Do Dividend Taxes Affect Firm Behaviour? Evidence from Publicly-Listed OECD Firms

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Abstract

I study the impact of the US 2003 dividend tax cuts on listed firms' payout and investment behaviour. In 2000, listed US firms' investment constituted close to 20% of US Gross Fixed Capital Formation. However, consensus on the payout and investment response of listed firms to dividend taxes remains limited. Thus, studying the impact of a reduction in dividend taxation on publicly traded firms' investment and payout behaviour is paramount. Using consolidated company accounts of a panel of large listed firms from OECD countries between 1997 & 2006 and a matched difference-in-difference quasi-experimental design that accounts for the international ownership of firms, I document the causal effect of the 2003 US dividend tax cut on US corporate financial and investment behaviour. In line with the literature on non-listed firms, I find a null investment response. However, the null investment response goes against suggestive evidence previously reported in the literature for listed firms. Additionally contrary to the previous literature on listed firms, I find a null payout response. I document evidence for two channels that give rise to the null responses. The first is a financing channel previously discussed in the literature. The second is an newly documented international ownership of listed US corporations channel, using unique data on the residence of corporate owners.

Keywords: Capital Income Taxation; Dividends; Investment; Firm Behaviour.

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1 Introduction

Publicly-listed US corporations invest close to 20% of US Gross Fixed Capital formation.[1] This accounts for most of US corporate investment (Asker et al. 2015). Thus quantifying the efficiency distortions created by the tax system at this large economic margin is important. Academic debate on the efficiency of dividend taxation of listed firms is yet to arrive at a consensus. Theoretically, the effect of dividend taxation on corporate investment and payouts is ambiguous (King 1977, Poterba & Summers 1984).[2] Additionally, given that shareholders of listed firms are usually at the top of the income distribution, the taxation of profit distributions of listed firms can serve as an efficient redistribution tool if the firms face limited distortions as a result of this tax (Saez & Zucman 2022).

In this paper, I document the impact of the 2003 dividend tax cuts on US listed firms' investment and payouts. Introduced by the Bush administration, the temporary and unanticipated tax cuts promised to raise growth and investment (Auerbach & Hassett 2006).[3] According to the logic of the policymakers, the financing constraint for equity-dependent firms would be relaxed as a result of the reform, assuming that US domiciled firms respond to the US shareholders dividend tax incentives. If however, firms are not equity dependent and finance their investments using retained earnings, then we would not necessarily expect an investment response.[4] Subsequently, several academic and public writings concluded that US listed firms increased their dividend payouts, for example Microsoft. These results suggest that the reform succeeded in channelling trapped equity to productive firms (Chetty & Saez 2006). However, the challenge is to identify the causal effect of the dividend tax cuts on listed firms' behaviour by constructing a control group of similar firms that were unaffected by the US dividend tax cuts. I take up this challenge here by carefully constructing a control group that consists of non-US OECD listed firms around the 2003 tax cuts using

[1] Based on the sample of listed firms in this paper and $2.38 trillion dollars of Gross Fixed Capital formation in the year 2000.

[2] The ambiguity in standard neoclassical models of the firm stems because of the marginal source of financing in the firm. Firms that finance investment through retained earnings face no economic distortion and hence do not change their behaviour in response to a permanent tax change.

[3] These tax cuts were later extended multiple times before being made largely permanent by 2013.

[4] The temporary nature of the US dividend tax cuts may still lead to additional investment distortions (Korinek & Stiglitz 2009).

a semi-parametric difference-in-difference design.

There are two main innovations in this paper. Firstly, I compare the investment and payout behaviour of US publicly-listed firms to publicly-listed firms in other OECD countries over the period of the US dividend tax cuts. I focus on a subset of US listed firms which can be well matched with non-US firms with similar observed characteristics (size, age, employment and sector). This innovation implies that my empirical strategy is better than previous papers on listed corporations like Chetty & Saez (2005), Gourio & Miao (2010). Secondly, using unique data on the residence of owners of listed firms, I comment on two previously unstudied channels: the international spill-over effect of US dividend tax policy on non-US firms and the international ownership channel of the dividend tax irrelevance. I do this while highlighting the standard financing argument for the irrelevance of dividend taxation.

Using a semi-parametric difference-in-difference design and a panel of consolidated accounts of listed US and OECD firms between 1997 and 2006 from COMPUSTAT, I show that the elasticity of total payouts with respect to the net-of-tax rate is a statistically insignificant -0.27. The upper bound on my estimated elasticity is 0.19. This upper bound on the elasticity is less than half of the 0.5 that was previously documented in the literature. The previous estimates rely on time series comparisons with limited control groups (Chetty & Saez 2005). The elasticity of investment with respect to the net-of-tax rate is a statistically insignificant -0.08. This investment response is consistent with evidence for a sample of smaller privately-held US firms (Yagan 2015). Although my results differ from literature on publicly-listed firms (Chetty & Saez 2005, Gourio & Miao 2010), I document the trend reversal in US corporate payouts and investment around 2003 that is noted in the literature. However, this trend reversal was closely mimicked by comparable publicly-traded firms in other OECD countries, suggesting an important role for the global business cycle. Hence, based on my result, it is a stretch to attribute this trend reversal to changes in the US tax code. These results imply that US listed firms' investment behaviour is not distorted by dividend taxation.

Importantly, the difference-in-difference design in this paper invokes a parallel trends assumption that does not rely on the randomness of selection into the treatment (Abadie 2005). Instead, I assume that, had the US dividend tax cuts not taken place, US firms'

investment and total payouts would have conditionally trended similarly to their non-US OECD counterparts. I satisfy a test of whether this assumption holds for the pre-reform period. This assumption is theoretically defensible since the rebalancing exercise corrects for industry and size differences between US and non-US firms. This accounts for static differences between firms in both groups that influence the dynamic evolution of their investment and payouts. Additionally, the matching exercise preserves the cyclical nature of investment and total payouts across both US and non-US firms. Importantly, the matched firms are of a similar size, compete in the same markets, and their international nature implies that their organisational structures are similar. With the exception of Japan, no large changes occurred in the tax treatment of dividends in my control countries during the sample period.[5]

This paper is motivated by and informs multiple literatures related to dividend tax policy, corporate finance, and international ownership of equity. I explain four motivations for the paper.

Firstly, previous work on the behaviour of listed US firms in response to the 2003 dividend tax cuts found a large payout elasticity. Poterba (2004), using time-series evidence before the reform, estimates that the dividend tax cuts can potentially pay for themselves. Chetty & Saez (2005) use an event study comparing corporate payouts before and after the 2003 dividend tax cuts to assess the impact of the reform. They then corroborate their findings using a control group of US firms that are substantially owned by non-taxable institutions. However, firms owned by non-taxable institutions tend to be less volatile over the business cycle (Gompers & Metrick 2001). Hence they do not constitute a valid control group given that this period immediately follows the end of the dotcom bubble. I construct a control group to document the causal impact of the tax reform on dividend payouts and investments.

