Australian National University

Crawford School of Public Policy

TTPI

Tax and Transfer Policy Institute

When are investment tax breaks effective?

TTPI - Working Paper 1/2025
January 2025

Nu Nu Win
Australian National University &
Department of Treasury

Jonathan Hambur
Reserve Bank of Australia

Robert Breunig
Crawford School of Public Policy
Australian National University

Abstract

We investigate the effectiveness of seven business investment tax breaks under a range of different macroeconomic conditions using Australian tax and survey data over a 15-year period. Policies implemented during the 2009 Global Financial Crisis increased investment. Policies implemented during normal economic times mostly have no effect. Where present, responses are larger for unincorporated businesses, likely reflecting reduced efficacy of investment stimulus under Australia's dividend imputation system. We find no evidence that policies enacted to address COVID-19 had any effect on investment, perhaps because of the unique nature of the economic shock.

Keywords: Business tax breaks; Firm investment; Dividend Imputation; Counter-cyclical policy

**Nu Nu Win would like to gratefully acknowledge the support of the Sir Roland Wilson Foundation PhD Scholarship Program. The views are those of the authors and do not represent the Reserve Bank of Australia, Treasury or the Australian Government. All errors are our own.* *Corresponding author*: robert.breunig@anu.edu.au

Tax and Transfer Policy Institute

Crawford School of Public Policy

College of Law, Governance and Policy

+61 2 6125 9318

tax.policy@anu.edu.au

The Australian National University

Canberra ACT 0200 Australia

www.anu.edu.au

The Tax and Transfer Policy Institute (TTPI) is an independent policy institute that was established in 2013 with seed funding from the federal government. It is supported by the Crawford School of Public Policy of the Australian National University.

TTPI contributes to public policy by improving understanding, building the evidence base, and promoting the study, discussion and debate of the economic and social impacts of the tax and transfer system.

The Crawford School of Public Policy is the Australian National University's public policy school, serving and influencing Australia, Asia and the Pacific through advanced policy research, graduate and executive education, and policy impact.

1 Introduction

Business investment is crucial for economic growth and prosperity. It contributes to productivity, the key driver of living standards, by raising the amount of capital available per worker (capital deepening) and by embodying technological advances. Business investment also plays an important role in economic cycles: while it accounts for only around 11 per cent of GDP in Australia, it has long been recognized as one of the most volatile and significant components of output across the cycle (Kydland and Prescott, 1982). Given these long- and short-term considerations, policymakers around the world remain attracted to policies to promote investment both during 'normal' times and in response to adverse economic shocks.[1]

Australian policymakers are no exception. Over the past two decades they have introduced numerous investment tax breaks (ITB) to stimulate investment. These have been used as a structural tool to raise investment in normal times and as a cyclical tool to stimulate investment during downturns such as the global financial crisis (GFC) or the COVID-19 pandemic. While the various policies differed in detail, all increased the (net present) value of depreciation expenses that firms could use to lower their taxable income. Hence, such policies generate a fiscal cost in terms of forgone tax revenue. For example, the tax incentives introduced in 2019 in Australia to support investment for medium-sized businesses were estimated to cost the government $400 million over the four years following implementation (Australian Government, 2019). It is therefore important to understand the effects of these policies to determine their cost effectiveness. This understanding will also help with forecasting the impact of future policy proposals by providing a sense of macroeconomic effects.

In this paper, we examine the effectiveness in stimulating additional investment of seven distinct policies implemented in Australia between 2009 and 2021. Examining a suite of policies under varying macroeconomic conditions in one country allows us to better understand the macroeconomic context in which ITB policies can increase or fail to increase investment and to understand which design features may lead to program success or failure. We focus on identifying additional investment that would not have occurred in the absence of the policy, distinct from subsidies of existing investment plans or shifts in the timing of investment.

High-quality, unique data make this possible. We use survey data from the Australian Bureau of Statistics (ABS) Capital Expenditure (CAPEX) survey and administrative tax return data from the Australian Tax office, both accessed using the ABS Business Longitudinal Analysis Data Environment (BLADE). We use two different approaches to identifying the effects of these policies, both of which leverage the cut-offs for eligibility in firm size as captured by turnover: Difference in Differences (DD) and Regression Discontinuity Design (RDD).

We find strong evidence that the policies implemented during the GFC increased investment. However, our analysis suggests that other policies implemented during the 2010s and in response to the COVID-19 pandemic had little substantive effect on firm investment.

Our paper makes several contributions to the literature. First, it adds to our knowledge of business investment incentives. By considering seven distinct programs with different rules and eligibility criteria implemented under different macroeconomic conditions, but in a single country, it allows us to learn about heterogeneity of effects with respect to firm size, generosity, uncertainty and cyclical economic conditions.

Second, we provide evidence on the effect of dividend imputation in dampening the impact of ITBs, giving insight into the debate on the 'old' and 'new' views of corporate finance. We find that unincorporated businesses, not subject to imputation, respond far more strongly to ITBs. This is consistent with the 'old' view where the marginal source of funds is external, rather than internal. Incorporated companies responded to ITBs during the GFC, but less than unincorporated businesses. Incorporated firms may be heterogeneous in their approach to financing. Some may fund investment from retained earnings (the 'new' view) whereas others may be more dependent upon external funding. It may also be that ITBs affect investment through non-traditional channels, like easing financing constraints that may have been particularly binding during the GFC. Where funding is from non-domestic sources, which is the case for many of Australia's largest

[1] For example, the United States, Netherlands, and South Korea all used investment tax incentives during the 2009 Global Financial Crisis (GFC). More recently, the 2017 US tax reform incorporates a significant investment tax incentive in the form of full expensing.

incorporated firms, dividend imputation may be less relevant and ITB policies may decrease the cost of capital and stimulate investment.

Third, we provide the first comprehensive study of business investment incentives in Australia. For Australian policy-makers, our findings suggest that ITBs can be an effective counter cyclical tool, but their effectiveness is likely to be highly dependent on the nature of the economic downturn. For example, they seem more likely to be effective in a downturn caused by a dislocation in credit markets, than in a pandemic where the economy is partly shut down. Beyond counter-cyclical policy, smaller policies targeted at structurally raising the level of investment during normal economic times generate few apparent benefits. This does not mean that such policies will never be effective. For example, it may be the policies we studied were too small to have an appreciable effect or were implemented in periods of policy uncertainty. But the results do suggest that something would need to be different to expect a substantial investment response. Finally, we provide some initial evidence that corporate tax rate cuts for small businesses appear to have stimulated additional investment.

The rest of this paper is set out as follows. Section 2 gives an overview of the policies we investigate. Section 3 gives an overview of the literature on the effect of investment incentive policies. Section 4 provides an overview of our data. Section 5 outlines our identification strategies and Section 6 presents our results. Sensitivity analysis is included in Section 7 and Section 8 concludes.

2 Overview of investment incentives in Australia

The 'modern' system of turnover-based investment incentives started in 2007, with the unification of small business tax concessions under a common turnover test of $2 million. Amongst other benefits, these changes enabled businesses that satisfied the turnover test to access instant deduction or 'write off' for depreciating assets costing less than $1,000 and allowed the cost of most other assets to be pooled and depreciated at an accelerated rate. Since then, seven important changes have been made to business investment incentives (See Table 1). Three of these - the GFC tax credit and two COVID-related policies - were designed as counter-cyclical stimulus in response to adverse economic shocks. The other four policies expanded eligibility for ITBs in terms of asset value and firm size, in order to 'structurally' raise the level of investment to support productivity.[2] In all cases the ITBs applied only to machinery & equipment, while buildings were excluded. We use this exclusion both as a placebo test, and to differentiate ITBs from the effects of corporate tax rate cuts that coincided with some ITB policies.

[Table 1 here]

The first three policies relate to small businesses with a turnover of under $2 million.

The GFC investment tax credit (GFC 2009) offered a deduction for new business investment in addition to normal depreciation deductions. When the policy was first announced in December 2008 it afforded all businesses an extra deduction of 10 per cent (Swan, 2008). This was increased to 30 per cent in the March quarter of 2009 (Swan, 2009a). In May 2009 the policy was further changed, to allow for a differentiated benefit by firm size (Swan, 2009b). Small businesses were able to deduct a bonus 50 per cent for investment made from 13 December 2008 to 31 December 2009 for investments costing at least $1,000 (applied retrospectively to investments made before the adjustment to the policy). Other businesses were allowed to deduct a bonus 30 per cent on investments over $10,000 for investments committed to before 30 June 2009 and a 10 per cent deduction for those committed to during the second half of 2009. We focus on the second half of 2009 for our analysis, where the subsidy rates differed substantially by firm size.

The small business incentive 2012 (SBE 2012) gave businesses with a turnover up to $2 million an increase in the instant asset write off threshold from $1,000 to $6,500 from 1 July 2012. Legislation enacting the increase from $1,000 to $5,000 was contingent on the passage of the Mineral Resource Rent Tax legislation[3] and the

[2] See Appendix A for more details of these policies.

[3] https://www.ato.gov.au/businesses-and-organisations/corporate-tax-measures-and-assurance/large-business/in-detail/tax-transparency/corporate-tax-transparency-report-for-the-2014-15-income-year/minerals-resource-rent-tax

further increase to $6,500 was contingent on the passage of legislation related to emissions reductions. This increase in the asset threshold was subsequently unwound from 1 January 2014 and reset to $1000 until 2015 when the next set of incentives were introduced.

The small business incentive 2015 (SBE 2015) increased the asset threshold from $1,000 to $20,000 from 12 May 2015 to 30 June 2018. However, small businesses also received a 1.5 percentage point tax cut from July 1 2015. Firm behavior during this period will be affected by both the instant asset write off and the corporate tax rate cut. We focus on an estimation window up to December 2015 to avoid possible confounding with the Medium 2016 policy discussed below. Table 2 shows the evolution of the corporate tax rate that applied to small businesses during our analysis period. This period includes both changes to rates and changes to the definition of small business.

[Table 2 here]

The next two policies extended instant write off benefits to medium-sized businesses.

The expanded business incentive 2016 (Medium 2016) extended the instant asset write off to businesses with a turnover of up to $10 million for expenditure up to $20,000 from mid-2016. The same package also gave these businesses a tax rate cut from $30% to $27.5% (Table 2). Since these reforms were introduced at the same time, the policy period covers both policies.

The medium sized businesses 2019 (Medium 2019) policy followed Medium 2016 and the instant asset write off was extended to businesses with a turnover of less than $50 million, allowing them to fully expense capital investments made between 2 April 2019 and 30 June 2020 for assets that cost less than $30,000. However, this policy effectively ended on 11 March 2020 as the Australian government introduced a new investment incentive from 12 March 2020, in response to the COVID pandemic.

The large businesses incentive (COVID 2020) was available from 12 March 2020 to 6 October 2020. Under this policy, larger businesses had access to a range of new incentive measures. Firstly, businesses with a turnover of up to $500 million had access instant asset write off for capital assets costing less than $150,000. Coverage included past purchases - that is, for assets purchased after 2 April 2019 and before 31 December 2020. Second, an accelerated depreciation rate of 50 percent was available to businesses in the year the asset was purchased for assets costing more than $150,000 purchased in the 2019-20 and 2020-21 income years.

Full expensing measures for very large businesses (COVID 2021) came hot on the heels of this program and were available from 6 October 2020. This program allowed businesses with a turnover of up to $5 billion to immediately deduct the full cost of investment with no dollar limit.

We can make some general observations in relation to the generosity of these policies. The GFC investment tax credit was the most generous of the policies because asset values were uncapped, and businesses were able to 'double dip' - to access an extra deduction, together with normal depreciation allowances in future years. In contrast, all other tax incentives only allowed a bring-forward of depreciation deductions from future years, basically providing only a timing benefit. The COVID 2020 and 2021 policies were the most generous bring-forward policies with much higher or uncapped asset limits and cover for businesses with much higher turnovers than previous policies. The 2015 and 2016 policies were not only less generous, but were rendered less valuable by corporate tax rate cuts implemented at the same time. (Lower tax rates imply that each dollar of deduction is worth less in foregone tax.)

One caveat is the difficultly of measuring the impact of these policies on firms' cost of capital. This is because we do not observe the proportion of each firm's assets that fall below the asset cost cap, nor do we observe the tax life of these assets (with longer-lived assets benefiting more from instant write off). For example, firms that primarily use inexpensive assets (costing less than the cost cap) with long tax lives would benefit more than firms that use expensive assets (costing more than the cost cap) or firms that use assets with short tax lives.

3 Literature

Jorgenson and Hall (1967) first analyzed the impact of tax incentives on business investment and the

intervening (nearly) sixty years have seen many economies implement large-scale business tax reforms with an aim of stimulating investment, both in normal times and as a counter-cyclical tool. Questions around the efficacy and cost effectiveness of such measures remain a live part of the policy debate.

Investment tax incentives are generally thought to affect investment through two channels. The 'traditional' user cost channel, and the 'non-traditional' financing frictions channel.

The user cost channel postulates that firms will invest as long as the marginal benefit of investment is higher than the cost. Increasing tax deductions for investment expenditure reduces the cost (often referred to as the user cost of capital (UC)) by increasing the present value of depreciation allowances that firms can use to lower their tax. The UC, as expressed by Jorgenson and Hall (1967), is:

$$UC = P * M * \frac{1 - \tau Z}{1 - \tau} \tag{1}$$

where P is the real price of investment goods, M is the weighted average cost of funds (debt and equity), and τ is the business tax rate. Z is the ratio of the present value of future depreciation allowances, relative to the initial purchase price of the asset. These allowances permit businesses to deduct the cost of their capital investment from their taxable income over several years, with the period approximately reflecting the economic life of the asset. By increasing or bringing forward the timing of these allowances, investment incentives can increase Z and so lower the UC.

A permanent policy change that lowers the UC leads to a burst of investment as firms adjust to their new higher desired capital stock.[4] Investment may remain higher going forward as well, as firms invest to maintain the higher capital stock. Temporary policies may have a larger and more immediate effect as firms bring forward investment to the current period when it is cheaper (Abel, 1982). However, empirically this bring-forward effect may be hard to detect, particularly in the case of long-lived assets as the future 'hole' in investment could be spread over many years (House and Shapiro, 2008).

Imputation systems such as Australia's change these costs and benefits. In Australia, company tax operates as a withholding tax. When dividends are issued to individuals, those individuals receive a credit for the company tax paid. Individuals are taxed annually on all of their income and can claim the credits for tax already paid at the company level. This reduces the tax liability for dividends for those on a personal income tax rate higher than the corporate rate and results in a refund of corporate tax paid for those individuals whose personal, marginal rate is less than the corporate rate. In particular, as they provide the end owner of the company with a tax offset for the tax paid by the company, they can 'wash out' the benefits of the depreciation allowances. Dividend imputation has been removed in many countries, including the U.K. and New Zealand, in line with lowering corporate tax rates. They remain an important feature of the Australian system.

