The impact of investment incentives: evidence from UK corporation tax returns*

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Abstract

How do tax incentives affect firms' investment? Using confidential UK corporation tax returns, we provide new evidence on the effects of incentives in the form of depreciation allowances. We exploit a 2004 exogenous change in the qualifying thresholds for the first-year depreciation allowances (FYAs) and conduct a difference-in-difference analysis. Results suggest that the investment rate increased between 2.1 and 2.6 percentage points when firms became qualified for FYAs, relative to firms that never qualified. This implies an increase in investment rate of 11 per cent at the mean. We exploit exogenous variation in the timing of tax payments to show that this large effect is not due to an increase in available cash and hence, this is primarily a cost of capital effect. Firms respond rather quickly to FYAs, within 12 to 18 months. Firms also bunch just below notches in the cost of capital created by the qualifying thresholds, suggesting salience of the FYAs. Such behaviour does not drive our main results.

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1 Introduction

Higher investment rates and hence a higher level of the capital stock in the economy could increase productivity and national welfare. A relevant policy issue for any government is then how to stimulate firms' investment effectively. Many OECD countries have frequently used the corporation tax system to encourage investment. In the last 20 years, most developed economies have cut their statutory corporation tax rates and in some cases, they have also increased the tax deductibility of capital expenditures. More generous deductions for capital expenditure increase the present value of depreciation allowances and consequently reduce the user cost of capital, which in turn increases the incentive to invest (Hall and Jorgenson, 1967; Hayashi, 1982; and Summers, 1981). In the United States, accelerated first-year investment allowances for equipment (known as the bonus depreciation deduction) were available between 2002 and 2004 and then again, between 2008 and 2013 after the onset of the global financial crisis. In Germany between 2009 and 2010, depreciation for fixed assets could be temporarily calculated using the declining-balance method, increasing the proportion of total allowable deductions taken in early years with respect to the generally permitted straight-line method. In the United Kingdom, before the fiscal year 2008/09,[1] accelerated first-year capital allowances (FYAs) for investment in plant and machinery were available to small and medium-sized enterprises (SMEs) at a rate of 40 per cent compared to the standard rate of 25 per cent available to larger companies.

Many have advocated the use of accelerated depreciation to stimulate business spending on plant and equipment (for example, Feldstein, 2006). Nevertheless, it is not easy to demonstrate convincingly a causal effect of accelerated depreciation on investment spending, partly because any such tax incentives are likely to be correlated with other policies and macro-economic conditions that affect investment behaviour. As the main innovation of this study, we examine the impact of a change to the tax depreciation schedule for a group of companies in the United Kingdom that occurred due to an exogenous change in the qualifying threshold for FYAs. Specifically, in the UK, firms could qualify as an SME and hence claim FYAs if they were below two of three thresholds based on turnover, total assets, and number of employees. In 2004, implementing a Recommendation of the European Commission on the definition of SMEs, the UK increased the turnover and total assets thresholds more than twofold. We exploit this quasi-experimental setting where our control group is composed of companies that did not qualify before 2004 but

[1]The United Kingdom also introduced and expanded repeatedly the Annual Investment Allowance, a 100 percent allowance for expenditure on qualifying plant and machinery.

became qualified under the new thresholds from 2004. Companies which never qualified for FYAs throughout our sample period constitute the control group. We compare investment before and after 2004 across the two groups using a difference-in-differences analysis.

We find a substantial positive effect of higher depreciation allowances on firms' investment. Relative to the control group, FYAs raised investment in eligible assets of newly qualified companies by between 2.1 and 2.6 percentage points within 3 years of the change in the definition of SMEs. At the mean, this translates into an 11 per cent increase of investment. In contrast to much of the literature, we show that this large effect cannot be explained by a cash flow effect and hence, this is entirely a cost of capital effect. Our results can therefore be fully translated into an elasticity of investment with respect to the user cost of about 8.7.[2]

Our benchmark findings are in line with the large effects estimated in the recent literature on the impact on investment of the US bonus depreciation and the US domestic production activities deduction, with the caveat that the calculation of our elasticity does not include a cash flow effect. Regarding the effects of the bonus depreciation, House and Shapiro (2008) find an elasticity of investment with respect to the user cost of capital of between 6 and 14. Zwick and Mahon (2014) find a central estimate of an elasticity of 7.2. Investigating the US domestic production activities deduction, Ohrn (2015) finds an average elasticity of investment with respect to the user cost of 7.8.

Our elasticity of investment translates into a corresponding, much lower elasticity of the capital stock. Evaluated at the mean, we find an elasticity of the capital stock with respect to the user cost of around 1.6 – similar to Zwick and Mahon (2014) but rather higher than that found in the earlier literature of between 0.25 and 1 (see, for example, Auerbach and Hassett, 1992; Cummins, Hassett and Hubbard, 1994; Chirinko, Fazzari and Meyer, 1999; Desai and Goolsbee, 2004; Bond and Xing, 2015).

Our work differs considerably from previous studies and contributes to the literature in the following ways. First, we investigate the effects of tax incentives on investment using changes in the user cost of capital due to an exogenous policy change. More specifically, our research design is based on a variation in the qualifying threshold for claiming FYAs. In contrast, most of the studies on bonus depreciation use variation in the tax treatment of different kinds of assets to identify the impact of tax incentives on investment (see,

[2]The discount rate used in this calculation of the elasticity is 7 per cent, in line with the most recent literature on the effect of the tax system on investment (Zwick and Mahon, 2014 and Ohrn, 2015). For smaller firms such as those analysed here, the discount rate could be as high as 15 or 20 per cent (Poterba and Summers, 1995; Graham and Harvey, 2001; and Jagannathan, 2015). In this case, the elasticity would be 5.3 and 4.6, respectively.

for example, Desai and Goolsbee, 2004; Edgerton, 2010; House and Shapiro, 2008; Zwick and Mahon, 2014). This implies that identification in these studies is based on variation across different industries using different mixes of capital assets which are subject to different tax depreciation schedules and (or) have different life spans. In this context, as recognised by Zwick and Mahon (2014), industry-specific shocks may coincide with the provision of bonus depreciation, which leads to endogeneity problems.[3] In contrast, as the changes in the qualifying thresholds for the FYAs are not related to aggregate economic conditions or firm-level unobserved shocks, our estimation results are less likely to suffer from endogeneity biases.

Second, we analyse a permanent change in depreciation allowances: FYAs were regarded as a permanent feature of the UK tax code[4] and the increase in the qualifying thresholds was announced as permanent. In contrast, studies investigating bonus depreciation in the US (Desai and Goolsbee, 2004; Edgerton, 2010; House and Shapiro, 2008; and Zwick and Mahon, 2014) examine a temporary measure. With a permanent tax incentive, we can rule out the possibility that the large response we find is due to inter-temporal shifting whereby firms have an incentive to bring forward investment in order to capture the transient tax benefits.

Third, we use data from corporation tax returns. This has several advantages over accounting data. For example, tax returns allow us to control precisely for firms' marginal tax rates, which can differ substantially across UK companies due to a complex marginal rate structure described below. Both the marginal corporation tax rate and depreciation allowances affect the user cost of capital, and so it is useful to control for the marginal tax rate differences. Compared to accounting data, tax returns allow us to measure the marginal tax rate precisely.[5] Another advantage of using the tax return data is that we obtain precise information on firms' qualified investment in plant and machinery, which is generally not available in accounting data, especially for smaller, privately-held firms.[6] Moreover, with corporation tax returns we observe when firms were actually liable to pay the corporation tax, which is an important factor in identifying whether there was an effect on capital expenditure through the higher availability of cash arising from a lower

[3]Zwick and Mahon (2014) produce various robustness checks to show that this is not the case in their sample.

[4]FYAs were available to SMEs since 1997 and made permanent in 1998.

[5]Devereux *et al.* (2015) find substantial measurement error in the marginal tax rates based on financial statements, relative to those based on tax returns.

[6]Available accounting data from FAME (Bureau van Dijk) for UK privately held firms offer a more general measure of the stock of fixed assets including both equipment and structures. However, FYAs were not available for structures and buildings. For a discussion of the challenges involved when investigating investment behaviour using accounting data, see Bond and Van Reneen (2007).

tax payment. Zwick and Mahon (2014) also employ corporation tax returns. Although they also find a large response of the intensive margin of investment to tax incentives, our results cannot be explained by the same mechanisms (that is, by a cash flow effect), as we clarify below. Additionally, we also match the corporation tax returns with accounting data so that we have rich information on the financial and tax position of the firm.

Fourth, our study examines the investment behaviour of medium-sized, private UK firms rather than large, public firms that are the focus of most other studies.[7] The effectiveness of investment incentives could be rather different for smaller, private firms. For example, they could be more financially constrained. Moreover, small and medium firms may not be able to fully understand a complex tax code and therefore, the tax incentives may be less salient for them. In fact, we find that firms in our sample responded by investing more following the reduction in the user cost of capital and that they appear to understand the tax code well. As far as we know, ours is one of the few studies which specifically analyse the effectiveness of investment tax incentives for smaller private firms.

Fifth, we test whether the positive effects of FYAs on investment can be explained by the cash flow effect. Theoretically, FYAs not only reduce the user cost of capital but also bring a cash windfall to the qualified firms as more generous depreciation allowances reduce the tax liability. If a firm is financially constrained and if internal and external sources of finance are not perfect substitutes, this windfall could increase investment. There is a vast literature suggesting that firms' investment behaviour is correlated with cash flow. For the UK, Devereux and Schiantarelli (1990) and Bond and Meghir (1994) find that investment is positively related to cash flow. Regarding the cash flow effects of the bonus depreciation, in recent US evidence, Edgerton (2010) finds that firms with more cash flow respond more to more generous depreciation allowances, although Zwick and Mahon (2014) and Ohrn (2015) find that firms that are more likely to be cash constrained reacted more to bonus depreciation deductions. In the spirit of Fazzari, Hubbard and Petersen (1988), these authors split the sample to exploit cross-sectional variation in the cost premium for external finance using various indicators for the existence of financial constraints: for example, the size of the firm, whether the firm pays dividends and measures of cash flow derived from financial statements. A problem with this sample-split approach is that other sources of mis-specification may not be common across different sub-samples of firms (Bond and Van Reneen, 2007): for example, for smaller, younger

[7]For example, in Zwick and Mahon (2014) the average volume of firms' annual sales is US$180 million. In our sample, average annual sales volume is £15.7 million (about US$25 million) for the treated group. Other studies such as Edgerton (2010), Ohrn (2015) and less recent ones such as those surveyed in Bond and Van Reenen (2007) investigate large public firms.

firms current profitability could be a poor proxy for future profitability and hence size would proxy for the firm's growth opportunities which are positively correlated with investment. In other words, cash flow variables and sample splits could be detecting other sources of mis-specification in the underlying investment model. Farre-Mensa and Ljungqvist (2015) recently show that standard measures of financial constraints used in the literature do not identify accurately whether a firm is actually constrained with respect to its choices. We avoid the endogeneity problem associated with conventional indicators of financial status, by instead relying on variation in the timing of the corporation tax payments. In the UK, large companies pay their corporation tax in each current financial year by quarterly instalments with prepayments completed before the end of the reporting year. In contrast, smaller companies pay in arrears: the corporation tax payment is due nine months after the end of the financial year. Thus, for these smaller firms, the cash flow effect of FYAs would generally only arise nine months after the end of the financial year in which the expenditure is undertaken. If the cash flow effect is a dominant factor contributing to the increased investment in our treated firms, we would observe either a small or no increase in the investment rate in the first year in which more generous FYAs become available. In fact, we find that firms paying corporation tax in arrears increased their investment in the period before they paid their (reduced) corporation tax liability, suggesting that it is the lower user cost of capital, rather than the cash flow effect, that explains our findings.

Sixth, our research design also allows us to test for adjustment costs. Companies end their financial reporting year on different dates. If there are adjustment costs, those with a year-end closer to the announcement of more generous capital allowances will have less time to adjust their investment plans in the current year, and will therefore react more in their following reporting year. This is what we observe in our data. Our results also imply that, although firms do not react immediately to increased deduction allowances, they do so relatively quickly, within a maximum of between 12 to 18 months. This evidence points to the existence of positive, but low adjustment costs.

