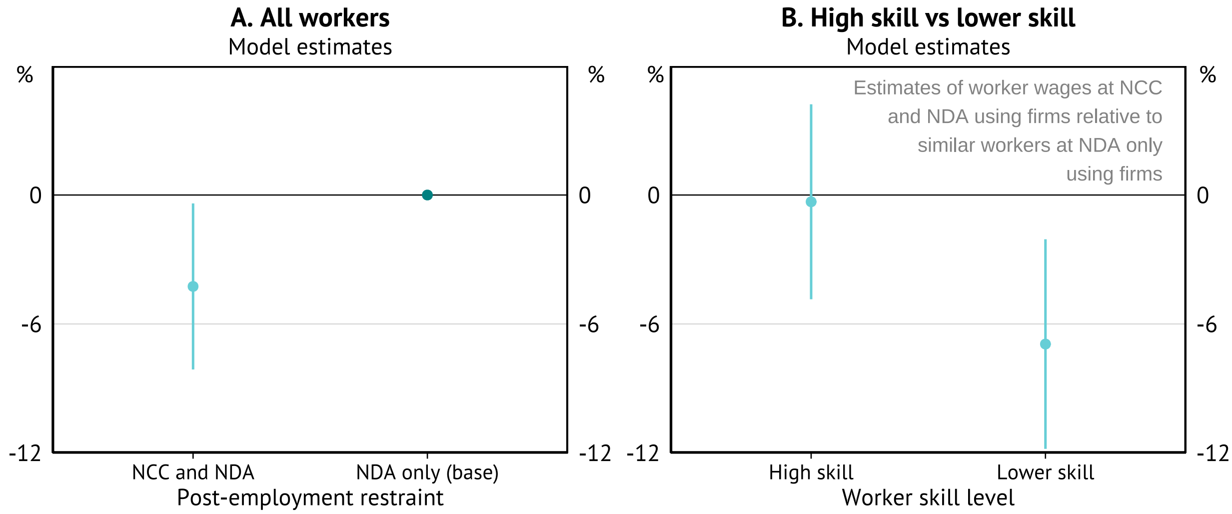
In this section, we present suggestive evidence that a decline in job mobility is associated with lower wages growth for workers with a NCC. This is consistent with the predictions of theoretical models where NCC cause a loss of worker bargaining power due to fewer outside employment opportunities (Gottfries & Jarosch, 2024; Shi, 2023). What's more, most workers with a NCC do not appear to receive a higher starting salary to compensate for their future lower wages growth. Putting these facts together, the average worker with a NCC earns 4% less than other similar workers with only an NDA (Figure 5 panel A, Appendix B.3 for methodology details). [12] This result is broadly consistent with evidence from the US (Box 1). The negative association with wages is concentrated among lower-skill workers (Figure 5 panel B).

10 The 2021 and 2022 financial years were very different periods of time – one still featured COVID lockdowns and JobKeeper support, the other the subsequent surge in the labour market – such that it is hard to figure what these temporary factors might have been. Our main results also remain similar in scale with the addition of controls for 'industry-time' effects that account for how different industries experienced this period (see Appendix A.3). We also find that in most other respects, firms that increased their use of NCCs were tracking similarly to those that did not change their use in terms of turnover, labour productivity, assets, capital expenditure, operating expenditure and research and development spending, conditioning on industry and firm size (see Appendix A.6). The lack of differences in observable characteristics does not rule out the possibility that there were differences in unobservable characteristics, but it does make it less likely if observable characteristics are correlated with potential unobservable confounders.

11 There are also factors that could cause our regression design to understate the effect of NCCs on job mobility. Rising NCC use at one firm may also reduce job turnover at other firms, since there are often 'chains' of separations and new hires across firms. As a result, rising NCC use in our 'treatment' group may be causing a decline in job mobility in our control group as well, leading us to underestimate the effect of treatment. Another possibility is that firms experiencing an environment of rising worker turnover in the first half of the 5-year period may have experienced a further rise in worker turnover absent their use of NCCs, if some sort of secular trend is at play.

12 For the interpretation of our results, note that we do not distinguish definitively between NCCs and 'non-solicitation' clauses that restrict the poaching of former clients and coworkers. Almost all firms that use NCCs also use non-solicitation clauses.

Figure 5: Non-compete use and worker wages



* The figure plots OLS estimates of Equation 2 with 90% confidence intervals. Values are multiplied by 100 for readability. Standard errors are corrected for two-way clustering at the firm and worker level. Regressions include occupation, state, remoteness area and industry fixed effects as well as a binned control for firm size, a second degree polynomial for worker age and tenure, and a control for worker gender. The NCC and NDA group includes workers at firms that use NDAs and NCCs for 76-100% of their workforce. The NDA only group includes workers at firms that do not use NCCs and use NDAs for 76-100% of their workforce. High skill workers are defined as those in occupations with ANZSCO skill level 1, which have skills commensurate with a bachelor degree or higher qualification. All other workers are defined as lower skill.
Sources: ABS; e61

To illustrate starting wages and subsequent wages growth, Figure 6 shows average relationships between (log) weekly wages and tenure for workers at a given firm. It compares wages at firms that make widespread use of both NDAs and NCCs, and firms that only make widespread use of NDAs.[13] Appendix B.3 provides further details on our methodology.

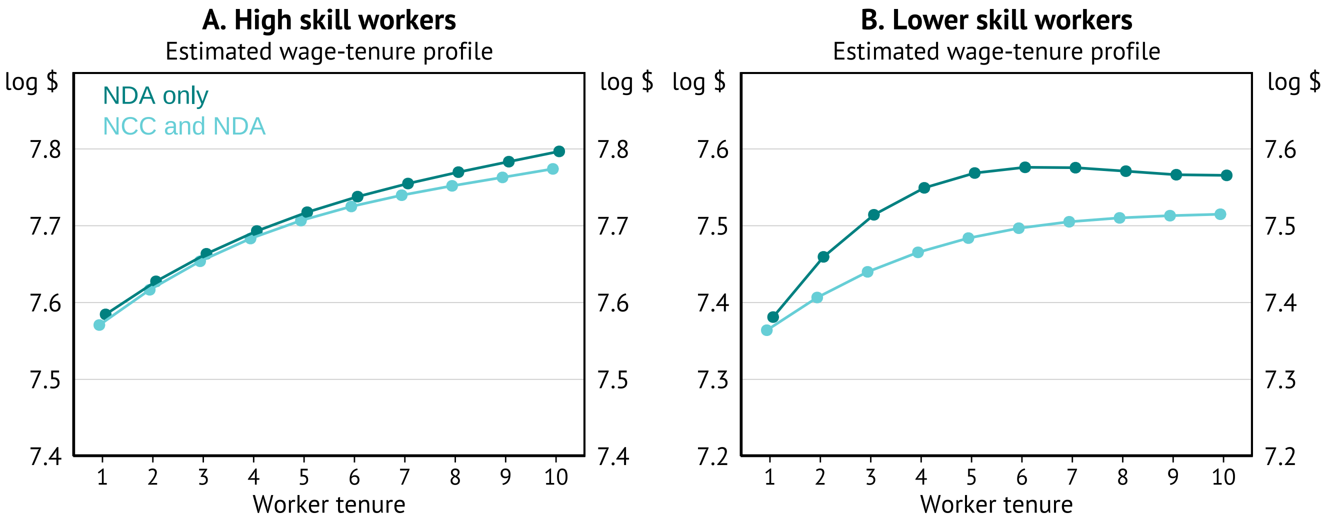
Results are presented separately for workers in high- and lower-skill occupations. For high-skill occupations, we find that workers at NCC using firms have similar starting wages and a broadly similar wages profile thereafter. By contrast, there is a stark difference in wages growth for workers in lower skill occupations. Lower skilled workers at firms that use NCCs have similar starting wages, but their wages grow much more slowly during the first 5 years of their employment.[14] After 5 years of tenure, our estimates imply that the presence of NCCs is associated with around a 10% lower wage level.[15]

The more benign outcomes for high-skill workers may indicate that there are positive effects of NCCs for these workers that offset a decline in bargaining power associated with lower job mobility (By the results above, lower job mobility appears to apply to both high- and lower-skill workers). Perhaps NCCs unlock greater investments in worker training or innovation activities for high-skill workers.

Interestingly, the gap between the wages of workers with an NCC and those with only an NDA appears to close somewhat for higher tenure, lower skilled workers. One potential explanation is that high-tenure workers at NCC using firms have higher wages growth as the result of increased training received earlier in their tenure. Unfortunately, we do not have the data on worker training to test this hypothesis and there are also other explanations that could explain this pattern. [16]

13 We define firms that make widespread use of NDAs and NCCs as those that apply them to 76-100% of their workforce – the highest use category in the ABS survey of firms. Almost 70% of firms that use NCCs fall into this 76-100% category. Almost all firms that make widespread use of NCCs also make widespread use of NDAs (Figure 1).
14 It is unlikely this result is driven by sample selection bias since if anything higher wages growth firms ought to be more incentivised to use NCCs.
15 This estimated difference is based on marginal effects at means calculated for workers with 5-years of tenure using the results of the regression reported in Table A.7.
16 For instance, lower tenure workers may receive the greatest potential benefit from using their outside options to negotiate higher wages.

Figure 6: Non-compete use and wage-tenure profiles



* The figure plots marginal effects at means based on the OLS estimation of Equation 3 reported in Appendix B. The NDA and NCC group includes workers at firms that use NDAs and NCCs for 76-100% of their workforce. The NDA only group includes workers at firms that do not use NCCs and use NDAs for 76-100% of their workforce. These estimates include occupation, state, remoteness area and industry fixed effects as well as a binned control for firm size, a second degree polynomial for worker age, a control for worker gender, and third degree polynomial for tenure interacted with a categorical variable for post-employment restraint use.
Sources: ABS; e61

Even with our efforts to control for observable and unobservable sources of selection bias, this analysis cannot pinpoint the causal effect on wages of workers signing a NCC. It is possible that firms and workers that sign both NCCs and NDAs remain different in important unobserved ways from those that sign NDAs alone. If so, our results would remain biased, to an extent that we cannot know. We do run additional tests on wages outcomes for 'job switching' workers that move into NCC firms vs NDA only firms. This effectively adjusts for any person-level fixed effect in the association of NCCs and initial worker wages. It continues to give results similar to those shown in Figure 6 (see Appendix A.4.1 for details of this job switching analysis).[17] Another caveat to have in mind is the potential for compositional changes in the relative 'quality' of workers at NCC using and NDA only firms at different tenures, beyond what the controls in our regression are able to adjust for. In Appendix A.8 we present some evidence of a 'survivorship bias' where higher-income workers are relatively more likely to leave NCC using firms at certain tenures, but the results do not neatly account for the relative wage-tenure profiles shown in Figure 6.

Policy implications

Our findings represent the first detailed empirical analysis of the relationship between NCC use, job mobility and workers' wages in Australia. We do not provide a complete cost-benefit assessment of non-competes and in particular we do not test directly the potential positive impacts of NCCs on investment and innovation. Nonetheless, our results have several important implications for policymakers.