Under the 'old view' of corporate finance, where businesses fund new investment through raising equity, the UC can be augmented for imputation as (Officer, 1994):

$$UC = P * M * \frac{1 - \tau(1 - \gamma)Z}{1 - \tau(1 - \gamma)} \tag{2}$$

Imputation is captured by the term $\gamma \epsilon [0, 1]$, which measures the value of a dollar of tax paid at the company level to shareholders. This value reflects the fact that, under dividend imputation, tax paid at the company level is a credit for personal income tax for resident shareholders. If all shareholders are domestic, as may be the case for small firms, and in the presence of full refundability of imputation credits, the value of γ is one – tax paid at the company level lowers shareholders' taxes dollar-for-dollar. In this case increases in Z do not affect the cost of capital nor investment. If some shareholders are non-residents, as may be the case for larger firms, γ may be below 1.

[4] Given tax incentives work through this UC channel, these policies have often been used to assess the effect of changes in the UC on investment more generally. They are seen as providing more plausibly exogenous variation than changes in financing costs (Cummins, Hassett and Hubbard, 1994) and are less prone to measurement error.

In contrast, the 'new view' posits that the marginal source of funds for firms is retained earnings. In this case the imputation system has little influence on the UC.

These two views of corporate finance lead to two very different conclusions regarding whether firms that are subject to imputation will respond differently to an ITB than those which are not. Under the old view, firms not subject to imputation (i.e. unincorporated businesses) should respond more strongly. Under the new view, both firm types should respond similarly.[5] As such, firm responses to ITBs can be used to assess which view of corporate finance is most consistent with the empirical evidence.

Numerous studies have examined similar ITBs outside of Australia. Empirical studies in the US (House and Shapiro, 2008; Zwick and Mahon, 2017) and UK (Maffini, Xing and Devereux, 2019) found strong investment responses to tax incentives (and associated decreases in the user cost of capital).

House and Shapiro (2008) test if businesses increased their expenditure on longer lived assets, and for any change in aggregate investment behavior in response to a change in policy. To examine asset lives, they use data from the Bureau of Economic Analysis to construct a quarterly panel of investment quantities and prices by asset type. They match 36 asset types to the IRS depreciation schedule to track eligible and ineligible investment as well as short/long asset lives between 1959 and 2005. A structural model is then estimated to provide a baseline for investment activity, GDP and employment without the bonus investment. House and Shapiro (2008) then compare forecast errors across asset types. A strong response to the tax break was found in terms of the investment composition, with an investment supply elasticity of between 10 and 20 per cent. However, only modest increases in aggregate output and employment were found, as the policy narrowly benefited a small subset of investment goods.

Zwick and Mahon (2017) study the same bonus depreciation policy as House and Shapiro (2008) but use detailed IRS corporate tax return data that include information on investment in equipment and structures. In 2008, the sample represented about 1.8 per cent of the total population of 6.4 million C and S corporation returns. In each non-bonus year, the authors compute the share of eligible investment a firm reports in each asset-life and property category. They then compute the present discounted value of one dollar of deductions for eligible investment, and combine this with the eligible investment shares to construct an industry-average asset life. As the policy raises net present value of longer-lived investment by more, they use this variation in asset lives in a difference in difference regression to identify the policy effect. They find evidence that the policy affected investment. They also find evidence that the policy had a larger effect on the investment of firms that appeared to be financially constrained, indicating that the policy worked at least partly by loosening constraints.

Maffini et al. (2019) study UK corporate tax returns to analyze the impact of a 2004 change in policy that allowed more firms to access small business depreciation concessions, which in turn allowed for a 40 per cent depreciation rate compared to the 25 per cent available for larger companies. Similar to what we do, they exploit this exogenous change in the qualifying thresholds and compare the response for companies newly qualified for the greater depreciation deductions to those that remain ineligible. Consistent with the US studies, Maffini et al. (2019) find a substantial positive effect of more generous depreciation allowances on firms' investment. Relative to the control group, the depreciation allowances raised the investment rate in eligible assets of newly-qualified companies by between 2.1 and 2.5 percentage points within three years of the change.

Zhang and He (2018) and Liu and Mao (2019) analyze the effect of a permanent 17 per cent tax credit for fixed investment for six industries in Northwest China between 2004 and 2009. Using geographical restrictions to identify the policy effect, Liu and Mao (2019) found that the tax credit raised fixed investment of eligible firms by 28 per cent on average during 2004–2007, relative to 2001–2003, corresponding to a user cost elasticity of 1.84. The strong response by firms found by Liu and Mao (2019) is consistent with Zhang and He (2018), who found that, on average, the reform raised investment of the treated firms relative to the control firms by 38.4 per cent.

Australian evidence is sparse, partly due to historic data availability issues. Rodgers and Hambur (2018) analyze the effect of tax breaks for machinery and equipment during the GFC (the first of the seven reforms

[5] For a more detailed discussion see Sobeck, Breunig and Evans (2022).

we consider in this paper). They use RDD and DD methods around the turnover threshold to compare the investment of small and large businesses using business tax microdata and unpublished survey data from the Australian Bureau of Statistics.

Consistent with the difference in corporate taxation regimes (i.e., classical taxation in the US and dividend imputation in Australia) and the 'old view' of corporate finance, Rodgers and Hambur (2018) find a lower responsiveness of investment to the ITBs compared to Zwick and Mahon (2017), as measured by the elasticity of investment. However, they find a much larger elasticity, more in line with the US studies, when focusing on unincorporated businesses who face a classical taxation regime similar to US corporations. They find some evidence that incorporated companies also responded. Given the policy was targeted at smaller companies who have very few non-resident shareholders, they argue that under Australia's dividend imputation system, γ equals one for these companies. They conclude that their findings suggest the policies worked partly by relaxing financing constraints.[6] Our results presented below are consistent with this previous research on Australia, which only examined one of the seven policies that we examine.

4 Data

We merge survey data from the Australian Bureau of Statistics (ABS) with business taxation administrative data from the Australian Taxation Office (ATO) to construct our sample. This provides us with two different sources of investment data. The first is the Private New Capital Expenditure and Expected Expenditure (CAPEX) survey, covering around 10,000 firms each quarter. Consistently large investors as determined by the ABS (large mining companies, banks and a few other large firms) are surveyed every quarter, while the remainder are sampled with stratification by industry, location and employment size, with firm details drawn from the administrative business register. CAPEX data have the advantage that they provide investment by asset type: machinery and equipment (assets mostly eligible for the investment incentives we consider) and buildings, structures and mining (assets ineligible for the investment incentives we consider). However, the sample is too small for our RDD. Once combined with other taxation data (see below) we have a panel with around 90,000 businesses and around 667,000 observations spanning the period 2000-01 to 2018-19. See Appendix B for summary statistics of the data we use to analyze each policy.

Our second source of investment data is capital expenditure reported in firms' Business Activity Statements (BAS). These are available quarterly covering the period 2001-2021. As these data are reported by almost all firms, the sample is very large[7]. However, eligible and ineligible investment are not separately reported in these data.

We consider both the intensive and extensive margins. The intensive margin is the (natural) log of investment. For the extensive margin, our dependent variable is the log odds ratio for the share of firms investing, calculated at the industry division-turnover category level, as in Zwick and Mahon (2017).[8] The extensive margin is particularly relevant for the analysis of small business policies, as only around 25 per cent of small firms (<20 staff) invest each quarter based on tax data, compared to around 80 per cent of large firms (200+ staff). In our RDD design we focus only on the intensive margin.

These investment data are combined with other annual tax data covering a rich set of information on business type, turnover, assets, liabilities and ownership structure. The tax data also include details of which businesses connect to others through a business group identifier. This aggregation is useful since the legislated turnover tests are on an 'aggregated turnover' basis, which includes the annual turnover of not just the primary business but also any other controlled or affiliated businesses in related business groups. Lastly, the tax data allow us to differentiate between different legal structures: companies, partnerships,

[6]The broader literature on the value of γ is mixed. Many large Australian companies raise equity from international investors, who derive no benefit from dividend imputation. As such, γ is unlikely to be 1 for these businesses. Applied evidence based on samples of listed companies finds a range of values for γ, see Swan (2019); Sobeck et al. (2022). At the same time, studies overseas have found evidence in support of the 'new view' (i.e. γ is 0), with Yagan (2015) finding evidence that cuts to dividend taxes in the US in 2003 had little effect on investment.

[7]From 2017/18 small firms below $10 million in turnover no longer needed to report on their capital expenditure in the BAS. However, all policies from this point on were aimed at larger firms, so this does not affect our analysis.

[8]More formally, it is defined as $\ln \frac{P_{n,s,t}(Investment_{i,t}>0)}{1-P_{n,s,t}(Investment_{i,t}>0)}$, where n indicates industry, s size and t time

trusts and individual entities. These distinctions are important in the Australian context given companies are subject to a dividend imputation regime, which may limit the value of ITBs to the firm.

We exclude the mining sector to eliminate any potential effects from the mining boom. We also exclude the public sector (based on ABS Standard Economic Sector Classifications of Australia). We exclude the finance sector from the RDD estimates, due to conceptual difficulties in using tax data to measure investment for this sector.

5 Identification Strategies

To identify any effect of the ITBs we use both a difference-in-differences approach and a regression discontinuity approach. As explained below, each identify different impacts and rather than focus on one, we use both to see if there is any effect of the ITB policies. Our assessment of the impact of the policies will be based on an examination of the combined evidence across these approaches.

5.1 Difference in differences

Our first approach is a DD model. We exploit the timing of the policies and the exogenous qualifying turnover thresholds to identify the impact of the policy by comparing qualifying and other firms during and outside of the policy period. The treatment and control groups are summarized in Table 3. The financial year in Australia runs from July to June.

[Table 3 here]

Our regression takes the following form.

$$Inv_{ijt} = \alpha + \beta_1 d_t^P + \beta_2 d_i^T + \beta_3 (d_t^P \times d_i^T) + \theta_i + \gamma X_{ijt} + D_t + IND_j \times D_t + \epsilon_{ijt} \quad (3)$$

where $Inv_{i,t}$ is our measure of investment (intensive or extensive margin) for machinery and equipment for business i in industry j in period t, d_t^P is a dummy that equals one in the periods of operation of the relevant policy, and dummy d_i^T is equal to one for businesses that meet the relevant turnover test. We also include a number of controls in the model to help with identification and with precision. First, we include firm fixed effects θ_i to capture time-invariant firm-specific factors influencing investment. We also include additional time-varying, firm-level controls to capture other factors that could be influencing investment, captured in $X_{i,t}$. Specifically these are full-time equivalent employment and deciles of the distribution of firm turnover. We also include a vector of quarterly dummies, D_t, to account for seasonal patterns in investment. And we include industry*time fixed effects ($IND_j \times D_t$) to capture heterogeneous industry level trends and conditions, which might be particularly important during the GFC and COVID as different industries were differentially exposed to these shocks. Our identification strategy compares small and large businesses within the same industry.[9] Standard errors are clustered at the firm level for intensive margins and industry levels for extensive margins.[10]

To define a firm's eligibility, we turn to the tax code. The code states that a firm is small if their revenue in a period is below a certain threshold. Several different periods can be used for this assessment under the tax code eligibility rules:

1. Revenue in year *t*;
2. Revenue in year *t-1*; or
3. Revenue in year *t-2* if the firm had a reasonable expectation when it undertook the investment that revenue in year *t* would be below the threshold.

[9] Industry is measured at the division level and takes 19 possible values. The inclusion of these industry dummies will also control for differing asset lives across industries, which determine how beneficial the various policies will be for different industries.

[10] We focus on the average effect of policies in the treatment period (which covers two through five quarters, depending upon the policy). If we instead estimate individual effects for each quarter, our conclusions are similar.

This final category, which combines revenue in year *t-2* with expectations about revenue in year *t*, was designed for firms that may have had a windfall gain in year *t-1* and for whom revenue in year *t-2* is more "representative" of their normal revenue. Given firms may not know time *t* sales during the year when investment decisions are made, they are allowed to rely on a reasonable expectation. We don't use firms that rely on this rule for the DD regressions, but they are used for the RDD identification in combination with information about actual turnover in time t and $t-1$ as discussed below.

For the purposes of the DD regressions, we use revenue in year *t-1* in assessing whether the firm qualifies for the policy, as in Rodgers and Hambur (2018). As discussed below, for our RDD regressions we take a different approach, exploiting the third category noted above to get very tight identification.

As noted above, one concern with identifying the effects of these ITBs is that some coincided with changes in the corporate tax rate for firms based on the same threshold. Table 2 shows the changes in the tax rate over time. Of particular concern, the changes in 2015/16 and 2016/17 coincide perfectly with the 2015 and 2016 ITBs. The reduction in the corporate tax rate would have simultaneously lowered the value of ITBs for companies, and potentially stimulated additional investment. As such, this may affect our estimated effects for these policies. To assess the degree to which this might be affecting our results, we provide separate estimates for assets which were or were not eligible for the ITB (see Section 7.1 below).

5.2 Regression discontinuity design

Another way to exploit the exogenous eligibility threshold is an RDD approach. Similar to DD, this involves looking at differences in outcomes for eligible and ineligible firms. But instead of comparing outcomes more broadly between these groups, it focuses on an area (η) around the policy threshold cut-off, comparing outcomes for firms just above and just below the threshold to determine any discontinuity at the threshold.

The advantage of this approach is that the identification is sharper and requires relatively weaker assumptions–namely that in the absence of treatment there would be no discontinuity at the threshold.[11] However, the approach requires more data and the identified effect, while sharp, is very local and can be hard to extrapolate to an aggregate effect. Given the data requirements, we use the BAS data described above for the RDD estimates.

For identifying the relevant cut-off we adopt the approach of Rodgers and Hambur (2018). This approach focuses on firms that would qualify based on whether their turnover was above or below the threshold in year *t-2*. This helps to minimise any possibility that firms could manipulate their way into treatment.

To isolate these firms, we remove firms that could qualify for the incentives based on their incomes in tax year *t* or *t-1* (i.e. businesses with turnover under the turnover limit in those years). Amongst the remaining firms, the only firms that could use their income in *t-2* to meet eligibility are those that had a reasonable expectation that their income in *t* would be below threshold. The tax rules note that the best way to assess this is revenue in *t-1* (Australian Taxation Office, 2017). That is, only firms with revenue a little bit above the threshold in *t-1*, could say they reasonably expect to be below the threshold in *t*.