Secondly, another line of work focusses on the effect of the dividend tax cuts on smaller privately-held US firms using an internally valid research design with S-corporations, which are taxed on a pass-through basis at the personal income tax rate, as a control group for privately-held C-corporations (Yagan 2015). However, given the size distribution of the control group, this evidence is only "suggestive" for the sample of large publicly-listed US firms

[5]I describe how I deal with dividend tax changes in other OECD countries later in the paper.

studied here. Documenting the behavioural response of listed US firms to the 2003 dividend tax cuts is the main contribution of this paper. Note that the size of the median firm in my sample is close to double that of the 90th percentile firm in Yagan (2015). As such, the behaviour of the larger publicly-listed firms is more important for understanding the macroeconomic implications of US dividend taxes. Studying this sample thus allows us to understand the aggregate implications of the tax cuts. Separately, the incentives affecting listed corporations are different compared to unlisted corporations. There is a larger mismatch between owners and managers in listed firms compared to smaller unlisted firms. This might imply a reduced incentive for the managers to payout profits to shareholders in listed firms. Listing also allows access to international investors with a larger set of demands, that are unrelated to the US tax code. In light of these considerations, it is surprising that the best available evidence for listed firms documents a significant dividends response as highlighted above Poterba (2004), Chetty & Saez (2010). This paper reassesses this surprising finding, using an arguably better constructed control group.

Thirdly, an additional motivation for this paper comes from the home equity bias literature (Coeurdacier & Rey 2013). This literature casts doubt on the assumption of perfect international capital mobility. If that literature is a relatively more accurate representation of reality, I should observe that US domestic dividend taxes levied on shareholders of US firms should matter for US corporate decisions more than non-US corporate decisions.

A final motivation of the paper is to delineate between two potentially relevant arguments for understanding the behavioural response of firms to dividend taxation in an increasingly globalised world. If the stated aim of the policy is to increase US investments, jobs, and growth, a reduction in the dividend tax might not affect US listed firms' decisions because their owners are not necessarily US tax residents. Hence, those non-US owners have no incentive for the firm to increase investments or payouts. I call this the ownership channel for why dividend taxes might not matter. This channel is much more important in my sample of listed firms compared to smaller privately-held firms in Yagan (2015). Additionally, there is the standard argument discussed in the literature related to the marginal source of financing. I call this the financing argument for why dividend taxes might not matter.

Additional results in the paper also show that there is a small increase in payouts imme-

diately after the reform for one year. This is in contrast with the literature on smaller US firms, which finds a positive statistically significant effect on dividend payouts for multiple years after the tax cuts until 2008 (Yagan 2015). This might be explained by bigger firms updating their beliefs about the permanency of the dividend tax cuts after the re-election of the Bush administration quicker relative to smaller firms.

I also show that the most equity dependent US listed firms do not respond to the dividend tax cuts, suggesting that these firms are too internationally owned to change their payout and investment policy in response to the dividend tax cuts. In the data, the mean foreign ownership relative to reported outstanding shares of these equity-dependent US domiciled firms is 23%. Furthermore, the most domestically owned US listed firms do not respond to the dividend tax cuts, suggesting that these might be firms financing their investments through retained earnings. In the data, the mean cash-holdings as a fraction of total assets is on average higher for the most US domiciled domestically-owned firms relative to the the least US domiciled domestically-owned firms.[6]

When using the fraction of US resident owners in March 2003 as the measure of treatment intensity instead, I find that the most US-owned firms (regardless of firm domicile) do not differentially increase their dividend payouts or investments compared to less US-owned firms in the period after the reform relative to before the reform. This suggests that listed firms in this sample are predominantly new view firms, financing investment through retained earnings. Note that both the identification strategies adopted in the paper are interesting and answer slightly different questions. Firm domicile-based treatment (adopted in the main result of the paper) asks whether US corporate investment and payouts responded to the tax policy. This is a test of the stated aim of "Bush tax cuts," namely to spur investment and growth. The second identification strategy asks whether US-owned firms potentially responded, abstracting from the international ownership irrelevance of dividend taxes.

Taken together, the evidence in this paper is in line with the view that listed firms finance their investment through retained earnings. Even though this may imply limited efficiency costs of taxing profit distributions of listed corporations, such a policy may affect the incen-

[6]The comparisons in this paragraph are based on cash-holdings, quintiles of domestic ownership, quintiles of US ownership and quintiles of measure of equity dependence.

tives of the firm to go 'public.' This remains an open important question that needs to be understood for the design of optimal policy. There is also evidence that the international ownership of US listed firms has limited the potential effect of the tax cuts, given that corporate boards have to respond to the diverse needs of the ultimate owners, who may be foreign enough for the dividend tax rate to be irrelevant. This difference is probably what explains the difference in payout elasticities reported in this paper and those reported by (Yagan 2015) for a sample of private firms that are unlikely to be as internationally owned as my sample.

Ultimately the famous fable of Microsoft, implementing its first dividend payouts in response to the 2003 tax cuts and multiplying its total payouts by 4 by 2007, is incomplete. Concurrently, Vodafone in the UK almost doubled its profit distributions; Nokia in Finland did the same; and, Panasonic increased its investments. Given these reasonable counterfactuals, it seems unlikely that the secular trend reversal in payouts and investment in the US was driven by changes to the US tax code. At best, there was an increase in payouts by listed US firms for one year immediately after the reform perhaps reflecting the announced temporary nature of the tax cut. The null response could have been driven by two reasons. Either US listed firms are too internationally owned for the US dividend tax rate to be relevant for their decisions, or US listed firms are financing their investments through retained earnings.

Two caveats are worth noting. Firstly, the tax cuts happened in a period of political uncertainty after 9/11 and in the run-up to the Iraq war. Heightened uncertainty is known to affect investment and payouts (Bloom et al. 2007). If we believe that the heightened risk is US-specific, then this implies that there may have been a muted investment and pay-out response compared to that expected under the "traditional view" of the firm in more normal times. However, this heightened uncertainty arguably started with the September 11 events and, given that there are no significant deviations from the parallel pre-trends of matched firms after 9/11 and before the dividend tax cuts studied here, it is unlikely that this matters for my analysis. Secondly, corporate boards might not react to the US dividend tax code because shareholders' tax incentives are not internalised by corporate managers. This is a related but slightly different argument to the international ownership of US equity argu-

ment. It requires a strong degree of mismatch between the interests of the owners and the managers.

Literature Background: Theory & Evidence on Dividend Taxation

The analysis presented in this paper draws on multiple literatures analysing dividend tax cuts in the US. The earlier theoretical work on this dates back to King (1977) and is summarised by Auerbach (2002). Early empirical work employing event study approaches and announcement effects is summarised by Auerbach & Slemrod (1997).