First, they show that use of NCCs is associated with a reduction in job mobility and lower wages growth for the workers covered. Low job mobility had already been linked to the period of low wages growth in the 2010s (Quinn, 2019). Wages growth was persistently lower than could be explained by other standard macroeconomic determinants (Cassidy, 2019). More recently, even as a very tight labour market in 2022 and 2023 drove an increase in nominal wages growth, real wages went backwards. Moreover, workers that change jobs also contribute to the diffusion of ideas, the creation of new firms and the supply of labour to fast-growing high-productivity firms. Our results are not causal, but they are consistent with a view that the proliferation of NCCs has reduced the quality of matches between workers and firms and contributed to the recent low levels of job mobility, wages growth and productivity growth.

17 For completeness, we also look at how wages growth differs at firms that increased their use of NCCs. That is, we apply a similar method to the job mobility analysis above, but to wages growth. We do not find significant differences in wages growth between workers at firms that increased their use of NCCs and other workers in our sample. A limitation with applying this method to wages growth analysis is that with only a limited time period (mid 2021 to late 2023) there is limited time for the effects of new NCC use to affect wages. Our findings in Figure 4 suggest that the full negative association between NCC use and wages takes several years to eventuate as an employee remains at the firm.

Second, NDAs appear to offer some similar potential benefits to NCCs, with fewer downsides for job mobility and worker wages. NDAs have some similar properties to NCCs: they prevent workers from disclosing confidential information gained during the course of employment to another firm. Along with the patent system, they offer an alternative way for firms to protect trade secrets. In theory, NDAs could incentivise firms to share proprietary information more widely across the organisation and innovate more. US evidence suggests that a combination of NDAs and NCCs does not protect trade secrets any more than NDAs alone (Cowgill et al., n.d.). If a similar finding were to hold in Australia, then it would be notable that NDA use does not appear to have a significant negative relationship with job mobility and that wages growth for workers with only an NDA is higher than for those with an NCC in addition over the first few years of tenure.

Third, workers in lower-paid and lower-skill occupations do not appear to be immune to the negative consequences of NCCs for job mobility even though NCCs may not be legally enforceable for many of them. In fact, the negative association of wages growth and NCC use is most acute in lower-skill occupations. Many of the traditional arguments in favour of NCCs – that they foster innovation by protecting trade secrets, or that they incentivise firms to make costly investments in worker training – are harder to make for these workers. On face value, this suggests that workers in lower-skill occupations are experiencing the pain of NCCs without even the prospect of the gain.

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A.1. Estimates of the share of workers covered by NCCs and NDAs

Estimating worker coverage of restraints from a firm-side survey

Since firms report the share of their workforce subject to each restraint in ranges (e.g. 76-100% as opposed to an exact number; see Appendix B.1 for more details of the data), it is necessary to make a few assumptions to construct an aggregate estimate of prevalence. This results in range of estimates for the share of workers subject to each constraint. Our approach can be broken down into three steps.

Step 1: Estimating the extensive margin

We start by estimating the extensive margin of NCC and NDA use − whether a firm uses these restraints at all. Here our key assumption is that we treat "unsure" responses as missing at random and drop them from our analysis. While a firm reporting that they are unsure whether they use a particular clause may indicate that they do not use it at all, we find that very large firms, who likely have more sophisticated HR departments, are the most likely to report that they are unsure. This suggests that unsure responses may instead reflect the fact that organisations may be uncertain whether a particular branch, team or store applies these restraints.

Step 2: Estimating the intensive margin

Next, for each firm that reports using NCCs, we estimate the intensive margin of their use − the average share of a firm's workforce who are covered by each clause for firms applying the clause. Here we again treat unsure responses as missing at random and drop them from our analysis. Because the shares reported by firms are in ranges, we develop three estimates for each firm at this stage: low − taking the share at the bottom of each range (e.g. 76-100% -> 76%), mid − the middle of each range (e.g. 88%) and high − the top of each range (e.g. 100%).

Step 3: Producing an aggregate estimate

The third step involves combining the above two estimates at the firm level into an aggregating estimate. This last step is conducted in two stages. First, we produce an aggregate estimate for firms of a particular size, weighting the firm level estimates by the number of workers employed at that firm in STP at the start of the 2023 financial year. Next, we produce a aggregate figure from these firm size specific averages by taking a weighted mean where the weights are determined by the relative employment shares of each firm size set out in the ABS Employee Earnings and Hours May 2023 release.

This estimate improves on the original estimate in Andrews et al. (2024), but remains subject to some uncertainty, especially regarding the treatment of "unsure" responses, but also selection into the survey (30% of firms in the EEH did not respond at all to this voluntary survey) and as a result of the fact that each firm reported their use of these restraints in ranges.

Table A.1: Estimated share of workers subject to restraint clauses

	Lower	Central	Upper
Non-disclosure	48%	56%	65%
Non-compete	15%	18%	21%

Sources: ABS Short Survey of Employment Conditions, e61 analysis

A.2. Raw data relationships

In the simplest possible comparison, we find that NCC using firms have slightly higher wages and much higher rates of job mobility than other firms. Box 2 describes the likely sources of selection bias that drive these results. Once we take steps to account for observable and some unobservable differences between workers and firms that choose to use NCCs in our empirical analysis, we find the opposite is true: NCC use is associated with a decline in job mobility and lower wages.

Figure A.1: Raw data

* The figure plots the mean mobility rates and the mean weekly wages of workers employed by firms that participated in the SSEC survey. The sample mirrors that used in our regression analysis. We drop workers who were not at the firm at the beginning of the year and workers who regularly earn below the full-time weekly minimum wage. Further details on the sample used in our analysis can be found in Appendix B.
Sources: ABS; e61

A.3. Main results

A.3.1 The effect of increased NCC use

This section provides a summary of our main results (Tables A.2 to A.5) and additional charts to support our analysis. These results provide additional detail behind our headline findings presented in the main body of the report in Figures 3 and 4.

Table A.2: NCC mobility results

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. var.	Separations	J2J transitions	J2J within division	J2J within sub-division	Job gap 6+ months	log(Wages)
Increased NCC x 2023	-0.018*	-0.009	-0.005	-0.005*	0.022**	-0.005
	(0.01)	(0.006)	(0.003)	(0.003)	(0.009)	(0.015)
Decreased NCC x 2023	0.047	0.031	0.037**	0.032**	-0.016	-0.004
	(0.06)	(0.029)	(0.018)	(0.014)	(0.047)	(0.043)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Occupation FE	Yes	Yes	Yes	Yes	Yes	Yes
Area FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year FE	No	No	No	No	No	No
R2	0.109	0.086	0.049	0.034	0.067	0.389
Within R2	0.014	0.009	0.002	0.001	0.027	0.07
N	1,091,257	1,091,257	1,012,763	1,012,763	91,420	1,091,257

Notes: This table shows the OLS estimation of Equation 1 using two calendar years of data - 2021 and 2023. The interaction between 2023 and the increased (decreased) NCC use dummy captures how job mobility changed between 2021 and 2023 for workers at firms that increased (decreased) their use of NCCs relative to workers at firms who did not change their use. Control variables include a binned control for firm size, a control for worker gender, a second-degree polynomial for worker age and a dummy for whether the worker had just joined the firm (<12 months of tenure). Standard errors are reported in parentheses and are corrected for two-way clustering at the firm and individual level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.3: NDA mobility results

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. var.	Separations	J2J transitions	J2J within division	J2J within sub-division	Job gap 6+ months	log(Wages)
Increased NDA x 2023	-0.009	-0.002	-0.003	-0.002	-0.004	0.007
	(0.01)	(0.006)	(0.003)	(0.002)	(0.01)	(0.013)
Decreased NDA x 2023	-0.039	-0.013	0.003	0.001	0.004	-0.023**
	(0.028)	(0.022)	(0.013)	(0.014)	(0.03)	(0.012)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Occupation FE	Yes	Yes	Yes	Yes	Yes	Yes
Area FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year FE	No	No	No	No	No	No
R2	0.112	0.089	0.051	0.035	0.067	0.383
Within R2	0.014	0.01	0.002	0.001	0.025	0.07
N	1,008,093	1,008,093	936,814	936,814	85,030	1,008,093

Notes: This table shows the OLS estimation of Equation 1 using two calendar years of data - 2021 and 2023. The interaction between 2023 and the increased (decreased) NDA use dummy captures how job mobility changed between 2021 and 2023 for workers at firms that increased (decreased) their use of NDAs relative to workers at firms who did not change their use. Control variables include a binned control for firm size, a control for worker gender, a second-degree polynomial for worker age and a dummy for whether the worker had just joined the firm (<12 months of tenure). Standard errors are reported in parentheses and are corrected for two-way clustering at the firm and individual level. * p < 0.10, ** p < 0.05, *** p < 0.01.

Table A.4: Lower skill workers

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. var.	Separations	J2J transitions	J2J within division	J2J within sub-division	Job gap 6+ months	log(Wages)
Increased NCC x 2023	-0.015	-0.01	-0.009*	-0.01**	0.015	-0.017
	(0.01)	(0.009)	(0.005)	(0.005)	(0.013)	(0.015)
Decreased NCC x 2023	0.058	0.043	0.038	0.034*	0.031	0.043
	(0.05)	(0.032)	(0.023)	(0.019)	(0.031)	(0.03)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Occupation FE	Yes	Yes	Yes	Yes	Yes	Yes
Area FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year FE	No	No	No	No	No	No
R2	0.13	0.105	0.045	0.044	0.08	0.447
Within R2	0.015	0.011	0.002	0.001	0.024	0.051
N	603,386	603,386	552,312	552,312	54,793	603,386

Notes: This table shows the OLS estimation of Equation 1 using two calendar years of data - 2021 and 2023. The interaction between 2023 and the increased (decreased) NCC use dummy captures how job mobility changed between 2021 and 2023 for workers at firms that increased (decreased) their use of NCCs relative to workers at firms who did not change their use. Control variables include a binned control for firm size, a control for worker gender, a second-degree polynomial for worker age and a dummy for whether the worker had just joined the firm (<12 months of tenure). Standard errors are reported in parentheses and are corrected for two-way clustering at the firm and individual level. * p < 0.10, ** p < 0.05, *** p < 0.01.

Table A.5: High skill workers

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. var.	Separations	J2J transitions	J2J within division	J2J within sub-division	Job gap 6+ months	log(Wages)
Increased NCC x 2023	-0.023	-0.008	0.001	0.001	0.037***	0.014
	(0.015)	(0.006)	(0.003)	(0.002)	(0.012)	(0.025)
Decreased NCC x 2023	0.033	0.011	0.035**	0.028**	-0.073	-0.057
	(0.08)	(0.034)	(0.014)	(0.012)	(0.068)	(0.063)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Occupation FE	Yes	Yes	Yes	Yes	Yes	Yes
Area FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year FE	No	No	No	No	No	No
R2	0.091	0.068	0.062	0.03	0.086	0.303
Within R2	0.012	0.007	0.002	0.001	0.032	0.102
N	487,871	487,871	460,451	460,451	36,627	487,871

Notes: This table shows the OLS estimation of Equation 1 using two calendar years of data - 2021 and 2023. The interaction between 2023 and the increased (decreased) NCC use dummy captures how job mobility changed between 2021 and 2023 for workers at firms that increased (decreased) their use of NCCs relative to workers at firms who did not change their use. Control variables include a binned control for firm size, a control for worker gender, a second-degree polynomial for worker age and a dummy for whether the worker had just joined the firm (<12 months of tenure). Standard errors are reported in parentheses and are corrected for two-way clustering at the firm and individual level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A.3.2 The relationship between NCCs and workers' wages

In this section we present the results for our analysis of the relationship between NCC use and workers' wages. These results provide further detail to support the analysis in the main body of the report on worker wages (Figure 5) and wage dynamics (Figure 6).