Seventh, we investigate whether FYAs are salient to firms. Recent literature has examined the salience of the tax code for companies. For example, in the US, Yagan (2015) finds that corporate investment does not react to changes in dividend taxation while Zwick and Mahon (2014) find that bonus depreciation provisions are salient. We test for the salience of FYAs provisions in the UK by observing whether firms bunch at the notches in the cost of capital created by the existence of qualifying thresholds. We find that companies bunched just below the qualifying turnover and total assets thresholds.

Nevertheless, we show that companies bunching at qualifying thresholds do not affect our benchmark results, implying that FYAs are effective in stimulating investment for a wider group of companies and not only for firms that managed to manipulate their financial accounts.

Overall, our results show that tax incentives in the form of first-year depreciation allowances can effectively stimulate investment. The large, positive effect of the first-year allowances on investment is not driven by the windfall of higher post-tax cash flows, suggesting that the reduction of the user cost of capital due to the FYAs is the main driver of the observed increase in investment. We find firms in our sample understood the depreciation allowances provisions in the tax code well and that they responded quickly once they became qualified for the FYAs, indicating that adjustment costs, although present, are not prohibitive.

2 The UK system for capital allowances

Until fiscal year 2007/08,[8] the basic structure of the UK system of depreciation allowances for plant and machinery, called capital allowances, consisted mainly of two types of allowances: writing-down allowances (WDAs) for the life of the asset applied at a rate of 25 per cent on a reducing balance basis and FYAs available only to SMEs (Tiley and Loutzenhiser, 2013).[9] FYAs for plant and machinery were introduced in 1997 and made permanent in 1998. WDAs and FYAs could be claimed only for expenditures in plant and machinery.[10] SMEs could generally claim 40 per cent FYAs on their investment,[11] although small but not medium firms could temporarily claim FYAs of 50 per cent in some years (Table 1).[12]

[8]In the UK, the fiscal year for corporate income tax purposes runs from 1 April to 31 March.

[9]The legislation on capital allowances is contained within the Capital Allowances Act (CAA) 2001, as later amended (Tiley and Loutzenhiser, 2013).

[10]Spending on certain assets such as cars, long-life assets and plant or machinery for leasing did not qualify. There is no statutory definition of 'plant' or 'machinery' in CAA 2001 but CAA 2001 provides a list of exclusions (CAA01/S21 & 22), essentially most buildings and structures, and a specific list of included items (CAA01/S23) such as computer software, expenditure on the provision or replacement of integral features. The Her Majesty's Revenue and Customs (HMRC) Capital Allowances manual states that machinery includes machines and working parts of machines (motor vehicles, lathes, computers and similar electronic devices). Plant is more difficult to define: the manual refers to the CAA 2001 list of excluded and included assets and to some tests established by case law. For a discussion of what plant and machinery is, see Tiley and Loutzenhiser (2013).

[11]If FYAs were claimed, WDAs could not be claimed in the same year for the same asset. WDAs would be claimed starting from the second year of life of the assets.

[12]For businesses in the charge to corporation tax the increase to 50 per cent for small businesses (not medium) applied to spending incurred between 1 April 2004 and 31 March 2005 and on or after 1 April 2006 (Table 1, first column). Increasing the rate of the first-year allowances also for medium-

There are different definitions of small and medium sized businesses in UK corporate and tax law but the relevant one for claiming FYAs is derived from EU law (Freedman, 2003). Table 2 sets out the definition.[13] To be classified as small or medium for claiming FYAs, a company needed to satisfy two of three criteria, encompassing turnover, total assets and the number of employees in the current and preceding financial year.[14]

In May 2003 the European Commission adopted a Recommendation concerning the definition of SMEs whereby the financial ceilings on turnover and total assets were substantially increased with respect to the 1996 definition.[15] On 10 December 2003, the UK Chancellor of the Exchequer announced the implementation of the new European definition, increasing the thresholds for defining small and medium-sized businesses to the maximum allowed under European Union regulations. This meant that the upper limits for qualifying as an SME for FYAs purposes were more than doubled for the turnover and total assets thresholds (Table 2, bottom panel). On that occasion, it was announced that the change in qualifying thresholds would have effect for all qualifying expenditure undertaken in any company financial year ending on or after 30 January, 2004. That is, for newly qualified firms, the FYA rate of 40 per cent applied to qualifying expenditure undertaken even before the announcement, as long as the year end of the financial year was on or after January 30, 2004.[16]

On 21 March 2007 as part of the Budget, the Chancellor unexpectedly announced a large overhaul of the capital allowances system to take effect from the following fiscal year (2008/09) and this entailed the repeal of FYAs for plant and machinery expenditures incurred on or after 1 April 2008.[17] In the same period, the corporate statutory tax rate

sized enterprises from 40 to 50 per cent would have carried the high Exchequer cost of £180 million for 2004/05. That additional cost was deemed too high. For more details, see Standing Committee on the Bill (2004).

[13]Section 247 of the 1985 Companies Act implementing EU Directive 78/660/EEC.

[14]Once a company qualified as SME, in order not to qualify subsequently, it had to be above two of the thresholds for at least two consecutive years. If a firm was part of a group, the test was done at the group level. The Companies' Act definition of SMEs provides thresholds for qualifying as a small enterprise (Table 2, column 2) and as a medium enterprise (Table 2, column 3). For identification purposes, we employ variations in the thresholds for medium enterprises only. The same thresholds were relevant for claiming SMEs research and development (R&D) tax credits (see Section 8 for further discussion of this point). The lower thresholds for qualifying as small enterprises were relevant also for the audit exemption but are not relevant for our analysis. Even lower thresholds were set for VAT registration, the VAT flat rate scheme and the cash and annual accounting scheme (Crawford and Freedman, 2010).

[15]The Recommendation is the European Commission Recommendation of 6 May 2003 concerning the definition of micro, small and medium-sized enterprises (2003/361/EC). It replaced the previous Commission Recommendation of 3 April 1996 on which the old UK definition of small and medium-sized enterprises was based (96/280/EC).

[16]For all companies, whether they qualify for FYAs or not, general WDAs rates are apportioned if the accounts span two fiscal years with different WDAs rates. Apportionment was not required for FYAs in the first year of implementation of the new thresholds.

[17]From fiscal year 2008/09, the rate of WDAs was also cut to 20 per cent and the FYAs were replaced

also changed (Table 3). The standard rate of corporation tax was cut from 33 to 30 per cent in 1999 and to 28 per cent in 2008. The UK system levied a reduced rate (known as the small company rate - SCR) on profits under £300,000. The SCR was reduced from 21 per cent to 20 per cent in 1999/00, to 19 per cent in 2002/03 and then increased again in 2007/08 to 20 per cent and to 21 per cent in 2008/09. For firms with profits between £300,000 and £1,500,000, a system of marginal relief[18] operated, which raised the marginal rate to over 32 per cent in most years. Finally, a starting rate of between zero and 10 per cent applied to profits below £10,000 between 2001/02 and 2005/06, and another, higher, marginal rate was applied to the remaining profit below £50,000.[19] This corporation tax schedule gives rise to both cross-sectional and time variation in the marginal rates.

3 Identification strategy and data

3.1 Identification strategy

The changes in the turnover and total assets thresholds for claiming FYAs detailed above implied that some firms which did not qualify for the scheme until the reform subsequently did qualify. We employ this exogenous change in the qualifying thresholds to identify a treatment group of firms which did not qualify for the FYAs prior to 2004 but which became qualified for expenditure undertaken in reporting years ending from January 30, 2004 onwards. Larger firms which never qualified for FYAs constitute our main control group.[20]

Before 2004, under the old regime, firms in the treatment and control groups could

by the Annual Investment Allowance, a £50,000 allowance available to all businesses (Maffini, 2013).

[18]For profits between £300,000 and £1.5 million, the corporation tax is calculated at the main rate minus the marginal relief. The marginal relief is calculated as follows. Marginal relief = (£1.500,000 - Profits) x Standard Fraction. This implies that an effective marginal rate of between and 19 and 32.5 per cent has been applying on profits in excess of £300,000 and below £1,500,000 (Table 3). The standard fraction was 1/40 in 2001/02 and 2007/08, 11/40 between 2002/03 and 2006/07 and 7/440 in 2008/09.

[19]When the starting rate was available, the marginal relief applicable on profits between £10,000 and £50,000 was 1/40 in 2001/02 and 11/40 between 2002/03 and 2006/07.

[20]In Section 5, we conduct robustness checks on the definition of control and treatment group to address the concern that larger firms could be subject to a different investment trend. In principles, we could employ firms that were medium-sized before the reform as another control group. For such firms, the rate of FYAs was always 40 per cent throughout our sample period (Table 1). On inspection of the investment trends for the treated group and the medium-sized firms, we find that the parallel trend assumption before treatment does not hold. This is not surprising as medium firms were treated differently from the treated group before the reform and hence, they are likely to behave differently. Firms that remained small throughout the sample period cannot be used as control group because the rate of FYAs for them was increased to 50 per cent multiple times (Table 1).

claim only WDAs for expenditure on plant and machinery at the rate of 25 per cent. From 2004 onwards they faced different schedules for capital allowances. Firms in the treatment group could depreciate qualified expenditures at 40 per cent in the first year, and thereafter at a declining balance rate of 25 per cent. This implies an increase in the present value of allowances of qualifying expenditure of just over 4 per cent of the expenditure, which in turn implies a reduction in the user cost of capital of about half of a percentage point.[21] Larger firms in the control group could only use WDAs at 25 per cent on a declining balance basis.

For our benchmark analysis, we allocate firms into the treatment and control group on the basis of turnover only, since data on turnover is available for most firms. In robustness checks, we use both turnover and total assets to determine whether or not a firm is treated (see Section5). As we show below, using turnover and total assets jointly significantly reduces the sample size. We do not use the number of employees as data on employees is available for only 10 per cent of our sample. More specifically, we allocate companies to the treatment group if their turnover was above £11.2 million (the old threshold) in each year before 2004 and below £22.8 million (the new threshold) in each year after the reform. We allocate firms into the control group if their turnover was above £11.2 million in each year before the reform and above £22.8 million in each year after the reform.

Four factors could influence the effectiveness of our identification strategy. First, the control group contains only large firms, which could behave differently from the treated firms for non-tax reasons. It is therefore important to establish that the investment trend is similar for the treated and control groups before 2004. Figure 1 shows the evolution of the average investment rate calculated as current investment (I_t) as a share of the stock of fixed assets at the beginning of the period (K_{t-1}). In the sample, companies have different reporting year ends. For firms with a year end in December,[22] the first qualifying year for FYAs is 2004. For most other firms it is 2003. In Figure 1, we code Year 1 to be the first reporting year in which a firm was able to claim the FYAs under the new qualifying thresholds.[23] The average investment rate of both groups declined in the year prior to the reform, but increased more for the treated immediately after the reform. In Section 5, we present a number of robustness checks on the composition of the control group, such as including only the smaller firms amongst those that did not

[21] The calculations are based on a standard definition of the cost of capital, assuming a discount rate of 7 per cent and a tax rate of 30 per cent (see Appendix A).

[22] This accounts for around two thirds of our sample.

[23] Firms in the control group are classified accordingly.

qualify, (Section 5.1), and using propensity score matching techniques (Section 5.2).

Second, in principle there may have been two types of anticipation effect. First, by reacting to the December 10, 2003 announcement of higher thresholds, firms which had a December 31, 2003 year end could have delayed investment from 2003 to 2004, in order to benefit from the FYA available in 2004. However, this is unlikely in practice for two reasons. First, if companies were responding to the announcement on December 10, 2003, they had very little time (about 20 days) to adjust their expenditure before the end of the month, when their reporting year ended. Second, under reasonable assumptions about the discount rate, it was, in any case, not worth delaying investment in this way.[24] That is because whilst delaying investment would permit the firm to claim an FYA, as a consequence it would also have to wait for an additional year until it received the benefit from any capital allowance. Under reasonable assumptions, the present value of allowances would have fallen if the firm had delayed claiming allowances. It therefore seems unlikely that investment of companies with a December 2003 year end would have fallen in anticipation of the reform being enacted for accounting year ends on or after January 30, 2004. Nonetheless, we test for this effect directly. Figure 2 suggests that firms with an incentive to postpone investment from 2003 to 2004, that is firms with December year-end do not appear to have done so: in 2003 their investment rate is only slightly lower than that of non-December firms and they did not invest more than other treated firms in 2004. Below we test these differences with a t-test and in a triple-difference regression framework (Section 5.4).