The paper builds on previous empirical work by Poterba (2004), Chetty & Saez (2005, 2010), Campbell et al. (2013) and Blouin et al. (2011) analysing the behavioural responses of publicly-traded US firms in response to the 2003 dividend tax cuts by constructing a valid control group to study the effects of this policy reform. As opposed to the earlier literature, to identify the causal effect, I rely on a parallel trends assumption between US and non-US publicly-traded firms. Poterba (2004) relies on a time series analysis that requires strong exogeneity of the tax policy. Chetty & Saez (2005) compare firm outcomes before and after the reform and corroborate those findings using a sample of firms substantially owned by non-taxable institutions. This paper is also related to the literature on smaller firms studied by Yagan (2015), Alstadsæter et al. (2017), Boissel & Matray (2022), using alternative internally valid research designs based on differences in the legal status of firms.

OECD firms have been used previously as a control group to provide evidence on the impact of corporation tax reforms (Bond & Xing 2015). Jacob & Jacob (2013) and Becker et al. (2013) assess the effect of different episodes of dividend tax changes in the OECD. There are four main differences between their analysis and this paper. Firstly, I discuss and analyse the residence of corporate ownership in light of the response of firms to the dividend tax cuts for my sample of firms. The second difference relates to the focus in this paper on the 2003 US dividend tax cuts. The third difference is econometric, I focus on constructing a valid control group for the tax cut analysed. Finally, I document null effects of the dividend tax cuts as opposed to their results, suggesting important behavioural differences for US and non-US matched counterparts in my sample.

My results are consistent with work by (Bond et al. 2007) on changes to UK dividend

taxation. In fact, the results documented for the UK heightens my belief that these results are not driven by the view that US dividend tax rates influence global dividend behaviour, because of for example portfolio reallocation (Kawano 2014). That is, the control group is not likely to be indirectly affected by the US tax cuts in a relevant way. In studying the robustness of these results, I make use of the doubly-robust diff-in-diff estimator (due to Sant'Anna & Zhao (2020)) that nests the inverse probability weighting approach with the outcome regression approach of Heckman et al. (1997). This estimator is consistent if either of the matching algorithm or the outcome regression model are correctly specified.

The paper sheds empirical light on previously discussed theoretical mechanisms such as the "new view" as opposed to the "old view" of the firm, the agency view of the firm discussed by Chetty & Saez (2010), the "new view" with temporary announcements (Korinek & Stiglitz 2009), and firm maturity theories (Sinn 1991). My findings are consistent with a new view firm that finances its investment through retained earnings. Thus, the economic distortions caused by dividend taxation on these firms appear to be limited. My findings are also consistent with a dividend tax irrelevance view due to the international ownership of listed firms. Importantly, the paper considers a partial equilibrium design and doesn't talk to macroeconomic adjustments implied by the tax change. Gourio & Miao (2010), Anagnostopoulos et al. (2012) study the general equilibrium implications of such tax cuts in a complete and an incomplete markets environment à l'Aiyagari (1994), respectively. Problematically however, these models seem to frequently get the empirical facts wrong. Investment rises in Gourio & Miao (2010) in response to the tax change for a model calibrated to COMPUSTAT data and falls in Anagnostopoulos et al. (2012). Dividend responses similarly vary. This implies we need a new quantitative model to reconcile the findings of the micro-empirical literature with the macro-literature.

More generally, the paper is related to the literature on the taxation of capital income. The traditional result in this literature supports a zero tax rate on capital à la *Chamley-Judd*, based on an infinite elasticity of capital assumption in a complete markets world in the long-run.[7] Although I document an elasticity of capital with respect to dividend taxation equal to 0 in this paper, an important assessment is whether the dividend tax cuts distort

[7]Interestingly the estimated elasticity for capital is 0 in this paper.

incentives of firms to become public through issuing an IPO. This remains an open avenue for future work.

2 Institutional Background

The Jobs and Growth Tax Relief Reconciliation Act of 2003 (JGTRRA) was introduced in the US House of Representatives in February 2003. President Bush signed this Act into law in May 2003, applied retroactively starting January 2003. This was the second tax code change of the later dubbed "Bush Tax Cuts", with the explicit aim of incentivising investment and promoting growth. The JGTRRA reduced the top statutory rate of dividend taxation by 23.9 percentage points on US tax resident shareholders. [8] The dividend tax proposal was largely unanticipated (Auerbach & Hassett 2006).

The earlier tax cut in 2001 had reduced income tax rates and introduced temporary accelerated depreciation for equipment and light structures. The JGTRRA accelerated the phase-ins of these two earlier reforms. The reduction in the income tax rates does not directly affect publicly-traded companies. However, the expansion of the temporary accelerated depreciation is meant to lead to a positive effect on investment. Given that I find no effect on investment and no differential trends in 2001-2002 between OECD and US firms, I conclude that this did not matter for my result. Additionally, the American Jobs Creation Act of 2004 introduced a tax holiday for multinational US firms. This allowed US firms to repatriate cash-holdings abroad specifically to be invested rather than paid out to shareholders.[9] This means that the effect I estimate is an upper bound on the effect of the dividend tax cuts. Given my null findings, I conclude that the effect of the dividend tax cuts on US payouts and investment is small.

The top tax rate on capital gains was also reduced by a much smaller 5 percentage points. The relevant theoretical measure is the differential treatment of dividends relative to capital gains, which highlights the differential disadvantage of distributing profits to shareholders.

[8] There are multiple methods to report the change in the tax rate. I follow Yagan's definition that the top statutory rate fell from 44.7% to 20.8% in order to remain close to the literature. The definition relies on the OECD report based federal and average state tax rates.

[9] There is empirical evidence that in fact firms were nonetheless able to repurchase stocks (Blouin & Krull 2009).

Since the reduction of the capital gains tax rate is very small, I choose to report elasticities with respect to the change in dividend taxes to remain in line with the literature. If anything, this implies that the elasticities I report are slightly overestimated in absolute terms since I attribute the entire effect to a change in the dividend tax only.

Initially, the tax reform was temporary with a sunset clause mandating its reversal in late 2008. Later towards the end of 2005, Congress proposed an extension to 2011. This was signed into law by President Bush in May 2006. A further temporary extension occurred in 2010 by President Obama, which was later made permanent in 2013.[10] Non-US tax resident owners of US domiciled firms are not liable for dividend taxes and, hence, were unaffected by the reform.[11]

3 Data

3.1 Sample & Variable Definitions

I use COMPUSTAT North America and Global to construct a panel dataset of comparable US and OECD consolidated company accounts around the 2003 tax policy reform. Using COMPUSTAT's exchange rate database and the GDP deflator series from the World Bank's national accounts data, I convert foreign company accounts to constant 2000 US Dollars. I use the fiscal year definition of COMPUSTAT. This relies on the 12-month company account that ends before June in the following fiscal year. For perspective, the latest accounting period for the fiscal year, defined as, 2002 is between 1 June 2002 to 31 May 2003. Given that the reform only marginally passed the senate vote on the 15th of May 2003, was signed into law on the 28th of May 2003, and the law was not anticipated (Auerbach & Hassett 2006), it is unlikely that a firm in the fiscal year 2002 was able to increase payouts or investment in the second quarter of calender year 2003.[12]

[10] The final permanent extension excluded all individuals with taxable income above $400,000 & married couples above $450,000. A federal rate of 20% applied to those individuals.

