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The Real Effects of Debt Covenants: Evidence from Australia

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Abstract

I study how the use and structure of debt covenants affect real business activity and pass-through of monetary policy using a newly constructed dataset of corporate debt covenants in Australia. I find that exposure to debt covenants disciplines firms' investment and staff expenses even in the absence of covenant breaches. In addition, covenants with interest coverage limits appear to amplify the transmission of monetary policy shocks while other types of covenants appear to mitigate transmission. As such, the shift from interest coverage limits to other types of covenants that appears to have occurred since the late 2000s may have lowered the responsiveness of investment to monetary policy, and in turn accounted for some of the surprising weakness in non-mining investment over the past decade.

JEL Classification Numbers: G1, E2, E5

Keywords: debt covenants, financing friction, investment, employment, monetary policy

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1. Introduction

Financial frictions can be important determinants of macroeconomic outcomes. They can lead to sub-optimal credit allocation, create a smaller and less efficient economy (Berk, Stanton and Zechner 2010; Agrawal and Matsa 2013) and amplify macroeconomic shocks (Bernanke, Gertler and Gilchrist 1999; Jeenas 2019). A key cause of financial frictions is informational asymmetries and misalignment in incentives between lenders and borrowers (Stein 2003). This paper studies the macroeconomic consequences of debt covenants, a contractual device designed to address these information frictions in debt financing, using Australia as a case study.

There are a number of reasons to focus on the role of debt covenants. First, while they facilitate access to credit in the presence of information frictions, they also restrict businesses' behaviour by setting conditions they need to satisfy (Jensen and Meckling 1976; Aghion and Bolton 1992; Tirole 2006). These conditions typically involve maintaining financial indicators within certain bounds, and therefore place some limits on the businesses' activities. This means that covenants can directly affect businesses' behaviour even in the absence of default.

Second, debt covenants are widely used in financial contracts (Bradley and Roberts 2015), and violations of their terms occur more frequently than defaults or bankruptcies (Dichev and Skinner 2002; Nini, Smith and Sufi 2012). As such, they can have first-order effects on economic activity. This was brought into sharp focus after the global financial crisis (GFC) and again following the COVID-19 pandemic.

Third, despite their importance, little is known about corporate debt covenants outside of the United States (Nini *et al* 2012; Lian and Ma 2021) and the United Kingdom (Moir and Sudarsanam 2007; Chatterjee *et al* 2021), reflecting a lack of information about corporate debt covenants. My paper is the first to examine the macroeconomic implications of corporate debt covenants in Australia, employing the newly constructed dataset for Australian non-financial listed firms outlined in Nguyen (2021).

In this paper I first show how exposure to covenants in debt contracts can affect investment and employment expenses as firms try to avoid breaching the covenants ('disciplining channel'). This influence occurs over and above any direct effect of actual breaches ('punishing channel'), suggesting that covenants can have far reaching implications for firm behaviour even if breaches remain rare. These findings provide further evidence for the disciplining channel of covenants outside of the US context, which is explored in Nini, Smith and Sufi (2009) and Adler (2021). My paper also supplements the empirical literature that focuses on the effects that a breach can have on business activity (Chava and Roberts 2008; Nini *et al* 2009; Roberts and Sufi 2009; Falato and Liang 2016).

Having demonstrated the substantive direct effects of covenants, I study their role in the transmission of monetary policy, building on the approach of Greenwald (2019) and using monetary policy shocks developed in Beckers (2020) to address endogeneity concerns. I find that the transmission of monetary policy to real business activity is amplified where the firm is subject to an interest coverage requirement, as higher interest rates have a direct effect in making the requirement more binding (and vice versa). This is akin to the financial accelerator mechanism outlined in Kiyotaki and Moore (1997) and Bernanke *et al* (1999). In contrast, transmission is dampened when the firm is subject to covenants that limit the stock of debt to some multiple of

assets or earnings, as interest rates do not directly influence whether the requirements bind or not. By focusing on the debt covenants, my paper also contributes to the literature on the role of financial frictions in the transmission of macroeconomic shocks, and more generally the macroeconomic role of corporate financing (Christiano, Motto and Rostagno 2014; Jeenas 2019; Ottonello and Winberry 2020; Singh, Suda and Zervou 2022).

Finally, I quantify the aggregate effects of changes in the types of covenants used on the transmission of monetary policy. Using a simple counterfactual, I show that the shift in the composition of covenants away from interest coverage requirements over time has lowered the aggregate responsiveness of non-mining investment by around 15 to 25 per cent. While the calculation is not precise, it demonstrates the potential importance of covenants in explaining part of the weakness in non-mining business investment over the 2010s despite the availability of finance and relatively low borrowing costs in Australia (Debelle 2017; Lowe 2018; Dynan 2021). That said, more needs to be done to understand the reasons behind the apparent shift in the composition of covenants, that is, whether it reflects an actual shift or changes in reporting.

2. Corporate Debt Covenants in Australia

For this paper I use the database of corporate debt covenants in Australia that I previously constructed in Nguyen (2021). To create this database I employed text analytic techniques to extract information about the prevalence, types and violations of debt covenants by non-financial listed Australian companies from their publicly available annual reports, collected from the Connect4 website. For details on the construction of the data, see Appendix A. Figure 1 shows an example of debt covenants being mentioned in a firm's annual report and reveals the three financial covenants the firm was subject to: equity ratio, leverage ratio and interest cover (or coverage) ratio. Figure 2 shows an example of a firm that breached its banking covenants for the financial year.

Figure 1: An Example of Covenants Being Mentioned in a Firm's Annual Report

'In addition to the eligible collateral, the Group has several general and financial undertakings which it must comply with including an Equity Ratio covenant, a Leverage Ratio covenant and an Interest Cover Ratio covenant.'

Source: Connect4

Figure 2: Mentions of Breaches in Covenants in a Firm's Annual Report

'For the year ended 30 June 2017, the Consolidated Entity made a loss of $7,337,000 (2016: profit of $5,317,000) and was in breach of its banking covenants as disclosed in Note 20.'

Source: Connect4

One drawback of this method is that it relies on the firm self-reporting, which may lead to selection bias. Australian companies are not required to report the existence of covenants and, indeed, they may have incentives *not* to report them. For instance, financially vulnerable firms may want to avoid any signal of their poor financial health. Alternatively, financially strong firms may have an incentive to mention covenants and draw attention to their compliance. This means that large and financially stable firms might have self-selected into the sample by reporting on their debt covenants, and in turn, might have become over-represented in the sample. On the other hand, the Australian

Accounting Standard on 'Financial Instruments: Disclosures' requires disclosures of non-remedied covenant breaches, as they have material effects on the classification of debt in the financial statements (AASB 2022). This means that firms need to disclose instances of covenants breaches in their public reports. The self-reporting bias hasn't necessarily been constant over time. ASIC's financial reporting guideline has increasingly emphasised the importance of disclosing information that 'investors would want to know' in their financial reports (ASIC 2020). This increase in financial reporting standards might contribute to an increase in the reported exposure to debt covenants over time.

In addition to the prevalence of debt covenants, the annual reports also allow for extraction of the types of debt covenants a firm was subject to in the reporting period. I categorise the types of debt covenants into:

1. **Interest coverage covenants (ICC):** set a minimum on the interest coverage ratio (ICR), which is the ratio of firm earnings (usually EBITDA (earnings before interest, taxes, depreciation and amortisation)) to interest payments on total debt (not just on the debt from the loan facility that imposed the covenants). Alternative names that often appear in the reports include fixed charge coverage and debt service coverage with slightly different calculations.

2. **Other earnings-based covenants (OEC):** limit the stock of debt to be no more than some multiple of earnings. Examples include debt-to-earnings, debt-to-EBITDA and debt-to-cash flow ratios.

3. **Asset-based covenants (ABC):** restrict the firm's maximum amount of debt or minimum amount of equity by requiring that the firm maintains leverage below or net worth above certain thresholds. They are usually called gearing (debt-to-equity) or leverage (debt-to-assets) ratios.

To assess how the prevalence of different types of debt covenants varies across different types of firms, I match the data on covenants with balance sheet information from the Morningstar database of Australian listed companies. The matched sample contains roughly 17,000 observations from 3,742 unique non-financial listed firms between 2002 and 2020. Table 1 presents summary statistics of firms without covenants (but having debt), firms subject to each type of covenant and firms that have breached covenants, by their financial characteristics. On average, roughly 25 per cent of the sample (more than 4,600 firm–year observations) reported debt covenants, of which around 13 per cent also reported having breached them. Roughly 30 per cent of firms reporting debt covenants specify which types of covenants they are subject to. ICC appears to be the most common type of covenant (60 per cent of reporting firms), followed by ABC (50 per cent) and OEC (30 per cent) (Table 1). Note that the sample contains only non-financial listed firms, which are inherently larger and of different financial characteristics compared to the broader population of firms. To the extent that larger, more complex firms are more likely to have access to loans entailing debt covenants instead of collaterals, the share of debt covenants is potentially more prevalent in this sample.