Table A.6: NCC cross-sectional wage differences

	(1) All workers	(2) All workers	(3) High skill	(4) Lower skill
Dep. var.	log(Wages)	log(Wages)	log(Wages)	log(Wages)
NDA and NCC	-0.043*	-0.043*	-0.003	-0.069**
	(0.023)	(0.023)	(0.028)	(0.03)
None	-0.041*	-0.042*	-0.03	-0.048**
	(0.022)	(0.022)	(0.024)	(0.023)
Controls	Yes	Yes	Yes	Yes
Year FE	Yes	No	Yes	Yes
Industry FE	Yes	No	Yes	Yes
Occupation FE	Yes	Yes	Yes	Yes
Area FE	Yes	Yes	Yes	Yes
Industry-year FE	No	Yes	No	No
R2	0.252	0.253	0.181	0.21
Within R2	0.074	0.074	0.097	0.063
N	1,316,981	1,316,981	705,076	611,905

Notes: This table shows the OLS estimation of Equation 2 using data from the 2022, 2023 and 2024 financial years. The NDA and NCC variable capture how worker's wages differ at firms that use NDAs and NCCs for 76-100% of their workforce, relative to firms that do not use NCCs and use NDAs for 76-100% of their workforce. Similarly, None captures the difference between firms that do not use either NCCs or NDAs and those that only use NDAs. Control variables include a binned control for firm size, a control for worker gender, a second-degree polynomial for worker age and a second-degree polynomial for worker tenure. Standard errors are reported in parentheses and are corrected for two-way clustering at the firm and individual level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.7: NCC use and worker wage dynamics

	(1) All workers	(3) High skill	(4) Lower skill
Dep. var.	log(Wages)	log(Wages)	log(Wages)
None	0.487	0.256	0.525
	(0.44)	(0.404)	(0.441)
NDA and NCC	0.054	-0.186	0.359
	(0.353)	(0.348)	(0.434)
Tenure/10	0.902***	0.556***	1.210***
	(0.173)	(0.133)	(0.249)
$(\text{Tenure}/10)^2$	-0.104***	-0.046*	-0.157***
	(0.03)	(0.024)	(0.044)
$(\text{Tenure}/10)^3$	0.004***	0.002	0.007***
	(0.001)	(0.001)	(0.002)
Tenure/10 x None	-0.514	-0.365	-0.448
	(0.355)	(0.367)	(0.296)
Tenure/10 x NDA and NCC	-0.282	0.057	-0.625**
	(0.211)	(0.229)	(0.278)
$(\text{Tenure}/10)^2$ x None	0.079	0.056	0.059
	(0.055)	(0.059)	(0.048)
$(\text{Tenure}/10)^2$ x NDA and NCC	0.044	-0.011	0.100**
	(0.038)	(0.045)	(0.048)
$(\text{Tenure}/10)^3$ x None	-0.004	-0.003	-0.003
	(0.002)	(0.003)	(0.002)
$(\text{Tenure}/10)^3$ x NDA and NCC	-0.002	0.000	-0.005**
	(0.002)	(0.002)	(0.002)
Controls	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes
Occupation FE	Yes	Yes	Yes
Area FE	Yes	Yes	Yes
Industry-year FE	No	No	No
R2	0.248	0.153	0.23
Within R2	0.067	0.068	0.068
N	1,559,553	806,943	752,610

Notes: This table shows the OLS estimation of Equation 3 using data from the 2022, 2023 and 2024 financial years. Tenure is rescaled, dividing by 10, to display the coefficient scale properly. The NDA and NCC variable capture how worker's wage dynamics differ at firms that use NDAs and NCCs for 76-100% of their workforce, relative to firms that do not use NCCs and use NDAs for 76-100% of their workforce. Similarly, None captures the difference between firms that do not use either NCCs or NDAs and those that only use NDAs. Control variables include a binned control for firm size, a control for worker gender, a second-degree polynomial for worker age and a second-degree polynomial for worker tenure. Standard errors are reported in parentheses and are corrected for two-way clustering at the firm and individual level. * p < 0.10, ** p < 0.05, *** p < 0.01.

A.4. Additional analysis

A.4.1 Wage gains from job switching

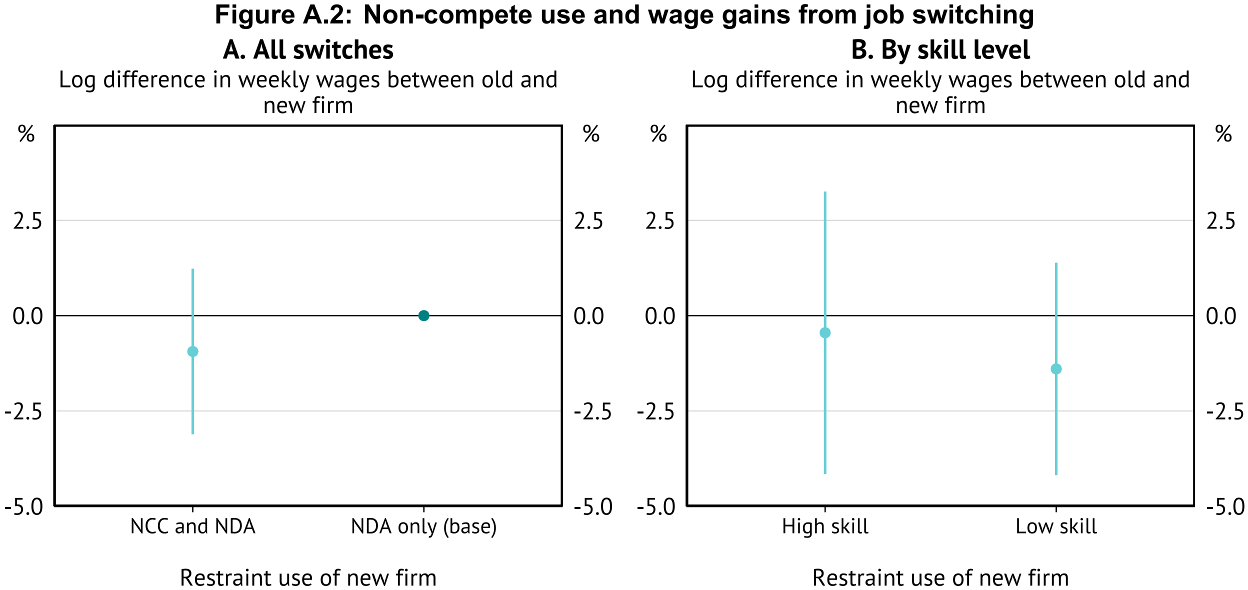
This section investigates whether workers recevie a higher starting salary when they sign a NCC. Theory predicts that when workers sign a NCC, their wage should be less 'backloaded' because their employer will need to compete less against outside offers later on (Shi, 2023). Knowing this, theory predicts that when workers join a new firm and sign a NCC, they will negotiate a higher wage initially to compensate for lower wage growth later (Shi, 2023).

But are workers actually negotiating a higher starting salary when they sign a NCC? In our analysis of wage dynamics, we found no evidence that workers were receiving higher starting salaries at firms with widespread use of NCCs (Figure 6). However, it could be that there are unobserved differences between workers that confound this analysis of wage levels.[18]

To better test whether workers receive wage gains when signing a NCC we look at data on actual job switches. We examine jobs switches where a worker leaves a firm that makes high use of NDAs, but no use of NCCs. This ensures that we are not partly capturing the effect of leaving an NCC using firm on the wage gains from job switching.[19] By comparing wages before and after a job switch, we can effectively control for time-invariant person-level characteristics that may affect wages (e.g. natural talent).

We find that when workers move from an NDA using firm to an NCC using firm, they do not receive a larger pay bump than workers moving to an NDA only using firms (Figure A.2). If anything, workers receive slightly smaller wage gains when they switch to a firm that makes widespread use of both NCCs and NDAs (76-100%), compared to workers who switch to firms that only makes widespread use of NDAs. These results are similar for both high and lower skill workers.

Figure A.2: Non-compete use and wage gains from job switching



* The figure plots OLS estimates of Equation 4 with 90% confidence intervals. Values are multiplied by 100 for readability. Standard errors are corrected for two-way clustering at the firm and worker level. Regressions include occupation, year, state, remoteness area and industry fixed effects as well as a binned control for firm size, a second degree polynomial for worker age and tenure, and a control for worker gender.
Sources: ABS; e61

18 For instance, if workers who choose to join NCC using firms are less educated than those that join NDA only firms, this could lead to a level shift in their wage growth profile that would obscure any initial gains.

19 For example, workers leaving an NCC using may move to a different industry to avoid breaching an NCC, which could have negative consequences on their wages. Alternatively, workers leaving NCC firms may be a selected sample of workers moving to much higher paying jobs if these are the only jobs where the risk of having the NCC enforced is worth it.

Table A.8: Wage gains from job switching, relative to moving to a firm that uses NDAs

	(1) All switches	(2) High skill	(3) Lower skill
Dep. var.	$\log(\frac{\text{Wages dest.}}{\text{Wages orig.}})$	$\log(\frac{\text{Wages dest.}}{\text{Wages orig.}})$	$\log(\frac{\text{Wages dest.}}{\text{Wages orig.}})$
To NDA and NCC using firm	-0.009	-0.004	-0.014
	(0.013)	(0.023)	(0.017)
To no NDA or NCC using firm	-0.023*	-0.031	-0.018
	(0.012)	(0.019)	(0.017)
Controls	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes
Occupation FE	Yes	Yes	Yes
Area FE	Yes	Yes	Yes
Industry-year FE	No	No	No
R2	0.068	0.066	0.095
Within R2	0.046	0.041	0.048
N	4,633	2,110	2,523

Notes: This table shows the OLS estimation of Equation 4 using data from the 2022, 2023 and 2024 financial years. The NDA and NCC variable capture how worker's wage gains from job switching differ when they move to firms that use NDAs and NCCs for 76-100% of their workforce, relative to moving to firms that do not use NCCs and use NDAs for 76-100% of their workforce. Similarly, no NDA or NCC captures the difference between moving to a firm that does not use either NCCs or NDAs, to one that only uses NDAs. Control variables include a binned control for the size of the firm a worker leaves, a control for worker gender, a second-degree polynomial for worker age and a second-degree polynomial for worker tenure. Standard errors are reported in parentheses and are corrected for two-way clustering at the firm and individual level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A.4.2 Mobility results by worker wage tercile

Tables A.9 to A.11 present out main results for the effect of increased NCC use broken down by worker income tercile. They show that workers of all income levels experience similar declines in job separations, although the effect is only statistically significant for the highest earning group. Similarly, the effect on time out of employment between jobs is similar in scale for workers of all income levels, but only significant for the highest earning group. Overall, these results suggest that the effect of NCCs is similar across income groups, but strongest for high-wage workers. Given the imprecision of our estimates we cannot statistically rule out the possibility that these effects on mobility are the same on workers of all income levels.