A second anticipation effect could be that firms foresaw the increase in thresholds and the precise timing of the UK government's response, following the European Commission's earlier announcement in May 2003. This also seems very unlikely in practice. The European announcement was a technical change, and it was unclear how or whether the UK would react, let alone the precise rules and timing that would be implemented. We have been unable to find any evidence in UK media that even tax professionals expected the announcement on December 10. However, if firms had anticipated the December 2003 announcement correctly and if they were already in their first qualifying year at the time of anticipation (for example, firms with February 2003 year-end), they could have started increasing the investment prior to the announcement, since any additional investment would have received the FYA if the financial accounts ended on or after January 30, 2004.[25] Contrary to this, we find that firms already in their first qualifying

[24]See Appendix A for a derivation of this argument.

[25]It is possible that firms anticipated the announcement incorrectly, but this is hard to test without knowing what the incorrect anticipation was.

year at the time of the European Commission's May or the UK government's December announcements increase their investment rate only in the following year, that is, in their qualifying year 2 (Section 4.4).

It could also be argued that only the recording of investment expenditure in the tax return was brought forward and not real investment expenditure. If this were possible, independently of the date of end of their reporting year, all firms could have shifted recorded investment from Year 0 to Year 1. In Section 5.5, we change our dependent variable and use a measure of investment taken from the balance sheet which should be less sensitive to the timing of tax incentives.

The third element which could influence our identification strategy is that the government could have increased the qualifying thresholds for FYAs because the investment rate in capital stock for the firms in the treated group was relatively low. Again, the UK government instead seemed to be responding to the technical change introduced by the European Commission, rather than targeting a specific group of firms. However, if the investment rate of the treated group were low, then treated firms may have had plans to anyway increase their investment post-reform, as a reversion to the longer-run mean, irrespective of the availability of FYAs. In this case, after an initial increase, the investment rate would decline until the optimal level of capital stock is reached. In Section 5.6 we investigate this by showing that when we take the longer-run averages by studying a longer period (2002/03 to 2008/09 instead of 2002/03 to 2006/07), the investment rate of the treated increases even further, showing no sign of reversion to the mean after the increase in the first three years.

Finally, firms with a high propensity to invest regardless of the FYAs could manipulate their size indicators to qualify for FYAs – and the incentive for such manipulation increased with planned investment. To examine this possibility, we analyse whether firms bunched just below the qualifying thresholds. We do find evidence of bunching, but we also show that our main results are robust to the exclusion of firms located in the bunching region (Section 7).

3.2 Data

We merge two datasets to construct our sample. The first is the universe of UK corporation tax returns (CT600 forms) made available for the period 2001/02 to 2008/09 on a confidential basis in the Datalab of the UK tax authority (Her Majesty's Revenue and Customs - HMRC). This contains detailed information on firms' turnover, capital allowances claimed, taxable income - and hence on the applicable marginal statutory

corporation tax rate - and whether the firm paid the corporation tax in arrears or in quarterly instalments. Additionally, tax returns data provide precise information on firms' investment in plant and machinery.[26] The second dataset is FAME (Bureau Van DijK) which collects publicly available balance sheet and income statement information for UK companies. We use information from FAME on total assets, fixed assets and cash flows of the firm.

We restrict the sample to the fiscal years 2002/03 to 2006/07.[27] This is because in March 2007, the UK Chancellor announced a large overhaul of capital allowances to be phased in fiscal year 2008/09.[28] Some companies may have reacted to such reform in 2007/08. Hence, we exclude fiscal years from 2007/08 onwards. To exclude outliers, we drop observations in the top percentile of the distribution of investment rate and of the growth rate of turnover. We construct a balanced panel which requires each firm to have non-missing observations throughout the sample period (2002/03 to 2006/07). The final sample consists of 17,365 firm-year observations for 3,473 firms (Table 4).[29] In our benchmark sample, 906 firms belong to the treatment group and 2,567 firms to the control group.

Table 5 compares the two groups of firms in terms of their turnover, growth rate of turnover, total assets, growth rate of total assets, profitability, loss carry-forwards, marginal tax rate, investment rate, tax component of the user cost of capital,[30] taxable income, and cash flow.[31] As expected, firms in the treatment group are smaller on average

[26]This is a major advantage of tax return data, since other data sources generally do not report investment, and do not break down capital or investment between equipment and buildings.

[27]As we will explain in Section 4.1, the dependent variable (current investment over fixed assets at the beginning of the period) requires one lag and therefore, we will not be able to include fiscal year 2001/02 in the regressions as this is the very first year in the HMRC corporate tax returns dataset.

[28]2007 Budget report announced on 21 March 2007.

[29]Table 1.B in Appendix B explains in detail how we construct the sample. The sample drops by 1 million observations when we require firms to have more than one observation before and after the reform. The sample drops further from 1,278,872 observations to 39,945 observations when we drop small firms. As explained in Section 3.1, small firms cannot be used in our quasi experimental setting because they underwent different changes in their capital allowances regime (see Table 1) and for this reason, they cannot be used as a control group. Our working sample will be much smaller than the full HMRC database because the majority of companies in the UK are small but our identification strategy is based on identifying two specific groups of firms which are relatively large in size with respect to the universe of UK companies. In the main dataset gathering the universe of UK tax returns, 96 per cent of the observations could be classified as referring to a small company as they report turnover below the threshold for qualifying as a small enterprises (£5.6 million) while 99 per cent report turnover below the threshold for medium enterprises (£22.8 million). The requirements for the treated group are specific: companies need not to qualify before the reform and need to qualify after the reform. This implies that treated firms are relatively large (within the class of SMEs) as they were too large to qualifying before the reform. The never qualifying companies are even larger.

[30]See Appendix A for a definition of the tax component of the user costs of capital.

[31]See Table 2.B in Appendix B for a definition of the variables.

in terms of turnover and total assets than those in the control group. Table 5 also shows that the average growth rate of turnover of treated firms is negative whilst that of the control group is positive. This raises the possibility that some firms entered the treatment group because their turnover had genuinely shrunk (and hence were likely to invest less) or because they manipulated their recorded turnover. Consequently, we control for the growth rate of turnover to avoid any downward bias caused by shrinking firms. In Section 8 we also study the possible manipulation of turnover or assets by firms that may want to qualify for FYAs. Other variables such as profitability and the dummy for loss carry-forwards are more comparable between the two groups. In terms of investment rate, firms in the control group have a higher average gross investment rate and a slightly higher percentage of observations with a positive investment rate. We further compare the investment rate of firms in the two groups before and after the reform. Table 6 shows that for the treated, the average investment rate for the periods after the reform (19.2 per cent) is significantly higher than that for the periods before the reform (16.9 per cent). The t-test shows that we can reject the null of equal means at the five per cent level. This is not the case for the never-qualified firms. For them, we cannot reject the null that the average investment rate after the reform (20.5 per cent) equals that before the reform (20.3 per cent).

4 Empirical models and results

4.1 Benchmark analysis

We estimate a regression model of the following form

$$\frac{I_{it}}{K_{i(t-1)}} = \alpha + \beta_1 d_{it}^R + \beta_2 d_i^T + \beta_3 d_{it}^R * d_i^T + \underline{\gamma}\underline{X}_{it} + \epsilon_{it} \tag{1}$$

where $\frac{I_{it}}{K_{i(t-1)}}$ is the gross investment rate calculated as the ratio of current investment expenditure in plant and machinery to fixed assets at the beginning of the period. d_{it}^R is a dummy equal to one if the financial year ends on or after 30 January 2004, when the new thresholds for qualifying for FYAs applied. d_i^T is a dummy equal to one for the treatment group. The parameter of interest in the difference-in-difference analysis is the coefficient β_3 on the interactive term $d_{it}^R * d_i^T$. It captures the effect of the treatment in our quasi-natural experiment. In our regressions, we also control for a vector of firm characteristics $\underline{X}_{it}$, which includes the growth rate of turnover, profitability, the marginal

corporation tax rate and the growth rate of total assets.[32]

We discuss the importance of controlling for the growth rate of turnover below. We control for profitability because more profitable companies are likely to invest more. We control for the marginal tax rate to isolate the effect of capital allowances from that of the tax rate. We control for the growth rate of total assets as slow-growing or shrinking companies may self-select into the treatment group. We first estimate equation (1) using an OLS and report the results in columns (1) to (5) of Table 7. Columns (6) to (9) report within-groups estimation results. Column (1) reports the benchmark estimation results when we do not control for any firm characteristics. It indicates that prior to the reform, the investment rate of the treated group is 3.4 percentage points lower than that of the control group on average. The estimate of β_3 suggests that after the reform, the investment rate of the treated group increased on average by 2.1 percentage points more than that of the control group. Finally, on average, there is no evidence of a common increase in investment rate for both groups as the estimated coefficient on the dummy d_{it}^R is not statistically different from zero.

Some firms could have entered the treatment group because they were shrinking in size, others because they manipulated their size indicators to qualify for FYAs. The former type of firms is also likely to have a lower propensity to invest, which could bias our estimate of β_3 downward; while the latter type is likely to have a higher propensity to invest and this may bias our estimate of β_3 upward. In columns (2) to (5), we control for the growth rate of turnover to address these concerns.[33] In column (3), we add a set of year dummies to control for common business cycle effects. In column (4) we add sector dummies, and in column (5) we employ a full set of sector-year dummies. This controls for the effect of the price component of the user cost of capital on investment.[34] When capital goods are in high demand, their price could increase, driving up the user cost of capital potentially undoing the effect of higher capital allowances. In all these columns, the estimated coefficient β_3 remains positive and significant at the 5 per cent level with a magnitude close to that in column (1). The magnitude of the estimated treatment effect varies very little across different specifications, suggesting that the benchmark result in column (1) is unlikely to be confounded by these factors. Nonetheless, we observe a slight increase in the size of the estimated coefficient β_3 when controlling for the growth rate of turnover, consistent with the hypothesis that shrinking firms with a lower propensity

[32]Total assets include equipment but also structures, buildings and intangible assets.

[33]We further address the behaviour of firms manipulating their reported turnover and total assets more specifically in Section 8.

[34]See Appendix A for a description of the price component of the user cost of capital.

to invest enter the treatment group and as a result, it is likely that we underestimate the treatment effect in column (1). For the same reason, the coefficient on the treated dummy (d_i^T) becomes smaller in absolute terms when controlling for the growth rate of turnover.

We repeat our analysis using the within-groups estimator to control for firm-specific fixed effects (columns (6) to (9)). The results remain robust. Taken together, Table 7 indicates that after firms in the treatment group qualified for the FYAs, their investment rate increased by between 2.1 and 2.3 percentage points relative to firms in the control group. At the mean, this translates into an 11 per cent increase in investment and into an elasticity of investment with respect to the user cost of about 8.7.[35]

4.2 Controlling for additional observables

In Table 8, we control for observable characteristics of the companies. In column (1), we control for the marginal corporation tax rate: the higher the marginal tax rate, the higher the user cost of capital and the lower investment, all else equal. The sign of the coefficient on the marginal tax rate is positive (though not significant), which may reflect the correlation between the broadly progressive UK marginal tax rate schedule and profitability. Once we control for profitability (column (2)) the sign of the coefficient on the marginal tax rate turns negative. The coefficient on profitability remains positive and highly significant. To deal with the potential endogeneity of current profitability, in column (3) we employ the lagged value of profitability. The coefficient is positive and highly significant showing that more profitable firms also invest more. In column (4), we control for the growth rate of total assets and unsurprisingly, firms which grow faster also invest more. In column (5), we use a within-groups estimator.