Table 1: Financial Statistics by Usage and Type of Debt Covenants

Non-financial listed firms, median

	None[a]	Covenants[b]	Breaches	ICC	OEC	ABC
Revenue ($ million)	14.0	104.5	108.9	93.5	91.7	119.8
Debt ($ million)	6.0	61.7	63.3	68.5	44.6	79.3
Cash ($ million)	5.0	15.2	13.7	13.7	10.5	17.9
Assets ($ million)	53.0	290.4	315.7	362.0	236.8	412.5
Investment ($ million)	6.5	44.5	39.8	36.8	32.6	55.9
Staff expenses ($ million)	2.5	18.4	21.5	18.8	18.8	25.1
Return on equity ratio	0.1	0.2	0.2	0.2	0.2	0.2
Debt-to-equity ratio	0.2	0.5	0.5	0.5	0.5	0.4
Debt-to-EBITDA ratio	0.1	3.0	3.1	3.4	3.4	3.2
Interest coverage ratio	2.5	6.1	5.7	5.7	6.6	5.7
Share of breaches (%)	na	13	na	11	8	8
Observations	12,567	4,613	592	726	485	686

Notes: (a) Firms without covenants but having debt in the year.
(b) Including firms not specifying the types of covenants and not equal to the total of ICC, OEC and ABC since they are not mutually exclusive.

Sources: Author's calculations; Connect4; Morningstar

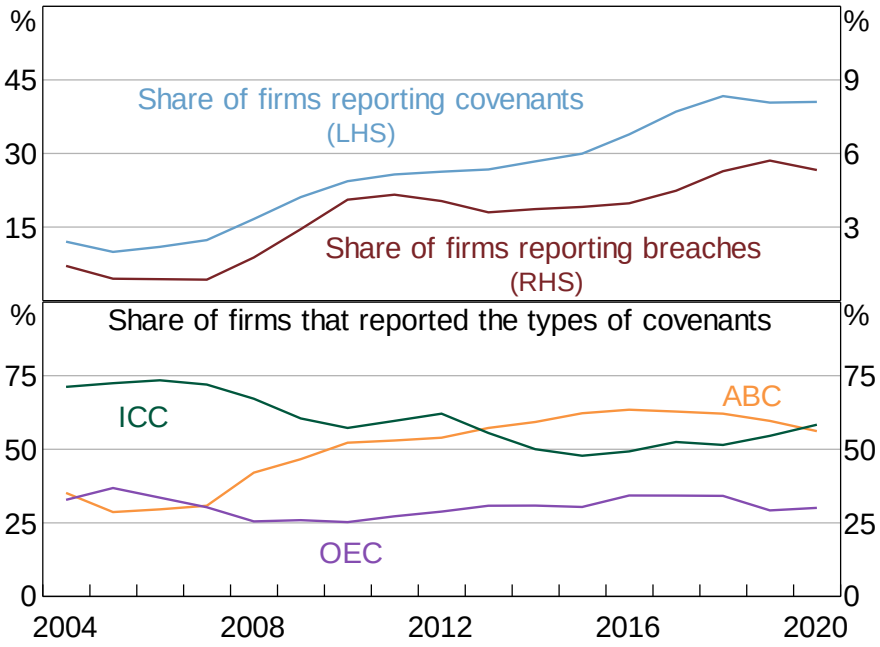
Table 1 also presents the median value for a number of financial measures across different covenant configurations. First, similar to the United States (Greenwald 2019; Lian and Ma 2021), firms with covenants in Australia tend to be much larger, in both revenue and assets, than firms without covenants. They are also more leveraged (higher debt-to-equity ratio), but more profitable (higher return-on-equity ratio) and have higher interest coverage ratios. This is unsurprising since larger firms tend to borrow more and have more consistent earnings to cover the cost of debt financing. Interestingly, on average, there is not a substantial difference in financial characteristics for firms that have experienced a breach in covenants, suggesting that violation instances are not unique to firms with specific financial circumstances.

Turning to different types of covenants, Australian firms reporting ICC and OEC (both are earnings-based) tend to be slightly smaller (in both revenue and balance sheet) than those reporting ABC. Interestingly, firms reporting ABC tend to be less leveraged, indicating that they have generally stronger balance sheets or that the ABC have restricted their opportunity to leverage their assets. On the other hand, firms reporting OEC appear most profitable with the highest median return-on-equity and interest coverage ratios. The breaching rate is only slightly different across covenant types, limiting concerns about structural shifts in reporting of covenant types due to breaches.

Figure 3 shows that the share of firms reporting debt covenants has steadily increased over time, from around 10 per cent in the early 2000s to nearly 40 per cent in the late 2010s. As discussed earlier, this could be due to the trend towards greater transparency in corporate reporting or an increase in the usage of covenants in debt contracts (or both). The structure of debt covenants among reporting firms also appears to have gone through a significant change around the GFC. In the early 2000s, more than 70 per cent of reporting firms were subject to ICC but the prevalence of ICC has steadily declined in the past decade. On the other hand, ABC has become more commonly used over time while the prevalence of OEC has stayed constant. In comparison, corporate debt covenants in the United States, the United Kingdom, Japan and Finland are predominantly earnings based (Niskanen and Niskanen 2004; Moir and Sudarsanam 2007; Kochiyama and Nakamura 2014; Lian and Ma 2021), and the share of earnings-based covenants has remained constant over the years (Greenwald 2019).

Figure 3: Trends in Corporate Debt Covenants

Non-financial listed firms, three-year moving average



Sources: Author's calculations; Connect4; Morningstar

Interestingly, the characteristics of firms subject to specific covenants are stable over time (Table B2). If the increase in reporting of ABC were due to greater reporting requirements, we might expect some change in the nature of the firms, for example, an increase of 'safer' firms that formerly had no need to disclose reporting. However, since no such shift is evident, it appears that the increase is not just driven by this trend in reporting.

Another possible explanation is that the shift in covenant composition away from ICC towards ABC could reflect considerations during the debt contracting process. Asset-based covenants have been shown to reduce the need to renegotiate loan terms as they better align the incentives of shareholders with those of lenders by putting restrictions on how much wealth is kept within the firm (Christensen and Nikolaev 2012). The shift could also reflect the increased use of unsecured loans relative to secured loans, with lenders putting restrictions on the total pool of assets subject to claims with asset-based covenants, rather than securing against specific collateral. In fact, the

unsecured share of loans in the sample has increased from around 8 per cent before the GFC to 12 per cent after the GFC.

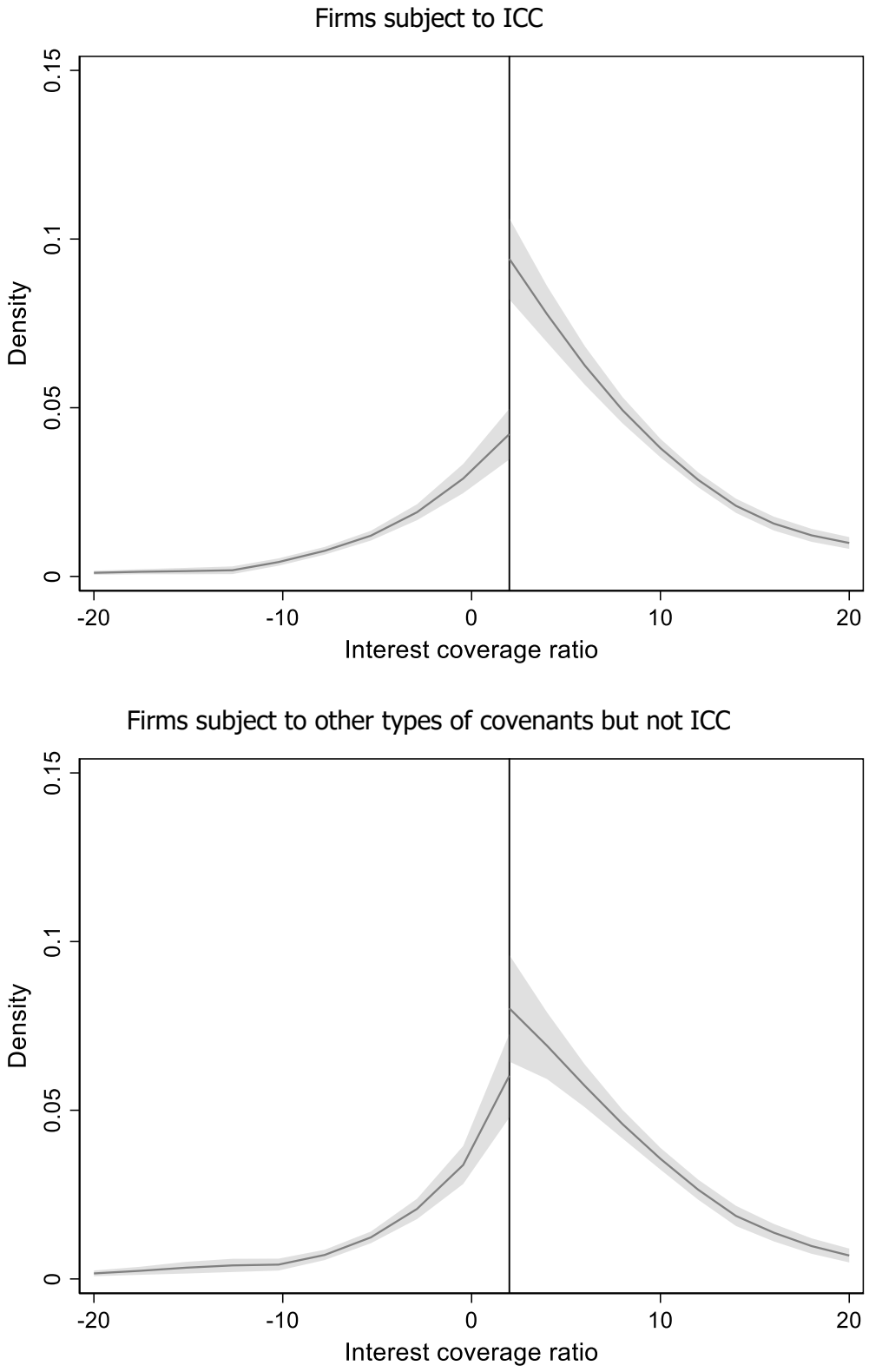
In terms of covenant violations, the fraction of firms reporting breaches of covenants rose sharply during the GFC, with a slight further rise in the late 2010s. As firms have to report instances of breaches, this rise will not reflect changes in reporting standards, though part of it may simply reflect increased use of covenants. The breaching rate, conditional on firms reporting covenants, is more stable and in line with the trends in the United States, where 10 to 20 per cent of firms reported breaches every year (Nini *et al* 2012). However, the conditional rate is potentially subject to bias due to selective reporting.