Table A.9: Lowest wage tercile

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. var.	Separations	J2J transitions	J2J within division	J2J within sub-division	Job gap 6+ months	log(Wages)
Increased NCC x 2023	-0.016	-0.004	-0.005	-0.007	0.017	-0.005
	(0.012)	(0.006)	(0.005)	(0.005)	(0.017)	(0.006)
Decreased NCC x 2023	0.08	0.069**	0.056*	0.045*	0.018	-0.003
	(0.05)	(0.032)	(0.029)	(0.026)	(0.057)	(0.007)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Occupation FE	Yes	Yes	Yes	Yes	Yes	Yes
Area FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year FE	No	No	No	No	No	No
R2	0.138	0.099	0.042	0.048	0.097	0.151
Within R2	0.018	0.013	0.002	0.001	0.02	0.011
N	363,598	363,598	348,063	348,063	30,736	363,598

Notes: This table shows the OLS estimation of Equation 1 using two calendar years of data - 2021 and 2023. The interaction between 2023 and the increased (decreased) NCC use dummy captures how job mobility changed between 2021 and 2023 for workers at firms that increased (decreased) their use of NCCs relative to workers at firms who did not change their use. Control variables include a binned control for firm size, a control for worker gender, a second-degree polynomial for worker age and a dummy for whether the worker had just joined the firm (<12 months of tenure). Standard errors are reported in parentheses and are corrected for two-way clustering at the firm and individual level. * p < 0.10, ** p < 0.05, *** p < 0.01.

Table A.10: Middle wage tercile

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. var.	Separations	J2J transitions	J2J within division	J2J within sub-division	Job gap 6+ months	log(Wages)
Increased NCC x 2023	-0.017	-0.007	-0.005	-0.005	0.024	0.003
	(0.014)	(0.009)	(0.004)	(0.004)	(0.017)	(0.003)
Decreased NCC x 2023	0.018	-0.006	0.02	0.026***	-0.126	-0.02***
	(0.055)	(0.027)	(0.014)	(0.008)	(0.108)	(0.004)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Occupation FE	Yes	Yes	Yes	Yes	Yes	Yes
Area FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year FE	No	No	No	No	No	No
R2	0.127	0.097	0.05	0.047	0.103	0.109
Within R2	0.012	0.008	0.002	0.001	0.028	0.018
N	359,457	359,457	328,936	328,936	26,482	359,457

Notes: This table shows the OLS estimation of Equation 1 using two calendar years of data - 2021 and 2023. The interaction between 2023 and the increased (decreased) NCC use dummy captures how job mobility changed between 2021 and 2023 for workers at firms that increased (decreased) their use of NCCs relative to workers at firms who did not change their use. Control variables include a binned control for firm size, a control for worker gender, a second-degree polynomial for worker age and a dummy for whether the worker had just joined the firm (<12 months of tenure). Standard errors are reported in parentheses and are corrected for two-way clustering at the firm and individual level. * p < 0.10, ** p < 0.05, *** p < 0.01.

Table A.11: Highest wage tercile

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. var.	Separations	J2J transitions	J2J within division	J2J within sub-division	Job gap 6+ months	log(Wages)
Increased NCC x 2023	-0.021*	-0.017*	-0.003	-0.002	0.025*	0.021
	(0.012)	(0.01)	(0.004)	(0.004)	(0.014)	(0.027)
Decreased NCC x 2023	-0.006	-0.009	0.011	0.007	-0.023	-0.038
	(0.066)	(0.032)	(0.011)	(0.012)	(0.077)	(0.049)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Occupation FE	Yes	Yes	Yes	Yes	Yes	Yes
Area FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year FE	No	No	No	No	No	No
R2	0.147	0.109	0.083	0.039	0.097	0.192
Within R2	0.02	0.01	0.002	0.001	0.032	0.026
N	368,202	368,202	335,764	335,764	34,202	368,202

Notes: This table shows the OLS estimation of Equation 1 using two calendar years of data - 2021 and 2023. The interaction between 2023 and the increased (decreased) NCC use dummy captures how job mobility changed between 2021 and 2023 for workers at firms that increased (decreased) their use of NCCs relative to workers at firms who did not change their use. Control variables include a binned control for firm size, a control for worker gender, a second-degree polynomial for worker age and a dummy for whether the worker had just joined the firm (<12 months of tenure). Standard errors are reported in parentheses and are corrected for two-way clustering at the firm and individual level. * p < 0.10, ** p < 0.05, *** p < 0.01.

A.5. Robustness

Several robustness checks of our results are reported in Tables A.12 to A.15:

1. In Table A.12 we test the sensitivity of our main mobility results to the inclusion of industry-year fixed effects. Reassuringly, our results are robust to these additional controls with our points estimates increasing slightly in magnitude and statistical significance. This suggests that our findings are not driven by industry-specific shocks, such as the hospitality industry being more exposed to COVID-19 lockdowns, which may have been correlated with increased NCC use.
2. In Table A.13, we test how our main mobility results change if we restrict our control group to only include firms that make at least some use of NCCs, as opposed to all firms who use either NCCs or NDAs. Restricting our analysis to only NCC using firms reduces the statistical significance of most of our results. The one result that is robust to this change in control groups is our estimate of the effect of increased NCC use on the probability that workers switch to a firm within the same industry subdivision (J2J within sub-division).
3. In Table A.14 we test the sensitivity of our main mobility results to the choice of time period used in our analysis. We find that restricting the analysis to a comparison of the 2022 and 2023 calendar years (rather than 2021 and 2023) also reduces the size and statistical significance of our results, although our results for job separations, J2Js and time out of the workforce (job gap) remain similar in magnitude. The fact that our results are affected by this change is largely unsurprising given it is likely that NCC use continued to increase at some of these firms through 2021 and 2022. This means that the size of the effect of increased NCC use would have been smaller between 2022 and 2023.
4. In Table A.15, we test the sensitivity of our cross-sectional wage results to the use of alternate samples and specifications.
   - In column (1), we show that our cross-sectional wage results are robust to the inclusion of industry-year FE.
   - In column (2), we show that the inclusion of a control for each firm's labour productivity reduces the size and statistical significance our wage results. However, this change in our estimates appears to be driven purely by a change in the composition of our sample of firms and workers in our analysis.

     When we restrict the sample of firms to only include those with data on their labour productivity (column (3)), we find effects on wages that are even smaller than our estimates that include a control for labour productivity. This suggests that the change in our estimates is being driven by the change in the sample of firms, rather than because controlling for labour productivity explains the difference in wages between NCC and NDA using firms.

     The firms that are missing a measure of labour productivity are mostly larger firms in the education and health sectors. Because we rely on Business Activitiy Statements (BAS) data for a timely measure of firm output, we only capture firms that report GST payments. Many firms in the education and health services industries are not captured by BAS data as many medical and education services are exempt from GST.[20] The extent to which this change in our sample affects our wage results suggests that the impact of NCCs on wages may be particularly strong in these sectors.
   - In column (4), we show how our results change if we compare workers are firms that make any use of NCCs and NDAs, to workers at firms that do not use NCCs, but do make some use of NDAs. This differs from our main analysis because we remove the requirement that firms widely use each restraint (apply them to 76-100%). This change increases the number of firms and workers in our sample. However, it means that we move from having a high degree of confidence that a worker has either an NCC or NDA, to a much lower degree of confidence that they are subject to these restraints (most firms that do not use these restraints widely apply them to only 10-20% of their workforce).

     We find that using this expanded sample, workers at NCC and NDA using firms are paid slightly more than workers at NDA only using firms, although the effect is statistically insignificant. This is opposite in sign to the effect that we found when using our preferred method (ie looking only at firms that widely use the restraints). While we are not able to determine exactly why our results change, one possibility is that firms that apply NCCs sparingly are higher paying firms (e.g. because they are more productive, or are just more generous with how much they pay their workers), which could mean that even if those workers who sign NCCs are still left worse off, this effect is drowned out by differences between the 80%+ of workers who do not sign an NCC. It could also be the case that if firms are deploying NCCs

20 BAS data is collected for GST reporting purposes and is much more timely that Business Income Tax data, which future research may be able to use.

sparingly, they are more likely to be negotiating in good faith and properly compensating workers in return for signing an NCC.

- In column (5), we show that our results are similar when we restrict the comparison to focus on wages in the 2024 financial year, which is the period of time where we are most confident in our measure of firm's NCC use.

Table A.12: Mobility: Adding industry-year fixed effects

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. var.	Separations	J2J transitions	J2J within division	J2J within sub-division	Job gap 6+ months	log(Wages)
Increased NCC x 2023	-0.019*	-0.011*	-0.007	-0.008*	0.026***	-0.005
	(0.01)	(0.007)	(0.005)	(0.004)	(0.008)	(0.016)
Decreased NCC x 2023	0.056	0.033	0.035*	0.03**	-0.013	-0.015
	(0.06)	(0.03)	(0.019)	(0.015)	(0.051)	(0.044)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Occupation FE	Yes	Yes	Yes	Yes	Yes	Yes
Area FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year FE	Yes	Yes	Yes	Yes	Yes	Yes
R2	0.11	0.087	0.05	0.035	0.067	0.397
Within R2	0.015	0.01	0.002	0.001	0.014	0.068
N	1,018,645	1,018,645	1,012,763	1,012,763	88,671	1,018,645

Notes: This table shows the OLS estimation of Equation 1 using two calendar years of data - 2021 and 2023. The interaction between 2023 and the increased (decreased) NCC use dummy captures how job mobility changed between 2021 and 2023 for workers at firms that increased (decreased) their use of NCCs relative to workers at firms who did not change their use. Control variables include a binned control for firm size, a control for worker gender, a second-degree polynomial for worker age and a dummy for whether the worker had just joined the firm (<12 months of tenure). Standard errors are reported in parentheses and are corrected for two-way clustering at the firm and individual level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.13: Mobility: Only NCC using firms

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. var.	Separations	J2J transitions	J2J within division	J2J within sub-division	Job gap 6+ months	log(Wages)
Increased NCC x 2023	0.001	-0.002	-0.004	-0.005*	0.006	0.00
	(0.01)	(0.007)	(0.003)	(0.003)	(0.009)	(0.016)
Decreased NCC x 2023	-0.001	0.01	0.022*	0.021*	-0.055	-0.044
	(0.029)	(0.023)	(0.013)	(0.012)	(0.068)	(0.043)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Occupation FE	Yes	Yes	Yes	Yes	Yes	Yes
Area FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year FE	No	No	No	No	No	No
R2	0.098	0.082	0.027	0.028	0.051	0.414
Within R2	0.018	0.012	0.002	0.001	0.016	0.076
N	484,599	484,599	481,272	481,272	50,540	484,599

Notes: This table shows the OLS estimation of Equation 1 using two calendar years of data - 2021 and 2023. The interaction between 2023 and the increased (decreased) NCC use dummy captures how job mobility changed between 2021 and 2023 for workers at firms that increased (decreased) their use of NCCs relative to workers at firms who did not change their use. Control variables include a binned control for firm size, a control for worker gender, a second-degree polynomial for worker age and a dummy for whether the worker had just joined the firm (<12 months of tenure). Standard errors are reported in parentheses and are corrected for two-way clustering at the firm and individual level. * p < 0.10, ** p < 0.05, *** p < 0.01.