[11] Non-US tax resident owners of US domiciled firms are liable to a dividend withholding tax (normally which is 15% for individuals from treaty countries). All OECD nations have tax treaties with the US. There are further tax discounts if the firm happens to be a subsidiary of a foreign firm. The dividend withholding tax was unaffected by the reform.

[12] I also explore a robustness check where I count all accounts as treated if they are reported after January 2003.

Variables were defined to be in line with the literature (Yagan 2015, Hennessy & Whited 2005). US firms are firms incorporated in the US. Similarly, OECD firms are defined as firms incorporated in OECD countries apart from the US. The extensive margin of dividends is defined as whether the firm has declared any positive cash dividends on all equity capital of the company. Total dividend payout ratio is defined as total cash dividends from the income statement on all equity divided by the book value of total assets at the end of the previous accounting period. Total payout ratio is defined as total cash dividends plus share buybacks (non-negative annual changes in treasury stock) divided by lagged total assets.[13] Total assets is defined as current assets plus net property, plant, and equipment plus other non-current assets, including intangible assets, deferred items, and investments and advances. Investment is defined as capital expenditure (CAPX). This is the cash outflow or funds used for additions to the company's property, plant and equipment, excluding amounts arising from acquisitions, reported in the Statement of Cash Flows. I define the outcome variable as investment divided by lagged total assets. As a robustness exercise, I scale dividend payouts and investment with lagged gross sales as opposed to lagged total assets.

The firm's age is defined as years since the company's initial public offering. I divide firms into 12 sectors based on the first digit Standard Industrial Classification of economic activity (SIC) codes. I then divide the largest sector (manufacturing) into 19 further subcategories. I define lagged sales as the total revenue reported in the previous accounting period. Lagged Income corresponds to income of a company after payment of all expenses in the previous year. Lagged sales growth is the growth rate in revenue in the previous year. I define lagged cash holdings as any immediately negotiable medium of exchange or any instruments normally accepted by banks for deposit and immediate credit to a customer's account in the previous year. Short-term investments are currently marketable investments as presented in the current asset section of the balance sheet that can be transferred into cash in a short period of time. Acquisition and R&D expenditure correspond to cash-outflow of funds used for acquisition of a company in the current year or effects of an acquisition in the previous year, and costs incurred during the year relating to the development of new

[13]This follows Blouin et al. (2011), Skinner (2008), Edgerton (2013), among others who use non-negative annual changes in treasury stock as a measure of share buybacks.

products and services.

I use Factset's unique ownership data from March 2003 on listed securities to construct measures of domestic ownership, US ownership, institutional ownership based on percentage of outstanding shares owed to reported owners that fit these criteria. Ownership data is notoriously incomplete (De La Cruz et al. 2019). As such, there is a fraction of ownership that is not documented. One way to deal with that is to measure reported ownership percentages that are owed to the different categories relative to the total reported ownership percentage. I do that in the appendix. However, I stick to reported percentages in the main text.

For the tax variables, I use data provided by the Centre for Business Taxation on corporate tax rates in OECD nations. For dividend tax rates, I use marginal tax rate data provided by the OECD tax database from 2000.[14]

I focus on firms reported in COMPUSTAT for 6 continuous years preceding 2003. This is to avoid differential changes in firm composition before the reform in the US relative to other OECD countries. In line with the literature (e.g. Hennessy & Whited (2005)), I exclude firms whose industry classification is in utilities (SIC codes between 4900 and 4999) and finance (SIC codes between 6000 and 6999) due to additional regulation considerations for these firms. I also restrict my sample to firms with more than $2M of total assets (constant 2000 US Dollars).

Following Yagan (2015), I winsorise (top-code) the outcome variables to exclude extreme outliers. The winsorisation process top-codes the highest 5% (unless otherwise specified). I allow the outcome distribution to change by winsorising the top 5% separately before and after the passage of the reform. I also allow the top 95th-percentile to vary between US and OECD firms. This is done to allow the outcome distribution to be different between treatment and control groups, before and after the tax cuts. Winsorising has two benefits. Firstly, it excludes extreme observations that are likely the result of accounting errors. Secondly, it reduces the skewness of the outcome variable distribution. This increases the efficiency of the estimator (Rivest 1994). I also restrict my sample to firms whose ratio of assets to sales ranges between 0.01 and 100. Finally, I follow Gourio & Miao (2010) in excluding firms with

[14]More information about the Centre for Business Taxation data is described in Devereux et al. (2002) and more information about the OECD tax database is available through `https://stats.oecd.org/index.aspx?DataSetCode=TABLE_II4`.

negative investment and negative dividend payouts.

The final constructed dataset has unique information about firm decisions, ownership, and tax policy at the country-level. We can thus study the impact of JGTRRA on listed firms' investment and financial decisions.

3.2 Facts about US and OECD Listed Firms

US Listed Firms' Investment relative to Total US Corporate Investment

Figure 1 reports total investment in billions of US Dollars by all non-financial corporations and by non-financial listed corporations. Close to 50% of overall US investment is made by listed firms. Listed firms are number close to a tenth of US incorporations. Understanding their response to the dividend tax cuts of 2003 is thus significantly important. Previous work on the topic relies on time series comparisons around the 2003 dividend tax cuts. This paper constructs a control group for listed US corporations and tries to understand some of the puzzles in the literature through this new identification strategy.