The use and structure of debt covenants also vary across industries. Debt covenants are most used in the real estate sector, while the materials (including mining) and energy sectors have the least use of covenants. ABC appear more popular in capital-intensive sectors (e.g. real estate and utilities), while ICC and OEC are more prevalent in services sectors (e.g. commercial and professional services, communication services and IT). In addition, the utilities and health care sectors stand out as having the largest shares of firms reporting a covenant breach (Figure B1).

3. Direct Effects of Covenants

In this section I explore the direct effects of debt covenants on real business activity and aim to disentangle the mechanisms. As discussed above, the direct effects of debt covenants can stem from the '*ex post* punishing channel' following a breach or the '*ex ante* disciplining channel' in the absence of any breach. The latter happens when firms try to manage their activity and financial position to avoid breaching covenants. Figure 4 (top panel) plots the density of the interest coverage ratio by firms subject to ICC in the data, showing a significant bunching right before 2, which is a common minimum critical threshold. By contrast, there is no significant evidence of manipulation of the ratio by firms subject to other types of covenants but not ICC (bottom panel). This suggests that firms try to avoid breaching ICC and, as such, supports the disciplining role of debt covenants.

Figure 4: Manipulation around ICR Threshhold of 2



Notes: The lines represent local polynomial estimates of order 2 and the shaded areas represent 95 per cent confidence intervals around the ICR cut-off of 2 (vertical line). See Catteneo, Jansson and Ma (2020) for details.

Sources: Author's calculations; Morningstar

To examine the overall direct effects of debt covenants, I first estimate the following baseline empirical model:

$$\Delta y_{i,t} = \alpha_i^0 + \beta^0 Cov_{i,t-1} + \mathbf{X}'_{i,t-1}\mu^0 + \sum_s \theta_s^0 I_{i,s} t + \varepsilon_{i,t} \quad (1)$$

The dependent variable $\Delta y_{i,t}$ represents real business activity of firm i in year t. For investment, it is measured by the log difference in fixed assets (property, plant, equipment and machinery) between t and $t-1$. For employment, it is measured by the log difference in staff and employees expenses. $Cov_{i,t-1}$ indicates whether firm i was subject to debt covenants in time $t-1$. The parameter of interest β^0 traces the relationship between the firm's real business activity in period t and its exposure to covenants in period $t-1$. The random error term $\varepsilon_{i,t}$ is allowed to be correlated within firm observations and potentially heteroskedastic with the standard errors being clustered at the firm level.

The simple reduced-form approach faces several empirical challenges. First, there is a potential reverse causality issue; that is, business activity causes exposure to debt covenants. On one hand, firms and creditors may anticipate a future drop in business activity and insure against that by incorporating debt covenants, contributing to a negative correlation between outcomes and covenants. On the other hand, firms could have hiring and investing strategies that lead to extra borrowing and, in turn, the covenants associated with the loan facilities. Similarly, firms may try to reduce expenses prior to loan applications and return to normal activity subsequently. In the latter two cases, the reverse causality should work against finding a negative link between business activity and exposure to covenants.

To address these potential endogeneity issues, I include the vector of covariates $\mathbf{X}'_{i,t-1}$ to control for time-varying firm-level financial measures such as the usual proxies for firm size (total assets), performance (total revenue), credit (debt), financial structure (gearing ratio) and liquidity (cash holding). Inclusion of firm fixed effects, α_i^0 controls for the unobserved time-invariant firm-level components (e.g. its origin and mission, managerial abilities and risk preferences, as well as sector and associated sector-level characteristics that do not vary over time). To the extent that the observed firm-level time-varying financial measures and firm-specific fixed effects inform the firm and creditors about its future conditions, this helps reduce potential reverse causality bias. These firm-level controls also help address the potential selection bias in the reporting of debt covenants by firms' characteristics as discussed in Section 2.

Lastly, both the actual usage and the reporting of debt covenants may change over time due to changes in aggregate economic and credit conditions, such as the tightening in credit availability and financial regulations during and following the global financial crisis, as well as changes in financial reporting standards. As these factors are likely to vary across industries, I include the sector-level time trends $I_{i,s}t$ where $I_{i,s}$ indicates the sector s of firm i. Even with all these controls, it is important to emphasise that identification relies on the underlying assumption that exposure to debt covenants does not coincide with firm-level temporary shocks that are not captured by the controls.

As an alternative to unpack the channels of the direct effects, I employ an empirical strategy similar in spirit to a difference-in-differences framework. I first focus on firms that were not recently exposed to nor breaching any covenants. More specifically, I estimate the following equation:

$$\Delta y_{i,t} = \alpha_i^1 + \beta^1 Dis_{i,t-1} + \mathbf{X}'_{i,t-1}\mu^1 + \sum\nolimits_s \theta_s^1 I_{i,s} t + \varepsilon_{i,t} \quad (2)$$

where $Dis_{i,t-1} = \begin{cases} 1, & Cov_{i,t-2} = 0 \ \& \ Cov_{i,t-1} = 1 \ \& \ Breach_{i,t-1} = 0 \\ 0, & Cov_{i,t-2} = 0 \ \& \ Cov_{i,t-1} = 0 \end{cases}$.

That is, the *ex ante* effect of debt covenants is identified by comparing the control group (not previously nor currently exposed to debt covenants) with the treatment group (not previously but currently exposed to debt covenants with no breaches). The additional condition that no covenant has been violated in period $t-1$ eliminates confounding effects of a breach and allows for an effective evaluation of the marginal effects of debt covenants as a disciplinary device. Figure 5 shows that both the control and treatment groups follow parallel and stable trends in investment and employment prior to the treatment group being exposed to covenants (before $t-1$). Moreover, there are no discernible differential trends in their financial statistics both before and after the exposure treatment (Figure C1). Formal testing of the difference between the time trend coefficients suggests that the time trends in investment and employment before the exposure shock are statistically similar between the two groups, confirming that they are otherwise comparable (Table C1).