To meet this final requirement, we remove firms with income in *t-1* above a certain threshold. We use a ceiling of 1.25 times the threshold. The selection rules we use to choose our estimation sample are summarized in Table 4.

[Table 4 here]

In summary, we focus on the subset of firms with turnover above the threshold in year *t*, and between the threshold and 1.25 times the threshold in year *t-1*. This group would qualify for the ITB only if their turnover was below the threshold in *t-2*. So we have a clean eligibility cut-off at the threshold to separate qualifying and non-qualifying firms based upon their revenue at time *t-2*.

The RDD can be thought of as estimating a local polynomial for the conditional mean, estimated precisely on either side of the threshold. We estimate this function using local linear, non-parametric regression with a

[11] For discussions of the theory and practice of the RDD approach, and how it approximates random assignment, see Imbens and Lemieux (2008) and Lee and Lemieux (2010).

triangular kernel. We implement the automatic bandwidth selection procedure proposed by Calonico et al. (2014) with a triangular kernel. We also also use their bias adjusted p-values.[12] The results are robust to selecting different bandwidths.[13] For all RDD regressions we cluster standard errors at the firm level.

6 Results

Below we summarize our empirical results. We split the results into groups of policies: macro-stabilisation policies; structural small business policies; and structural medium business policies.

6.1 Macrostabilisation policies

First, focusing on the graphical evidence for the DD approach (see Figure 1) we see clear evidence of an effect for the GFC policies: investment for both groups followed a similar pattern, before diverging in the second half of 2009 when the differential tax incentive was in place (dark grey). This was particularly evident at the intensive margin.

[Figure 1 here]

In contrast, focusing on the COVID-era policies (see Figure 2), we see that over this period all firm size groupings follow a broadly similar investment path. Given the overlapping nature of these policies we visualise them differently to the other DD results. We show investment for each group, where the shaded colour corresponds to the largest firm now eligible for a tax concession. If the policies were affecting investment, we would expect to see each line pick up, relative to the line for the next size group, when we enter the relevant shaded period. However, there is no evidence for an increase in investment as each group becomes eligible for the incentive, suggesting little evidence of an effect from the policy. All lines follow fairly similar paths.

[Figure 2 here]

These findings are borne out in the regression analysis (Table 5). The GFC tax incentive seems to have successfully raised investment on the intensive margin, with treated firms increasing spending substantially on eligible investment. The effects are larger for unincorporated businesses, consistent with previous findings and the fact that the imputation system should wash out much (if not all) of the benefit for domestically owned companies. This would also be supportive of the 'old view' of corporate finance. Nevertheless, the fact that there is some response among companies suggests that either the ITB is working through other channels, like easing cashflow constraints, or that there is heterogeneity in the source of funding for incorporated firms. Overseas investors do not benefit from imputation and the ITB would lower the cost of capital for firms with overseas ownership or investment.

We see a statistically significant effect on the extensive margin for unincorporated firms suggesting that the policy increased the number of unincorporated firms who made investments. We see a positive effect for incorporated firms as well but the t-value of 1.3 falls below standard significance levels. For all of the DD results we show robust standard errors clustered at the firm (intensive margin) or industry (extensive margin) level; for the RDD results we show robust p-values.

[Table 5 here]

In contrast, we do not find statistically significant results for the COVID policies.[14] There is even some evidence suggesting a negative effect at the intensive margin for the 2020 policy, though the confidence interval is wide.

Turning to the RDD approach, graphically we see a clear shift down in investment above the GFC policy threshold (Figure 3). This is evident whether we focus on average investment within small sales buckets (blue dots), or a line of best fit using a linear (green dots) or local polynomial model (orange line). This is

[12] We implement this using the `rdrobust` package in STATA.

[13] These results are available from the authors on request.

[14] Due to small sample sizes at higher turnover ranges, we do not include results by firm type for the COVID policies.

also evident in the regressions, with a large fall in investment above the threshold, suggesting the ITB raised investment for eligible firms (Table 6).[15]

[Figure 3 here]

[Table 6 here]

Interestingly, based on the RDD, most of the effect occurred in the first quarter of the policy (i.e. September quarter 2009), with very little evidence of an effect in the second. In some sense this is surprising, since the policy imposes a long period between the outlay and receipt of the tax benefit. The latter only happened after the company lodged its return at the end of the tax year (post June 2010). The finding could reflect an announcement effect. However, it could also reflect uncertainty around further changes to the policy, given the policy was changed several times over the preceding year in various government announcements.

The lack of an offsetting effect in the quarters following the end of the policy suggests either the policy led to an increase in investment, rather than just a bring-forward of future investment, or that the bring forward effect was quite drawn out and not readily observable. These results are consistent with Rodgers and Hambur (2018).

While our findings are qualitatively similar to Rodgers and Hambur (2018), the magnitudes differ. This reflects two factors. For the DD we use an indicator variable to identify the 2009 GFC tax credit in contrast to their measure of the intensity of treatment. This modeling choice is for consistency within our paper across the different policies we consider. For RDD we are using quarterly investment data from the BAS, rather than annual income tax data. This allows us to narrow our focus to the affected quarters.

Unfortunately, we cannot assess the COVID polices using RDD because the sample of firms close to the thresholds for the COVID-19 policies is too small. This is due to the very large turnover thresholds–the density of firms becomes very small.

The difference in results across the GFC and COVID policies may reflect the different nature of the economic shocks. The GFC was a financial shock that may have impacted credit supply but did not severely slow real activity in Australia (unemployment did not rise above 6%). This could have meant that generous measures which increased access to funds through the tax system were more likely to be successful. In contrast, the COVID shock resulted in lockdowns across Australia and disruptions in machinery and equipment production globally. These two factors together may have hampered the ability and incentives for business to source and install new capital purchases to take advantage of the investment tax breaks.

6.2 Structural small business policies

We can also compare the 2009 policy results with other small business policies to interrogate the differences in effects across the policies. For both the 2012 and 2015 policies, very little graphical evidence suggests a policy effect in the DD set-up: investment for the treated and control groups follow similar paths pre-treatment, with no change post treatment (see Figures 4 and 5).

[Figures 4 and 5 here]

Consistent with this, the DD regression analysis suggests the 2012 policy did not significantly raise investment (Table 7). The 2015 policy appears to have had an effect on investment on the intensive margin for unincorporated businesses. However, the impact is weaker than that of the GFC policy. Furthermore, the 2015 policy may be confounded by corporate tax rate cuts for small businesses implemented at the same time. As discussed in section 7.1, we find some evidence that these tax cuts increased investment for incorporated companies. This could have led to some spillovers to unincorporated businesses, so the evidence of any

[15] In the main text, we present graphs of the RDD results for the impact in the first quarter of the policy. In Appendix C, Figures C.1 through C.3, we show RDD estimates of the policy impact in the second quarter for which the policy was in place. Some evidence suggests a small effect for the SBE 2015 policy (Figure C.3) but this policy is confounded by the small business tax cuts we discuss elsewhere in the paper.

effect of the 2015 policy should be treated with caution.[16,17] The estimates for an impact on the extensive margin are positive but not statistically significant at conventional levels.

[Table 7 here]

The RDD results are similar to the DD results. No evidence suggests the 2012 policy affected investment, even when focusing only on unincorporated businesses (Figure 6 and Table 8). The one significant coefficient (a negative effect of the policy for companies in the June 2013 quarter) may simply be noise. When estimating so many regressions there will be some apparently significant results even when the true impact is zero.

The 2015 policy is associated with some increase in investment, though only for unincorporated businesses (Figure 7 and Table 9) and with some time lag. The apparent negative impact of the policy in the June 2015 quarter, before the policy began, may be a sign that firms held off investment in anticipation of the policy. If this were the case, however, we would expect to see a strong impact of the policy in the first two quarters where it was in place (September and December 2015). However, there is no measurable impact of the policy in the first 6 months. Any impact appears to occur in March and June 2016 quarters, the period six to twelve months after the policy was introduced. Again, the possibility of confounding effects of corporate tax rate cuts and the announcement of the subsequent medium 2016 policy should be kept in mind.

[Tables 8 and 9 and Figures 6 and 7 here]

These lack of responses, particularly for the 2012 policy, may seem surprising in the context of the GFC results, given their identical target groups. However, the 2012 changes happened in a context of heightened political uncertainty, with legislation enacting the increase in the asset cost cap from $1,000 to $5,000 contingent on the passage of the Mineral Resource Rent Tax legislation and the further increase in the cap to $6,500 contingent on the passage of legislation related to emissions reductions. Uncertainty around these other policies, and that the Opposition committed to unwinding these policies if it won government before the expiration of the tax break, may have led businesses to hold off on investment.

Some evidence suggests the 2015 policy had a small effect for unincorporated businesses. However, the evidence is weaker and macroeconomically less significant, given that we find smaller aggregate effects than for the GFC policy. This could reflect a number of factors: heightened political uncertainty with the Australian Senate blocking government legislation and culminating in the first double dissolution election since 1987; the smaller nature of the policy; the introduction of the policy at the same time as corporate tax cuts that lowered the value of the investment tax break; and looser financial conditions, which may have meant less scope for the policy to influence companies.

6.3 Structural medium business policies

Focusing on the 2016 and 2019 medium firm policies, which cover firms at the $10 million and $50 million thresholds, respectively, again little graphical evidence suggests an effect in the DD approach (See Figures 8 and 9). The pre-trends for the extensive margins are quite volatile, suggesting these results should be interpreted with caution.[18]

[Figures 8 and 9 here]

Turning to the regression results, some evidence suggests an effect at the extensive margin in the DD approach for all firms, though as noted, we are cautious in interpreting this result (Table 10). Moreover, as discussed below, these results might reflect the effect of coincident corporate tax cuts (section 7.1 below). Meanwhile the 2019 policy had no discernible effect on the intensive margin. The large negative effect on

[16]Some unincorporated businesses, such as trusts, are pass-through companies for corporate entities, thus there may be an affect on investment for unincorporated businesses. We attempt to account for this using ABS information on business links. However, we cannot rule out the possibility that the corporate tax cuts could also affect investment of unincorporated businesses.

[17]We end our DD regression in December 2015 because the tax cuts announced in May 2016 for firms with turnover up to $10 million may have affected the behavior of our control group. Results are broadly similar if we include the March and June quarters of 2016 in the regression.

[18]As noted above, the share of firms investing is much higher once we move to medium and larger firms. As such, the extensive margin becomes a less relevant metric.

the extensive margin for unincorporated firms is imprecisely estimated and, as mentioned above, subject to substantial year-on-year volatility even in the absence of any policy changes.

[Table 10 here]

We also show the RDD results for the 2016 policy in Table 11, with no evidence of an effect. However, firms are sparser around the $10 million cut-off, as reflected in the larger optimal bandwidth and smaller observation count. There may be too few firms to precisely estimate an RDD model for this policy.

[Table 11 here]

The large negative coefficient in the June 2016 quarter, before the policy came into effect, could again reflect firms holding off on investment in anticipation of the policy. Although not statistically significant, the negative coefficients in the four following quarters suggest some investment increase for treated firms below the threshold. The sample sizes for these regressions are quite small. Again, there may be some weak evidence here for a response of unincorporated firms but there is no evidence of any impact for companies.

7 Robustness

In this section we discuss a number of threats to the identification of our models and conduct several robustness checks to assess the sensitivity of our findings and to aid in interpretation. These include the potential for firms to adjust their behavior to utilise the policy, and other policies that may have affected behavior.

7.1 The impact of corporate tax cuts

The 2015 and 2016 results may be confounded by corporate tax cuts implemented for the policy groups in 2015 and 2016. These were introduced coincident with the ITB, and had the same exact size cut-off for eligibility (see Tables 1 and 2).

In order to consider whether these policies had an effect, we exploit the fact that the ITB only applied to machinery and equipment, while the tax cuts lowered the cost of capital for any investment. We re-estimate the DD models, but for buildings and structures investment, as reported in the CAPEX data. While there is no response of this ineligible investment to the GFC policy, we do see a significant increase for the 2015 policy at the intensive margin (Appendix D). This suggests the corporate tax cuts themselves may have increased investment. However, no evidence suggests an effect following the 2016 tax rate changes. We need to exercise caution in interpreting the RDD results for the SBE 2015 and Medium 2016 policies as the simultaneous nature of the tax cuts means we are picking up a combined effect of both policies.

7.2 Other policies

We know the corporate tax rate changes potentially affect our estimates of the 2015 and 2016 ITB policies but perhaps other policies also create differential incentives to invest at the threshold.

One possibility is that such policies pre-existed the ITB. This is unlikely to affect the DD results, as it will be captured by the pre-treatment difference in outcomes. But it could affect the RDD results. To consider this possibility we estimate the RDD for some quarters prior to the introduction of the policy. These are included in the tables which present the RDD results. No estimated difference in investment emerges at the threshold before the policies were in place, suggesting that pre-existing policies do not explain the results.

Another way to account for potentially confounding policies or factors that affect firms at the threshold is to look at other firm-level metrics that could be affected by those policies, but which should be less affected by the investment tax incentives. Table 12 examines firms' revenue and wages, focusing on quarters with evidence of a significant investment response. We find no evidence of a discontinuity in these other observables, providing further evidence that the effects we are finding reflect the investment policies.

[Table 12 here]

7.3 Validity tests - selection into treatment

One of the requirements for valid RDD is that businesses cannot manipulate the forcing variable, revenue, to change their treatment status. If businesses can select into treatment, the assumption that businesses just above and below the cut-off are identical would be violated. Businesses with a higher ex ante propensity to invest would have a greater incentive to select into treatment.

In our case, manipulation of the forcing variable – revenue measured over a year before the announcement of the differential tax break – is improbable. Consistent with this, no graphical evidence emerges of bunching of businesses just below the threshold (Figure 10). These graphical results are confirmed by the formal test proposed by Cattaneo et al. (2020).

[Figure 10 here]

The policy rules create an incentive for firms to manipulate their way into eligibility for the policy in a way that would remove them from our treatment and control groups. In particular, businesses with revenue above the cut-off in *t-2* and who forecast revenue just above the threshold in *t-1* or *t*, might have the incentive to manipulate their revenue in year $t-1$ or t to qualify for the tax breaks. Recall that we dropped these businesses from our estimation sample–see section 5.2.

This could create a situation where our control group is comprised of firms with a lower propensity to invest than the true population of firms just above the threshold. The firms for whom manipulation into the policy is most valuable (those with the highest propensity to invest) are most likely to be absent from the control group.

We test the possibility of selection effects on the control group in two ways. One is to replace current investment in the regression with 2-year lagged investment. This test of a pre-determined co-variate should provide some sense of any inherent differences between the treated and non-treated groups.