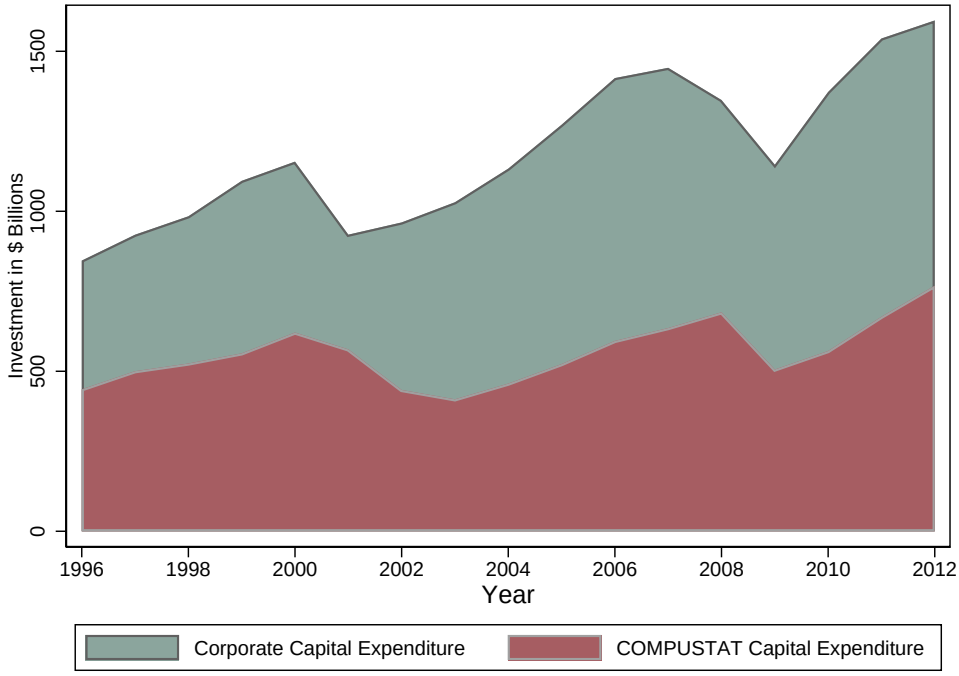
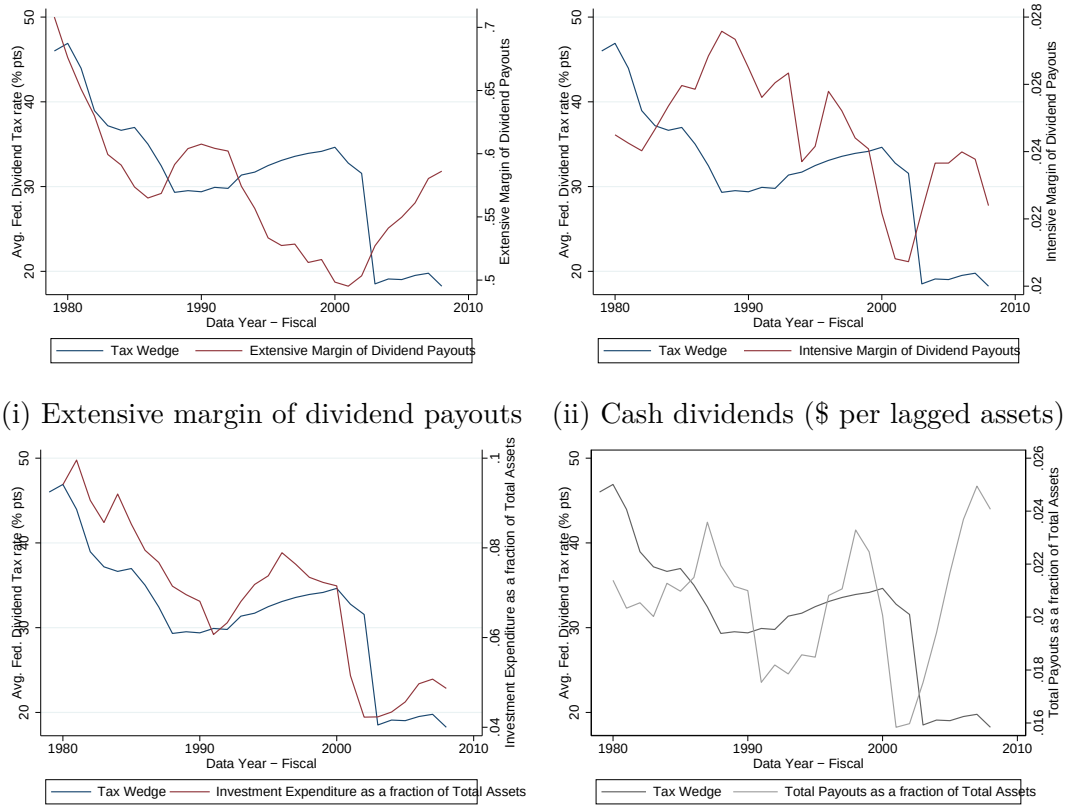


Figure 1: Listed Non-financial firms nominal investment accounts for 50% of total non-financial nominal Corporate Investment over the period studied.
Source. FRED, COMPUSTAT, and Author's calculations (Board of Governors of the Federal Reserve System (US) 1946).

Long-term Evolution of US Firm Behaviour & Taxation of Dividends



(i) Extensive margin of dividend payouts (ii) Cash dividends ($ per lagged assets)

(iii) Investment ($ per lagged assets). (iv) Total Payouts ($ per lagged assets).

Figure 2: Long-run payout and investment behaviour by US firms & the US tax on dividends.

Notes. Based on NBER's TAXSIM model (Feenberg & Coutts 1993) and COMPUSTAT North America data. The average federal tax rate is defined as the dollar weighted average marginal tax rate as calculated for a sample of US tax payers allowing for changes in income distribution through time.

Figure 2 plots the long-run time series behaviour of average unmatched dividend payouts, investment, and total payouts of COMPUSTAT firm-level data. This is done without any restrictions on the presence of firms in all or some of the sample periods. It is clear from the figures the extent of cyclicality observed in the investment and total payouts. It also happens to be the case that the average federal tax rate on dividends also followed a relatively cyclical pattern. This observation has two implications. Firstly, without a plausible control group, it would be difficult to argue that the rise in payouts or, indeed investment, after 2002 is attributable to the 2003 dividend tax cuts. Specifically, it is not clear whether, for example, the sudden rise in corporate total payouts and investments around 2003 is because

of the dividend tax cuts or the result of a cyclical recovery following the end of the dotcom bubble. A similar pattern is seen in the time series trends presented by Yagan (2015) for both C-corporations and S-corporations. The older literature (Auerbach & Slemrod 1997, MacKie-Mason 1990, Poterba & Summers 1984) based on simple before and after policy shock comparisons with limited control groups favoured the "traditional" view of the firm, in which dividend taxes matter for financial and investment decisions of firms. Poterba (2004) is latest contribution in this literature suggesting that dividend tax cuts might pay for themselves. Secondly, the control group ought to follow the same cyclical pattern. The assumption required for the effect to be causal is that, in the absence of the US tax reform, the conditional means for the two groups of firms would have trended similarly before and after 2003.

Aside from cyclicality, investment expenditure as a fraction of lagged assets followed a downward trend over the period. This pattern is documented in previous literature (Gutierrez & Philippon 2017). More immediately after 2002, total payouts, dividends and investments all rose sharply.

The more recent literature on publicly-traded firms usually relies on institutionally-owned firms or control groups that depend on corporate cash-holding sizes regimes or unaffected industries. When these comparisons are made, it is frequently not obvious why or indeed whether the trends between the two groups would have had the same cyclical pattern over the business cycle in absence of the reform.[15]

Summary Statistics for the Main Sample

Table 1 reports the unweighted unwinsorised summary statistics for the characteristics and outcome variables in 2002 separately for the US and OECD firms analysed in this paper. All monetary values are in millions of 2000 USD and all values are annual. The publicly-traded firms in these samples are extremely large. The median US incorporated firm has lagged total assets equal to $163.6M (constant 2000 dollars). For reference, the

[15]For example, if institutionally-held firms are endogenously selected to be more liquid and less volatile (as is documented in Gompers & Metrick (2001)) in order to hedge against the business cycle, then it is clear that this identification strategy will bias our views towards believing that a cut in dividend taxes at the bottom of the cycle will affect corporate decisions. This is especially relevant during this period in the run-up to the great financial crisis.