Similarly, I attempt to pin down the *ex post* effect by comparing the control group (previously and currently exposed to debt covenants but no breaches) with the treatment group (previously and currently exposed to debt covenants with current breaches only) as follows:

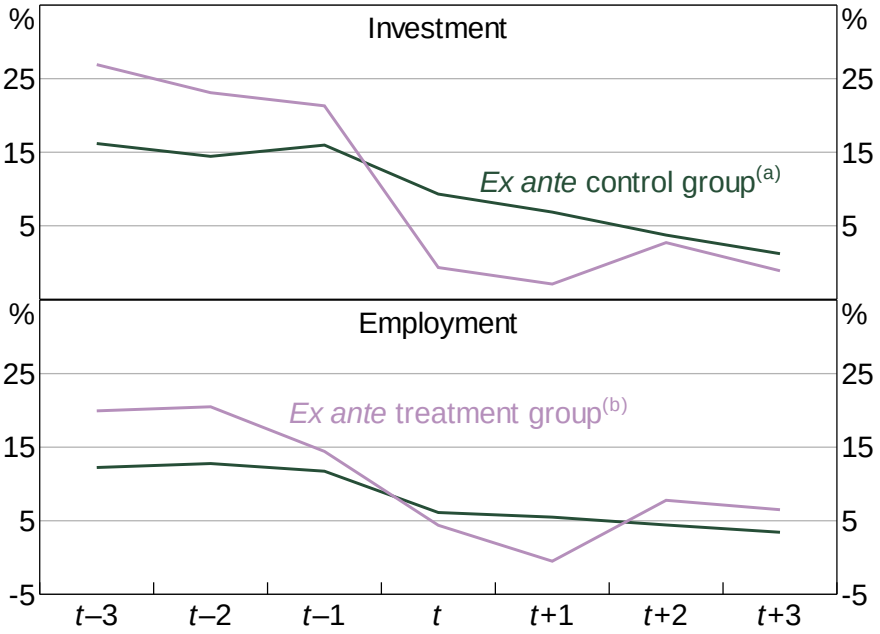
$$\Delta y_{i,t} = \alpha_i^2 + \beta^2 Pun_{i,t-1} + \mathbf{X}'_{i,t-1}\mu^2 + \sum\nolimits_s \theta_s^2 I_{i,s} t + \varepsilon_{i,t} \quad (3)$$

where $Pun_{i,t-1} = \begin{cases} 1, & Cov_{i,t-2} = Cov_{i,t-1} = 1 \ \& \ Breach_{i,t-2} = 0 \ \& \ Breach_{i,t-1} = 1 \\ 0, & Cov_{i,t-2} = Cov_{i,t-1} = 1 \ \& \ Breach_{i,t-2} = Breach_{i,t-1} = 0 \end{cases}$.

However, unlike what's observed for *ex ante* treatment, trends in investment, and to a lesser degree employment, prior to the *ex post* treatment vary significantly between control group and treatment group (Figure 6). Moreover, firms' financial statistics experience noticeable deterioration around the breach with an even larger deviation by firms who could not survive following a breach (Figure C2).

Figure 5: Activity around *Ex ante* Treatment

Non-financial listed firms, average

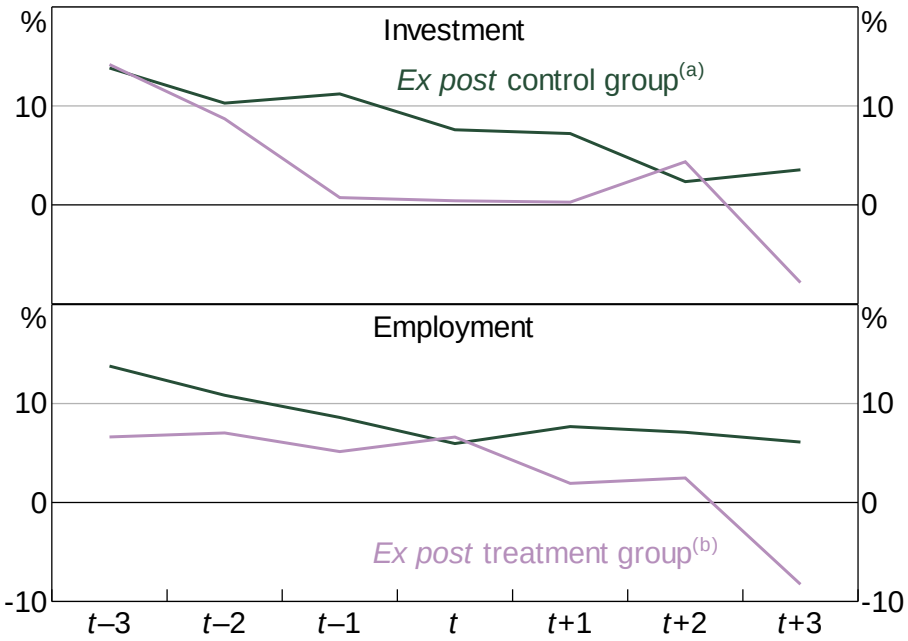


Notes: (a) Firms not exposed to covenants in both $t-2$ and $t-1$.
(b) Firms not exposed to covenants in $t-2$, exposed in $t-1$ but no breach in $t-1$.

Sources: Author's calculations; Connect4; Morningstar

Figure 6: Activity around *Ex post* Treatment

Non-financial listed firms, average



Notes: (a) Firms exposed to covenants but no breach in both $t-2$ and $t-1$.
(b) Firms exposed to covenants in both $t-2$ and $t-1$, no breach in $t-2$ but breaches in $t-1$.

Sources: Author's calculations; Connect4; Morningstar

Turning to the empirical results, Table 2 presents the estimates of relevant coefficients in Equations (1), (2), and (3) for investment and employment. All specifications include the full set of firm-level controls, firm fixed effects and sector-level time trends. The first column shows that, overall, being subject to covenants is associated with a more than 11 per cent drop in investment and more than 6 per cent drop in staff expenses. That is equivalent to $50 million in investment and $3 million in staff expenses, on average. Equation (2) isolates the *ex ante* disciplining mechanism and shows that the exposure treatment is associated with an 11 per cent drop in investment and a 9 per cent drop in staff expenses. By contrast, estimates of the *ex post* effect are not particularly significant, which is unsurprising given the differing trends prior to the covenant violation (Figure 6). Overall, these results suggest the importance of debt covenants as an *ex ante* disciplining device, over and beyond the effects of their violations.

Table 2: Direct Effects of Debt Covenants on Investment and Employment

Overall and across different channels

	Equation (1) Overall (β^0)	Equation (2) *Ex ante* (β^1)	Equation (3) *Ex post* (β^2)
Investment	–0.114***	–0.118**	0.021
	(0.037)	(0.055)	(0.132)
Observations	5,577	2,966	1,120
Staff expenses	–0.065***	–0.092*	–0.095*
	(0.024)	(0.050)	(0.057)
Observations	4,007	2,069	846
Firm fixed effects	Yes	Yes	Yes
Financial measures	Yes	Yes	Yes
Sector time trends	Yes	Yes	Yes

Notes: Clustered standard errors at firm level are shown in parentheses. ***, ** and * denote statistical significance at the 1, 5 and 10 per cent levels, respectively.

Sources: Author's calculations; Connect4; Morningstar

To ensure that the estimates are not driven by mining firms with large investment and very low likelihood of being exposed to covenants, I estimate all three equations on non-mining firms and find that the *ex ante* effects of debt covenants appear even stronger on non-mining investment. As seen in Section 2, firms subject to covenants are substantially larger than those not subject to covenants. To check the results against potential underlying factors associated with smaller firms that are not captured by their financial statistics, I also estimate the models using a sample of larger firms only. They are defined as having annual revenue greater than $2 million before 2015 and $10 million after 2015. The estimates are broadly similar to the full sample. Finally, there may be underlying factors that lead to firms never engaging in debt covenants. I address this concern by removing firms not subject to covenants throughout their lifespan in the sample. Results suggest much stronger direct effects of debt covenants, particularly through the disciplining channel. All robustness checks can be found in Appendix D.

4. Indirect Effects of Covenants – Monetary Policy Transmission

I now turn to how debt covenants can amplify or mitigate the transmission of monetary policy to real business activity. This may differ based on the type of covenant, given interest rate changes may or may not change the associated constraint.

Changes in interest rates directly affect (variable-rate or floating) interest payments on outstanding debt and, in turn, the interest coverage ratio. That then moves firms closer to or further from the threshold associated with their ICC. Therefore, for firms subject to ICC, how much debt can be issued without violating these covenants and how much scope the firms have to expand and take on other expenses is highly sensitive to changes in monetary policy.

Conversely, changes in interest rates may have minimal effects on ratios used for other types of covenants: for OEC there may be some indirect effect via earnings, while for ABC there will be little to no effect since the ratios are measured with book values to avoid feedback from market prices of assets. If firms with such covenants are already constrained, changes in interest rates may have little effect on their behaviour. As such, OEC and ABC may dampen the effects of monetary policy.

To empirically test the indirect effects of debt covenants, I modify Greenwald's (2019) framework and split firms into three categories: subject to ICC, not subject to ICC but to either OEC or ABC or both (NICC), and the residual group of firms not subject to any covenants (NC).[1] To measure the heterogeneous transmission across different covenant configurations, I employ the panel local projections method of Jordà and Taylor (2016) and estimate the following regression for dependent variable y and horizon h:

$$\Delta_h y_{i,t+h} = \alpha_i^h + \sum_{Cov} I_{i,t-1,Cov}\left(\beta_{0,Cov}^h + \beta_{1,Cov}^h \epsilon_t\right) + \left(\gamma_0^h + \gamma_1^h \epsilon_t\right)\mathbf{X}'_{i,t-1} + \theta^h t + \sum_k \delta_k^h \epsilon_{t-k} + \eta_{i,t+h} \quad (4)$$

where $\Delta_h y_{i,t+h} = y_{i,t+h} - y_{i,t-1}$ is the response of the variable of interest (log investment and log staff expenses) in year $t+h$ to a monetary policy shock in year t, notated as ϵ_t. The categorical variable $I_{i,t-1,Cov}$ indicates the firm's covenant configuration in year $t-1$, which is either (1) ICC, (2) NICC or (3) NC.