Throughout different columns of Table 8, the estimated coefficient β_3 hardly changes and its magnitude remains similar to that reported in Table 7. Since results using OLS and within-groups estimators are very similar, from now on we present results only from the within-groups estimation.

4.3 The extensive margin

We next test whether higher FYAs affected the extensive margin of investment: are firms more likely to invest in years after the reform? Table 9 reports the marginal effects from a Probit model (columns (1) to (4)), a Logit model (column (5)) and a linear

[35] We assume a discount rate of 7 per cent.

probability model (column (6)), where the dependent variable is a dummy that equals 1 if a firm made non-zero investment in plant and machinery in a particular year, and 0 otherwise. Throughout these columns, we fail to identify any significant effect at the extensive margin: whether or not firms undertook investment seems to be unrelated to the availability of the FYAs.

The lack of effect on the probability of undertaking investment for the treated firms can be explained by two factors. First, in our sample, the percentage of observations with zero investment is very low: only around 7 per cent (Table 5). Most firms invested for at least three years during the sample period. Examining transitions, firms investing in year *t-1* are highly likely (96 per cent of them) to invest in year *t*, and 47 per cent of firms not investing in year *t-1* also invest in year *t*. Second, if there are fixed costs of starting an investment project, a small reduction in the user cost of capital may not be sufficient to encourage a sizeable number of firms to initiate new investment. For these reasons, it is not surprising that we do not find any effect of the reform on the probability of making investment, although the literature sometimes finds a significant effect (for example, Zwick and Mahon, 2014).

4.4 Timing

Another relevant question for policy makers is how fast the tax incentives could become effective. Answers to this question depend on various factors, including the magnitude of costs associated with adjustments of the capital stock. Our dataset provides an exogenous variation to assess the size of adjustment costs associated with investment. As already mentioned, companies have different end dates for their reporting year. Since the new thresholds for FYAs were announced in December 2003 and became effective on January 30, 2004, companies in our treated group with accounts ending earlier in 2004 (for example, in February and March) had less time to adjust their investment plans for Year 1 than those in the treated group with accounts ending later in 2004. For example, when the new thresholds were announced in December 2003, firms with December year-end were still in their qualifying Year 0. Firms with year-end in July 2004 had eight months before the end of their Year 1 while firms with year-end on January 31, 2004, only had a little over one month before the end of qualifying Year 1 to adjust their investment plans for that year.

If adjustment costs are relevant, companies with financial accounts ending earlier in 2004 would be more likely to have increased their investment in Year 2 than in Year 1. In comparison, companies with accounts ending later in 2004 would be more likely

to have increased their investment in Year 1. To test this, in Table 10, we split the sample between firms with accounts ending between January and June (column (1)) and those between July and December (column (2)). The results show that firms increased investment with a time lag as we expected: the first group only increased their investment in Year 2 and 3 while the latter group already increased their investment in Year 1.[36] In columns (3) and (4) we increase the sample by relaxing the requirement of having a balanced panel: companies only need to appear two years after the reform and two years before that. The pattern of the results is similar, although we now observe a statistically significant increase in investment rate for the first group of firms in Year 2. These results are also consistent with firms with year ends in the first part of the year not increasing investment in 2003 in anticipation of the reform announcement.

Our analysis therefore shows that companies did not react immediately to the availability of more generous FYAs, consistent with the presence of adjustment costs. Nonetheless, companies reacted relatively quickly, within 12 to 18 months from the announcement of the new rules. Combined with previous results, this suggests that FYAs were not only effective in stimulating investment but they did so rather quickly.

5 Robustness checks

We now present a series of robustness tests of the results.

5.1 Alternative control groups

By construction, firms in the control group are larger than firms in the treatment group, and hence could be subject to different trends. Although Figure 1 shows that the investment rates of the two groups have a comparable trend before the reform, it is useful to check whether our results are robust when we drop the largest firms in the control group. Table 11 show the results for the baseline model when we drop the top 5 per cent (column (1)) and the top 50 per cent (column (2)) of companies in the control group in terms of total assets.[37] The estimated coefficient β_3 on the interactive term remains positive and

[36] We have also split the sample between firms with end date of accounts between January and May and between June and December. This generates similar results: the coefficient β_3 for Year 1 for the group with year-end between January and May is not statistically significant (0.039) with a standard error of 0.043. The coefficient for Year 2 is still not significant but much larger, at 0.080, with a standard error of 0.052. For the firms with year-end between June and December, the coefficient β_3 for Year 1 is statistically significant at 5 per cent (0.031^{**}) with a standard error of 0.014. The coefficient for Year 2 is not significant (0.013), with a standard error of 0.015.

[37] The percentiles refer to the distribution of total assets for the firms in the control group. We have carried out the same exercise for the same percentiles of turnover. The results are virtually identical to

statistically significant in both columns, with a magnitude of between 2.3 and 2.4, very close to our benchmark results of Table 7 and 8.

5.2 Propensity score matching

Next, we match companies in the control group to companies in the treatment group by constructing a propensity score (for being treated) based on observed firm characteristics. Among the different matching techniques used, we report results using the method which yields the best balancing properties, the kernel matching method.[38] To construct the propensity score, we employ a Logit model which regresses the dummy indicating the treatment group on the average pre-treatment growth rate of turnover, the natural logarithm of total assets, profitability and sector dummies (Table 3.B in Appendix B).[39] The balancing properties of the matching method used are reported in Table 4.B in Appendix B.[40] The t-test for equal means in the treated and non-treated groups indicates that for most variables used in the Logit regression, including the sector dummies, the null of equal means cannot be rejected and that the standardised bias is small. This indicates good balancing properties. Only the standardised bias for the growth rate of turnover has a larger value but this is not extremely high (about 11 per cent).[41] By construction, companies in the treatment group are smaller in terms of assets and turnover than companies in the control group. It is therefore unsurprising that it is not possible to achieve a perfect match based on these two parameters. The matching works very well on profitability and sector dummies, which are not directly used in the construction of our treatment and control group. Column (3) of Table 11 reports the weighted regression

those of Table 11. Results are not reported here for brevity.

[38]The method is applied restricting the sample to ensure common support. We also try other matching methods: a nearest-neighbour (NN) matching without replacement with common support (without caliper), NN with replacement and no common support (without caliper), caliper matching with distance set to 0.1 (and robustness checks on the distance up to 1), radius matching with common support and kernel matching without common support. An analysis of the balancing properties of such matching procedures shows that the balancing is much less satisfactory than that achieved with the kernel method reported. The results are very similar to our previous specifications but for brevity, they are not reported here.

[39]The average pre-treatment value of the covariates is calculated across qualifying Year 0, -1 and -2 if available. We also tried to include other covariates in the regressions such as the rate of growth of total assets, the natural logarithm of turnover and the marginal tax rate but their coefficients are insignificant

[40]The average pre-treatment value of the covariates is calculated across qualifying Year 0, -1 and -2 if available. We also tried to include other covariates in the regressions such as the rate of growth of total assets, the natural logarithm of turnover and the marginal tax rate but their coefficients are insignificant.

[41]The next method with the best balancing properties is the caliper matching (distance 0.1) with common support. The absolute value of the standardised bias there is above 10 for both the rate of growth of turnover (14.1) and for the natural logarithm of total assets (-11.5). The bias for the profitability parameter is -2.2.

results for the matched sample. The estimated coefficient β_3 on the interaction term is positive, statistically significant and very close in magnitude to those estimated in our benchmark specifications.

5.3 An alternative treatment group

Next, we consider the definition of treatment group. To allocate firms to a new treatment group, we employ two thresholds jointly: the one on total assets and the one on turnover. For this new treatment group, we require companies to be above both qualifying thresholds before the reform and below both thresholds after the reform in each year.[42] This is a stricter requirement than that used in our benchmark analysis, which implies less measurement error in our classification. The drawback of using a stricter classification is that we are left with a much smaller number of firms: 237 in the treatment group (versus 906 in the benchmark specification). Column (4) of Table 11 shows that, when employing the new definition of treatment and control group, the coefficient β_3 remains positive and highly significant but the estimated magnitude of the coefficient more than doubles that in the benchmark estimations. This suggests that our benchmark estimates are likely to provide a lower bound for the magnitude of the effect of increased FYAs on firms' investment.

5.4 Anticipatory effect

We have discussed how the possibility that firms may have anticipated the reforms may affect the results. Specifically, firms with a reporting year-end of December 31, 2003 could conceivably have delayed qualifying expenditure to 2004 in order to receive the benefit of the FYA. If this anticipatory effect is driving the estimated tax effect on investment, we should observe that the investment rate of firms with a December year-end drops relative to other treated firms in Year 0, and increase more after the reform. Figure 2 shows that investment for December year-end firms did drop slightly more in Year 0 than that of other firms but the difference is very small. Additionally, investment of December firms did not increase more than other treated firms' investment in Year 1.[43] As a further

[42]More specifically, to be assigned to the new treatment group, we require companies to report turnover above £11.2 million and total assets above £5.6 million in each year before the reform (that is, in Year -1 and 0). For years after the reform (that is, Year 1 to 3), companies are required to report turnover below £22.8 million and total assets below £11.4 million. The control group is also constructed using both turnover and total assets thresholds. Firms in the control group report turnover and total assets above the old thresholds both in year -1 and 0 and above the new thresholds in Year 1, 2 and 3.

[43]We also tested with a t-test whether in qualifying Year 0, mean investment of treated December firms is different from mean investment of treated non-December firms. We cannot reject the null that

robustness check, we construct a dummy with value 1 if a firm's reporting period ends in December. We interact this dummy with our difference-in-differences specification to derive a triple-differences regression. In column (5) of Table 11, the estimated coefficient on the triple interaction (Treatment * December year-end * Years after the reform) is insignificant, indicating that the behaviour of December firms is not different from that of non-December firms and hence, it does not affect our benchmark results. In column (6) we include in the benchmark difference-in-difference regression two new variables: the interactions (Treatment * December year-end * Year 0) and (Treatment * December year-end * Year 1). The former indicates the different behaviour of December firms in 2003, when it is possible that they invested less than other treated firms. The latter indicates the differential behaviour of December firms in 2004, when it is possible they invested more than other treated. The coefficient on both variables is close to zero and statistically insignificant, indicating that firms with a possible incentive to delay investment from 2003 to 2004 did not do so.[44]

5.5 An alternative measure of investment rate

A further concern is that, for the treated group, the observed higher investment rate after the reform could be the result of overstating or relabelling investment expenditures in the tax return rather than an increase in actual investment. To address this issue, we use a measure of investment taken directly from the balance sheet: the growth rate of fixed assets calculated as the difference between the natural logarithm of fixed assets at time t and the natural logarithm of fixed assets at time *t-1*.[45] Such a measure should be less sensitive to the timing of tax incentives, especially for private firms such as those analysed here which are not required to publish their accounts.[46] This new dependent variable allows us to test whether the effect we find in the previous analysis is driven

mean investment for the two groups is equal (t-test is 0.5723 with $\Pr(|T| > |t|) = 0.5673$). We run the same test for mean investment of the two groups for qualifying Year 1 and again, we cannot reject the null that mean investment of December firms is equal to mean investment of non-December firms (t-test is 0.292 with $\Pr(|T| > |t|) = 0.7704$).

[44] In further robustness checks we constructed a dummy for firms with year-end between September and December. In a triple difference approach, we interacted the dummy with our benchmark specification and the coefficient on the variable "Treatment * Years after the reform * year-end between September and December" is insignificant (0.022) with a standard error of 0.015, indicating that firms with late year-end do not behave differently from other treated firms. Finally, we have also dropped December firms from our sample. The coefficients on β_3 remains positive and highly significant at 0.043*** with a standard error of 0.017. The sample is reduced to 1,024 companies.

[45] In the regressions, covariates such as the rate of growth of turnover and of total assets are calculated in the same way.

[46] If the firm manipulates both the financial statements and the tax returns, our test will not estimate the change in real investment.

by over-reporting of investment in tax returns. Column (7) of Table 11 shows that the coefficient β_3 remains positive and significant with a magnitude of 2.4 percentage points, again very close to that of our benchmark specifications.