Table 13 shows these results, focusing on the periods where the investment response was statistically significant. We find no evidence of a difference in the earlier investment propensity of firms either side of the threshold.

[Table 13 here]

Still, we may be concerned that even if the earlier propensity to invest was the same, it may not be the case during the period of interest. As a further test, we rely on the assumption that businesses are only likely to be able to manipulate their revenue to a certain extent. Businesses with revenue just above the threshold may be able to manipulate their revenue down, but businesses with revenue well above the turnover limit could not.

As shown in Rodgers and Hambur (2018) Figure 6, some evidence emerges that firms manipulated their sales in year *t-1* to qualify for the GFC policy. Specifically, a small spike appears in the number of firms with turnover just below the $2m in the 2008-2009 financial year, followed by a dip just after. It appears most of this came from firms with 'potential' turnover between $2m and $2.1m.

To account for this we follow Rodgers and Hambur (2018)) and re-estimate the model, only keeping firms with revenue in *t-1* or *t* more than 5 per cent above the threshold (equivalent to $100,000 for the $2m threshold). The logic is that it would have been more difficult for this subset of firms to manipulate turnover to be below the threshold. This reduces the concern that the control group only contains those firms that did not have the incentive to manipulate into the policy using turnover in *t* or *t-1*.

Table 14 shows the results, again focusing on the statistically significant periods. The results are quite similar, suggesting that manipulation is not driving the findings.

[Table 14 here]

7.4 Repeated treatment

As Australia has implemented a number of different ITB policies over recent years, one concern could be interaction between the various policies and overlap of the time periods covered by the policies. For example, firms that took up the GFC policy may have had less incentive to invest using the 2012 policy, if they had already made all the investment they desired.

We do see some evidence of overlap. For example, around half of the treated group in our GFC regressions were in our treated group for the 2012 policy. However, both our results and Rodgers and Hambur (2018) provide very limited evidence of bring forward over the 1-2 years following the GFC policy. We think the likelihood that this is driving our results for the 2012 policy is very low.

8 Conclusions

Various tax incentives have been provided to businesses in Australia over the past 15 years, both as a stimulus tool and as a structural tool to raise aggregate investment. Such incentives are inherently costly, making it important to understand whether they are effective in stimulating additional investment.

We evaluate seven policies over a 12 year period using difference-in-difference and regression discontinuity designs to identify the causal impact of investment tax breaks on firm investment. Our time period covers 'normal' economic times, the GFC and the COVID pandemic period. This approach, of examining a range of policies, at different time periods in one country, allows us to draw conclusions about heterogeneous effects of investment tax breaks freed from issues of poor comparability often apparent in cross-country studies.

ITBs seem to be somewhat less effective in Australia, compared to many other countries. This appears related to the existence of dividend imputation, which lessens the value of the ITB to company shareholders and means that the policies mainly affect small unincorporated businesses. This is an important insight about the relationship between ITBs and other elements of the design of the overall tax system. Our finding is consistent with the old view of corporate finance, where firms fund themselves externally.

Overall, our results suggest the effectiveness of the policies studied is mixed at best, and probably highly dependent on the nature of the policy and macroeconomic conditions. In Australia, ITB policies as economic stimulus may be most effective during downturns which are driven by a shock to credit supply, rather than a sharp decrease in demand. ITBs appear to loosen cash-flow constraints for businesses in periods of credit dislocation. In 'normal' times they seem to have little effect. Policies targeted at smaller firms appear somewhat more effective: unincorporated businesses in particular are far more responsive. Policy uncertainty, reflected in frequent policy changes and uncertainty about the continuation of a policy, may undermine effectiveness.

Finally, while not the focus of our paper, we find evidence that corporate tax rate cuts led to an increase in investment, consistent with theory and previous empirical evidence from cross-country and other studies.

Tables

Table 1: Investment incentive policies

Policy	Eligibility criteria Investment tax benefit policy	
	TO limit Instant write off	Asset limit Extra deduction
GFC 2009	$2m ×	Uncapped, expenditure minimum ✓
SBE 2012	$2 million ✓	$6,500 ×
SBE 2015	$2 million ✓	$20,000 ×
Medium 2016	$10 million ✓	$20,000 ×
Medium 2019	$50 million ✓	$30,000 ×
COVID 2020	$500 million ✓	$150,000 ×
COVID 2021	$5 billion ✓	Uncapped ×

SBE: small business entities

Table 2: Corporate tax cuts

	Small business rate	Small business threshold
2014-15	30%	$2m
2015-16	28.5%	$2m
2016-17	27.5%	$10m
2017-18	27.5%	$25m
2018-19	27.5%	$50m
2019-20	27.5%	$50m
2020-21	26%	$50m
2021-22	25%	$50m

Table 3: Summary of DD Identification Strategies

Policy	Treated	Control	Quarters
GFC policy 2009	TO < $2m	$2m < TO < $5m	Sep 2009 - Dec 2009
SBE 2012	TO < $2m	$2m < TO < $5m	Sep 2012 - Dec 2013
SBE 2015	TO < $2m	$2m < TO < $5m	Sep 2015 - Dec 2015
Corporate tax cut	TO < $2m		Jun 2015
Medium 2016	$2m < TO < $10m	$10m < TO < $20m	Sep 2016 - Jun 2017
Corporate tax cut	TO < $10m		Sep 2016
Medium 2019	$10m < TO < $50m	$50m < TO < $60m	Jun 2019 - Mar 2020
Corporate tax cut	TO < $50m		Sep 2018
COVID 2020	$50m < TO < $500m	$500m < TO < $600m	Mar 2020 - Jun 2020
COVID 2021	$500m < TO < $5b	$5b < TO < $6b	Sep 2020 - Jun 2021

Quarters refer to the end month of the quarters during which we evaluate a policy impact.

Table 4: Summary of RD Identification Strategies

	TO in *t-2*	TO in *t-1*	TO in *t*
GFC, 2012 & 2015 Policies			
Dropped		TO < $2m	TO < $2m
Policy	$2m-$\eta$ < TO < $2m	$2m < TO < $2.5m	
Control	$2m < TO < $2m+$\eta$	$2m < TO < $2.5m	
2016 Policy			
Dropped		TO < $10m	TO < $10m
Policy	$10m-$\eta$ < TO < $10m	$10m < TO < $12.5m	
Control	$10m < TO < $10m+$\eta$	$10m < TO < $12.5m	

Table 5: Difference-in-difference results: Stimulus measures

	Intensive margin			Extensive margin		
	All entities	Companies	Unincorps	All entities	Companies	Unincorps
GFC Policy 2009	0.456***	0.414**	0.870***	0.207*	0.176	0.274**
SE	(0.136)	(0.168)	(0.238)	(0.113)	(0.132)	(0.139)
Observations	7226	4660	2520	46069	24758	21293
COVID 2020	-0.291*			0.138		
SE	(0.150)			(0.219)		
Observations	17701			25643		
COVID 2021	0.404			0.546		
SE	(0.267)			(0.449)		
Observations	5040			6147		

Regressions include controls, except for COVID 2021 intensive margin results which are estimated without controls due to small sample sizes.
Mining is excluded.
Cluster robust standard errors in parentheses
* p < 0.1, ** p < 0.05, *** p < 0.01

Table 6: RDD results: GFC policies

	Mar 2009	Jun 2009	**Sep 2009**	**Dec 2009**	Mar 2010
All firms					
Beta	-0.114	0.006	-0.404**	-0.0358	-0.022
Robust p-value	(0.49)	(0.79)	(0.02)	(0.82)	(0.88)
Bandwidth	328,163	326,907	246,555	467,689	508,608
Effective observations	2,106	2,421	1,692	2,875	2,713
Unincorporated					
Beta	0.005	-0.292	-0.570**	-0.032	0.187
Robust p-value	(0.96)	(0.31)	(0.03)	(0.96)	(0.45)
Bandwidth	315,877	366,361	275,067	385,634	466,038
Effective observations	867	1,084	813	1,089	1,081
Companies					
Beta	-0.127	0.177	-0.154	-0.036	-0.155
Robust p-value	(0.51)	(0.30)	(0.35)	(0.78)	(0.41)
Bandwidth	374,530	353,778	313,475	368,003	560,025
Effective observations	1,332	1,490	1,173	1,457	1,685

Robust *p*-values in parentheses
* p < 0.1, ** p < 0.05, *** p < 0.01

Table 7: Difference-in-difference results: Small business measures

	Intensive margin			Extensive margin		
	All entities	Companies	Unincorps	All entities	Companies	Unincorps
GFC Policy 2009	0.456***	0.414**	0.870***	0.207*	0.176	0.274**
SE	(0.136)	(0.168)	(0.238)	(0.113)	(0.132)	(0.139)
Observations	7226	4660	2520	46069	24758	21293
SBE 2012	0.0911	0.0308	0.321	0.0431	0.0757	-0.0311
SE	(0.125)	(0.165)	(0.200)	(0.087)	(0.095)	(0.110)
Observations	7253	4704	2521	49474	25920	23534
SBE 2015	0.14	0.070	0.440*	0.138	0.137	0.154
SE	(0.129)	(0.159)	(0.230)	(0.100)	(0.120)	(0.105)
Observations	3911	2631	1270	24172	13298	10857

Regressions include controls. Mining is excluded.
Cluster robust standard errors in parentheses
* p < 0.1, ** p < 0.05, *** p < 0.01

Table 8: RDD results: 2012 SBE policy

	Mar 2012	Jun 2012	**Sep 2012**	**Dec 2012**	**Mar 2013**	**Jun 2013**	**Sep 2013**	**Dec 2013**
All firms								
Beta	-0.149	-0.0386	-0.0474	0.162	0.186	0.171	-0.0419	-0.112
Robust p-value	(0.53)	(0.78)	(0.85)	(0.25)	(0.26)	(0.14)	(0.93)	(0.61)
Bandwidth	372,005	421,748	379,932	345,167	380,623	406,370	316,471	411,141
Effective observations	2,765	3,177	2,713	2,556	2,639	2,952	2,402	2,990
Unincorporated								
Beta	0.298	-0.163	-0.149	0.040	0.203	-0.118	-0.121	0.011
Robust p-value	(0.69)	(0.60)	(0.70)	(0.66)	(0.24)	(0.72)	(0.92)	(0.81)
Bandwidth	381,694	482,329	360,767	327,458	434,046	400,103	290,010	367,851
Effective observations	1,298	1,557	1,255	1,170	1,383	1,371	1,059	1,344
Companies								
Beta	-0.288	0.091	0.040	0.187	0.151	0.527**	0.148	-0.173
Robust p-value	(0.21)	(0.77)	(0.91)	(0.32)	(0.70)	(0.02)	(0.41)	(0.43)
Bandwidth	436,224	390,962	401,357	456,602	362,765	323,714	410,413	539,201
Effective observations	1,664	1,614	1,470	1,631	1,312	1,317	1,531	1,813

Robust *p*-values in parentheses
* p < 0.1, ** p < 0.05, *** p < 0.01

Table 9: RDD results: 2015 SBE policy

	Mar 2015	Jun 2015	**Sep 2015**	**Dec 2015**	**Mar 2016**	**Jun 2016**
All firms						
Beta	-0.126	0.226*	-0.012	0.055	-0.306*	-0.279*
Robust p-value	(0.47)	(0.08)	(0.90)	(0.61)	(0.07)	(0.07)
Bandwidth	402,386	344,157	376,464	407,926	326,053	368,267
Effective observations	3,234	3,161	3,129	3,412	2,915	3,333
Unincorporated						
Beta	0.011	0.484**	-0.235	-0.356	-0.518**	-0.567**
Robust p-value	(0.76)	(0.01)	(0.40)	(0.10)	(0.03)	(0.01)
Bandwidth	381,886	386,019	375,716	352,855	315,226	329,403
Effective observations	1,482	1,591	1,408	1,436	1,277	1,399
Companies						
Beta	-0.258	-0.021	0.180	0.446**	-0.136	-0.0512
Robust p-value	(0.20)	(0.92)	(0.52)	(0.03)	(0.47)	(0.76)
Bandwidth	334,682	400,725	385,850	350,061	451,190	484,585
Effective observations	1,492	1,875	1,728	1,688	1,955	2,154

Robust *p*-values in parentheses
* p < 0.1, ** p < 0.05, *** p < 0.01

Table 10: Difference-in-difference results: Medium-sized business measures

	Intensive margin			Extensive margin		
	All entities	Companies	Unincorps	All entities	Companies	Unincorps
Medium 2016	-0.0735	0.0166	-0.618**	0.295**	0.293**	0.295
SE	(0.120)	(0.139)	(0.260)	(0.122)	(0.142)	(0.178)
Observations	8663	6846	1772	18962	14062	4873
SBE 2019	0.0344	0.0837	-0.135	-0.289	-0.211	-0.784**
SE	(0.116)	(0.125)	(0.253)	(0.203)	(0.200)	(0.336)
Observations	7978	6732	1212	13646	11026	2593

Regressions include controls. Mining is excluded.
Cluster robust standard errors in parentheses
* p < 0.1, ** p < 0.05, *** p < 0.01

Table 11: RDD results: Medium 2016 policy

	Mar 2016	Jun 2016	**Sep 2016**	**Dec 2016**	**Mar 2017**	**Jun 2017**
All businesses						
Beta	0.099	0.293	-0.247	0.179	0.140	-0.140
Robust p-value	(0.78)	(0.14)	(0.36)	(0.49)	(0.47)	(0.49)
Bandwidth	2,156,505	2,296,991	1,702,082	2,275,956	1,970,100	1,451,920
Effective observations	1,154	1,224	1,039	1,218	1,140	95
Unincorporated						
Beta	-0.026	0.921**	-0.342	-0.186	-0.302	-0.561
Robust p-value	(0.79)	(0.03)	(0.60)	(0.75)	(0.62)	(0.19)
Bandwidth	1,497,854	1,765,229	1,969,430	1,850,328	2,080,014	1,673,069
Effective observations	293	331	326	333	338	334
Companies						
Beta	0.144	0.017	-0.272	0.330	0.401	0.0576
Robust p-value	(0.69)	(0.98)	(0.33)	(0.27)	(0.16)	(0.99)
Bandwidth	2,663,093	2,315,771	1,499,775	2,357,502	1,638,978	1,619,548
Effective observations	874	852	670	874	712	695