Table A.14: Mobility: Comparing 2022 to 2023 (rather than 2021 and 2023)

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. var.	Separations	J2J transitions	J2J within division	J2J within sub-division	Job gap 6+ months	log(Wages)
Increased NCC x 2023	-0.012	-0.005	0.001	0.001	0.013	-0.013
	(0.009)	(0.005)	(0.002)	(0.002)	(0.008)	(0.01)
Decreased NCC x 2023	0.036	0.034	0.03*	0.031**	-0.003	0.012
	(0.036)	(0.023)	(0.017)	(0.014)	(0.034)	(0.02)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Occupation FE	Yes	Yes	Yes	Yes	Yes	Yes
Area FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year FE	No	No	No	No	No	No
R2	0.103	0.084	0.032	0.034	0.054	0.381
Within R2	0.013	0.01	0.002	0.001	0.017	0.073
N	1,102,186	1,102,186	1,092,668	1,092,668	92,015	1,102,185

Notes: This table shows the OLS estimation of Equation 1 using two calendar years of data - 2021 and 2023. The interaction between 2023 and the increased (decreased) NCC use dummy captures how job mobility changed between 2021 and 2023 for workers at firms that increased (decreased) their use of NCCs relative to workers at firms who did not change their use. Control variables include a binned control for firm size, a control for worker gender, a second-degree polynomial for worker age and a dummy for whether the worker had just joined the firm (<12 months of tenure). Standard errors are reported in parentheses and are corrected for two-way clustering at the firm and individual level. * p < 0.10, ** p < 0.05, *** p < 0.01.

Table A.15: Wages: Alternate controls and specifications

	(1) Industry-year FE	(2) Prod. Control	(3) Prod. Sample	(4) All firms	(5) FY 2024
Dep. var.	log(Wages)	log(Wages)	log(Wages)	log(Wages)	log(Wages)
NDA and NCC	-0.043*	-0.017	-0.011	0.012	-0.047*
	(0.023)	(0.027)	(0.027)	(0.025)	(0.024)
None	-0.042*	-0.032	-0.047*	-0.04**	-0.03
	(0.022)	(0.025)	(0.025)	(0.02)	(0.026)
Controls	Yes	Yes	Yes	Yes	Yes
Year FE	No	Yes	Yes	Yes	Yes
Industry FE	No	Yes	Yes	Yes	Yes
Occupation FE	Yes	Yes	Yes	Yes	Yes
Area FE	Yes	Yes	Yes	Yes	Yes
Firm productivity	No	Yes	No	No	No
Industry-year FE	Yes	No	No	No	No
R2	0.253	0.359	0.352	0.254	0.254
Within R2	0.074	0.114	0.104	0.073	0.071
N	1,316,981	394,782	394,782	1,778,233	448,388

Notes: This table shows the OLS estimation of Equation 2 using data from the 2022, 2023 and 2024 financial years. The NDA and NCC variable capture how worker's wages differ at firms that use NDAs and NCCs for 76-100% of their workforce (except in model (1)), relative to firms that do not use NCCs and use NDAs for 76-100% of their workforce. Similarly, None captures the difference between firms that do not use either NCCs or NDAs and those that only use NDAs. Control variables include a binned control for firm size, a control for worker gender, a second-degree polynomial for worker age and a second-degree polynomial for worker tenure. Regression (2) includes an additional control for each firm's labour productivity measured as turnover per worker. Standard errors are reported in parentheses and are corrected for two-way clustering at the firm and individual level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A.6. Trends in firm characteristics

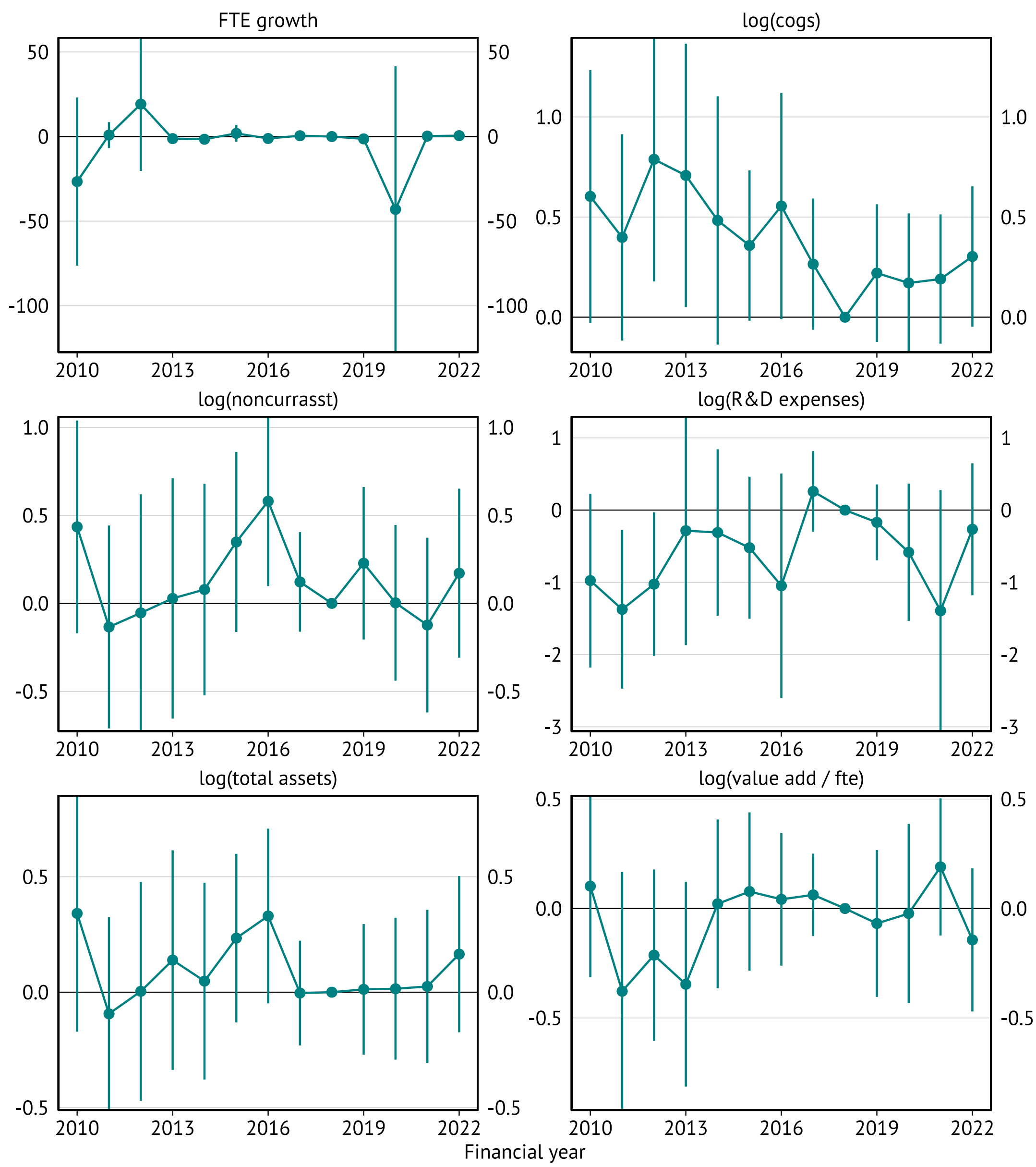
A key assumption in our analysis is that firms that increased their use of NCCs were on largely similar trajectories to firms that did not change their use. We have already shown in Figure 2 that firms who increased their use of NCCs appeared to do so for a reason: they experienced an increased in worker turnover. But did they experience changes in other variables?

If firms that increased their use of NCCs also made other changes at the same time (e.g. expanded their workforce, or made changes in the use of intermediate inputs or other business practices), this would cast doubt on our interpretation of the effects we estimate on job mobility as the impact of increased NCC use.

In Figure A.3 and A.4 we plot the trajectory of various firm characteristics measured in Business Income Tax (BIT) and quarterly Business Activity Statements (BAS) data. We find that in most respects, firms that increased their use of NCCs were tracking similarly to those that did not change their use, including in terms of turnover, labour productivity (value add per FTE), assets, capital expenditure, operating expenditure and research and development spending.

Figure A.3: Trends in firm characteristics (BIT)

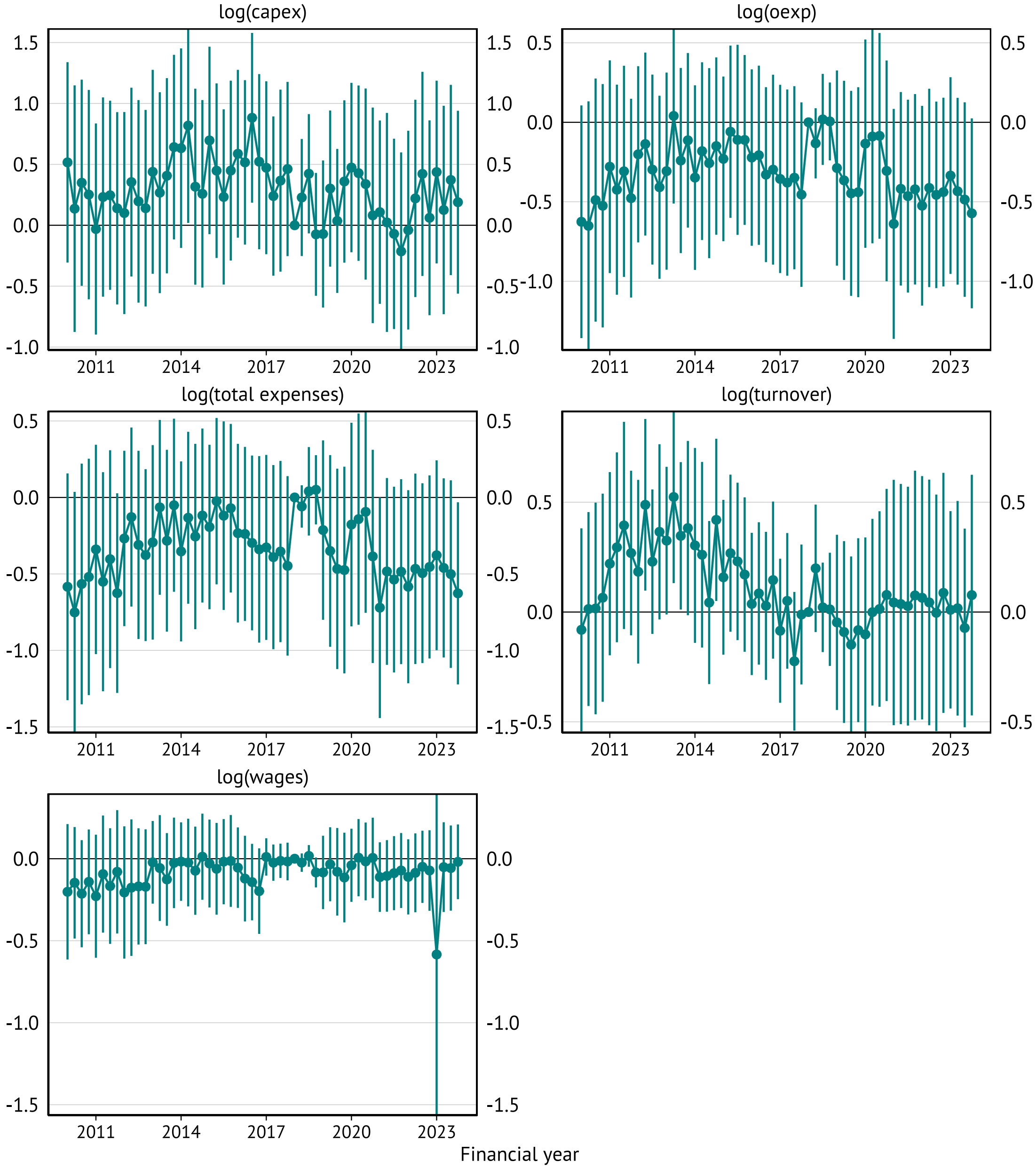
Firms that increased their use of NCC vs those that remained the same