A drawback of the dependent variable used here is that it groups together investment in plant and equipment with investment in structures and buildings to which FYAs do not apply. Given no obvious change in the trend of investment in structures and buildings in the sample period, this aggregated investment rate remains informative about the effects of the FYAs. The results in this section suggest that the FYAs were effective in stimulating real investment rather than only providing incentives for firms to relabel their investment.

5.6 Testing for reversion to the mean

The government could have targeted the treated group because their capital stock and investment were lower than those of firms in the control group. If the firms in the treated group were already planning to increase their investment and capital stock in the future, the effects we estimate would simply be due to a reversion to the longer-run mean and not to the effect of more generous capital allowances. In this case, the investment of the treatment group would only be higher for a short period. If the treated group aimed to achieve a higher optimal level of capital stock, their investment rate will increase initially and gradually decline until the optimal level of capital stock is reached.

To address whether our results are driven by such a reversion to the mean, in column (8) of Table 11 we use a longer time period, extending the period after the reform by two years, although, as noted in Section 3.2, any effects of the 2004 reform may be confounded by subsequent reforms. The results suggest that when using a longer time period, the coefficients β_3 marginally increases with respect to our benchmark specifications. This implies that there is no evidence of reversion to the mean: after three years, investment rate of the treated remains higher than that of the control group.

5.7 Placebo tests

We carry out two placebo tests. In the first, we use firms that were medium-sized prior to the reform as the treated group. We continue to use the never-qualified firms as the control group. For both groups, the rate of FYAs did not change: for medium-sized firms the rate was 40 per cent throughout our sample period and for large firms which could not claim FYAs, the rate remained at 25 per cent. Column (9) of Table 11 shows that

the estimated coefficient β_3 is close to zero and not statistically significant.

In the second placebo test, we create an alternative treatment group from the set of larger firms that never qualified for FYAs (and which are therefore in our benchmark control group). The definition of the new treatment group is somewhat arbitrary here. In column (10), we allocate firms in the new treatment group if their turnover is above £11.2 million (old threshold) in each year before the reform and between £45.6 (double the actual new threshold) and £91.2 million in each year after the reform. Companies in the new treatment group have always enjoyed WDAs at 25 per cent and hence, their investment behaviour should not be different from that of the control group. Our results are consistent with this hypothesis: in column (10), the coefficient β_3 on the interaction between the treatment and the post-reform dummy is not statistically different from zero.[47]

6 Is the cash flow effect important?

Capital allowances may affect investment via two main channels - by lowering the cost of capital, and for cash constrained firms, by increasing the availability of cash due to reduced tax liabilities. In this section, we exploit variation in the timing of actual corporation tax payments to test the relative importance of these two channels. In the UK, larger firms with taxable income above £1.5 million in the current and preceding financial years are not allowed to pay in arrears. Instead they are required to pay the corporation tax liability by quarterly instalments within their financial year. However, the majority of smaller UK companies can settle their corporation tax liability nine months after their accounting year end. Cash savings due to a reduced tax liability would only arise when actual taxes are paid and therefore, if the cash flow effect dominates the user cost effect, we would expect to see no change in investment in Year 1 for treated firms paying their corporation taxes in arrears. We test this hypothesis in Table 12. In column

[47]We also explored with other similar placebo tests, identifying a treatment group within the set of large, non-qualifying companies and varying the definition of treatment. We increase the upper bound of the turnover threshold. To do this, we refer to the distribution of turnover for the benchmark sample and we employ the 90th percentile as an upper bound for the definition: we allocate firms in the new treatment group if their turnover is above £11.2 million in each year before the reform and between £45.6 and £209 million for each year after the reform. The coefficient β_3 derived from a within-group estimation is negative and statistically significant at 5 per cent (-0.21**) with a standard error of 0.008. Finally, we decrease the lower bound. We refer to the distribution of turnover of the never qualifying using the 10th percentile: we allocate firms in the new treatment group if their turnover was above £11.2 million in each year before the reform and between £24.3 and £91.2 million for each year after the reform. The coefficient β_3 derived from a within-group estimation is close to zero (0.004) and not statistically significant with a standard error of 0.008.

(1) a company is defined as paying in arrears if its taxable income in Year 0 and Year 1 is below £1.5 million. As a robustness check, in column (2), companies are defined as paying in arrears if their taxable income is below £1.5 million in each year in our sample period.[48]

In column (1) and (2) of Table 12, we show that firms with accounts ending in the second half of the calendar year (from July to December)[49] and settling their tax liabilities in arrears still react strongly to the reform in their Year 1: the estimated coefficient on the interaction between the treatment dummy and the dummy for Year 1 is positive and significant. This indicates that the cash flow effect on investment is unlikely to be important for these firms.[50]

This finding seems to be at odds with some previous studies (for example, Zwick and Mahon, 2014) who find the cash-flow effect associated with bonus depreciation to be important.[51] However, our results could arise if firms in our sample were not financially constrained: throughout our sample period (2002/01-2006/07), the UK economy was expanding at an average GDP growth rate of 3 per cent per annum and hence, was in a boom period.[52] In this period, credit was also easily available.[53] By contrast, bonus depreciation in the US was implemented during two economic downturns.[54] Nonetheless,

[48]Overall, in the main regression sample, 2,285 firms always pay in arrears (66 per cent) and 1,188 firms always pay in instalment. Nonetheless, the regressions in this section focus on firms with year-end between July and December. See column (1) and (2) of Table 12 for the number of firms paying in arrears used in such regressions.

[49]As explained in Section 4.4, firms with year-end between July and December should have sufficient time to adjust their investment plans in Year 1.

[50]In further robustness checks, we employed a within-groups estimator in a triple difference approach where we constructed a dummy for firms always paying in instalments and interacted it with the benchmark difference-in-difference model. The sample contains both firms paying in arrears and in instalments. All firms have a year-end of accounts between July and December. The coefficient on the triple interaction (Treated * always paying in instalments * Year 1) is close to zero and not statistically significant (-0.007) with a standard error of 0.040, indicating that firms paying in instalments and firms paying in arrears responded in a similar way to more generous capital allowances in Year 1. The coefficient on the variable (Treated * Year 1) remains positive and significant at 5 per cent (0.037**) as in column (2) of Table 12, with a standard error of 0.016. The sample increases to 15,230 observations for 3,046 firms.

[51]Devereux and Liu (2015) also find a cash flow effect on investment for very small and new UK corporations.

[52]Long-term profile of Gross Domestic Product (GDP) in the UK, ONS, 23 August 2013. Available at: http://www.ons.gov.uk/ons/rel/elmr/explaining-economic-statistics/long-term-profile-of-gdp-in-the-uk/sty-long-term-profile-of-gdp.html

[53]Lending to UK businesses was rapidly expanding between December 2003 (when higher thresholds were announced) and December 2006, often growing at double digits yearly rates. By comparison, in the years during and following the global financial crisis (between March 2009 and December 2013), lending contracted by about 4.2 percent per year (Bank of England, 2009, 2010).

[54]In 2002 and 2003, when the bonus depreciation deduction was in force, the debt of the US corporate sector did not grow: the rate of growth was 0.1 in both years and it declined sharply by 5.4 and 1.2 per cent in 2010 and 2011 respectively when the bonus depreciation was introduced for the second time (Federal Reserve, 2004, 2012).

some firms in our sample appear to be financially constrained as indicated by a standard cash flow measure.

As a robustness check, we use more conventional cash flow measures to identify firms that are likely to be financially constrained, although Farre-Mensa and Ljungqvist (2015) find that standard measures of financial constraints[55] may not identify very well whether a firm is actually financially constrained. We adopt a triple-difference approach where we interact a dummy indicating financially constrained companies with our benchmark specification of equation (1). We assume companies are more likely to be financially constrained (a) if they had negative lagged cash flow (Table 12 - column (3)), or (b) if they had a lagged cash flow below the sample mean (Table 12 - column (4)).[56] Cash flow is defined as the sum of profits before tax plus depreciation as a share of total assets.[57] In Table 12, we find that firms classified as financially constrained do not reacted differently from firms that are unconstrained. In other literature, it is sometimes argued that smaller firms are more likely to be financially constrained and hence, firm size seems to be another indicator of firms' financial status. This does not seem to be true in our dataset as Figure 3 shows that the relationship between size and cash flow is non-monotonic. For this reason, we do not employ size as a measure of financial constraints.[58] As another approach, we identify firms in our sample that belong to a corporate group using ownership information in FAME.[59] Such firms are less likely to be financially constrained if there is an efficient internal, group capital market and hence, they should react less than stand-alone firms if the cash flow effect is important. In column (5) of Table 12 we implement a triple difference approach with a dummy for

[55]Such measures are for example, credit rating, paying dividends, cash flow measures from the financial statements, age, size and leverage or a linear combination of them as in the Kaplan-Zingales, Whited-Wu, and Hadlock-Pierce indices.

[56]Among the treated, 17 per cent of the observations have negative lagged cash flow and 48 per cent have lagged cash flow above the mean. In the control group, 12 per cent of the observations have negative lagged cash flow and 49 per cent have lagged cash flow above the mean. Mean cash flow for the sample is 0.77 and the median is very close, at 0.075 so that using the median does not change our results.

[57]See Table 2.B in Appendix B. We used the lagged value of cash flow to avoid endogeneity problems.

[58]As an indicator of financial constraints, the literature often uses the fact that a firm distributes dividends or not. In our sample, we have no information on dividend distribution.

[59]Using the ownership information available in FAME, we define companies as part of a group if they directly or indirectly own more than 50 per cent of some subsidiaries or if they are owned directly or indirectly by another corporation. If one of the entities in the group is a foreign entity, the company is classified as belonging to a multinational group. Otherwise, it belongs to a domestic group. When ownership information in FAME is not available but the company records infra-group debt or claims group relief in the tax return, it is classified as belonging to a group, although we cannot say whether it is a domestic or a multinational group. Firms without subsidiaries or corporate owners are classified as stand-alone companies. By this definition, in our sample 90 per cent of the firms (3,120) are part of a corporate group and the rest are stand-alone companies. Among the treated, 753 firms (83 per cent) are part of a group and 153 are stand-alone. For the control group, 2,367 (92 per cent) are part of a group and 200 are stand-alone.

standalone companies interacted with the benchmark specification. The coefficient β_3 remains positive, significant and close in magnitude to our benchmark specifications. The coefficient on the triple interaction (Treated * Years after reform * Stand-alone company) is negative but statistically insignificant, indicating that there is no evidence that stand-alone firms which are more likely to be financially constrained, respond more to generous capital allowances.

As discussed in Section 1, conventional indicators of financial constraints may suffer from endogeneity. We avoid such problems by using heterogeneity across firms regarding the timing of settling their actual corporation tax liability to identify the cash flow effect associated with the FYAs.

7 Bunching at qualifying thresholds and the endogeneity of the treatment status

The presence of higher FYAs available only for companies below a certain size threshold creates notches in the cost of capital.[60] To qualify for FYAs, companies may manipulate their size. If this is the case, we should observe an abnormal mass to the left of the threshold in the distributions of turnover or total assets, and missing mass to the right.[61]

This is exactly the behaviour we observe in our sample. The top panel of Figure 4 shows that companies bunched just to the left of the old turnover threshold (£11.2m) before the reform (Year 0, -1, -2) but that this excess mass disappeared after the reform (bottom panel of Figure 4). Instead, companies bunched at the new turnover threshold (£22.8m) in Year 1, 2 and 3 (bottom panel of Figure 5), though not before the reform (upper panel of Figure 5).[62] The same pattern appears for the total assets thresholds.[63] Companies bunching at the thresholds seem to have understood the tax provisions, suggesting that the FYAs were salient to them.[64]

[60]For a definition of a tax notch, see Slemrod (2013).

[61]We cannot analyse the distribution of the number of employees as the variable is missing for 90 per cent of our firm-year observations and the information is generally reported only for larger firms (above 250 employees). This implies that the distribution will not be representative of the universe of firms.