Similar to the empirical model employed to study the direct effects of covenants, I control for firm-level time-varying financial measures, firm fixed effects and time trends. As suggested by the varying financial statistics across covenant groups, selection into different covenants is potentially non-random. That is, firms whose business strategies expose them to changes in interest rates differently may select into different covenants. For instance, large firms in capital-intensive industries with more volatile streams of earnings are often subject to interest coverage limits. However, they are also more likely to invest in high-yielding projects that face fiercer credit rationing and, in turn, are more likely to be particularly sensitive to changes in interest rates (Barea Lugo 2006). To address this potential endogeneity issue, I include interactions between the firm-level controls (including firm size and firm sector) and monetary policy shocks. This allows for firms with different financial situations and in different industries to react differently to the same shocks and, to the extent that selection is correlated with my controls, alleviates the selection bias. Since business activity is likely

1 I exclude firms not specifying the exact types of covenants. These firms share virtually similar financial and non-financial characteristics as firms reporting the types of covenants, suggesting little concern for bias in sample selection.

to respond to past monetary policy, controlling for the lagged measures of monetary shocks ϵ_{t-k} can help improve the precision of my estimates.[2]

Another key departure from Greenwald (2019) is that instead of changes in the cash rate, I employ the newly available series of monetary policy shocks constructed by Beckers (2020). This is because changes in the cash rate are not exogenous, but instead reflect systematic responses to economic conditions, which may have confounding effects on business activity. This is concerning in my case as ICC and OEC are affected by earnings, which depend on aggregate economic conditions, while ABC are not. Therefore, the effects of monetary policy need to be separated from macroeconomic and financial conditions. The monetary policy shocks are constructed following the method of Romer and Romer (2004), but augmented to control for the systematic response of the central bank to financial conditions, as represented by credit spreads (as in Caldara and Herbst (2019)). They are then purged of financial market expectations for the cash rate to ensure that changes in monetary policy are plausibly exogenous to and unanticipated by private agents. I aggregate the shocks up to an annual frequency to match with the annual data on covenants and finances (Appendix E).

The primary statistics of interest are the differences between the coefficients attached to monetary policy shocks on firms of different covenants configurations ($\beta^h_{1,Cov}$), for each horizon h :

$$D^h_{ICC-NC} = \beta^h_{1,ICC} - \beta^h_{1,NC}$$

$$D^h_{NICC-NC} = \beta^h_{1,NICC} - \beta^h_{1,NC}$$

$$D^h_{ICC-NICC} = \beta^h_{1,ICC} - \beta^h_{1,NICC}$$

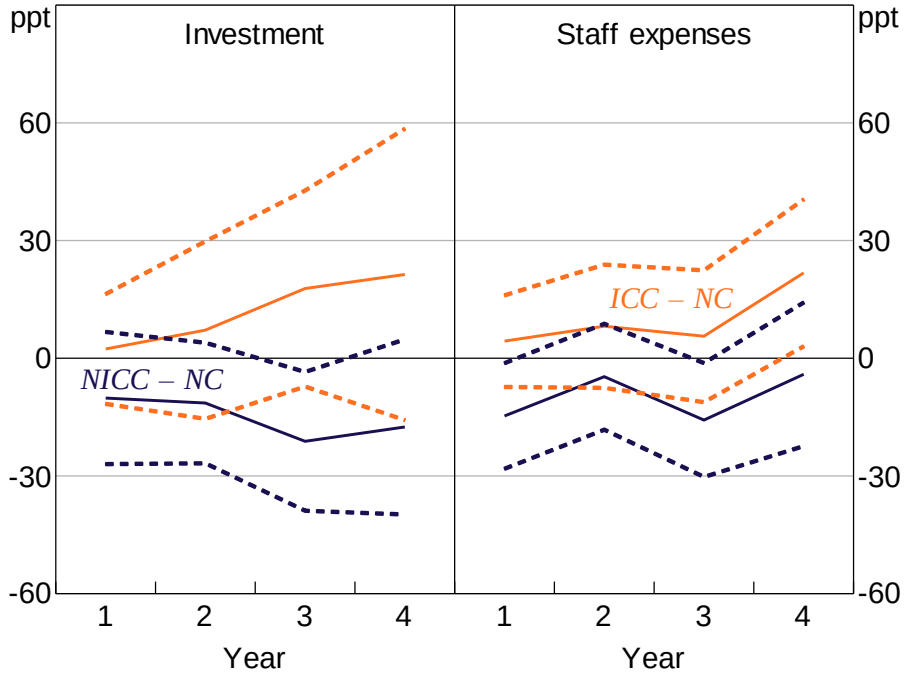
In other words, the statistics measure the responses of firms subject to a particular covenant configuration relative to otherwise-equivalent firms subject to a different covenant configuration.

The left panel of Figure 7 plots the differential coefficients for investment up to four years after an expansionary monetary policy shock. While imprecise, the estimates suggest a role for ICC in amplifying transmission of monetary policy to investment; firms subject to ICC respond more strongly to shocks relative to firms not subject to any covenants. In contrast, responses of NICC firms appear weaker than otherwise equivalent non-covenants firms, suggesting that other types of covenants dampen the effect of monetary policy on investment. Similarly, transmission of monetary policy to firms' staff expenses is amplified by ICC and mitigated by NICC (right panel).

2 I employ $k = 2$ but results are robust using fewer or more lags.

Figure 7: Differential Responses by Covenant

To a 100 basis point expansionary monetary policy shock



Notes: Measured as the differences between the coefficients attached to each covenant configuration dummy interacting with monetary policy shock. Dashed lines depict 95 per cent confidence intervals.

Sources: Author's calculations; Beckers (2020); Connect4; Morningstar

5. Aggregate Effects of Covenant Composition

The differential responses by covenant configuration beg the question: has the effectiveness of monetary policy changed due to changes in the structure of covenants over time? According to Figure 3, the prevalence of ICC relative to other types of covenants has declined over the past decade. As such, the role of covenants in amplifying monetary policy transmission post-GFC might have been dampened by the shift away from ICC towards NICC. Moreover, the overall usage of covenants has increased, which could also affect transmission.

In this section, I quantify the effect of the shift in the composition of covenants by using the coefficients from the full sample regression in Equation (4) and information on the composition of covenants to capture the average partial response to a monetary policy shock. I examine how this has changed over time to quantify how changes in composition affected monetary policy pass-through. Specifically, I compute the average partial responses[3] of business activity in each year of the data as follows:

$$Responses_t^h = ICC\ share_{t-1} * \beta_{1,ICC}^h + NICC\ share_{t-1} * \beta_{1,NICC}^h + NC\ share_{t-1} * \beta_{1,NC}^h$$

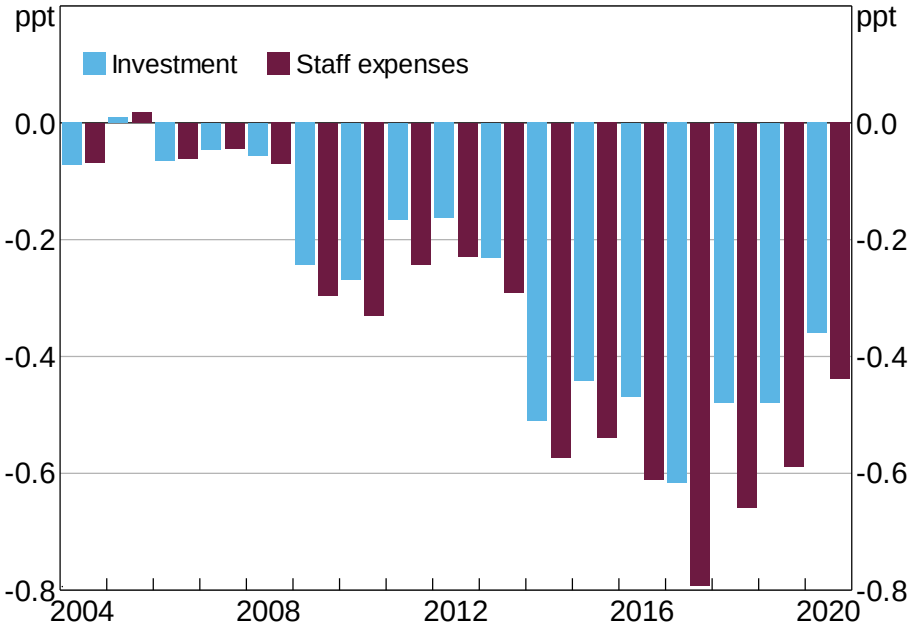
That is, the average partial response in time t is the sum of coefficients $\beta_{1,Cov}^h$, estimated for each covenant configuration using Equation (4), across configurations and weighted by the composition

3 Impulse responses of business activity to monetary policy shocks, abstracting from the transmission of shocks via firms' characteristics.

of covenants in the previous year (as a share of all firms). I then compare the average partial response in any given year with that in 2003.