Robust *p*-values in parentheses
* p < 0.1, ** p < 0.05, *** p < 0.01

Table 12: RDD results for revenue and wages

	Sep 2009	Dec 2015	Mar 2016	Jun 2016
Wages				
Beta	0.0386	0.0296	0.0700	0.0660
Robust p-value	(0.38)	(0.52)	(0.12)	(0.11)
Bandwidth	337,338	384,609	373,948	372,523
Effective observations	5,131	7,917	7,781	7,771
Revenue				
Beta	-0.017	-0.030	0.0.10	-0.026
Robust p-value	(0.67)	(0.12)	(0.84)	(0.32)
Bandwidth	350,983	288,230	404,467	307,524
Effective observations	5.599	7,899	9,517	8,224

Robust *p*-values in parentheses
* p < 0.1, ** p < 0.05, *** p < 0.01

Table 13: RDD results using lagged investment

	Sep 2009	Dec 2015	Mar 2016	Jun 2016
Investment in *t*-2				
Beta	0.003	-0.065	-0.007	0.095
Robust p-value	(0.93)	(0.69))	(0.96)	(0.45)
Bandwidth	464,597	377,614	403,782	405,104
Effective observations	2,763	3,134	3,181	3,506

Robust *p*-values in parentheses
* p < 0.1, ** p < 0.05, *** p < 0.01

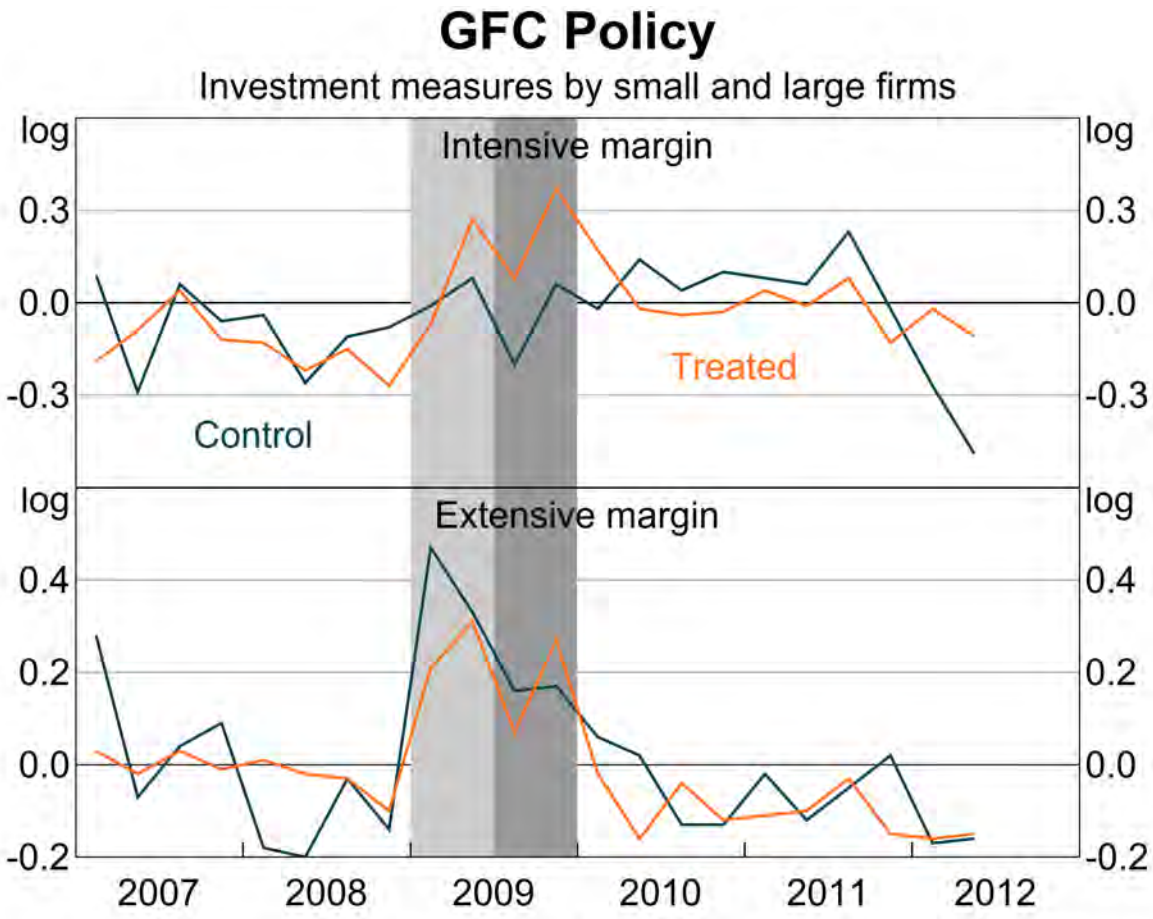
Table 14: RDD results - Donuts Manipulation Test Specification

	Sep 2008	Dec 2015	Mar 2016	Jun 2016
All firms				
Beta	-0.364*	-0.073	-0.500**	-0.197
Robust p-value	(0.05)	(0.79)	(0.02)	(0.35)
Bandwidth	306,648	426,061	298,216	522,424
Effective observations	1,584	2,763	1,969	3,014
Unincorporated				
Beta	-0.528*	-0.542**	-0.728**	-0.607**
Robust p-value	(0.06)	(0.04)	(0.02)	(0.03)
Bandwidth	315,977	335,454	311,429	386,911
Effective observations	705	1,198	1,010	1,208

Robust *p*-values in parentheses
* p < 0.1, ** p < 0.05, *** p < 0.01

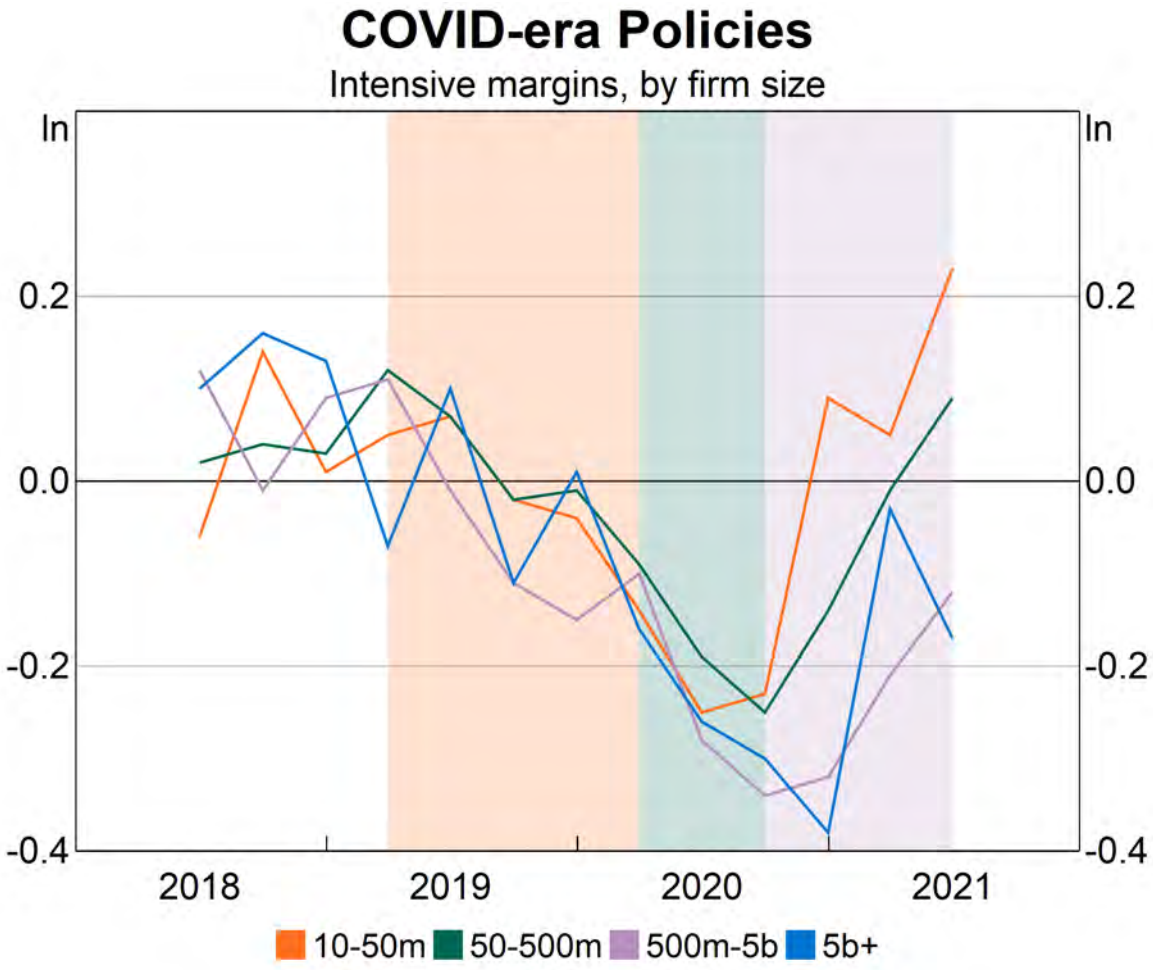
Figures

Figure 1: GFC policy intensive and extensive margins



* Light grey indicates period where treated and untreated received similair investment tax break. Dark grey indicates period with different tax breaks, the treatment period for regressions.

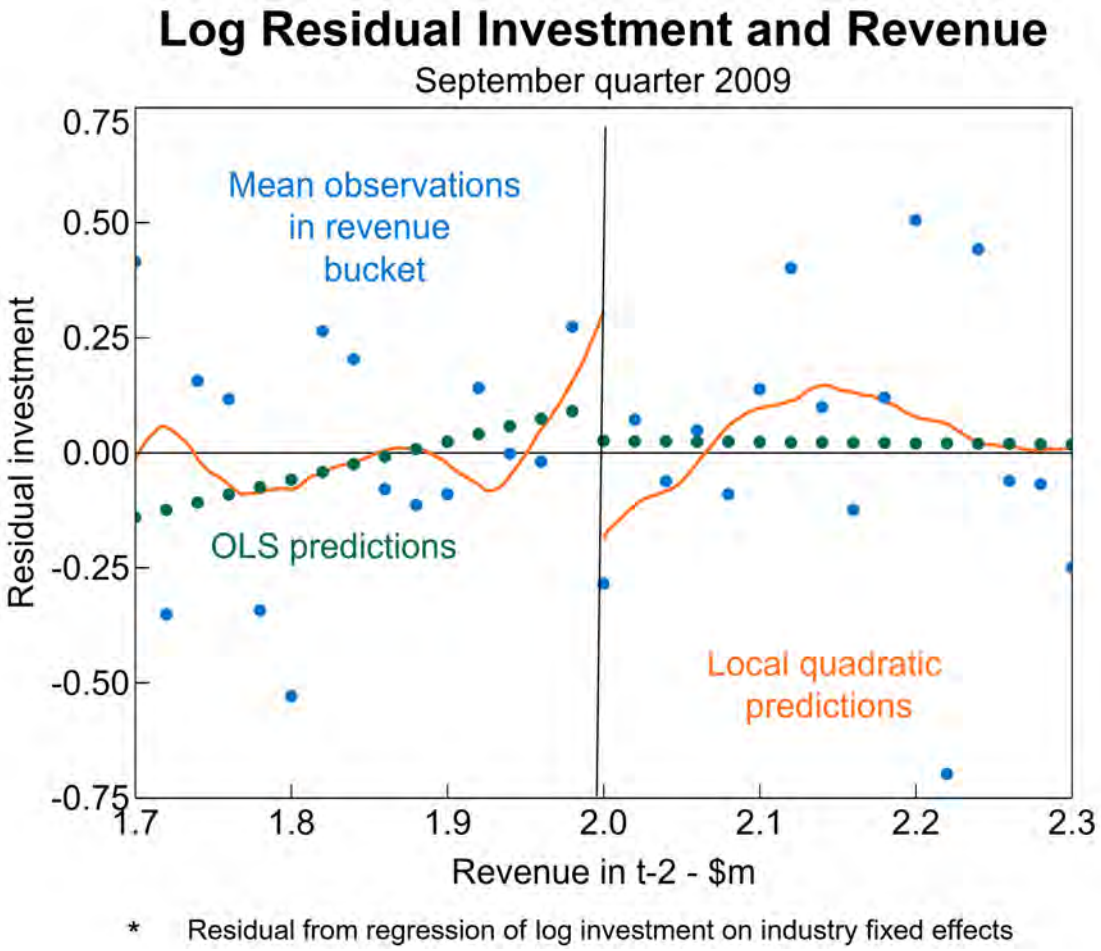
Figure 2: Post-GFC policies



* Size based on income in 2018/19. Controls for seasonal factors and size buckets. Shaded region indicates largest firms that were eligible for investment incentives