[62]The graphs above are drawn for a larger sample of companies (1,278,872) than our regression sample. With the smaller sample we employ in our regressions, we would not be able to present the graphs as the HMRC only allows us to reproduce bins containing more than 30 observations. When plotting the distribution of turnover and total assets for our regression sample, many of the bins contain less than 30 observations, especially on the right hand side of the thresholds where the missing mass is located. For a description of the sample containing 1,278,872 observations, see note i in Table 1.B in Appendix B.

[63]See Figure 1.B and 2.B in Appendix B.

[64]The same threshold is relevant for claiming R&D tax credits for SMEs. In our regression sample, the bunching firms do not claim R&D tax credits, according to their tax return.

The bunching behaviour of companies indicates possible self-selection into the treatment group. If these self-selected firms also had a higher propensity to invest regardless of the tax incentives, our estimated treatment effect would be biased upward. In the difference-in-difference analysis, to remove the possible bias, we have therefore also excluded firms that bunched below the new qualifying threshold for turnover.[65] Table 13 shows that, in this case, the estimated coefficient on the interaction between the treatment and post-reform dummies remains positive and highly significant and its magnitude is virtually the same as that in the benchmark results.[66] This implies that FYAs were effective in stimulating investment for a wider group of companies other than only affecting the behaviour of firms that were able to manage their size indicators.

8 Conclusion

Using confidential UK corporation tax return data, we analyse the effects of tax incentives on investment spending. We exploit a change in the user cost of capital for medium-size UK firms brought about by an exogenous variation in the qualifying thresholds for higher first year capital allowances. We find that access to more generous capital allowances increases firms' investment by between 2.1 and 2.6 percentage points, or 11 per cent, relative to firms that never qualified for the more generous treatment. In contrast to much of the rest of the literature, we do not find evidence of a cash flow effect and hence, this increase was due to the reduction in the user cost of capital, implying an elasticity of investment with respect to the user cost of around 8.7. This result remains robust throughout a number of tests. Although treated firms reacted to the more generous FYAs with a small time lag due to the existence of adjustment costs, they increased investment relatively quickly.

By investigating the distribution of turnover and total assets, we show that the somewhat complex UK legislation on capital allowances is salient to our medium-sized firms: companies bunch just below the qualifying thresholds on total assets and turnover. This suggests that our sample of firms understands investment tax incentives well. However, the positive effect on investment of treated firms is not driven by firms that managed to manipulate their size indicators.

[65] We define bunching as having trading turnover between £22 million and £22.8 million included at least once after the reform. This implies dropping 49 firms within the treated group (5.4 per cent of the treated).

[66] The same results are obtained if we drop companies clustering around the new threshold for total assets. For brevity, we do not report results here.

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Tables

Table 1: Rates of capital allowances for plant and machinery in first year (%)

	Small (always qualifying for FYAs)	Medium (always qualifying for FYAs)	Treated*	Large**
2001/02	40	40	25	25
2002/03	40	40	25	25
2003/04	40	40	25	25
2004/05	50	40	40	25
2005/06	40	40	40	25
2006/07	50	40	40	25
2007/08	50	40	40	25
2008/09	20	20	20	20

* Treated firms were large before the 2004/05 change in the definition of SMEs, so they could not qualify for FYAs. ** Large firms never qualified for FYAs.

Table 2: Conditions for qualifying as SME for FYAs purposes

Category	**Small**	**Medium-sized**
Before the reform: for financial years ending before 30 January 2004		
Annual turnover	not more than £2.8m	not more than £11.2m
Assets	not more than £1.4m	not more than £5.6m
Employees	not more than 50	not more than 250
After the reform: for financial years ending on or after 30 January 2004		
Annual turnover	not more than £5.6m	not more than £22.8m
Assets	not more than £2.8m	not more than £11.4m
Employees	not more than 50	not more than 250

Table 3: Corporate income tax rates (%)

Taxable income - (£)	2001/02	2002/03	2003/04	2004/05	2005/06	2006/07	2007/08	2008/09
Corporate income tax marginal rates*								
0 - 10,000	10	0	0	0	0	19	20	21
10,001 - 50,000	22.5	23.75	23.75	23.75	23.75	19	20	21
50,001 - 300,000	20	19	19	19	19	19	20	21
300,001 - 1,500,000	32.5	32.75	32.75	32.75	32.75	32.75	32.5	29.75
Over 1,500,000	30	30	30	30	30	30	30	28

*For taxable income between £300,000 and £1.5 million, the corporation tax is calculated at the main rate minus the marginal relief. The marginal relief is calculated as follows. Marginal relief = (£1.5m - Profits) x Standard Fraction. This implies that an effective marginal rate of between 19 and 32.5 per cent has been applying on profits in excess of £300,000 and below £1,500,000. The standard fraction was 1/40 in 2001/02 and 2007/08, 11/40 between 2002/03 and 2006/07 and 7/440 in 2008/09. When the starting rate was available, the marginal relief applicable on profits between £10,000 and £50,000 was 1/40 in 2001/02 and 11/40 between 2002/03 and 2006/07.

Table 4: Sample composition

	Observations	Companies	% of total companies
Treated	4,530	906	26%
Never qualifying	12,835	2,567	74%
Total	17,365	3,473	

Table 5: Descriptive statistics

	Observations	Mean	St. Dev.
Trading turnover			
TREATED	4,530	15,700,000	4,864,624
NEVER	12,835	169,000,000	828,000,000
Growth rate of turnover			
TREATED	4,530	-2.42%	0.17
NEVER	12,835	7.06%	0.18
Total assets			
TREATED	4,530	26,100,000	120,000,000
NEVER	12,835	219,000,000	4,020,000,000
Growth rate of total assets			
TREATED	4,530	-0.073%	0.632
NEVER	12,835	0.021%	0.721
Profitability			
TREATED	4,530	4.40%	0.08
NEVER	12,835	5.23%	0.09
Dummy for loss carry forward used in CT600			
TREATED	4,530	11.92%	0.32
NEVER	12,835	11.09%	0.31
Marginal tax rate			
TREATED	4,530	14.33%	0.15
NEVER	12,835	17.01%	0.15
Gross investment rate			
TREATED	4,530	18.28%	0.33
NEVER	12,835	20.45%	0.34
Perc. of positive values for gross investment rate			
TREATED	4,530	91.90%	0.27
NEVER	12,835	93.57%	0.25
Tax component of the user cost of capital			
TREATED	4,530	32.18%	0.010
NEVER	12,835	32.47%	0.027
Growth rate of the tax component of the user's cost of capital			
TREATED	4,026	-0.089%	0.019
NEVER	11,267	0.039%	0.021
Perc. of observations with zero taxable income			
TREATED	4,530	46.60%	0.50
NEVER	12,835	40.48%	0.49
Cash flow			
TREATED	4,407	0.063%	0.305
NEVER	12,527	0.082%	0.306

(i) Variables are defined in Table 2.B in Appendix B.

Table 6: Gross investment rate

	Mean	St. Dev.	**Mean**	St. Dev.	t-test	P(T<t)	DF
	Non-qualifying years		Qualifying years				
			TREATED				
	0.1692	0.3005	**0.1919**	0.3497	-2.2581	[0.0120]	4,528
Observations	1,812		2,718				
			NEVER QUALIFYING				
	0.2032	0.3370	**0.2054**	0.3370	-0.3563	[0.3608]	12,883
Observations	5,134		7,701				

(i) In the t-test for the equality of means, H_0: mean(non-qualifying years) - mean(qualifying years) = 0. H_a: mean(non-qualifying years) - mean(qualifying years) < 0. DF: degrees of freedom.

Table 7: Benchmark specifications

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dep var: $\frac{I_t}{K_{t-1}}$ - gross investment rate	OLS					FE			
Treated group - dummy	-0.034***	-0.019**	-0.019**	-0.018*	-0.018*				
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)				
Treatment dummy * dummy for years after reform	0.021**	0.023**	0.023**	0.023**	0.023**	0.021**	0.022**	0.022**	0.023**
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)
Dummy for years after reform	0.002	0.001				0.002	0.001		
	(0.005)	(0.005)				(0.005)	(0.005)		
Rate of growth of turnover		0.168***	0.168***	0.161***	0.160***		0.111***	0.112***	0.109***
		(0.018)	(0.018)	(0.018)	(0.018)		(0.019)	(0.019)	(0.019)
Year dummies			Yes	Yes				Yes	
Sector dummies				Yes					
Sector*year dummies					Yes				Yes
Constant	0.203***	0.192***	0.191***	0.372***	0.302***	0.194***	0.190***	0.191***	0.282***
	(0.006)	(0.005)	(0.006)	(0.005)	(0.056)	(0.003)	(0.003)	(0.005)	(0.057)
R-squared	0.001	0.009	0.009	0.043	0.048	0.001	0.005	0.005	0.014
Observations	17,365	17,365	17,365	17,365	17,365	17,365	17,365	17,365	17,365
Number of companies	3,473	3,473	3,473	3,473	3,473	3,473	3,473	3,473	3,473

(i) Standard errors in parentheses and robust to heteroskedasticity and within-firm serial correlation. (ii) ***, **, * significant at 1%, 5% and 10% respectively. (iii) For columns (6) to (9), the R-squared is the within R-squared. (iv) For the treatment group, the number of observations is 4,530 and the number of companies is 906. For the group of firms never qualifying, the number of observations is 12,835 and the number of firms is 2,567.

Table 8: Benchmark specifications, controlling for observables

	(1)	(2)	(3)	(4)	(5)
Dep var: $\frac{I_t}{K_{t-1}}$ - gross investment rate	OLS				FE
Treated group - dummy	-0.018*	-0.019*	-0.019*	-0.018*	
	(0.010)	(0.010)	(0.010)	(0.010)	
Treatment dummy * dummy for years after reform	0.024**	0.024**	0.026**	0.026**	0.025**
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)
Rate of growth of turnover	0.157***	0.149***	0.165***	0.161***	0.114***
	(0.018)	(0.018)	(0.018)	(0.018)	(0.020)
Marginal tax rate	0.036	-0.065**	-0.087**	-0.087**	-0.042*
	(0.023)	(0.026)	(0.025)	(0.025)	(0.024)
Profitability		0.323***			
		(0.053)			
Lagged profitability			0.470***	0.467***	0.300***
			(0.055)	(0.055)	(0.060)
Rate of growth of total assets				0.008	0.015***
				(0.005)	(0.006)
Sector*year dummies	Yes	Yes	Yes	Yes	Yes
Constant	0.454***	0.208***	0.369***	0.041	0.229***
	(0.167)	(0.016)	(0.014)	(0.218)	(0.057)
R-squared	0.048	0.053	0.061	0.062	0.020
Observations	17,365	17,365	17,365	17,365	17,365
Number of companies	3,473	3,473	3,473	3,473	3,473

(i) Standard errors in parentheses, robust to heteroskedasticity and within-firm serial correlation.
(ii) ***, **, * significant at 1%, 5% and 10%. (iii) For column (5), the R-squared is the within R-squared.
(iv) For the treatment group, the number of observations is 4,530 and the number of companies is 906. For the group of firms never qualifying, the number of observations is 12,835 and the number of firms is 2,567.

Table 9: Extensive margin (marginal effects)

	(1)	(2)	(3)	(4)	(5)	(6)
Dep var: dummy = 1 if gross investment rate > 0 (0 otherwise)	Probit				Logit	LPM
Treated group - dummy	-0.024***	-0.024***	-0.024***	-0.024***	-0.015**	-0.015**
	(0.009)	(0.009)	(0.009)	(0.009)	(0.006)	(0.006)
Treatment dummy * dummy for years after reform	0.004	0.004	0.005	0.005	0.006	0.006
	(0.007)	(0.007)	(0.007)	(0.007)	(0.005)	(0.008)
Rate of growth of turnover	0.006	0.006	0.006	0.006	0.009	0.010
	(0.012)	(0.012)	(0.012)	(0.012)	(0.009)	(0.012)
Marginal tax rate		-0.003	-0.013	-0.013	-0.007	-0.007
		(0.018)	(0.019)	(0.019)	(0.012)	(0.014)
Lagged profitability			0.041	0.040	0.025	0.026
			(0.028)	(0.028)	(0.019)	(0.019)
Rate of growth of total assets				0.001	0.0001	0.001
				(0.001)	(0.001)	(0.002)
Sector*year dummies	Yes	Yes	Yes	Yes	Yes	Yes
Pseudo R-squared	0.0325	0.0325	0.0329	0.0329	0.2686	0.252
Log likelihood	-4192.61	-4192.58	-4191.05	-4190.93	-3121.80	-3121.80
Observations	17,230	17,230	17,230	17,230	16,340	17,365
Number of companies	3,446	3,446	3,446	3,446	3,446	3.473

(i) Marginal effects reported. (ii) Standard errors in parentheses, robust to heteroskedasticity and within-firm serial correlation.
(iii) ***, **, * significant at 1%, 5% and 10%. (iv) The dependent variable is a dummy taking value one if investment is larger than zero.