Focusing first on investment, Figure 8 shows that the responsiveness of investment to a monetary policy shock has declined due to shifts in the covenant composition. The response of business investment one year after a 100 basis point monetary policy shock is estimated to be 0.35 percentage points smaller in 2020, compared to the case where the composition of covenants remained unchanged. To put this in context, the RBA's macroeconometric model MARTIN predicts that business investment declines by about 1.3 per cent over one year following a 100 basis point cash rate increase (Ballantyne *et al* 2020). As such, the shift in the composition of covenants is estimated to have lowered the responsiveness of firms' investment by around 15 to 25 per cent.

Figure 8: Average Partial Responses Relative to 2003
One year following a 100 basis point expansionary monetary policy shock



Note: Average partial responses in each year are calculated using the NC, ICC and NICC shares of firms in the year.
Sources: Author's calculations; Beckers (2020); Connect4; Morningstar

Focusing on non-mining firms only, I find the estimates to remain virtually unchanged (Figure F1), suggesting that changes in the composition of covenants could potentially account for part of the weakness in non-mining investment observed over the 2010s (Debelle 2017; Lowe 2018; Dynan 2021). Applying the results from my sample to the full economy seems reasonable, given my sample is made up of large firms, and large firms account for a large proportion of non-mining investment (Dynan 2021).

The compositional change in covenants includes both the increased reporting of covenants and the shift from ICC to NICC within firms subject to covenants. To the extent that the lower share of NC firms reflects the trend in reporting standards rather than a true increase in covenant exposure, the previous calculation may overstate the effects of the compositional change over time. Therefore I also take a more conservative approach in estimating the effect by keeping the share of NC firms unchanged from the level in 2003 and just allowing the ICC and NICC split within firms with

covenants to vary. Results suggest that the responsiveness of investment to monetary policy shock is 0.2 percentage points smaller in 2020 (Figure F2).

Similar results are found for staff expenses, with the average response to a monetary policy shock after one year being 0.4 percentage points lower in 2020, compared to the case where the composition of covenants remained unchanged. However, total staff expenses in the sample account for less than 10 per cent of total private sector labour costs according to the national accounts. As such, it is a stronger assumption that the changes can be extrapolated to the broader economy. However, to the extent that the same shifts have occurred amongst non-listed firms, this might still suggest some moderate aggregate effects.

One potential concern is that the decline in reported ICC relative to NICC might have been driven by a lower breaching rate of ICCs due to the low interest rate environment in Australia. In such a case, there may not be an underlying shift in the composition of covenants. However, the rate of breaches amongst firms with ICC has increased over the past decade, and at a much faster pace compared with NICC firms. Secondly, having removed breaching instances and keeping only firms routinely reporting covenants, the shift in the composition of covenants away from ICC towards NICC remains apparent (Figure G1). As such, there is no evidence of the trend being driven by breaches. Nevertheless, it is possible that the increase in the relative NICC share reflects changes in the reporting standard that caused the overall rise, to the extent that they disproportionately affected the NICC. In that case, the counterfactual analysis results should still be treated as an upper bound on the effect of the shifting composition of covenants.

More broadly, it is important to note that the estimates take a relatively simple, partial equilibrium approach to understanding the effect of changes in the composition of covenants. As such they should be thought of as indicative, not precisely estimated. Still, they do suggest that increased use of NICC could help explain part of the weakness in non-mining investment over the 2010s.

6. Conclusion

This paper studies the macroeconomic effects of debt covenants using a new dataset of corporate debt covenants in Australia. I first extend the strand of literature focusing on the consequences of covenant breaches by exploring the disciplining role of debt covenants. I find that firms exposed to debt covenants reduce their investment and staff expenses even in the absence of any breaches, suggesting that covenants can have substantive implications for firm behaviour even if rates of breach remain low. I then examine how the structure of covenants affects the transmission of monetary policy to real business activity. I find that interest coverage covenants, where the level of interest rates affects how much they bind, amplify the responses of investment and staff expenses to monetary policy shocks. By contrast, covenants that directly limit the stock of debt appear to dampen the transmission.

Interestingly, there seems to have been a compositional change in covenants over time among non-financial listed firms in Australia. The shift away from interest coverage covenants towards other types of covenants is in contrast with findings in the United States where the interest coverage share of debt covenants has stayed virtually constant since the late 1990s (Greenwald 2019). Using simple quantification, I find that the compositional change in covenants over time in Australia may have had a sizeable effect on the transmission of monetary policy to aggregate non-mining investment.

This potentially explains part of the surprising weakness in non-mining investment in Australia since the 2010s. The shift may have also made employment slightly less responsive to monetary policy. To the extent that the shift reflects a structural change, it has important implications for monetary policy. As such, future work should explore the potential causes of the shift in covenant composition.

Appendix A: Construction of Debt Covenants Data

I construct a database on the prevalence and types of debt covenants used by non-financial listed Australian firms by applying text analytic techniques to their publicly available annual reports, collected from the Connect4 website. I write a Python program to first convert the files into readable text and then extract relevant information from the text as follows:

1. I search for the term 'covenant' and its inflections in the text. If the search query returns non-empty results, I classify the firm as having debt covenants in that year.

2. I isolate the blocks of text surrounding the mentions of covenants. Figure 1 shows an example of an extracted block of text.

3. In each block of text, I search for keywords (and their inflections) that indicate the possible types of debt covenants (e.g. interest cover, gearing ratio, leverage ratio).

4. For each type of debt covenant, I count the appearances of its indicative keywords. If the counter returns a positive value, I classify the firm as having that particular type of covenant. In the example in Figure 1, the firm mentions three types of debt covenants: equity ratio, leverage ratio and interest cover ratio.

5. Finally, I determine if firms comply with or violate their covenants from the reports by counting the appearances of keywords such as 'breach' and 'violate' (and their inflections while incorporating negation). The example in Figure 2 suggests that the firm breached its financial covenants in the period to the date of the report.

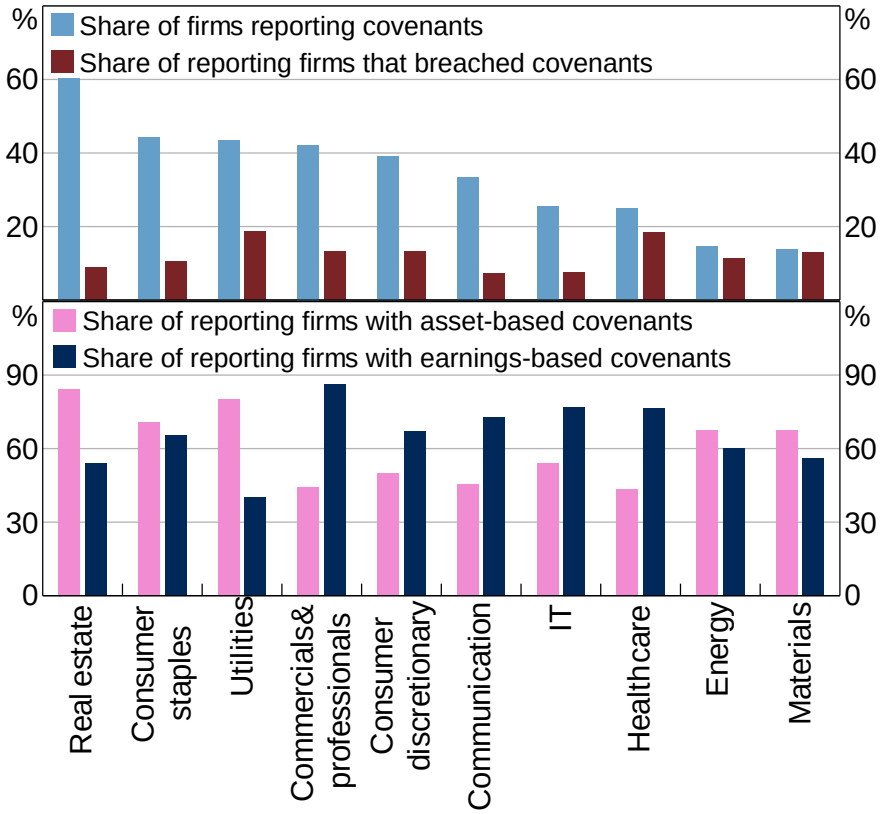
Appendix B: Other Descriptive Statistics

Table B1: Summary Statistics – Full Sample
Non-financial listed firms

	Mean	Median	Standard deviation
Revenue ($ million)	378	15	2,189
Debt ($ million)	321	4	2,018
Cash ($ million)	87	6	648
Assets ($ million)	1,135	59	6,802
Investment ($ million)	463	5	4,270
Staff expenses ($ million)	55	3	267
Return on equity ratio	0.1	0.1	13.0
Debt-to-equity ratio	1	0	15
Debt-to-EBITDA ratio	0	0	132
Interest coverage ratio	–5,282	4	329,514