Source: CAPEX microdata

Figure 3: Log Residual Investment and Revenue SEP 2009



* Residual from regression of log investment on industry fixed effects

Figure 4: 2012 policy intensive and extensive margins

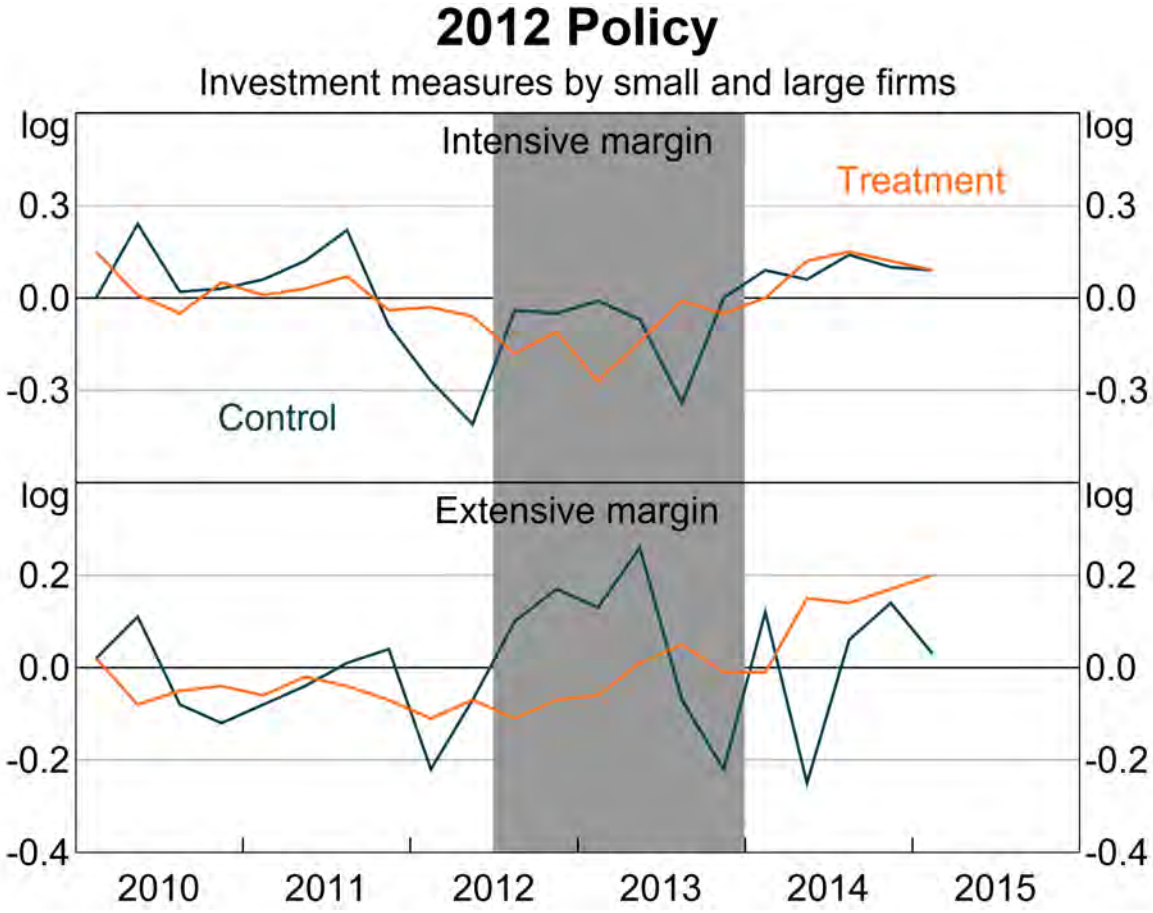
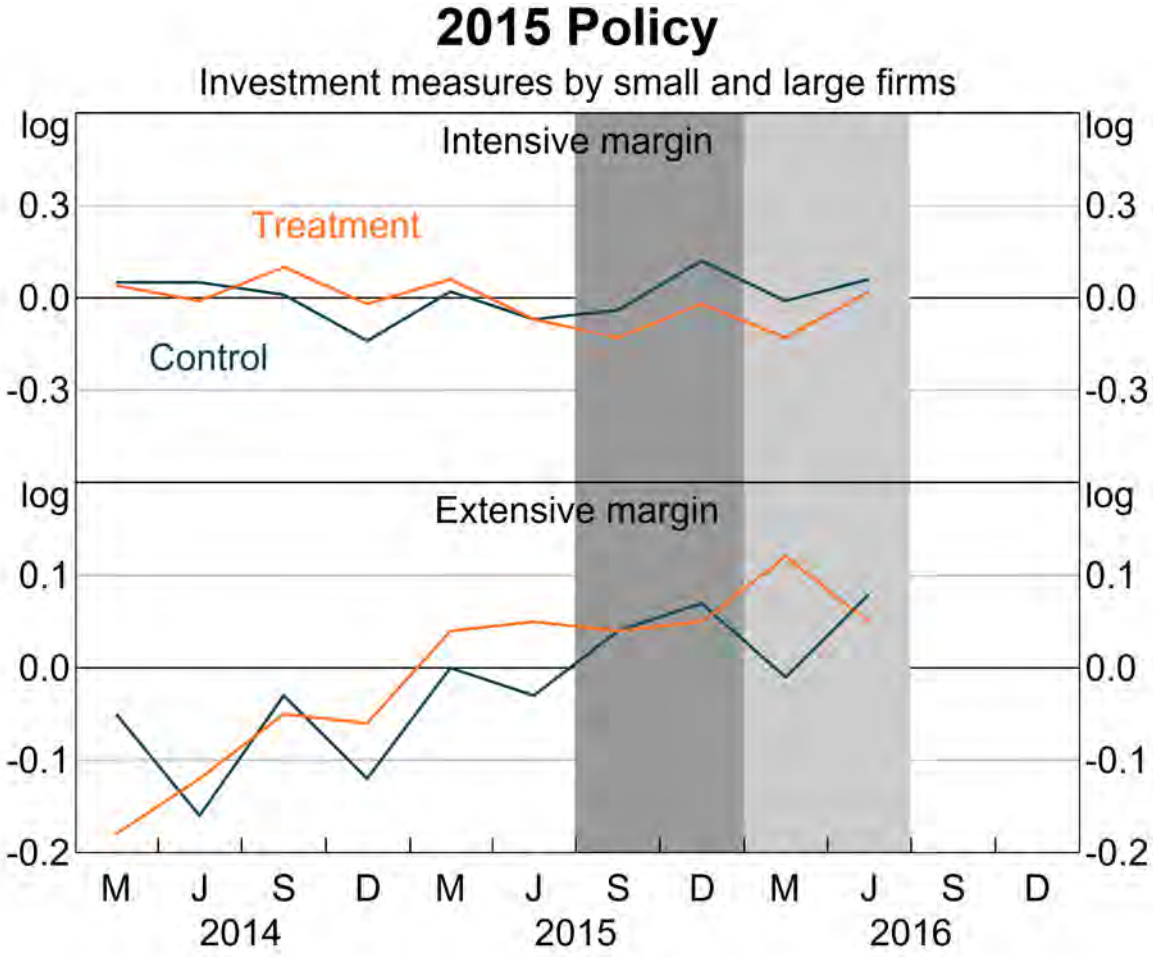
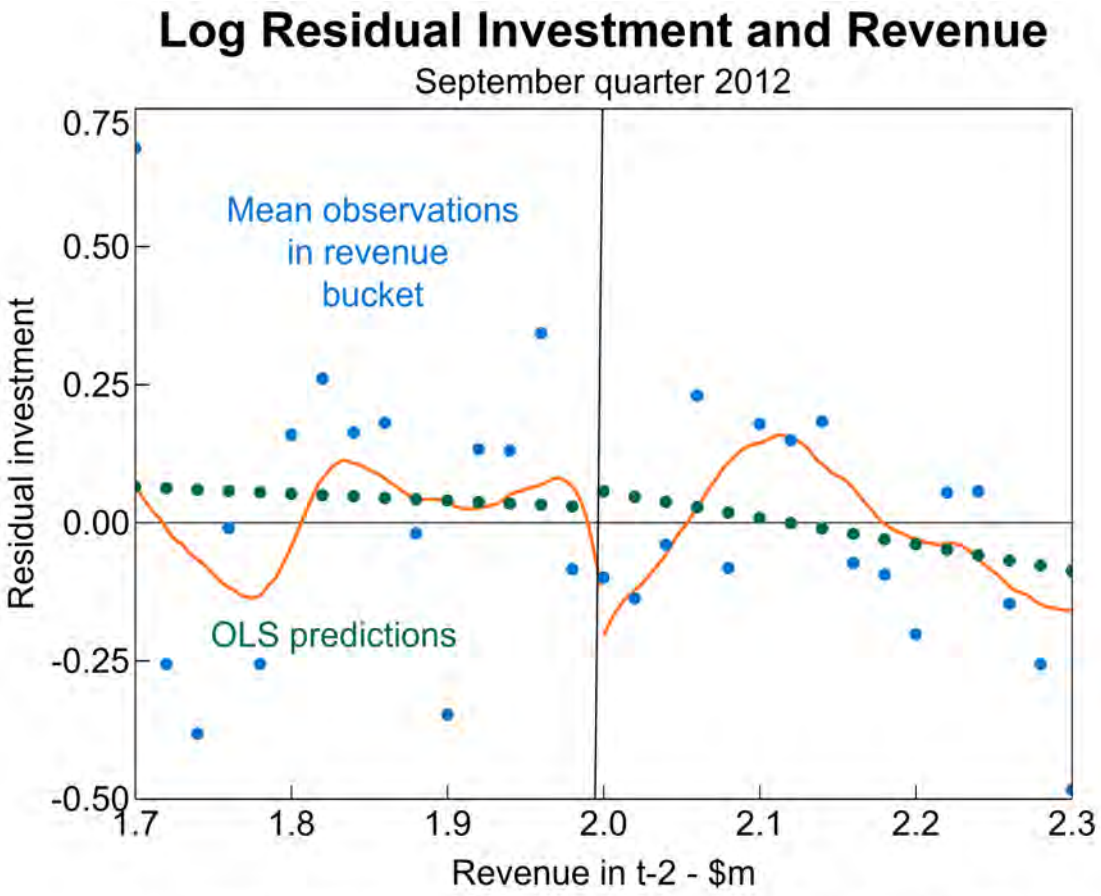


Figure 5: 2015 policy intensive and extensive margins



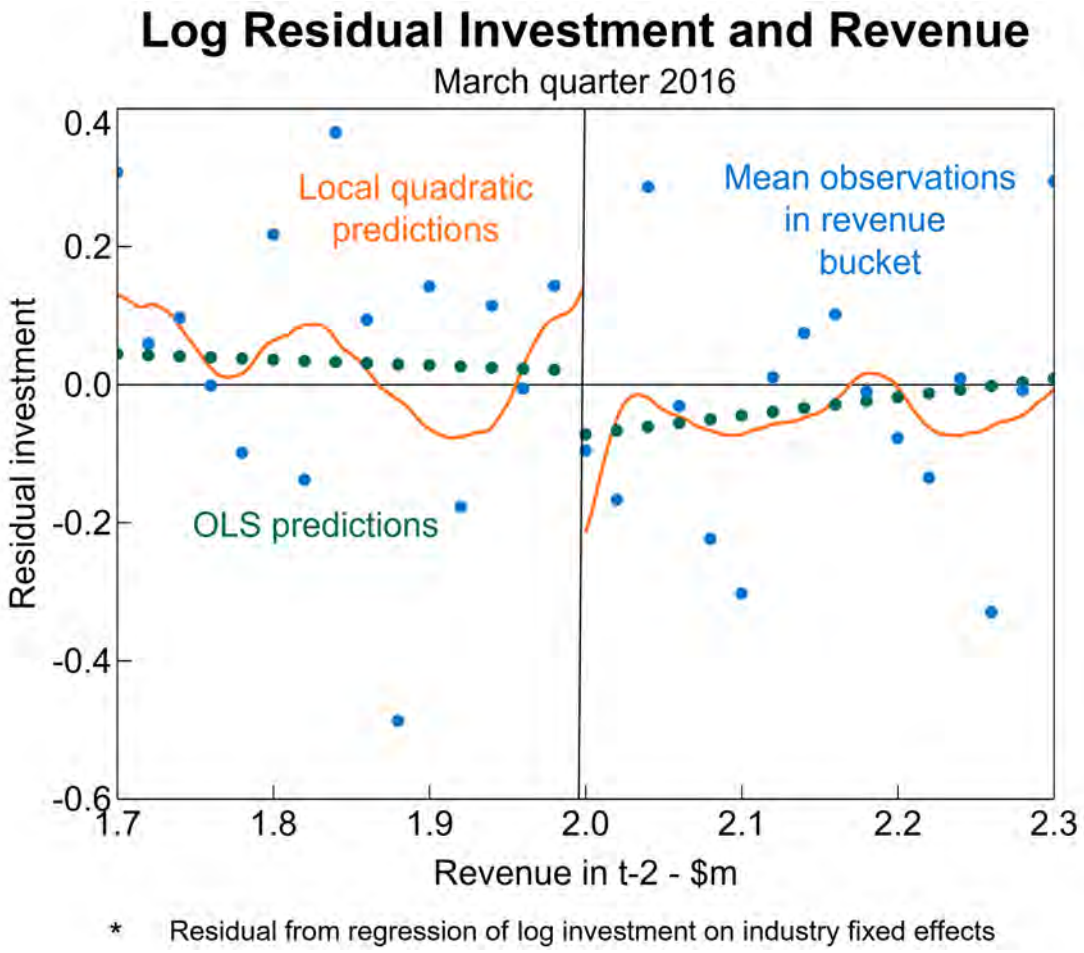
* Dark grey indicates treatment period for DD regressions, prior to any speculation of further support. Light grey indicates period where differential tax break remains in place, but excluded from DD regression.