* The figure plots OLS estimates with 90% confidence intervals. Values are multiplied by 100 for readability. Standard errors are corrected for clustering at the firm level. Regressions include industry (ANZSIC division) fixed effects as well as a binned control for firm size and a control for firm age. Cost of goods sold (COGS) is measured as total expenses minus depreciation, interest, wages and other non-sales expenses. Value add is measured as total income minus COGS. Non-current assets is the book value of total assets minus current assets, which we use a proxy for the firm's capital stock.
Sources: ABS; e61

Figure A.4: Trends in firm characteristics (BAS)

Firms that increased their use of NCC vs those that remained the same



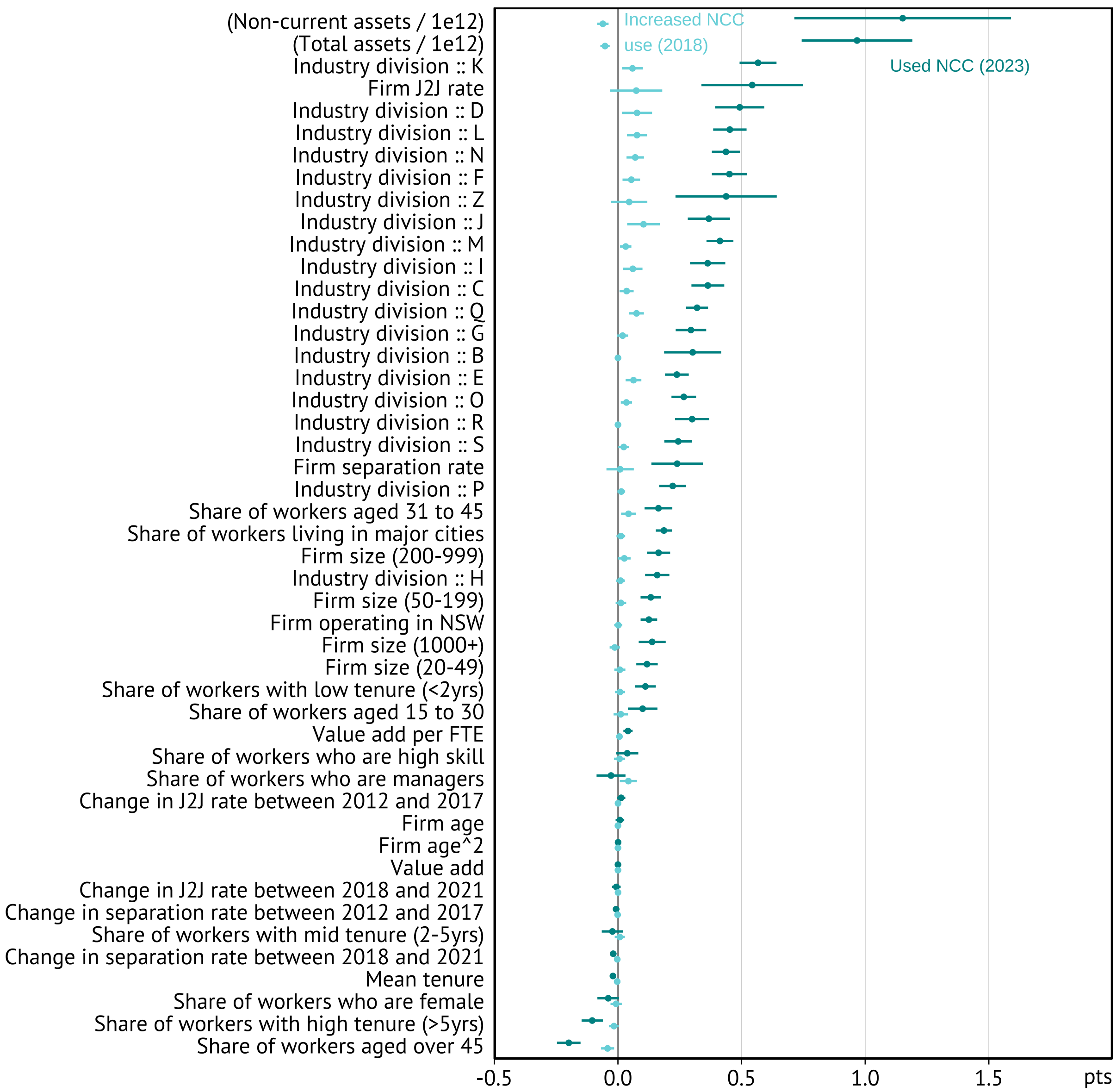
* The figure plots OLS estimates with 90% confidence intervals. Values are multiplied by 100 for readability. Standard errors are corrected for clustering at the firm level. Regressions include industry (ANZSIC division) fixed effects as well as a binned control for firm size and a control for firm age.
Sources: ABS; e61

A.7. Predictors of NCC use and increased use

Figure A.5 plots coefficients from a stepwise regression of each variable (separately) on a binary indicator variable for NCC use in November 2023 (using FY2023 data) and a binary indicator variable for increased NCC use in the five years up to November 2023 (using FY2018 data). It highlights that there are large differences between those firms that use NCCs and those that do not (but do use NDAs), in particular in industry of operation; job separation and J2J rates; worker characteristics including share of low tenure workers, worker age and share of workers living in a major city; and measures of firm size, including headcount and assets. There are also significant differences between firms that increased their use of NCCs and those that did not, although they are much smaller in scale. The results of this analysis help motivate the selection of controls we use throughout our empirical analysis.

Figure A.5: Predicting NCC use

Predictors of NCC use increased use



* Coefficients from regular stepwise regressions of each variable against NCCuse using 2023 (or latest) values and increased NCC use using 2018 values except when looking at future changes in separation and J2J rates.
Sources: ABS; e61

A.8. Wage dynamics and survivorship bias

In Figure 6, we showed that lower skilled workers at NCC and NDA using firms have slower initial wages growth than similar workers at firms that only use NDAs, but then there is a degree of re-convergence at higher tenures.[21]

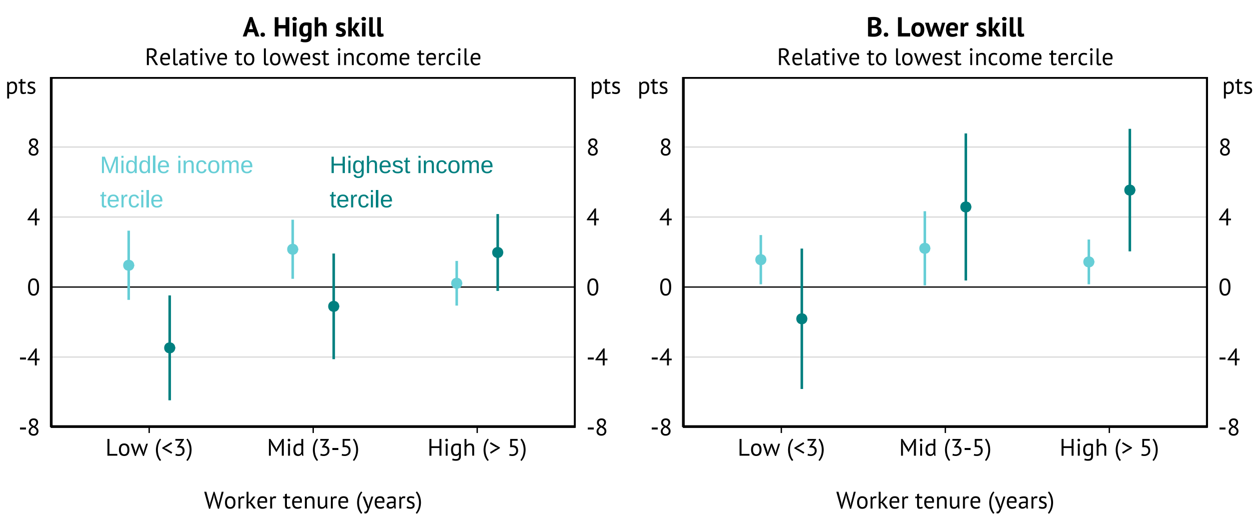
One potential explanation for this pattern is that there is survivorship bias in the sample of workers we use to estimate these wage-tenure profiles. If, at lower tenures, higher income workers are more likely to leave a NCC using firm than an NDA using firms, this could contribute to the divergence in the wage-tenure profiles because relatively fewer "high type" workers remain at NCC using firms at higher levels of tenure.

To investigate this possibility we look at whether for a given level of tenure, high-income workers are more likely to separate from an NCC using firm than an NDA only firm. Figure A.6 shows the results of regressions where the outcome variable is a binary indicator variable for whether the worker left the firm (separated from their job) and where we include as an explanatory variable an interaction between the income tercile of the worker and a categorical variable that classifies firms as NCC using or NDA only.

The coefficients displayed in Figure A.6 show the relative effect of a worker being at a NCC using firm (versus a base case of NDA only) and being in either the middle or highest income tercile on job separation probability (versus a base case of workers in the lowest income tercile). We focus on the results for lower skill workers, since that is where the divergence in wages is largest in Figure 6. These results show that higher income workers are more likely to separate from NCC using firms at mid and high tenures (the coefficient estimates for the effect on job separation probability are above zero). By contrast, there is no clear relationship at low tenure. This suggests that survivorship bias may be contributing to relative weakness in wages at NCC using firms at some tenures, but cannot be the primary cause of the relative shapes of the low-skill wage-tenure profiles in Figure 6. If survivorship bias was a dominant effect then we would expect to see continued divergence in wages for workers at high tenures, when instead we see some modest re-convergence.