Sources: Author's calculations; Connect4; Morningstar

Figure B1: Reported Debt Covenants by Sector
Non-financial listed firms, 2002–20



Sources: Author's calculations; Connect4

Table B2: Summary Statistics of ICC Firms

Non-financial listed firms

	2002–10			2011–20		
	25th percentile	Median	75th percentile	25th percentile	Median	75th percentile
Revenue ($ million)	35.6	110.9	330.8	19.8	82.4	289.6
Debt ($ million)	8.3	47.8	301.4	11.1	79.3	420.0
Cash ($ million)	1.9	9.1	30.3	4.5	15.9	50.3
Assets ($ million)	55.4	198.2	980.1	87.3	446.5	1,342.0
Investment ($ million)	6.6	29.8	210.7	3.5	38.1	288.1
Staff expenses ($ million)	5.3	17.5	60.8	3.1	20.2	81.7
Return on equity ratio	0.1	0.2	0.4	0.1	0.2	0.3
Debt-to-equity ratio	0.3	0.5	0.9	0.2	0.4	0.8
Debt-to-EBITDA ratio	0.9	3.6	8.0	0.8	3.4	8.1
Interest coverage ratio	2.8	5.8	12.6	2.1	5.5	13.2

Sources: Author's calculations; Connect4; Morningstar

Appendix C: Trends before and after Treatments

C.1 Test for parallel trends

To test for parallel trends before the covenants exposure treatment, I estimate the following regression on investment and staff expenses the years before the treatment:

$$\Delta y_{i,t} = \alpha_i + \beta_0 NoDis_{i,t-1} * trend + \beta_1 Dis_{i,t-1} * trend + \mathbf{X}'_{i,t-1}\mu + \varepsilon_{i,t}$$

The parameter of interest is the differential time trend coefficients, $\beta_0 - \beta_1$. The two groups follow parallel trends if the parameter is statistically significantly zero.

Table C1: Test for Parallel Trends between Control and Treatment Groups

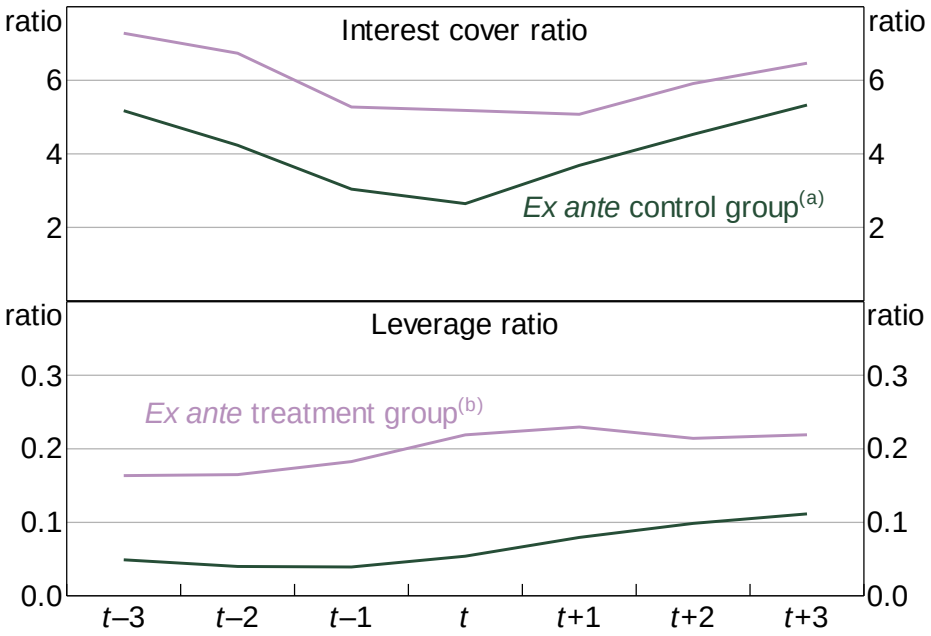
Wald test for the difference between time trends

	Time trend coefficients		Difference
	Control group (β_0)	Treatment group (β_1)	$\beta_0 - \beta_1$
Investment	–0.0065863	–0.0066515	0.0000652
	(0.0071332)	(0.0071293)	(0.0000284)
Staff expenses	–0.0114725	–0.0115148	0.0000423
	(0.0065714)	(0.0065660)	(0.0000206)

Note: Clustered standard errors at firm level are shown in parentheses.
Sources: Author's calculations; Connect4; Morningstar

Figure C1: Trends in Financial Statistics

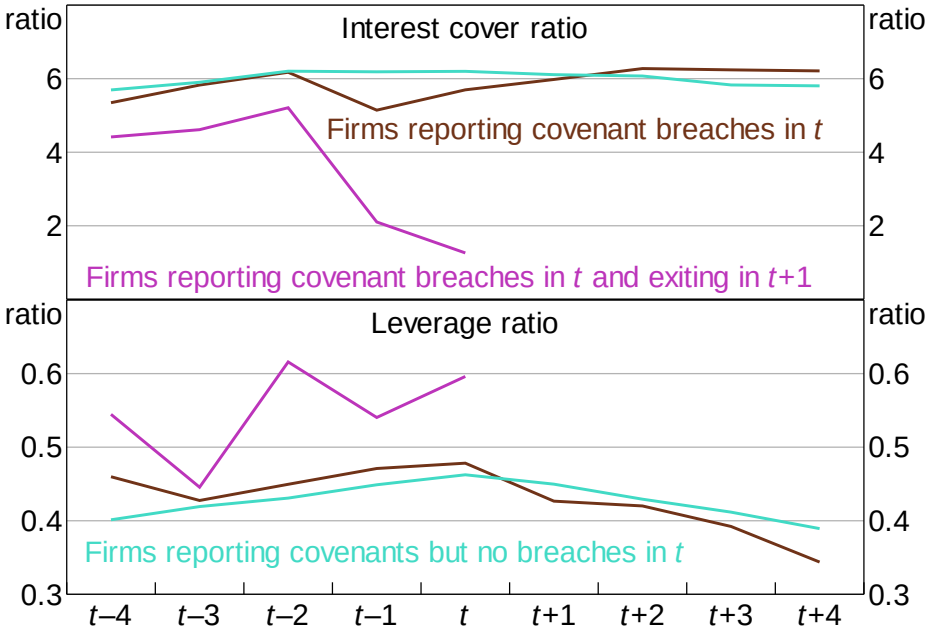
Non-financial listed firms, average



Notes: (a) Defined as firms not exposed to covenants in both $t-2$ and $t-1$.
(b) Defined as firms not exposed to covenants in $t-2$, exposed in $t-1$ but no breaches in $t-1$.
Sources: Author's calculations; Connect4; Morningstar

Figure C2: Financial Statistics and Covenant Breaches

Non-financial listed firms, average



Sources: Author's calculations; Connect4; Morningstar

Appendix D: Direct Effects – Robustness Checks

Table D1: Direct Effects of Debt Covenants – Non-mining Sample
Overall and across different channels

	Equation (1) Overall (β^0)	Equation (2) *Ex ante* (β^1)	Equation (3) *Ex post* (β^2)
Investment	–0.128***	–0.134**	0.033
	(0.037)	(0.054)	(0.135)
Observations	5,409	2,871	1,086
Staff expenses	–0.072***	–0.094*	0.105*
	(0.025)	(0.050)	(0.060)
Observations	3,901	2,009	823
Firm fixed effects	Yes	Yes	Yes
Financial measures	Yes	Yes	Yes
Sector time trends	Yes	Yes	Yes

Notes: Clustered standard errors at firm level are shown in parentheses. ***, ** and * denote statistical significance at the 1, 5 and 10 per cent levels, respectively.
Sources: Author's calculations; Connect4; Morningstar

Table D2: Direct Effects of Debt Covenants – Large Firms Sample
Overall and across different channels

	Equation (1) Overall (β^0)	Equation (2) *Ex ante* (β^1)	Equation (3) *Ex post* (β^2)
Investment	–0.111***	–0.102**	–0.006
	(0.033)	(0.051)	(0.140)
Observations	4,499	2,344	1,086
Staff expenses	–0.066***	–0.078	0.100*
	(0.024)	(0.053)	(0.054)
Observations	3,186	1,603	817
Firm fixed effects	Yes	Yes	Yes
Financial measures	Yes	Yes	Yes
Sector time trends	Yes	Yes	Yes

Notes: Clustered standard errors at firm level are shown in parentheses. ***, ** and * denote statistical significance at the 1, 5 and 10 per cent levels, respectively.
Sources: Author's calculations; Connect4; Morningstar

Table D3: Direct Effects of Debt Covenants – Sample with Firms Never Exposed Removed