90th-percentile firm studied in Yagan (2015) had total assets equal to $105M.[16] The median investment expenditure is $93.5M, as opposed to $250,000 in Yagan (2015). Yagan's control group of S-corporations simply does not have any common support for publicly-traded US firms. Hence, Yagan excludes publicly-traded firms from his main analysis and only explores suggestive evidence for publicly-traded firms in the appendix. Importantly, the larger firms studied here form a significant portion of US corporate investment and of US GDP. Thus, understanding their response to dividend taxation is crucial for policy design.

Table 1: Unweighted Summary Statistics for the Main Sample.

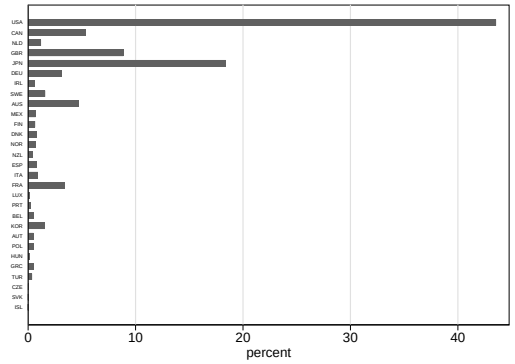
	US Firms				OECD Firms			
	Mean (1)	Median (2)	10th Percentile (3)	90th Percentile (4)	Mean (5)	Median (6)	10th Percentile (7)	90th Percentile (8)
Characteristics								
Lagged Sales	1,578.95	157.55	8.32	2,463.44	1,676.96	232.61	17.70	3,030.18
Lagged Total Assets	1,971.39	163.60	10.39	2,586.85	1,942.07	241.75	21.36	3,284.13
Lagged Income/Lagged Assets	-0.20	0.00	-0.53	0.10	-0.04	0.01	-0.13	0.08
Age since IPO	7.85	7.00	3.00	15.00	24.32	17.00	4.00	52.00
Employment	8.07	0.80	0.05	14.94	8.18	1.39	0.20	16.08
Lagged Sales Growth	0.13	0.01	-0.30	0.45	0.01	-0.01	-0.48	0.29
Lagged Cash/Lagged Assets	0.13	0.06	0.01	0.35	0.09	0.06	0.01	0.22
Lagged R&D expenditure/Lagged Assets	0.09	0.04	0.00	0.24	0.04	0.01	0.00	0.08
Lagged Short-term Investment/Lagged Assets	0.05	0.00	0.00	0.18	0.03	0.00	0.00	0.08
Lagged Acquisition/Lagged Assets	0.03	0.00	0.00	0.06	0.02	0.00	0.00	0.04
Lagged Leverage	0.73	0.28	0.00	2.18	1.05	0.54	0.00	2.53
Outcomes								
Investment	93.53	4.95	0.12	119.46	109.35	7.48	0.32	144.19
Investment(USD per Lagged Assets)	0.05	0.03	0.01	0.10	0.06	0.03	0.01	0.11
Dividends	25.38	0.00	0.00	14.46	25.10	1.65	0.00	33.29
Dividends (USD per Lagged Assets)	0.01	0.00	0.00	0.02	0.01	0.01	0.00	0.03
Total Payout (USD per Lagged Assets)	0.02	0.00	0.00	0.05	0.01	0.01	0.00	0.03
Number of firms			4,570				5,945	
Number of firm-years			55,705				82,944	

Notes. Table 1 presents unweighted unwinsorised summary statistic for firms incorporated in the US (more exposed to the tax cut) relative to firms incorporated in the OECD (less exposed to the tax cut) in 2002. This is based on an unbalanced sample of COMPUSTAT (North America & Global) publicly-traded firms tracked continuously for 7 years before the 2003 tax cuts. Firms with less than $2M were excluded from the sample, as were firms with an assets-to-sales outside the range (0.01 - 100). Additionally, firms in the finance and utilities sector were excluded from the sample. All monetary amounts are reported in millions of constant USD (2000).

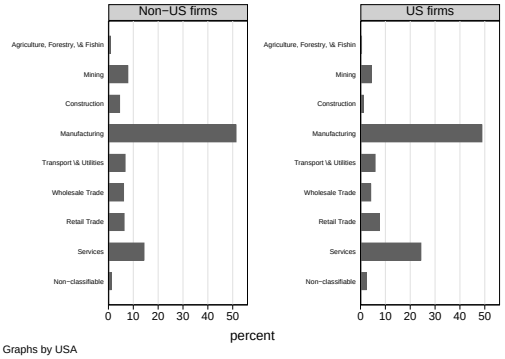
Table 1 shows that US publicly-traded firms are on average smaller and younger than their non-US counterparts along multiple dimensions. This fact is not a problem for the exercise since the matching algorithm used selects similar non-US control firms to compare to the US treated firms. Figure 3 shows the distributional characteristics of the variables in both US and OECD samples. Subfigure (vi) shows that US firms do not differentially drop-out from the sample by 2007 relative to other countries. There is substantial common support such that the matching exercise can yield observationally similar OECD firms that

[16]Yagan (2015) reports dollar amounts denominated in 2010. Hence, the firms in my sample are even larger than in his sample compared to the numbers reported in this simple comparison.

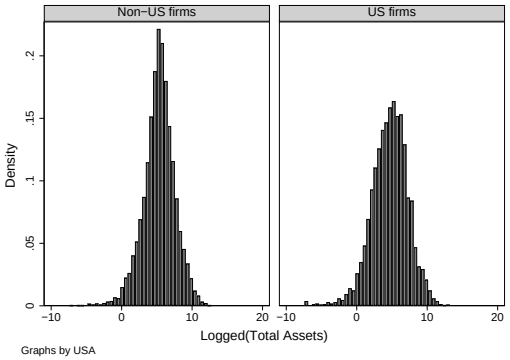
can be compared to US firms in the difference-in-difference setup.[17]



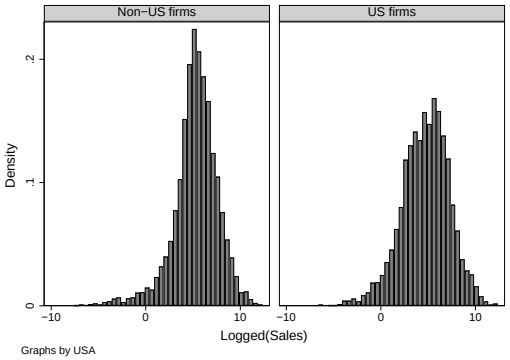
(i) Fraction of firms in the sample by country