Figure 6: Log Residual Investment and Revenue SEP 2012



* Residual from regression of log investment on industry fixed effects

Figure 7: Log Residual Investment and Revenue MAR 2016



* Residual from regression of log investment on industry fixed effects

Figure 8: 2016 policy intensive and extensive margins

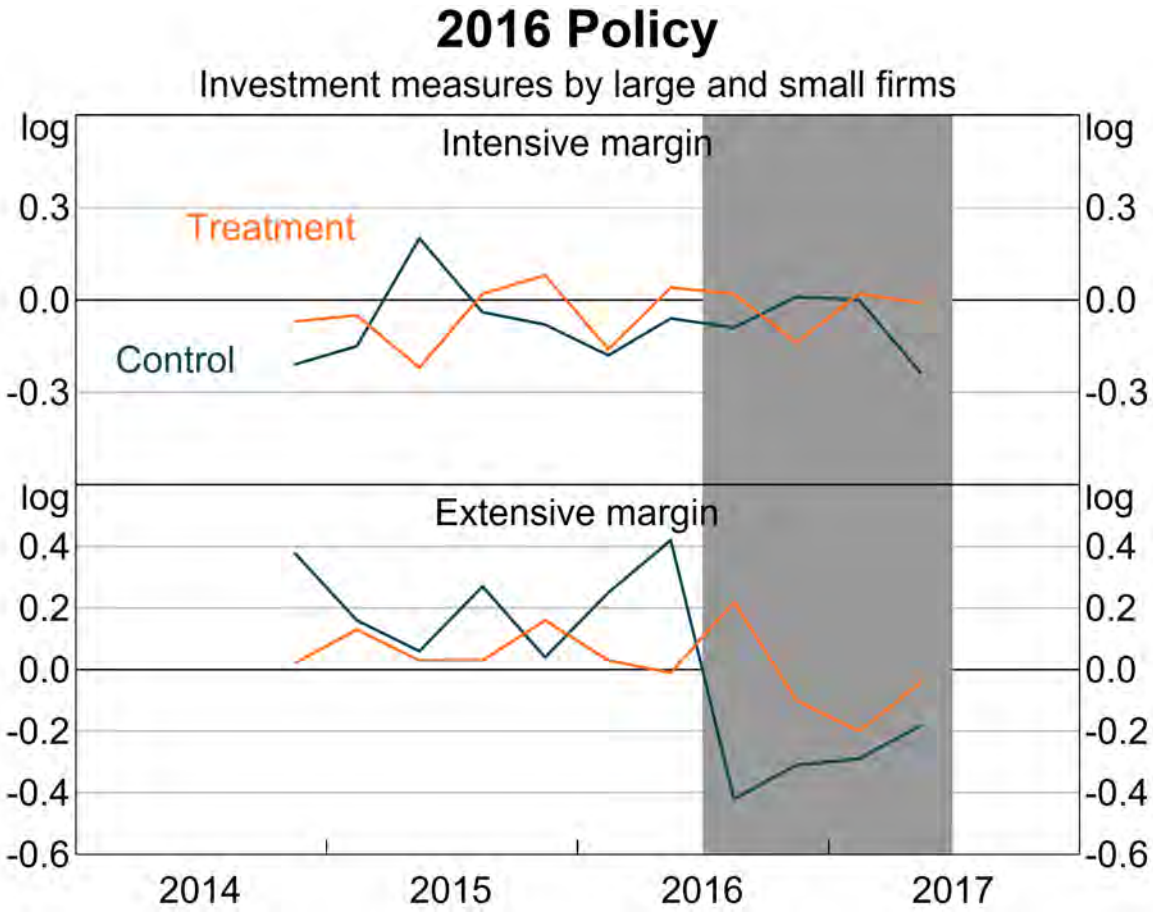


Figure 9: 2019 policy intensive and extensive margins

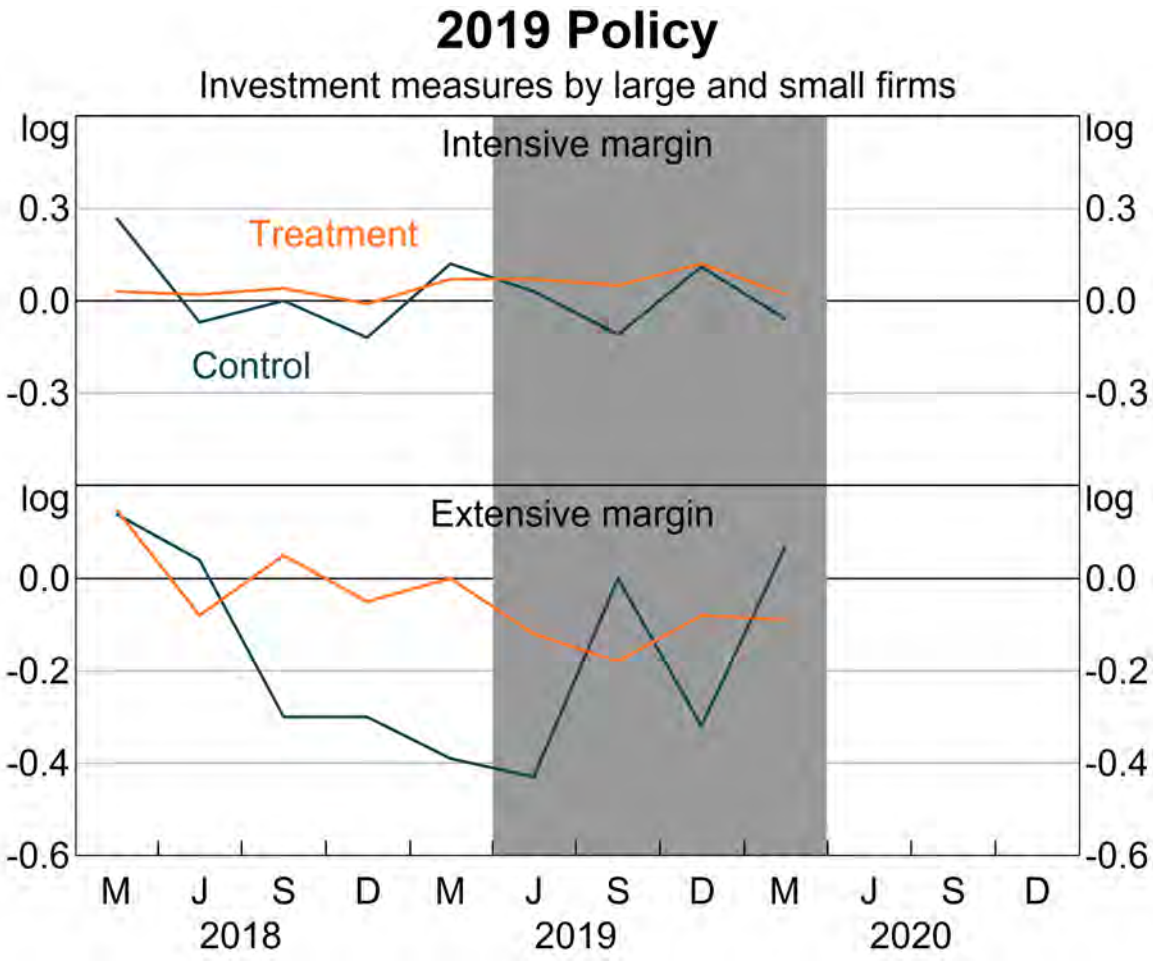


Figure 10: Number of Firms By Turnover at $t-2$

References

Abel, A. B. (1982). Dynamic effects of permanent and temporary tax policies in a q model of investment, *Journal of Monetary Economics* **9**(3): 353–373.

Australian Government (2019). Supplementary explanatory memorandum accompanying treasury laws amendment (increasing the instant asset write off for small business entities) bill 2019. Available at `https://www.aph.gov.au/Parliamentary_Business/Bills_Legislation/Bills_Search_Results/Result?bId=r6282`.

Australian Taxation Office (2017). Work out if you're a small business for the income year. Accessed on 4 January 2025 at https://www.ato.gov.au/business/small-business-entity-concessions/eligibility/work-out-if-you-re-a-small-business-for-the-income-year/.

Calonico, S., Cattaneo, M. D. and Titiunik, R. (2014). Robust nonparametric confidence intervals for regression-discontinuity designs, *Econometrica* **82**(6): 2295–2326.

Cattaneo, M. D., Jansson, M. and Ma, X. (2020). Simple local polynomial density estimators, *Journal of the American Statistical Association* **115**(531): 1449–1455.

Cummins, J. G., Hassett, K. A. and Hubbard, R. G. (1994). A reconsideration of investment behavior using tax reforms as natural experiments, *Brookings Papers on Economic Activity* **2**: 1–74.

House, C. L. and Shapiro, M. D. (2008). Temporary investment tax incentives: Theory with evidence from bonus depreciation, *American Economic Review* **98**(3): 737–68.

Imbens, G. W. and Lemieux, T. (2008). Regression discontinuity designs: A guide to practice, *Journal of Econometrics* **142**(2): 615–635.

Jorgenson, D. W. and Hall, R. E. (1967). Tax policy and investment behavior, *The American Economic Review* **57**.

Kydland, F. E. and Prescott, E. C. (1982). Time to build and aggregate fluctuations, *Econometrica* **50**: 1345–1370.

Lee, D. S. and Lemieux, T. (2010). Regression discontinuity designs in economics, *Journal of Economic Literature* **48**(2): 281–355.

Liu, Y. and Mao, J. (2019). How do tax incentives affect investment and productivity? Firm-level evidence from China, *American Economic Journal: Economic Policy* **11**(3): 261–91.

Maffini, G., Xing, J. and Devereux, M. P. (2019). The impact of investment incentives: Evidence from UK corporation tax returns, *American Economic Journal: Economic Policy* **11**(3): 361–89.

Officer, R. R. (1994). The cost of capital of a company under an imputation tax system, *Accounting & Finance* **34**(1): 1–17.

Rodgers, D. and Hambur, J. (2018). The GFC investment tax break. Reserve Bank of Australia Research Discussion Paper RDP 2018-07. Available at `https://www.rba.gov.au/publications/rdp/2018/pdf/rdp2018-07.pdf`.

Sobeck, K., Breunig, R. and Evans, A. (2022). *Corporate income taxation in Australia: Theory, current practice and future policy directions*, Tax and Transfer Policy Institute, Australian National University.

Swan, P. L. (2019). Investment, the corporate tax rate, and the pricing of franking credits, *Economic Record* **95**(311): 480–496.

Swan, W. T. (2008). Investment allowance to boost business investment. Treasurer's Press Release No 141, Canberra, 12 December.

Swan, W. T. (2009a). Small business and general business tax break. Press Release No 013, Joint Media Release with the Prime Minister and Minster for Small Business, Canberra, 3 February.

Swan, W. T. (2009b). Small business tax break boost. Press Release No 061, Joint Press Release with the Hon Dr Craig Emerson MP Minister for Small Business, Independent Contractors and the Service Economy, Canberra, 12 May.

Yagan, D. (2015). Capital tax reform and the real economy: The effects of the 2003 dividend tax cut, *The American Economic Review* **105**(12): 3531–3563.

Zhang, Lei, C. Y. and He, Z. (2018). The effect of investment tax incentives: Evidence from China's value-added tax reform, *International Tax and Public Finance* **25**(4): 913–945.

Zwick, E. and Mahon, J. (2017). Tax policy and heterogeneous investment behavior, *American Economic Review* **107**(1): 217–48.

Appendix A: Policy episodes

A.1 2008-09 GFC tax credit

The Government provided a temporary bonus income tax deduction for new investment in tangible depreciating assets undertaken between 13 December 2008 and 31 December 2009.

For small business entities (where turnover was less that $2 million), the Tax Break was 50% of their investment in an eligible asset undertaken on or before 31 December 2009. For all other taxpayers, the Tax Break was worked out using a rate 30% for investments committed to before 30 June 2009 and a 10% deduction for those committed to during the second half of 2009. The Tax Break can be claimed in the income year that the asset is first used or installed ready for use. The GFC tax break was in addition to any depreciation allowances for the asset in the year of investment and future depreciation deductions. New investment in relation to an asset needed to exceed a certain threshold before qualifying for the Tax Break. The new investment threshold is $1,000 for small business entities and $10,000 for all other taxpayers.

The investment tax break was announced in the Treasurer's Media Releases No. 141 of 12 December 2008 and No. 012 of 3 February 2009 and as part of the 2009-10 Budget of 12 May 2009. Amendments were passed in *Tax Laws Amendment (Small Business and General Business Tax Break) Act 2009*, which received Royal Assent on 22 May 2009. See `https://www.aph.gov.au/Parliamentary_Business/Bills_Legislation/Bills_Search_Results/Result?bId=r4094`.

For a pre-policy period over which we assess common trends we take the period from 1 July 2007 (21 June 2007 was the date of Royal Assent of legislation lifting the turnover threshold for small business concessions from $1 million to $2 million and simplifying a range of small business tax concessions under this one turnover test). The end of the pre-policy period is December 2008.

A.2 2012 Small business incentive

SBE taxpayers received an increase in the instant asset write off cost threshold from $1,000 to $6,500 for the 2012-13 income year and later income years. Simplification and consolidation of the small business depreciation pools were also implemented at the same time.

The change in asset threshold from $1,000 to $5,000 was announced on 2 May 2010 for the 2012-13 and later income years in response to the *Australian's Future Tax System* Review released in December 2009. On 10 July 2011, the Government announced that as part of the *Clean Energy Future* Plan the small business instant write-off threshold would be further increased from $5,000 to $6,500. The amendments were passed in the *Tax Laws Amendment (Stronger, Fairer, Simpler and Other Measures) Act 2012*, which received Royal Assent on 29 March 2012. SEE `https://www.aph.gov.au/Parliamentary_Business/Bills_Legislation/Bills_Search_Results/Result?bId=r4709`

With a change in government, and repeal of the Mineral Resource Rent Tax, the threshold was moved back to $1,000. The repeal of the MRRT was announced on 24 October 2013, and announcement of repeal of the higher threshold and related spending measures was announced on 18 July 2014. The changed in threshold was effective from 30 September 2014 with the passage of the *Minerals Resource Rent Tax Repeal and Other Measures Bill 2014*, which received Royal Assent on 5 September 2014. SEE `https://www.aph.gov.au/Parliamentary_Business/Bills_Legislation/Bills_Search_Results/Result?bId=r5327`

We take the date of announcement of the repeal of the MRRT as the end date of the policy since the original threshold increase was funded by the MRRT. Businesses would have anticipated repeal of other spending measures when the repeal of the MRRT was announced.

We define the policy period from 1 July 2012 to 31 December 2013 (quarter one 2012-13 to quarter two 2013-14). To look at pre-trends we take the period 1 January 2010 to 30 June 2012 (quarter two 2009-10 to quarter four 2011-12) which is the period after the end of the GFC investment incentive.

A.3 2015 Small business incentive

Small business entities (i.e, businesses with a turnover up to $2 million) were given an increased immediate deduction for a depreciating asset costing less than $20,000 from 12 May 2015 onwards. The temporary increase applied from 12 May 2015 until 30 June 2017. However, we only consider the policy through 31 December to avoid any potential influence from the introduction of the lower corporate tax rate for firms with turnover up to $10 million in May 2016.

The small businesses entities' tax rate was cut to 28.5% in 2015 by the *Tax Laws Amendment (Small Business Measures No. 1) Bill 2015*, which received Royal Assent on 22 June 2015. See `https://www.aph.gov.au/Parliamentary_Business/Bills_Legislation/Bills_Search_Results/Result?bId=r5465`.

A.4 2016 Small business incentive

The small business threshold was increased from $2 million to $10 million and the corporate tax rate was cut to 27.5% for businesses with a turnover less than $10 million from the 2016-17 income year onwards. This meant that business with a turnover up to $10 million could access instant write off provisions for assets costing up to $20,000. With the extension of that provision by 12 months, to 30 June 2018 (*Income Tax (Transitional Provisions) Act 1997*).

The tax cuts and change in the definition of small businesses were announced on 3 May 2016 as part of the 2016-17 Budget and implemented in *Treasury Laws Amendment (Enterprise Tax Plan) Act 2016*, which received Royal Assent on 19 May 2017.

For identification purposes we take 1 July 2017 as the end date of this policy. After this date, tax cuts (lowering the corporate rate to 27.5%) were implemented for businesses with a turnover of less than $25 million as per the *Treasury Laws Amendment (Enterprise Tax Plan) Act 2016*.

A.5 2019 Medium business incentive

The instant asset write off cap increased the maximum amount of allowable expenditure to $30,000 per asset between 2 April 2019 to 30 June 2020 (although this was overtaken by new provisions that commenced on 11 March 2020 and included businesses with a turnover of up to $50 million).

The corporate tax rate was cut to 27.5% for businesses with a turnover of less than $50 million from 1 July 2018 onwards, in accordance with legislation passed in 2016 (*Treasury Laws Amendment (Enterprise Tax Plan) Act 2016*).

We define the policy period as 2 April 2019 to 11 March 2020 (in financial year terms, quarter four of 2018-19 and the first three quarters of 2019-20). We define the treatment group as businesses with a turnover of between $10 million and $50 million for the 2018-19 financial year. We use businesses with a turnover of more than $50 million as the control group–see Table 3.

A.6 2020 Large business COVID incentive

The assets cost cap was increased from $30,000 to $150,000 and the turnover threshold was increased from $50 million to $500 million from 12 March 2020 to 30 June 2020. Proposal was announced in early March (either 12 March 2020, 18 March 2020 and 22 March 2020): (*Coronavirus Economic Response Package Omnibus Act 2020*). This policy was extended by six months to end on 31 December 2020 (announced in June 2020 and passed in (*Treasury Laws Amendment (2020 Measures No. 3) Act 2020*). However, this was overtaken by the expanded COVID temporary full expensing package, which started on 6 October 2020.