Overall and across different channels

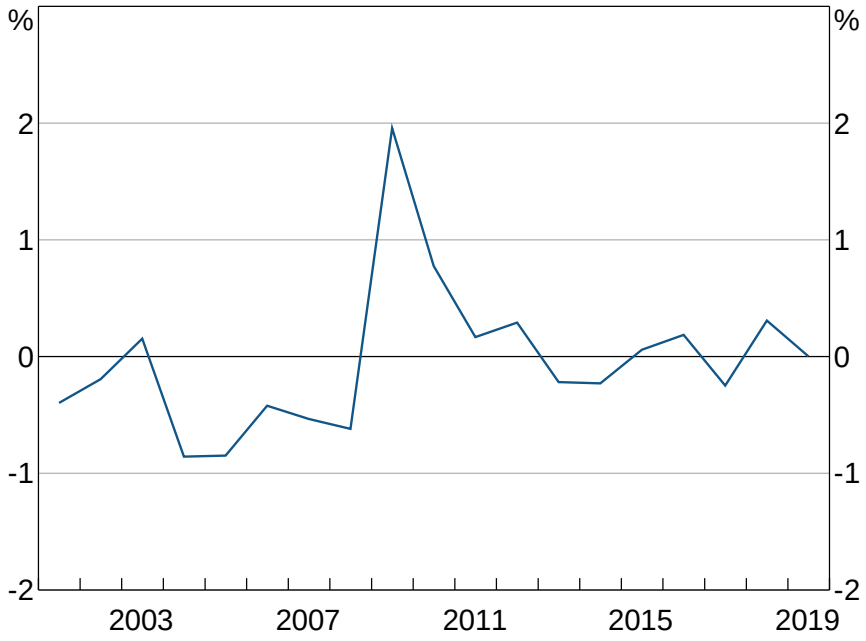
	Equation (1) Overall (β^0)	Equation (2) *Ex ante* (β^1)	Equation (3) *Ex post* (β^2)
Investment	–0.206***	–0.163**	–0.126
	(0.060)	(0.075)	(0.153)
Observations	1,978	472	1,040
Staff expenses	–0.103**	–0.056	0.105**
	(0.041)	(0.071)	(0.046)
Observations	1,479	330	786
Firm fixed effects	Yes	Yes	Yes
Financial measures	Yes	Yes	Yes
Sector time trends	Yes	Yes	Yes

Notes: Clustered standard errors at firm level are shown in parentheses. ***, ** and * denote statistical significance at the 1, 5 and 10 per cent levels, respectively.

Sources: Author's calculations; Connect4; Morningstar

Appendix E: Monetary Policy Shocks

Figure E1: Monetary Policy Shocks for Australia
Annualised

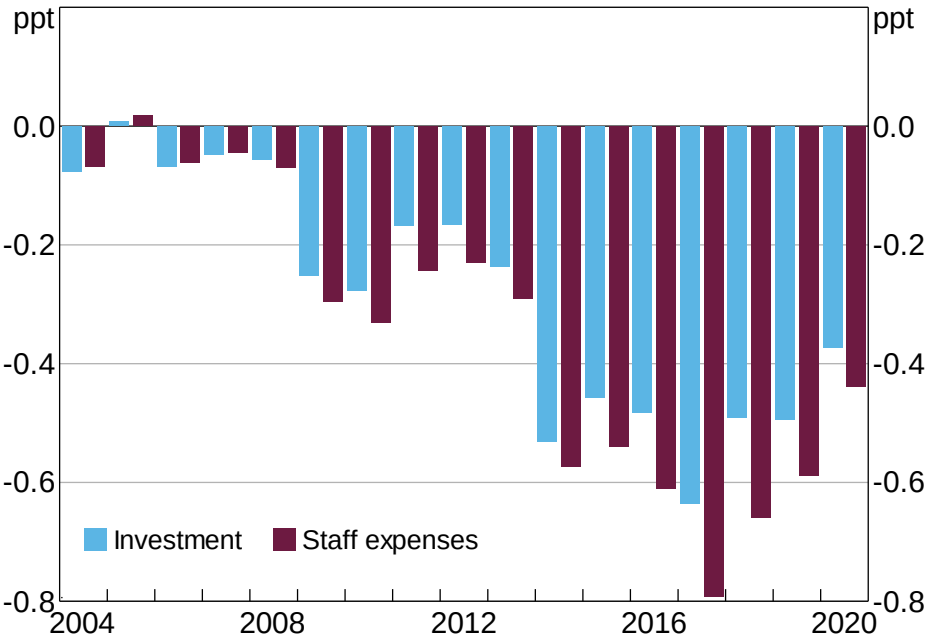


Sources: Author's calculations; Beckers (2020)

Appendix F: Average Partial Responses Relative to 2003

Figure F1: Average Partial Responses Relative to 2003 – Non-mining

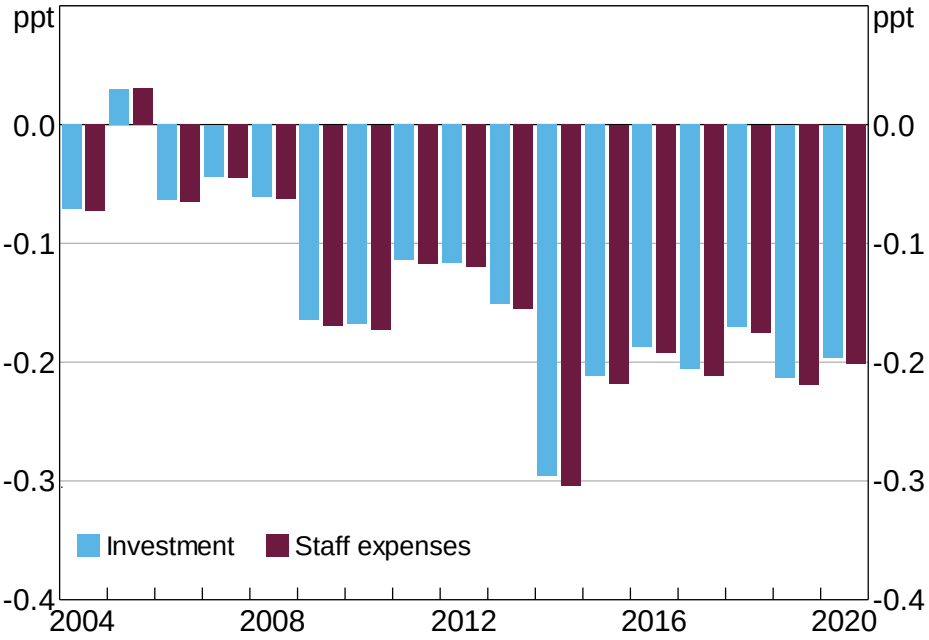
One year following a 100 basis point expansionary monetary policy shock



Note: Average partial responses in each year are calculated using the NC, ICC and NICC shares of firms in the year.
Sources: Author's calculations; Beckers (2020); Connect4; Morningstar

Figure F2: Average Partial Responses Relative to 2003

One year following a 100 basis point expansionary monetary policy shock

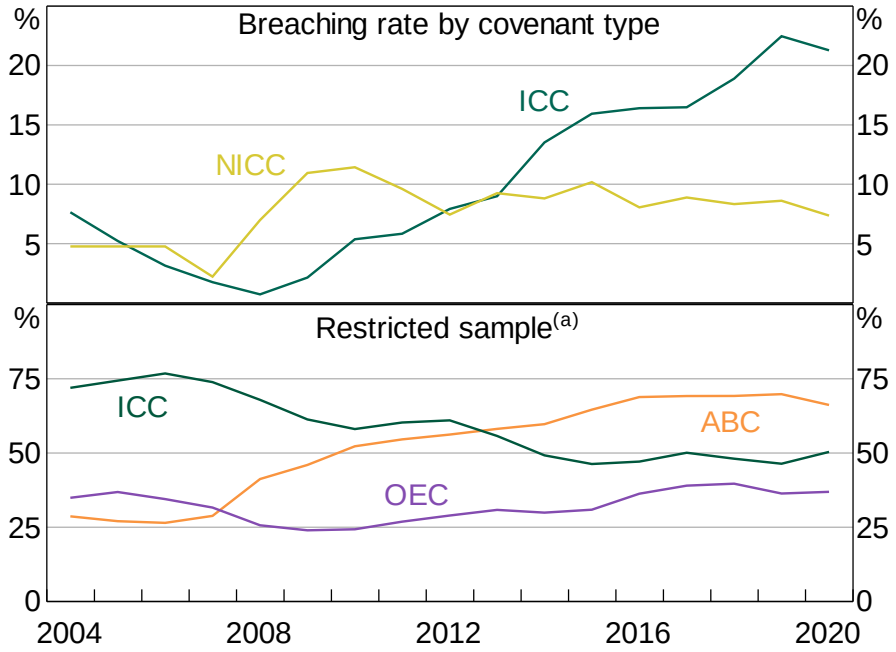


Note: Average partial responses in each year are calculated using the NC share of firms in 2003 and the ICC and NICC shares of firms subject to covenants in the year.
Sources: Author's calculations; Beckers (2020); Connect4; Morningstar

Appendix G: Trends Using Restricted Sample and Breaching Rates

Figure G1: Trends in Corporate Debt Covenants

Non-financial listed firms, three-year moving average



Note: (a) Firms that routinely reported debt covenants. Breaching instances and observations with no mention of covenant types were removed.

Sources: Author's calculations; Connect4; Morningstar

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