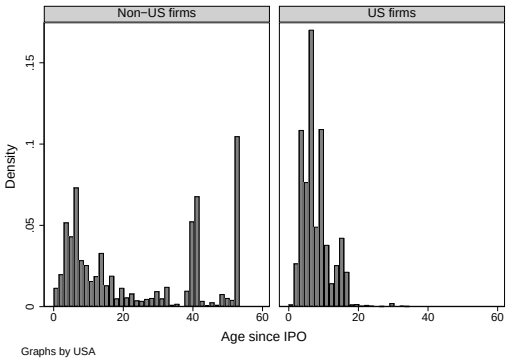
(ii) Industry Differences



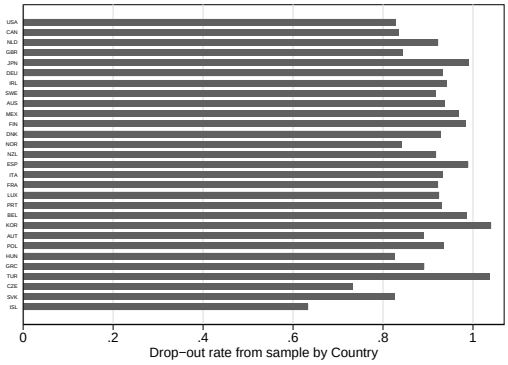
(iii) Distribution of logged assets



(iv) Distribution of logged revenues



(v) Distribution of Age since initial public offering



(vi) Average ratio of firms in the sample after 2002.

Figure 3: Distributional differences along relevant variables between US and OECD firms.

Notes. Distributional differences between US and OECD firms along multiple characteristics. US firms are younger and smaller than their OECD counterparts. US firms do not drop-out of the sample disproportionally relative to OECD counterparts. Industry classification is based on the Standard Industrial Classification code.

[17]This is an essential check for the matching exercise (see for e.g., Heckman et al. (1997))

Percentage of US Ownership by Firm Domicile

Figure 4 is bar chart of mean reported US ownership of firms by country of firm domicile in March 2003. The figure summarises two features that are carefully thought of in the identification strategy section and alluded to in the introduction. Firstly, a large fraction of US domiciled firms are owned by non-US tax residents. The red bar suggests that this number is just below 50%, assuming all unreported owners are non-US tax residents.[18] Non-US owners do not pay the domestic dividend tax rate. Instead, they pay a dividend withholding tax that is unaffected by the dividend tax cuts of 2003. Hence, one reason that some firms might not react to the tax cut is if they were owned predominantly by non-US tax residents who do not benefit from the tax cut. This dividend tax irrelevance for some US firms might explain the response of some firms. Despite that, defining treatment in terms of country of firm domicile helps us identify the effect of the policy relative to its target aim of increasing US firm's investment and employment.

Relatedly, some non-US domiciled firms are owned by US tax residents. Mean reported US ownership of non-US domiciled firms is summarised by the blue bars in Figure 4. The US owners of these firms are affected by the tax cut. This international spillover of the US dividend tax cut is a concern that is addressed in multiple ways in the analysis later on. Note that most countries with substantial sample sizes have little reported US ownership. This implies that the contamination worry of the control group is not extremely important. Both of these features of the comparison (the dividend tax irrelevance for international owners of US firms and the international spillover of the US tax cut) are carefully studied in the later sections.

[18]This assumption is highly problematic since there are many reasons for the ownership data to be missing for a firm shareholder, chief among which is due to small holdings. However, if we assume that non-US ownership is as equally missing as US ownership. Non-US ownership would be estimated at 20%. If we only account for reported foreign ownership, the estimate would be 5%. Other studies place this number at close to 20% De La Cruz et al. (2019).

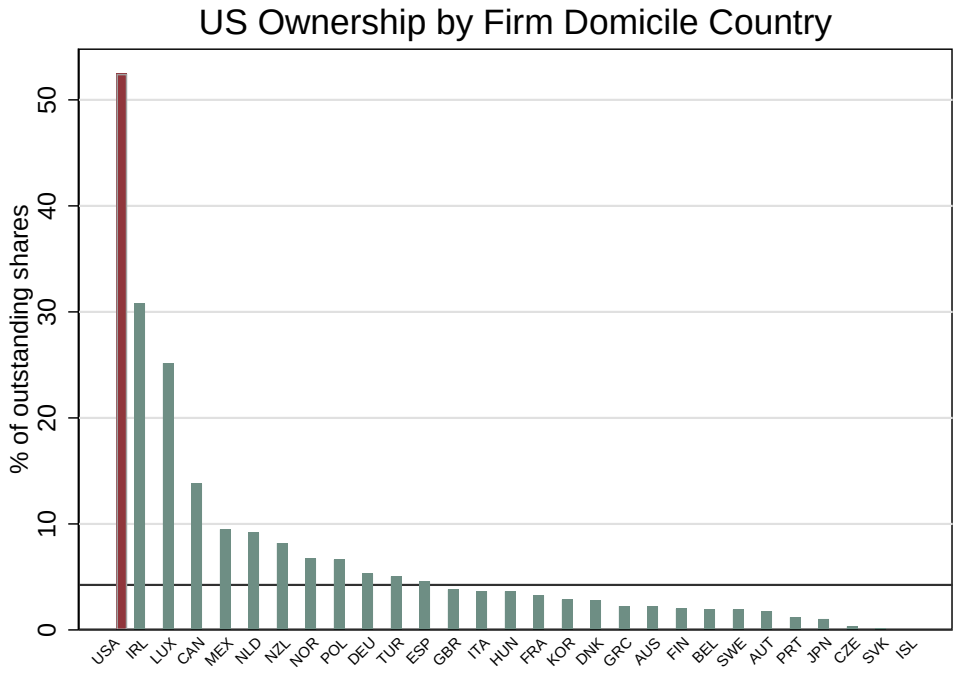
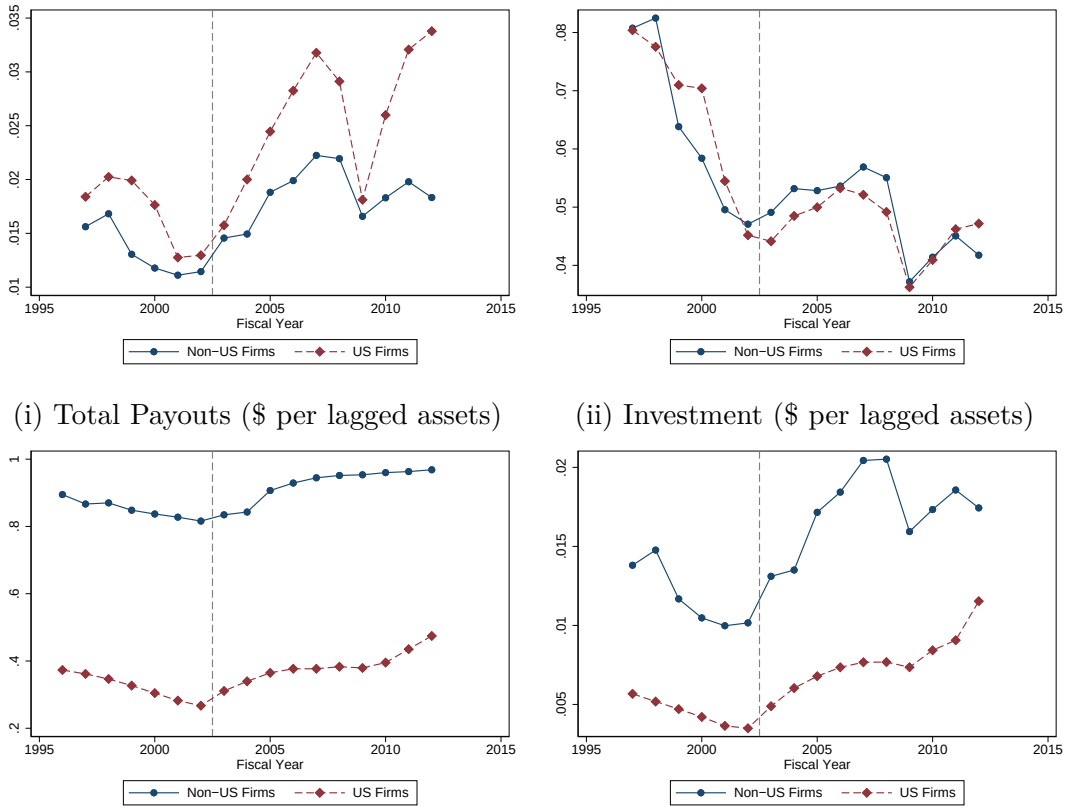