We define the policy period as 12 March 2020 to 5 October 2020 (in financial year terms, quarters two and three of 2019-20). We define the treatment group as businesses with a turnover of between $10 million and $50 million for the 2018-19 financial year. We use businesses with a turnover of more than $50 million as the control group.

A.7 2021 Very large business COVID incentive

The instant asset write off was expanded into Temporary Full Expensing (TFE). Under TFE, businesses with a turnover of less than $5 billion could deduct the full cost of eligible depreciating assets with no cap for assets first held, and first used or installed, between the 2020 Budget time (6 October 2020) and 30 June 2022. Loss carryback was also put in for entities with a turnover of less than $5 billion which allowed them to carry back a tax loss for the 2019-20, 2020-21 or 2021-22 income year and apply it against tax paid in a previous income year as far back as the 2018-19 income year. The policy was announced on 6 October 2020 and passed in (*Treasury Laws Amendment (A Tax Plan for the COVID-19 Economic Recovery) Act 2020*).

Businesses with a turnover of under $500 million were given a tax cut from 1 July 2021 with the new rate of 25%. This was in accordance with legislation passed in 2016 (*Treasury Laws Amendment (Enterprise Tax Plan) Act 2016*).

We define a policy period of 6 October to 30 June 2021, the latest observation in our data (the policy expired on 30 June 2023). We define a treatment group of businesses with a turnover between $500 million and $5 billion and use businesses with a turnover between $5 and $6 billion as the control group. These results are robust to changing the turnover bands.

Appendix B: Summary statistics

A.8 Summary statistics

Table B.1: Summary statistics for the $2m group (relevant for the 2009, 2012 and 2015 policies)

	Treated			Control		
	Mean	SD	Observations	Mean	SD	Observations
Investment ($'000s AUD)	156.68	1783.51	16,242	214.42	3305.30	5,052
Log odds investment	-1.40	0.87	75,542	0.27	1.75	10,123
Group turnover ($m AUD)	0.42	0.84	86,202	3.28	0.86	13,780
Employment	14.18	137.90	54,595	27.14	94.24	11,821

Note (a): Firms with group turnover under $2 million are included in the treated group. Firms with group turnover $2-$5 million are assigned to the control group.

Note (b) Only non-zero entries are used for investment.

Group turnover is the sum of the turnovers of all entities in the enterprise group to which a firm belongs.

Where a firm does not belong to an enterprise group, group turnover is equivalent to firm turnover.

Table B.2: Summary statistics for the $10m group (relevant for the 2016 policy)

	Treated			Control		
	Mean	SD	Observations	Mean	SD	Observations
Investment ($'000s AUD)	224.42	2817.32	6,223	268.32	1323.15	3,678
Log odds investment	0.06	1.48	13,017	1.12	1.87	5,702
Group turnover ($m AUD)	5.10	2.31	14,959	14.28	2.82	6,529
Employment	54.28	275.22	12,802	82.32	139.00	5,745

Note (a) Firms with group turnover between $2-$10 million are included in the treated group, Firms with group turnover $10-$20 are assigned to the control group.

Note (b) Only non-zero entries are used for investment.

Group turnover is the sum of the turnovers of all entities in the enterprise group to which a firm belongs.

Where a firm does not belong to an enterprise group, group turnover is equivalent to firm turnover.

Table B.3: Summary statistics for the $50m group (relevant for the 2019 policy)

	Treated			Control		
	Mean	SD	Observations	Mean	SD	Observations
Investment ($'000s AUD)	393.21	1956.87	6,953	531.55	1606.68	890
Log odds investment	0.84	1.60	11,151	2.29	1.94	1,118
Group turnover ($m AUD)	25.49	11.59	11,716	54.63	2.88	1,357
Employment	119.44	225.32	10,531	208.31	230.34	1,224

Note (a) Firms with group turnover $10-$50 million are included in the treated group. Firms with group turnover $50-$60 million are assigned to the control group.

Note (b) Only non-zero entries are used for investment.

Group turnover is the sum of the turnovers of all entities in the enterprise group to which a firm belongs.

Where a firm does not belong to an enterprise group, group turnover is equivalent to firm turnover.

Table B.4: Summary statistics for the $500m group (relevant for the 2020 policy)

	Treated			Control		
	Mean	SD	Observations	Mean	SD	Observations
Investment ($'000s AUD)	1355.62	5467.85	16,619	3413.06	11531.34	1,082
Log odds investment	1.64	1.54	21,938	3.09	1.60	1,208
Group turnover ($m AUD)	196.90	120.30	22,231	549.30	28.83	1,337
Employment	412.37	470.99	20,751	771.23	920.41	1,305

Note (a) Firms with group turnover $50-$500 million are included in the treated group. Firms with group turnover $500-$600 million are assigned to the control group.

Note (b) Only non-zero entries are used for investment.

Group turnover is the sum of the turnovers of all entities in the enterprise group to which a firm belongs.

Where a firm does not belong to an enterprise group, group turnover is equivalent to firm turnover.

Table B.5: Summary statistics for the $5b group (relevant for the 2021 policy)

	Treated			Control		
	Mean	SD	Observations	Mean	SD	Observations
Investment ($'000s AUD)	156.68	1783.51	2,079	214.42	3305.30	114
Log odds investment	-1.40	0.87	2,537	0.27	1.75	136
Group turnover ($m AUD)	0.42	0.84	2,582	3.28	0.86	154
Employment	14.18	137.90	2,553	27.14	94.24	151

Note (a) Firms with group turnover between $500 million to $5 billion are included in the treated group.

Firms with group turnover $5-$6 billion are assigned to the control group.

Note (b) Only non-zero entries are used for investment.

Group turnover is the sum of the turnovers of all entities in the enterprise group to which a firm belongs.

Where a firm does not belong to an enterprise group, group turnover is equivalent to firm turnover.

Appendix C: Additional RDD charts

Figure C.1: Log Residual Investment and Revenue DEC 2009

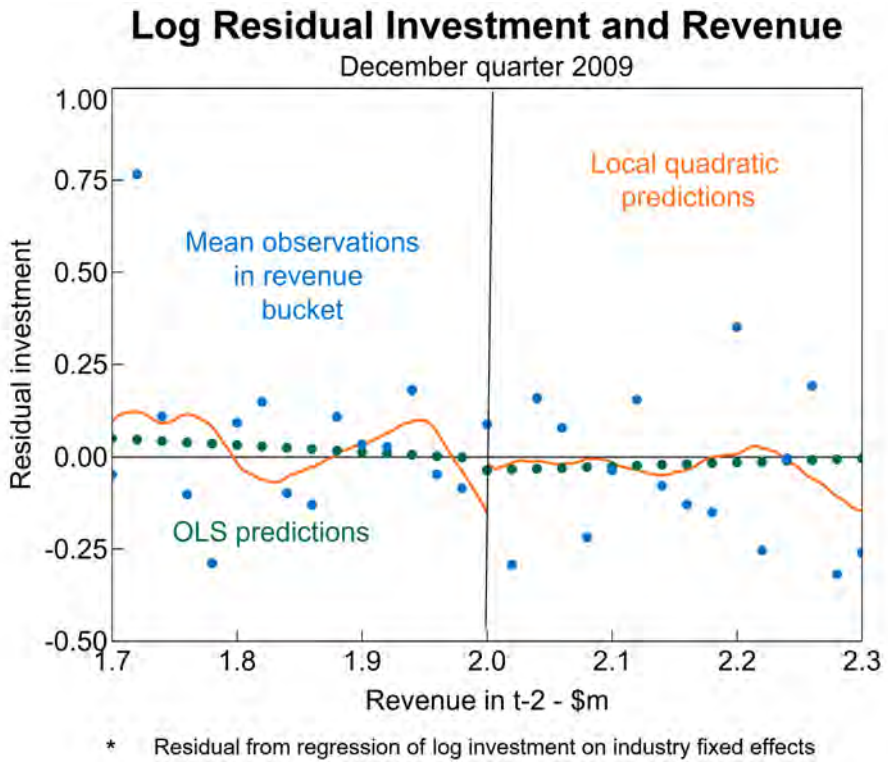
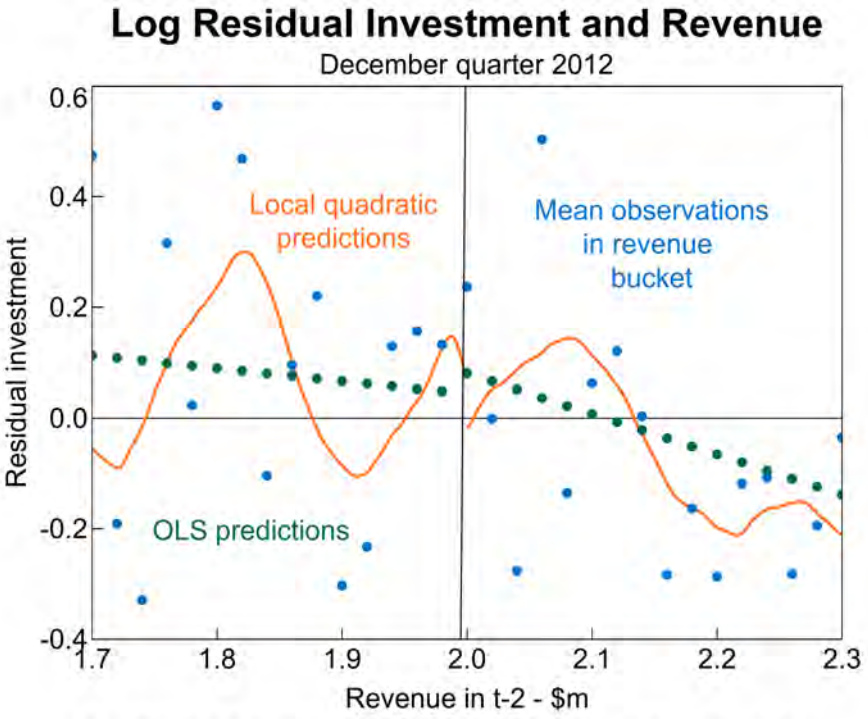
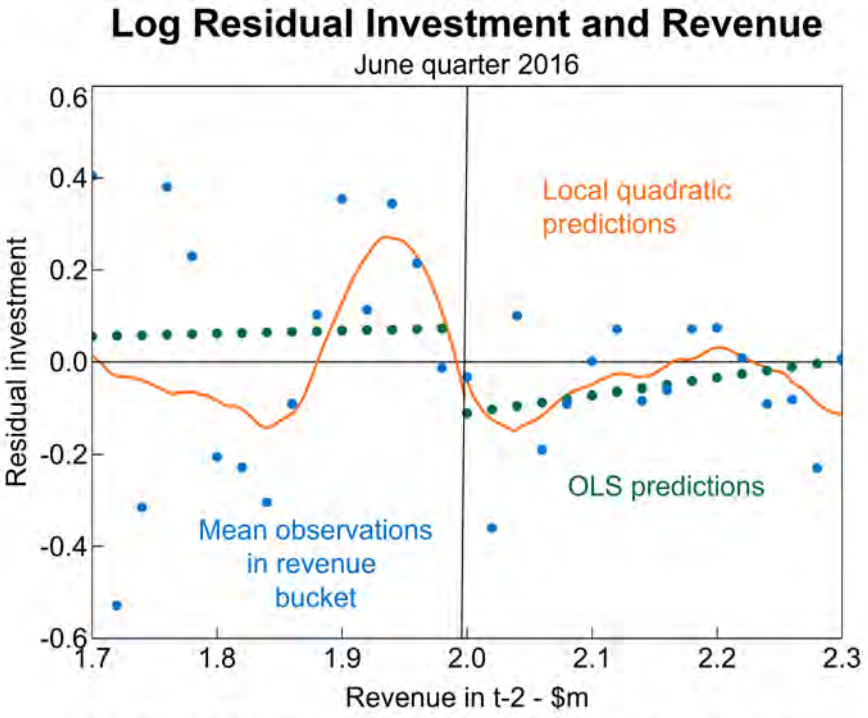


Figure C.2: Log Residual Investment and Revenue DEC 2012



* Residual from regression of log investment on industry fixed effects

Figure C.3: Log Residual Investment and Revenue JUN 2016



* Residual from regression of log investment on industry fixed effects

Appendix D: Testing for the effect of corporate tax changes

D.1 Corporate tax cuts

The 2015 corporate tax cut was a standalone measure announced at Budget on 12 May 2015 which gave incorporated businesses with annual turnover up to $2 million a 1.5 percentage point tax cut to 28.5 per cent. The Enterprise Tax Plan enacted a series of further tax cuts for small and medium sized companies across the ensuing six year–see Table 2. The tax cuts were announced as part of the 2015-16 Federal Budget.

To explore the possible effect of these tax cuts, we re-estimate the DD models using buildings and structures investment from the CAPEX data described above. This asset class is ineligible for the investment incentives that we study in this paper.

Looking at results for the GFC policy period, we can see there is no significant increase in investment in buildings and structures either on the intensive or extensive margins. There appears to be no little complementarity between investment in these asset types (that is, increases in eligible machinery and equipment has no effect on investment in buildings and structures). This suggests that looking at buildings and structures can be a useful placebo test for other ITB policies.

In contrast, for the 2015, and to a lesser extent 2016, there is evidence of an increase in buildings and structures investment, suggesting that firms reacted to the corporate tax cuts.[19]. As firms appear to be responding to the corporate tax cuts, we should be cautious about interpreting our findings regarding the effects of the 2015 and 2016 ITB.

	Intensive margin			Extensive margin		
	All entities	Companies	Unincorps	All entities	Companies	Unincorps
GFC Policy 2009	-0.017	-0.074		-0.242	-0.182	-0.449
SE	(0.440)	(0.526)		(0.173)	(0.172)	(0.285)
Observations	1250	734		25880	13330	12518
2015 Policy	0.665**	0.829*		0.010	-0.157	0.0702
SE	(0.312)	(0.448)		(0.226)	(0.269)	(0.333)
Observations	1317	761		16808	9040	4008
2016 Policy	0.171	0.294		0.219*	0.156	0.277
SE	(0.323)	(0.351)		(0.125)	(0.134)	(0.181)
Observations	1897	1493		10612	7651	2908

Results exclude mining but include no controls due to small sample sizes.

Standard errors in parentheses

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

Table D.1: Buildings investment, excluding mining

[19]We do not show intensive margin results for unincorporated firms because very few firms make these investments.