Figure A.6: Relationship between worker tenure and wages on job separation probability

Interaction between income tercile and NCC use on probability of leaving a firm



* The figure plots OLS estimates of Equation 1 with 90% confidence intervals. Values are multiplied by 100 for readability. Standard errors are corrected for two-way clustering at the firm and worker level. Regressions include occupation, state, remoteness area and firm fixed effects as well as a binned control for firm size, a second degree polynomial for worker age and tenure, and a control for worker gender. High skill workers are defined as those in occupations with ANZSCO skill level 1, which have skills commensurate with a bachelor degree or higher qualification. All other workers are defined as lower skill.
Sources: ABS; e61

21 Firms that use both restraints for 76-100% of their workforce.

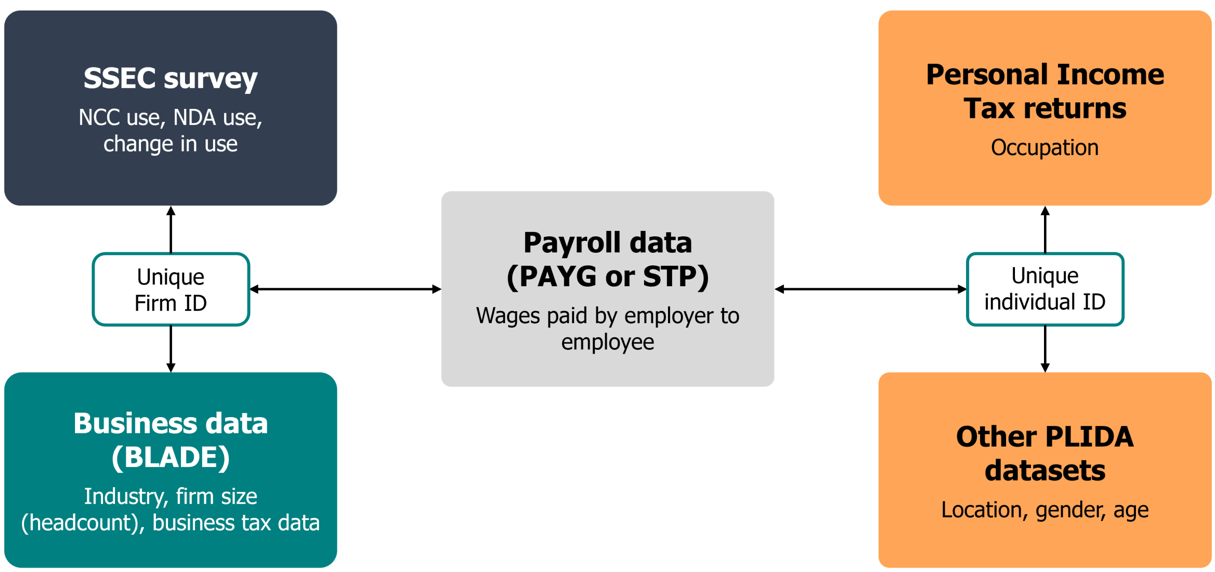
B.1. Data

We construct a Longitudinal Linked Employee-Employer Dataset (L-LEED) of all job-holders using administrative data from Single-Touch Payroll (STP) and pay as you go (PAYG) payment summary data.

For firms, we draw detailed information on their industry of operation, sales and expenses from business income tax files and business register information (BLADE data) (Business Longitudinal Analysis Data Environment (BLADE), 2001 - 2024). For workers, we collect information on their occupation from personal income tax return data and demographic data from the combined PLIDA location and demographic modules (Person Level Integrated Data Asset (PLIDA), 2001 - 2024). We use the STP and PAYG data to determine firm size (headcount) and workers' wages.

The various components of this L-LEED, and how they relate to one another, are summarised in Figure B.1 below. The rest of this section provides further details on the SSEC survey and the construction of the two versions of the L-LEED we use in our analysis.

Figure B.1: Longitudinal linked employee-employer dataset (L-LEED)



ABS Short Survey of Employment Conditions (SSEC)

In November 2023, the ABS surveyed approximately 7,000 Australian businesses on their use of post-employment restraint clauses. The survey was a short follow-up survey to the Employee Earnings and Hours (EEH) May 2023 survey. Although the SSEC was a non-compulsory survey, the ABS still managed to achieve a response rate of around 70% (Australian Bureau of Statistics, 2023).

The survey asked a range of questions related to post-employment restraint use by firms. This included the extent to which employers currently use the following post-employment restraint clauses:

- Non-competes
- Non-disclosure agreements
- Non-solicitation: coworkers
- Non solicitation: clients

The survey also asked employers to qualitatively described how their use of these restraints had changed over the last 5-years (increased, decreased, or remained the same), and how likely it was that they would include any of these conditions in future contracts. For further information on the SSEC, please see the ABS's online EEH methodology summary.

STP L-LEED

The main dataset used in our analysis is a L-LEED constructed from Single-Touch Payroll (STP) data. This STP data captures the weekly wages, superannuation and allowances paid to employees of employers registered with the STP system (see discussion of coverage below for more details). In the period of our analysis STP captures close to the universe of Australian employees.

To produce a dataset that is easier to work with, we aggregate the weekly STP data up to the calendar month level. We then use this monthly data to analyse labour market movements. In our regression analysis we then aggregate this monthly data up further to either the financial year or calendar year level.

We add to the STP data information on worker and firm characteristics. This includes information on a firm's industry, head-count (derived from STP) and workers' demographics, such as their gender, age, occupation, and their state and remoteness area of residence. In most instances this firm and worker demographic data does not cover the entire period covered by the STP data. In these cases we take the last reported observation for that worker or firm, which is generally from the 2021/22 financial year. We also derive several variables, including worker tenure and various measures of job mobility, which are discussed further in the next section.

From this combined data set we focus on the subset of workers employed at firms that participated in the SSEC and reported using either non-compete clauses or non-disclosure agreements. We argue that this comparison helps to control for *unobservable* differences between workers and firms because all of these firms have revealed a tendency to have something to protect (e.g. trade secrets) and thus ought to be different to firms that do not use NDAs or NCCs (Balasubramanian et al., 2024). The justification for this sample selection is discussed further in the main body of the text.

Finally, we drop from our wage analysis a small number of workers whose STP records are inconsistent with their PAYG records (for these workers we cannot accurately estimate their tenure) and workers who earn below the full-time minimum wage.

STP coverage

In our analysis we use STP data that covers the period from the beginning of January 2020 to the end of July 2024. During this time there were small changes to the group of employers and employees who were required to report data through the STP system. Prior to 1 July 2021 there were two main exemptions from STP reporting requirements. First, small employers (19 or fewer employees) were not required to report income for closely held payees through STP.[22] Second, employers in certain industries such as building, construction or cleaning, who make regular contributions to long service leave and redundancy schemes, were not required to report such payments (Australian Taxation Office, 2023).

In our analysis of the effect of NCCs on worker wages we focus on the period from 1 July 2021 when most STP reporting exemptions were closed. In our main analysis of job mobility we use a longer time frame that includes data prior to 1 July 2021. However, this analysis only includes firms that reported data through the STP system before and after the exemptions were lifted. We also show that our main results are qualitatively similar when using a shorter time period that excludes data from before 1 July 2021

Combined STP and PAYG L-LEED

To analyse job mobility trends over a longer time period we produce a second L-LEED that combines STP and PAYG payment summary records. This L-LEED is constructed at the annual level – the frequency of the PAYG data – by aggregating the weekly STP data to the financial year level. We then add the same information on workers and firms to this L-LEED as we did the STP L-LEED. We also filter the data in the same way, focusing on workers at firms who use either NCCs or NDAs and dropping workers who earn below the full-time minimum wage.

With this combined L-LEED, we use PAYG data to calculate job mobility trends over the period from the beginning of the 2010 financial year to the end of the 2021 financial year (the second last financial year for which we have PAYG records). We then

22 A closely held payee is an individual who is directly related to the entity from which they receive payments. These could include family members of a family business, directors or shareholders of a company, or beneficiaries of a trust (Australian Taxation Office, 2023).

use STP data to calculate job mobility trends for the 2022 and 2023 financial years. Crucially, when estimating whether a worker has left a firm, through either a job separation or job-to-job (J2J) transition, we do this within a given dataset (PAYG or STP), rather than relying on a combined version. For instance, to determine job separations and J2Js in the 2021 financial year, we use PAYG data from 2021 and 2022, rather than a combination of PAYG and STP data.

B.2. Measuring job mobility, tenure and wages

Job separations

We define a job separation as having occurred when a worker leaves a firm for any reason. In the STP L-LEED, we define this as when a worker ceases to be paid by a firm and is not paid again by that same firm for at least the next 12 months. In the combined PAYG and STP L-LEED, we define this as when a worker is not paid by the same firm in the following financial year.

Job-to-job transitions

Our definition of job-to-job transitions (J2Js) largely follows that of Wong (2024). The exact definition for J2Js differs slightly in the STP L-LEED and the combined PAYG and STP L-LEED due to differences in the granularity of the data.

J2Js in the STP L-LEED

In the STP L-LEED, we define a J2J as having occurred when a worker changes jobs without going through a period of more than 6-months of non-employment. This is defined using monthly STP data to identify when a person reports leaving a job (stops being paid by a firm) and starts being paid by a new firm. We choose to use monthly data for two reasons. First, the size of the STP data makes working with the weekly level data unwieldy. Second, there are inconsistencies in the STP data that suggest that it does not always accurately capture the last week of a worker's employment at a firm.[23] By using monthly data, we reduce the sensitivity of our J2J estimates to these inconsistencies.

The following scenarios are defined as J2Js in the STP L-LEED:

- $A \rightarrow B$: a person leaves job A and starts job B within 6 months. This includes when the person leaves job A and starts job B in the same month.
- $AB \rightarrow C$: a person leaves jobs A and B, and starts job C within 6 months.
- $A \rightarrow BC$: a person leaves jobs A and starts jobs B and C within 6 months.

The following scenarios are defined as job separations but not J2Js:

- $A \rightarrow \_$: a person leaves job A, but does not start a new job.
- $A \rightarrow B$: a person leaves job A and starts job B, but spends more than 6 months out of employment between jobs.
- $AB \rightarrow B$: a person leaves job A, but remains employed at job B.

To avoid short-term contract or casual work overstating the frequency of J2Js, we define a J2J as occurring only if the previous job was held for at least the past 4 weeks.

In our analysis we also examine J2Js that occur within an industry (ANZSIC) division or sub division. These are defined in the same way as above, but with the added condition that the origin and destination firms must be from the same industry.

J2Js in the combined PAYG and STP L-LEED

In the combined PAYG and STP L-LEED, we define a J2J as having occurred when an individual leaves one job and starts another within the same financial year, and does not hold a third job over the entire period. This measure differs from the measure in the STP L-LEED because we can not know for sure that the individual did not start the new job before leaving their old job, or set a specific time limit on the gap between the old job ending and the new job starting. We are also not able to capture J2Js when the new job is not carried forward into the next financial year, or where the old job was not carried forward from the last financial year.

23 The STP data we work with includes both raw information on the wages paid by a firm to a worker and an ABS generated measure of weekly compensation. At the end of employment spells these two measures often do not align, with the ABS generate measure often adding a week or two of additional payments.

The following scenarios are defined as J2Js in the combined PAYG and STP L-LEED:

- $A \rightarrow AB \rightarrow B$: a person was employed in job A last financial year, they then started job B and left job A this financial year, before carrying job B into the next financial year.
- $AC \rightarrow ABC \rightarrow B$: a person was employed in jobs A and C last financial year, they then started job B and left jobs A and C this financial year, before carrying job B into the next financial year.
- $A \rightarrow ABC \rightarrow B$: a person was employed in job A last financial year, they then started jobs B and C and left jobs A and C this financial year, before carrying job B into the next financial year.

The following scenarios are defined as job separations but not J2Js:

- $AC \rightarrow ABC \rightarrow BC$: a person was employed in jobs A and C last financial year, they then started job B and left job A this financial year, before carrying jobs B and C into the next financial year.
- $\_ \rightarrow AB \rightarrow B$: a person did not have a job last year, they then started jobs A and B this financial year, before carrying job B into the next financial year.
- $A \rightarrow AB \rightarrow \_$: a person was employed in job A last financial year, they then started job B and left jobs A and B this financial year, and did not carry any jobs into the next financial year.

Tenure

We estimate each worker's tenure at a given firm using a combination of STP and PAYG data. Tenure is defined as the number of consecutive financial years a worker has received a payment from a firm. In cases where there is an inconsistency between between the PAYG and STP data we treat the STP data as the source of truth For example, if a worker had a tenure of 10 years based on the PAYG data, but in STP their recorded tenure was only 1 year, we would record their tenure as 1 year. The exception to this is for the small number of workers who appear in the STP data in every year from 2020, but who are either not present in the PAYG data or have a lower tenure in the PAYG data than in the STP data. For these workers we are unable to accurately determine their true tenure and we drop them from our wages analysis where we use tenure as a key control.