Table 10: Timing: companies with different end date of accounts

	(1)	(2)	(3)	(4)
Dep var: $\frac{I_t}{K_{t-1}}$ - gross investment rate	Smaller sample (balanced)		Larger sample (not balanced after reform)	
	FE			
End of accounts	Jan-June	July-Dec	Jan-June	July-Dec
Treatment dummy * dummy for qualifying year 1	0.0001	0.036**	0.005	0.020**
	(0.035)	(0.014)	(0.031)	(0.009)
Treatment dummy * dummy for qualifying year 2	0.053	0.012	0.076**	-0.005
	(0.035)	(0.015)	(0.039)	(0.008)
Treatment dummy * dummy for qualifying year 3	0.086**	0.024	0.076**	0.026***
	(0.037)	(0.017)	(0.033)	(0.010)
Rate of growth of turnover	0.182***	0.156***	0.154***	0.116***
	(0.050)	(0.020)	(0.041)	(0.011)
Marginal tax rate	-0.053	-0.091***	-0.027	-0.012
	(0.078)	(0.026)	(0.069)	(0.017)
Lagged profitability	0.414***	0.479***	0.432***	0.269***
	(0.014)	(0.005)	(0.014)	(0.0001)
Rate of growth of total assets	0.024*	0.007	0.025*	0.0001
	(0.014)	(0.005)	(0.014)	(0.0001)
Sector*year dummies	Yes	Yes	Yes	Yes
Constant	0.359***	-0.080***	0.207***	0.296
	(0.038)	(0.017)	(0.020)	(627)
R-squared	0.097	0.065	0.099	0.073
Observations	2,135	15,230	2,448	17,811
Treated	630	3,900	746	4,650
Never qualifying	1,505	11,330	1,702	13,161
Number of companies	427	3,046	500	3,712
Treated	126	780	152	971
Never qualifying	301	2,266	348	2,741

(i) Within-groups estimator used. Standard errors in parentheses and robust to heteroskedasticity and within-firm serial correlation. (ii) ***, **, * significant at 1%, 5% and 10% respectively.
(iii) In column (1) and (3), the sample contains only companies with year-end between January and June included. In column (2) and (4), the sample contains only companies with year-end between July and December included.

Table 11: Robustness checks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Drop top 5%	Drop top 50%	PSM	Using turnover & total assets	December year-end companies		Dep. var: △ln(fixed assets)	Longer averages	Placebo I	Placebo II
Dep var: $\frac{I_t}{K_{t-1}}$ - gross investment rate	FE									
Treatment * Years after reform	0.023**	0.024*	0.024**	0.053***	0.044***	0.044***	0.024**	0.029***	0.006	-0.018
	(0.011)	(0.013)	(0.010)	(0.015)	(0.016)	(0.016)	(0.011)	(0.007)	(0.011)	(0.011)
Treatment * December year-end * Years after reform					-0.027					
					(0.020)					
December year-end * Years after reform					0.022*					
					(0.012)					
Treatment * December year-end * Year 0						-0.010				
						(0.017)				
Treatment * December year-end * Year 1						0.008				
						(0.017)				
Rate of growth of turnover	0.116***	0.123***	0.112***	0.098***	0.113***	0.113***	0.084**	0.0001	0.119***	0.105***
	(0.020)	(0.028)	(0.020)	(0.018)	(0.020)	(0.020)	(0.017)	(0.000)	(0.020)	(0.017)
Lagged profitability	0.299***	0.371***	0.300***	0.287***	0.300***	0.299***	0.257***	0.207***	0.269***	0.198***
	(0.061)	(0.078)	(0.061)	(0.067)	(0.060)	(0.060)	(0.061)	(0.034)	(0.058)	(0.043)
Marginal tax rate	-0.044*	-0.063*	-0.015***	0.034	-0.042*	-0.042*	-0.057**	0.014	-0.086***	-0.053**
	(0.025)	(0.034)	(0.006)	(0.026)	(0.024)	(0.024)	(0.028)	(0.014)	(0.028)	(0.023)
Rate of growth of total assets	0.015**	0.012***		0.016**	0.015***	0.015***	0.334***	0.001	0.025*	0.014***
	(0.006)	(0.005)		(0.007)	(0.006)	(0.006)	(0.021)	(0.001)	(0.014)	(0.005)
Sector*year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	-0.099***	0.289***	0.166***	0.279***	0.314***	0.204***	-0.108***	0.025	0.080***	0.141***
	(0.012)	(0.012)	(0.017)	(0.010)	(0.083)	(0.034)	(0.035)	(.)	(0.030)	(0.036)
R-squared	0.020	0.028	0.019	0.023	0.020	0.020	0.085	0.033	0.021	0.028
Observations	16,724	10,948	17,020	13,418	17,365		16,280	25,222	19,845	12.580
Treated	4,530	4,530	4,505	1,185	4,530		4,374	6,553	6,650	2,000
Never qualifying	12,194	6,418	12,515	12,233	12,365		11,906	18,669	12,835	10,580
Number of companies	16,724	10,948	3,404	2,733	3,473		3,472	3,473	3,897	2,516
Treated	906	906	901	237	906		906	906	1,330	400
Never qualifying	2,472	1,481	2,503	2,496	2,567		2,566	2,567	2,567	2,116

(i) Within-groups estimator used. Standard errors in parentheses and robust to heteroskedasticity and within-firm serial correlation.
(ii) ***, **, * significant at 1%, 5% and 10% respectively. (iii) In column (1) we drop the top 5% in terms of total assets of the companies never qualifying, in column (2) the top 50%. The percentiles refer to the distribution of total assets of the companies never qualifying. In column (3), we use a propensity score matching (PSM). In column (4) the treatment and control group are classified using both total assets and turnover. In column (5) and (6), we employ the sample used in the benchmark specification. In column (7), the dependent variable is ln(fixed assets). In column (8), we use longer averages: the sample includes Year -1 to Year 6, instead of Year -1 to Year 3. In column (9), we run the first placebo exercise where medium-sized firms are used as treatment group. In column (10), we run the second placebo exercise where the treated are defined as firms with total assets above £11.2m for every year before the reform and with total assets between £45.6m and £91.2m every year after the reform.

Table 12: Testing for the cash flow effect

Dep var: $\frac{I_t}{K_{t-1}}$ - gross investment rate	(1)	(2)	(3)	(4)	(5)
	In arrears in Year 1	Always in arrears	Negative cash flow	Cash flow above mean	Ownership structure
			FE		
Treatment dummy * Years after reform			0.025**	0.033*	0.027**
			(0.011)	(0.014)	(0.011)
Treatment dummy * dummy for qualifying Year 1	0.040*	0.037**			
	(0.016)	(0.017)			
Treatment dummy * dummy for qualifying Year 2	0.019	0.020			
	(0.017)	(0.018)			
Treatment dummy * dummy for qualifying Year 3	0.035*	0.039**			
	(0.018)	(0.019)			
Rate of growth of turnover	0.125***	0.142***	0.115***	0.115***	0.114***
	(0.024)	(0.028)	(0.020)	(0.020)	(0.020)
Marginal tax rate	-0.052*	-0.098***	-0.049*	-0.046***	-0.042*
	(0.027)	(0.031)	(0.024)	(0.025)	(0.024)
Lagged profitability	0.317***	0.426***	0.291***	0.295***	0.299***
	(0.088)	(0.146)	(0.060)	(0.061)	(0.060)
Rate of growth of total assets	0.013**	0.025*	0.016***	0.016**	0.016***
	(0.005)	(0.015)	(0.006)	(0.007)	(0.006)
Treatment dummy * Years after reform* Neg.lagged cash flow			0.012		
			(0.026)		
Treatment dummy * Years after reform* Lagged cash flow above mean				-0.014	
				(0.020)	
Treatment dummy * Years after reform* Stand-alone company					-0.026
					(0.030)
Neg. lagged cash flow			-0.033**		
			(0.016)		
Lagged cash flow above mean				0.010	
				(0.010)	
Neg. lagged cash flow - dummy * Treatment dummy			0.0001		
			(0.027)		
Lagged cash flow above mean * Treatment dummy				-0.005	
				(0.020)	
Years after reform * Neg. lagged cash flow			0.025**		
			(0.011)		
Years after reform * Lagged cash flow above mean				0.033**	
				(0.014)	
Years after reform * Stand-alone company					0.028
					(0.020)
Observations	12,145	10,100	16,971	16,971	17,365
Treated	3,670	3,395	4,424	4,424	4,530
Never qualifying	8,475	6,705	12,547	12,547	12,835
Number of companies	2,429	2,020	3,449	3,449	3,473
Treated	734	679	901	901	906
Never qualifying	1,695	1,341	2,548	2,548	2,567

(i) Within-groups estimator used. Standard errors in parentheses and robust to heteroskedasticity and within-firm serial correlation.
(ii) ***, **, * significant at 1%, 5% and 10% respectively. (iii) Sector-year dummies used. (iv) In columns (1) and (2), companies have year-end between July and December. In column (1) all companies pay in arrears in qualifying year 1. In column (2), all companies always paying in arrears. (v) In column (3) we run a triple difference specification with dummy = 1 if cash flow is negative (0 otherwise). In column (4), we run a triple difference specification where we interact a dummy = 1 if cash flow is above the mean cash flow (0 otherwise). Cash flow: (profit before tax + depreciation)/total assets (see Table 2.B). (vi) In column (3) and (4), among the treated, 741 observations have negative cash flow (17%) and 2,121 have cash flow above the mean (48%). In the control group, 1,546 observations have negative lagged cash flow (12%) and 6,113 observations have lagged cash flow above the mean (49%). (vii) In column (5), stand-alone companies are 353 and firms part of a group 3,120. For the treated, stand-alone companies are 17% of the sample (8% for the control group.

Table 13: Regressions excluding companies bunching at turnover thresholds

	(1)	(2)	(3)
Dep var: $\frac{I_t}{K_{t-1}}$ - gross investment rate	OLS		FE
Treated group - dummy	-0.037***	-0.021**	
	(0.010)	(0.010)	
Treatment dummy * dummy for years after reform	0.020**	0.026**	0.025**
	(0.010)	(0.011)	(0.011)
Rate of growth of turnover		0.160***	0.109***
		(0.018)	(0.019)
Marginal tax rate		-0.086***	-0.045*
		(0.025)	(0.024)
Lagged profitability		0.474***	0.303***
		(0.055)	(0.061)
Rate of growth of total assets		0.008	0.015***
		(0.005)	(0.006)
Dummy - years after reform	0.002		
	(0.005)		
Sector*year dummies		Yes	Yes
Constant	0.203***	0.300**	0.164***
	(0.006)	(0.144)	(0.034)
R-squared	0.001	0.064	0.020
Observations	17,120	17,120	17,120
Number of companies	3,424	3,424	3,424

(i) Standard errors in parentheses and robust to heteroskedasticity and within-firm serial correlation. (ii) ***, **, * significant at 1%, 5% and 10% respectively.
(iii) In all columns, companies bunching at the turnover threshold are dropped. These are companies with trading turnover between £22m and £22.8m at least once in Year 1, 2 or 3 are dropped. (iii) In column (1) and (2), we use an OLS estimator. In column (3), we use a within-groups estimator. For columns (3), the R-squared is the within R-squared.