Wages

STP records the total wages paid by a firm to a worker on a weekly basis, but not the total number of hours worked. As a result, we cannot calculate hourly wages and all the analysis in this note uses total weekly wage earnings as our measures of earnings. This means that some of the effects that we find may be driven by differences in hours worked, rather than wages paid per hour.

Throughout our analysis we convert all wage measures to real terms using quarterly Consumer Price Index data.

For our analysis of wage differences by worker tenure and between NCC and NDA using firms, we aggregate the weekly STP wage data up to the financial year level by taking the mean of each worker's weekly wages within the year.

For the job-switching analysis, we compare each worker's wages in their old and new job. We again smooth the wage analysis by taking the worker's mean wage over the last 12-months of their old job and the mean of their weekly wages over the first 12-months of their new job. Because workers often receive large sums in the final weeks of their employment (e.g. cashed out annual leave, or redundancy payments), we exclude the last calendar month of wages before the job switch, when calculating the mean wage of a worker at their old job.

B.3. Empirical approach

In this analysis we are interested in understanding the relationship between exposure to NCCs and the ability for workers to change jobs and bargain for higher wages. Our analysis can be divided into two sections: analysis of worker mobility (job separations, job-to-job transitions, time out of the employment between jobs), and workers' wages (cross sectional wage differences, returns to tenure, gains from job switching). Importantly, while in our main results we use different approaches for analysing mobility and wages, we do also include results where we apply the same methodology from the mobility analysis to the measure of wages. However, what we find is

Mobility analysis

We conduct our analysis of job mobility at the worker-firm-year level, estimating the effect of worker *i* being employed at a firm *j* that reported that they increased their use of NCCs over the 5-years to November 2023, relative to workers at firms that reported they did not change their use of NCCs over the same time period.We define a variable T_j as a dummy variable that is equal to one if firm *j* reported that they increased their use of NCCs over the period, and zero if the firm reported that they did not change their use. Our estimation equation is:

$$y_{ijt} = \delta_1 Year_t + \delta_2 T_j + \delta_3 (T_j \times Year_t) + X_{ijt} + \alpha_j + \gamma_{it} + \epsilon_{ijt} \tag{1}$$

The dependent variable y_{ijt} takes the value of our various worker-level mobility measures, including a dummy indicator variable for whether the worker separated from their job during the year, made a J2J, made a J2J to another firm within the same industry sub division, and a binary indicator for whether when a worker separated from their job they spent more than 6-months out of employment.

Our main coefficient of interest is δ_3, which captures the estimated relationship between being at a firm that increased their use of NCCs and job mobility overtime. For our longer term mobility analysis this interaction captures differential trends in job mobility between workers at the two groups of firms – those that changed their use relative to those that did not change their use.

For our headline findings, we run a version of this regression where we just examine the change in job mobility between the 2021 and 2023 calendar years (the apparent peak and trough of job mobility for those firms that increased their use of NCCs). In these regressions *Year* effectively becomes a dummy variable for 2023 and the interaction captures how job mobility changed between 2021 and 2023 for workers at firms that increased their use relative to workers at firms who did not change their use.

The matrix X_{ijt} contains a binned control for firm size (0-19, 20-49, 50-199, 200-999 and 1000+), as well as a control for a worker's gender, a second degree polynomial for their age and a dummy for whether the worker had just joined the firm (< 1 year of tenure). We include this control for short tenure workers to minimise the effect of workforce compositional changes affecting our results given that firms that increased their use of NCCs had just experienced a relative uptick in worker turnover.

Finally, we include a vector of firm fixed effects, α_j, to account for any unobserved time-invariant factor that may influence a firm's decision to increase their use of NCCs, while also affecting our various outcome measures. All specifications also include a vector of occupation, state and remoteness area fixed effects (γ_{it}). Standard errors are corrected for two-way clustering at the worker-firm level.

Wage analysis

Our wage analysis can be broken up into three parts:

- **Cross-sectional wage differences.** A comparison of the wages of workers at firms that use both NCCs and NDAs, with the wages of workers at firms that just use NDAs.
- **Wage dynamics.** An estimate of the returns to worker tenure for workers at firms that use both NCCs and NDAs, compared to workers at firms that just use NDAs.
- **Gains from job-switching.** An analysis of the gains for workers who, when they switch jobs, move from having an NDA to an NCC and NDA, relative to workers who move from having an NDA to an NDA.

Cross-sectional wage differences

We conduct our cross-sectional comparison of wages at the worker-firm-year level, estimating the effect of worker *i* being employed at a firm *j* that reported they applied NCCs and NDAs to between 76 and 100% of their workforce, relative to workers at firms that do not use NCCs and apply NDAs to between 76 and 100% of their workforce. The aim behind this analysis is to get as close as possible to comparing the wages of a worker with an NCC and NDA to a worker with just an NDA. We define a variable NCC_j as a dummy variable that is equal to one if firm *j* reported that they used NCCs and NDAs for 76-100% of their workforce (and that this use had remained stable over the last 5 years), and zero if the firm reported that they do not use NCCs, but did use NDAs for 76-100% of their workforce (and that this use had remained stable). Our estimation equation is:

$$(2) \qquad log(wages)_{ijt} = \beta_1 NCC_j + X_{ijt} + \theta_j + \nu_t + \gamma_{it} + \epsilon_{ijt}$$

The dependent variable $log(wages)_{ijt}$ is the log of the mean weekly wages of worker i at firm j in financial year t. Our main coefficient of interest is β_1, which captures the estimated difference in $log(wages)$ between workers at firms that use both NCCs and NDAs and workers at firms that only use NDAs.

The matrix X_{ijt} includes a binned control for firm size, as well as a control for worker gender and a second degree polynomial for worker age. We also include a vector of industry fixed effects θ_j, a vector of year fixed effects ν_t and a vector γ_{it} of occupation, state and remoteness area fixed effects based on a worker's place of residence. Standard errors are corrected for two-way clustering at the worker-firm level.

Wage dynamics

Our analysis of wage dynamics follows a similar approach to the cross-sectional analysis described above. The main difference is that we now add an interaction between worker tenure and the firm's use of NCCs (NCC_j). Following Shi (2023), our estimation equation is:

$$(3) \qquad log(wages)_{ijt} = \beta_1 NCC_j + \sum_{k=1}^{3} \delta_k Tenure_{ijt}^{k} + \sum_{k=1}^{3} \lambda_k (NCC_j \times Tenure_{ijt}^{k}) + X_{ijt} + \theta_j + \nu_t + \gamma_{it} + \epsilon_{ijt}$$

Our main coefficients of interest are β_1, δ_k and β_4, these collectively describe the estimated relationship between mean weekly wages, firms' use of NCCs and worker tenure. The other variables, X_{ijt}, θ_{jt}, ν_t and γ_{it} are the same as those described above in the cross-sectional analysis of wages. Standard errors are corrected for two-way clustering at the worker-firm level.

Gains from job-switching

We conduct our analysis of the wage gains from job switching at the worker-J2J level, estimating the effect of worker i moving from a firm j that uses NDAs for 76-100% of their workforce, to a firm k that uses NDAs and NCCs for 76-100% of their workforce, relative to moving to a firm that does not use NCCs, but does use NDAs for 76-100% of their workforce. We define a variable NCC_k as a dummy variable that is equal to one if firm k (the destination firm) reported that they NCCs and NDAs for 76-100% of their workforce (and that this use had remained stable), and zero if the firm reported that they do not use NCCs, but do use NDAs for 76-100% of their workforce (and that this use had remained stable). Our estimation equation is:

$$(4) \qquad log(wages)_{ik} - log(wages)_{ij} = \beta_1 NCC_k + X_{ijt} + \theta_j + \nu_t + \gamma_{ijt} + \epsilon_{ijt}$$

The dependant variable is the log difference in mean weekly wages between the worker's old job at firm j and their new job at firm k. The mean weekly wages of the worker at their old firm, $log(wages)_{ij}$, is calculated over the 12 months preceding the month in which the worker left their old job. We drop this last month of employment to avoid including lump sum leave or redundancy payouts in the calculation of mean wages. The mean weekly wages of the worker at their new firm, $log(wages)_{ik}$, is calculated over their first 12 months of employment. Although the first month at a new job may also include abnormal one-off payments, such as a signing bonus, we do not drop it from the analysis as it could be part of the mechanism through which workers who sign NCCs obtain higher wages.

The matrix X_{ijt} includes a binned control for firm size, as well as controls for worker gender, and second degree polynomials for worker age and their tenure at their old job. We also include a vector of industry fixed effects for the worker's old job θ_j, a vector of year fixed effects ν_t related to the year in which the J2J occurred, as well as a vector of occupation, state and remoteness area fixed effects as measured at the end of the worker's tenure at their old job (γ_{ijt}). Standard errors are corrected for two-way clustering at the individual and (origin) firm level.

Disclaimers

Business Longitudinal Analysis Data Environment (BLADE)

This paper uses unit record data held in the BLADE data environment which is hosted by the Australian Bureau of Statistics. The results are based, in part, on Australian Business Register (ABR) data supplied by the Registrar to the Australian Bureau of Statistics (ABS) under A New Tax System (Australian Business Number) Act 1999 and tax data supplied by the Australian Taxation Office (ATO) to the ABS under the Taxation Administration Act 1953. These require that such data are only used for the purpose of carrying out functions of the ABS. No individual information collected under the Census and Statistics Act 1905 is provided back to the Registrar or ATO for administrative or regulatory purposes. Any discussion of data limitations or weaknesses is in the context of using the data for statistical purposes, and is not related to the ability of the data to support the ABR or ATO's core operational requirements. Legislative requirements to ensure privacy and secrecy of this data have been followed. Only people authorised under the Australian Bureau of Statistics Act 1975 have been allowed to view data about any particular firm in conducting these analyses. In accordance with the Census and Statistics Act 1905, results have been confidentialised to ensure that they are not likely to enable identification of a particular person or organisation.

Person Level Integrated Data Asset (PLIDA)

The results of these studies are based, in part, on data supplied to the ABS under the Taxation Administration Act 1953, A New Tax System (Australian Business Number) Act 1999, Australian Border Force Act 2015, Social Security (Administration) Act 1999, A New Tax System (Family Assistance) (Administration) Act 1999, Paid Parental Leave Act 2010 and/or the Student Assistance Act 1973. Such data may only used for the purpose of administering the Census and Statistics Act 1905 or performance of functions of the ABS as set out in section 6 of the Australian Bureau of Statistics Act 1975. No individual information collected under the Census and Statistics Act 1905 is provided back to custodians for administrative or regulatory purposes. Any discussion of data limitations or weaknesses is in the context of using the data for statistical purposes and is not related to the ability of the data to support the Australian Taxation Office, Australian Business Register, Department of Social Services and/or Department of Home Affairs' core operational requirements.

Legislative requirements to ensure privacy and secrecy of these data have been followed. For access to PLIDA and/or BLADE data under Section 16A of the ABS Act 1975 or enabled by section 15 of the Census and Statistics (Information Release and Access) Determination 2018, source data are de-identified and so data about specific individuals has not been viewed in conducting this analysis. In accordance with the Census and Statistics Act 1905, results have been treated where necessary to ensure that they are not likely to enable identification of a particular person or organisation.