Figures

Figure 1: Average gross investment rate (I_t/K_{t-1})

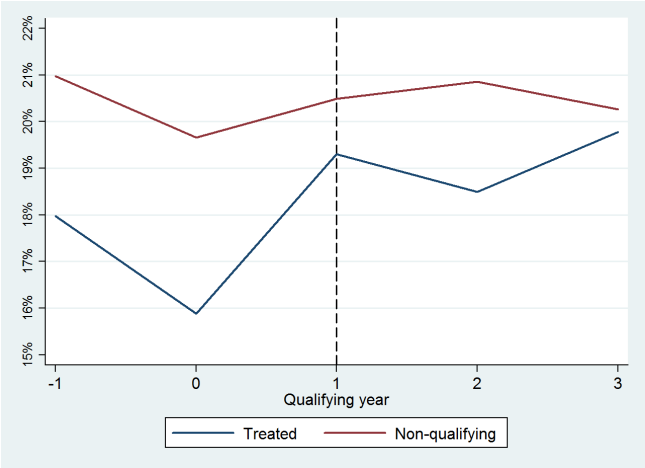


Figure 2: Average gross investment rate for December and non-December firms

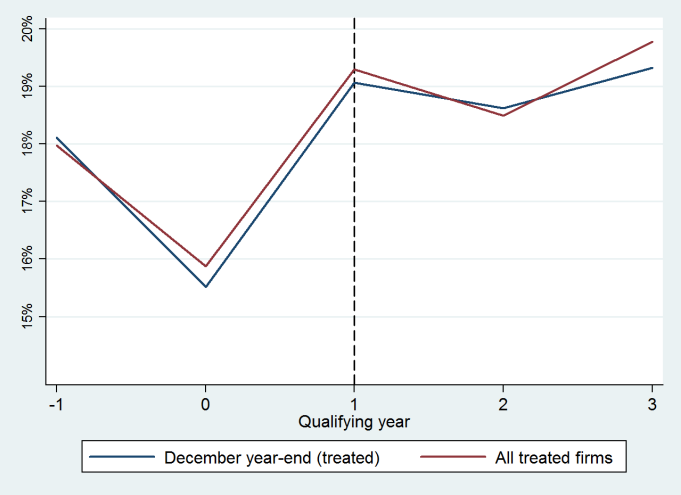
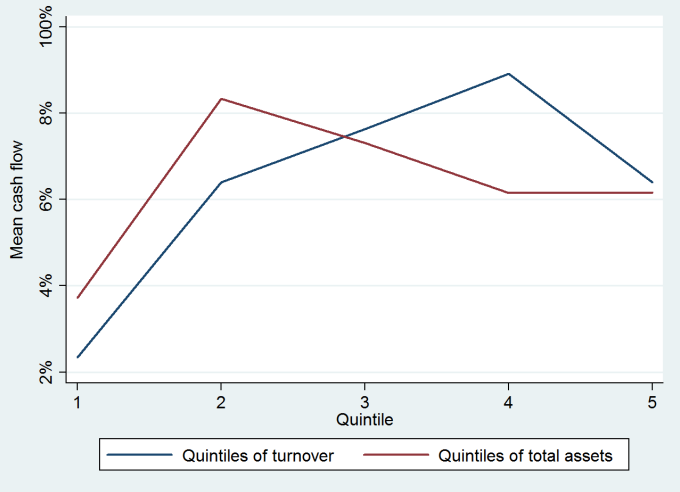


Figure 3: Mean cash flow, by quintile (treated only)



Note: cash flow calculated as defined in Table 2.B in Appendix B.

Figure 4: Distribution of turnover around old thresholds (between £10m and £12m)

Figure 5: Distribution of turnover around new thresholds (between £20m and £24m)

Appendix A - User cost of capital and elasticities

A.1 The user cost of capital

A standard definition of the user cost of capital, for investment financed by retained earnings, is[67]

$$c = \frac{(1-A)}{(1-\tau)}(r+\delta) \tag{2}$$

where A is the present value of the tax saving due to capital allowances for a unit increase in investment, τ is the marginal corporate tax rate (30%), r is the interest rate which we assume is 7% and δ is the economic rate of depreciation which we assume is 15%. $\frac{(1-A)}{(1-\tau)}$ is the tax component of the user cost of capital. $(r+\delta)$ is the price component of the user cost of capital.

Before the reform, the rate of tax depreciation is $d = 0.25$:

$$A = \tau d\{1 + \frac{1-d}{1+r} + (\frac{1-d}{1+r})^2 + (\frac{1-d}{1+r})^3 + ...\} = \frac{\tau d(1+r)}{(r+d)} = 0.8359\tau$$

In the case in which the asset receives an allowance of $f = 40\%$ in the first year, and then subsequently d on a declining balance basis, then

$$\begin{aligned} A &= \tau f + \frac{\tau d(1-f)}{1+r}\left\{1 + \left(\frac{1-d}{1+r}\right) + \left(\frac{1-d}{1+r}\right)^2 + \left(\frac{1-d}{1+r}\right)^3 + ...\right\} \\ &= \tau f + \frac{\tau d(1-f)}{r+d} = 0.8688\tau \end{aligned} \tag{3}$$

$$dA = 0.0328\tau; \quad c = 0.2354; \quad dc = -0.0031; \quad \frac{dc}{c} = -0.0131.$$

Our baseline result is $\frac{dI_t}{K_{t-1}} = 0.021$. The average investment rate for the treated is 0.1828, so that $d(\frac{I_t}{K_{t-1}})/\frac{I_t}{K_{t-1}} = 0.1149$. This implies that the elasticity of investment rate with respect to the user cost of capital is $e = -8.74$. For given $K_{(t-1)}$, the change in investment is equal to the change in the capital stock: $dI_t = dK_t$ so that the elasticity of

[67] See, for example, Devereux and Griffith (2003).

capital stock to the user costs is $E = \frac{dI_t}{dc} * \frac{c}{K_t} = e\frac{I_t}{K_t} = -1.598$

A.2 Anticipation effect

If the company has a December year-end, does ti have an incentive to postpone investment from December 2003 to January 2004? Assume discounting in the first year and $r = 7\%$

Option 1. The company declares £1 of investment in December 2003. The present value of allowances is approximately

$$A = \frac{0.25}{r + 0.25} = 0.78125 \tag{4}$$

Option 2. The company declares £1 of investment in January 2004. The company enjoys FYAs but no additional year of discounting. The present value of allowances is approximately

$$A = \frac{0.4}{(1+r)^2} + \frac{(1-0.4)}{(1+r)^2} * \frac{0.25}{r + 0.25} = 0.7588 \tag{5}$$

The fact that (3) is larger than (4) suggests no incentive to delay.

Appendix B

Table 1.B: Building the sample

	(1)	(2)
	Observations	Companies
Firms classifiable as small, medium, treated or never qualifying (2002/03-2006/07)	3,045,890	452,196
Drop if fixed assets zero or negative	(158,051)	
Remaining	2,887,839	448,076
Drop if only observations before or only after reform	(629,764)	
Remaining	2,258,075	332,655
Drop if only one observation before or after reform	(917,514)	
Remaining	1,340,561	190,293
Drop if firms changes month of end of accounts	(61,689)	
Remaining[i]	1,278,872[i]	181,333
Keep if medium, treated or never qualifying (drop small firms)	(1,238,927)	
Remaining	39,945	5,713
Drop if less than 5 observations between Year -1 and 3	(3,378)	
Remaining	36,567	5,133
Drop outliers	(12,552)	
Remaining	24,015	4,803

(i) The analysis of bunching in Figures 3 and 4 (and 1.B and 2.B) is derived on the sample with 1,278,872 observations.

Table 2.B: Definition of variables

	Variable	Box in tax return	FAME line
Investment	Gross investment rate ($\frac{I_{it}}{K_{i(t-1)}}$)		
constructed using	Qualifying expenditure for FYAs (I_{it})	118 & 121[i]	
	Fixed assets ($K_{i(t-1)}$)		37
Output	Rate of growth of turnover $\frac{turn_{it}-turn_{i(t-1)}}{turn_{it}}$	1	
Profitability	$\frac{Inc_{it}}{TA_{it}}$		
constructed using	Taxable income (Inc_{it})	37	
	Total assets (TA_{it})		70
Marginal tax rate			
constructed using	Taxable income (Inc_{it})	37	
Growth	Rate of growth of total assets $\frac{TA_{it}-TA_{i(t-1)}}{TA_{it}}$		70
Investment (FAME)	Rate of growth of fixed assets ($\ln(K_{it}) - \ln(K_{i(t-1)})$)		
constructed using	Fixed assets (K_{it})		37
Cash flow	$\frac{(P\&L\ after\ tax+depreciation)_{it}}{TA_{it}}$		
constructed using	P&L after tax		16
	Depreciations		21
	Total assets (TA_{it})		70
Tax payment method			
constructed using	Taxable income (Inc_{it} and $Inc_{i(t-1)}$)	37	
Sector	UK SIC 2007	First 2 digits	

(i) Box 118 contains the total expenditure for investment in plant and machinery incurred in the accounting period on which FYAs are claimed. Box 121 contains the total expenditure on machinery and plant but excludes any amount entered in box 118. For firms in the control group (never qualifying), I_{it} is calculated as box 121. For firms in the treatment group, I_{it} is calculated as the sum of box 118 and box 121. The reason is the following. FYAs were not granted for all plant and machinery (for example, cars and other miscellaneous items were excluded). From the tax return and for the periods before the reform, it is not possible to distinguish plant and machinery for which FYAs were granted from the total pool of plant and machinery recorded in box 121.

Table 3.B: Logit regression for propensity score

Dep var: dummy = 1 if companies is treated	Logit
Rate of growth of turnover	- 6.627***
	(0.475)
Ln(total assets)	-1.748***
	(0.099)
Profitability	-2.574***
	(0.621)
Sector dummies	Yes
Constant	29.069***
	(1.724)
Log-likelihood	-2503.05
Pseudo R2	0.3674
Observations	6,848

(i) Standard errors in parentheses and robust to heteroskedasticity and within-firm serial correlation.
(ii) ***, **, * significant at 1%, 5% and 10% respectively.
(iii) The number of observations is small because the matching is carried out on an average of pre-treatment variables. We have collapsed the pre-treatment sample to do so.

Table 4.B: Test of balancing property of kernel matching

	Treated	Never	%bias	t	$p > t$
Rate of growth of turnover	-0.020	-0.035	11.2	3.25	0.001
Ln(total assets)	16.288	16.361	-6.7	-2.57	0.061
Profitability	0.04861	0.0475	1.3	0.42	0.674

(i) We carried out the tests reported in this table using the Stata command -*pstest*. (ii) We have included also sector dummies, for all of them the null hypothesis of equality of means cannot be reject at levels below 5 per cent.

B.2 Figures

Figure 1.B: Distribution of total assets around old thresholds (£4m - £6m)

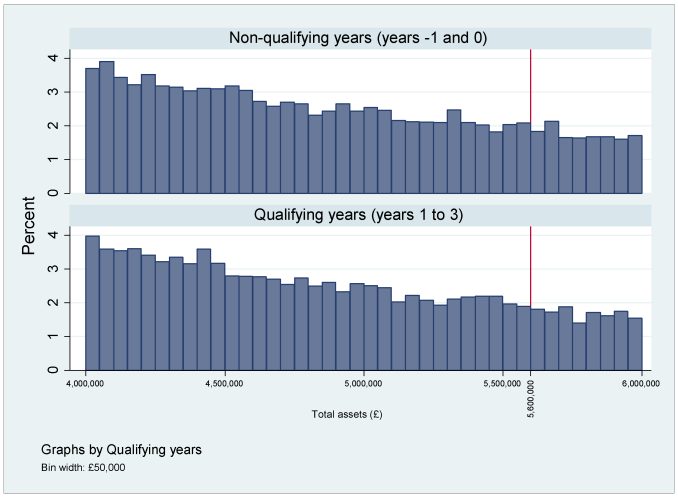


Figure 2.B: Distribution of total assets around new thresholds (£10m - £12m)