Figure 4: Reported US Ownership by Firm Country of Domicile.
Source. COMPUSTAT, Factset Ownership, and Author's calculations (Board of Governors of the Federal Reserve System (US) 1946). Horizontal line indicates OECD average. I calculate the same graph relative to total reported shares in Figure 10.

A first look at Payouts and Investment from 1997 to 2006

Figure 5 shows the evolution of the average winsorised outcome variables around the 2003 tax reform (marked by the dotted vertical line) for both firms incorporated in the US and OECD firms. The cyclical patterns for US firms are strikingly similar to those for non-US firms over this period. Figure 5 is generated without implementing any matching algorithm nor controlling for any confounders.

In the raw comparison, there could be some concern about different trends in the pre-reform period for the cash dividends variable and the total payout variable. This concern is dealt with using the matching algorithm as later presented in the results section in Figure 29.



(i) Total Payouts ($ per lagged assets)

(ii) Investment ($ per lagged assets)

(iii) Extensive margin of dividend payouts

(iv) Dividends ($ per lagged assets)

Figure 5: Unmatched plots of average financial and real behaviour of the firms by whether they were incorporated in the US.

Notes. This graph presents a plot of the evolution of the winsorised outcome variables around the 2003 tax cuts by whether the firm is incorporated in the US. The scatter-plot doesn't use the matching algorithm implemented in the main results section and focusses on the raw relationship around the reform. There are slight pre-trends in both the dividends and total payouts. This problem is not unique to this paper. Yagan (2015) suffers from a similar problem.

Additionally, it is clear that investment expenditure in the US tracked closely other OECD countries. This is despite the presence of a large dividend tax cut (close to 50%) in the US. The same is true for the extensive margin of dividend payouts. Accepting the presence of pretrends, I note that total cash dividend payouts actually increased in OECD nations relative to the US after the reform compared to before. A final thing to note is that share buybacks increased in the US to generate the pattern observed in total payouts. Later in the results section, I discuss, after the implementation of the matching algorithm, the

causal effect of the tax reduction in the US on the investment and financial behaviour of US firms.

4 Identification Strategy

4.1 Semi-parametric Difference-In-Difference Design

Estimation Technique

I weigh a sample of non-US OECD firms relative to US firms based on a set of characteristics in 1997: firm age; employment; sales; total assets; and 2-digit SIC sectors with added markers for manufacturing subcategories. I then trace the behaviour of the constructed control group of firms relative to their US counterparts over time before and after the reform in a difference-in-difference exercise. The estimation is done through two simple steps, intuitively described by Abadie (2005) as "first weight, then project."

The first step is the weighing. For this, I construct inverse probability weights (Abadie 2005) by estimating a cross-sectional probit regression for whether a firm is incorporated in the US in 1997 using firm characteristics (detailed earlier) as regressors. The propensity scores, $\hat{P} = P(\hat{US = 1}|X_i)$, are then used to estimate the weights as follows[19]

$$w_{it} = \begin{cases} 1 & \text{if US=1} \\ \frac{\hat{P}}{1-\hat{P}} & \text{if US=0} \end{cases} \tag{1}$$

Intuitively, these weights balance observable differences in characteristics that can affect the dynamic evolution of outcomes between the treated and control groups. This is done in the first sample period to avoid the introduction of endogeniety through colliders. An assessment of the matching exercise employed in the main results in this paper is available in Appendix A. The algorithm preforms well. There is common support as seen in Figure 12. Matched observations have the same distribution of propensity scores as the US firms. This matching method adopted here is in contrast to the outcome regression and doubly

[19]I require that $P(\hat{US = 1}|X_i) < 1$ in the sample used for the projection. This is a statement of the common support assumption. In practice, this assumption doesn't matter for the estimated results, since there is substantial common support between the two subsamples as seen in Appendix A.

robust estimation approaches considered by Heckman et al. (1997) and Sant'Anna & Zhao (2020), respectively. For robustness, in the appendix, I show the results do not change when estimating a doubly-robust model like Sant'Anna & Zhao (2020), which nests both the outcome regression approach and the inverse propensity weights. Under doubly-robust estimation, consistency requires only one of the specifications of the outcome regression or the propensity score model to be correct. Under inverse probability weighting, consistency requires the correct specification of the propensity score model relative to the true data-generation process.

The second step of the process is the estimation of the main outcome regression. The main regression specification used in the tables is given by

$$Y_{it} = \theta.US_i \times D_{post-03} + \alpha_t + \alpha_{US} + \boldsymbol{Z_{it}'} \cdot \boldsymbol{\beta} + \mu_{it}, \qquad \forall i, t \tag{2}$$

This regression is estimated by weighted least squares using the w_i weights from the previous step. Y_{it} is the outcome variable of interest. The two main outcome variables studied in this paper are total payouts and capital expenditures. Both variables are scaled by lagged assets (more details on the variables are available in the data section). $D_{post-03}$ is a marker for post-2003, US_i is a marker for whether the firm was incorporated in the US, and Z_{it}' is a potentially empty vector of control variables including additional firm characteristics and OECD corporate and dividend taxation changes.

I assume a conditional parallel trends assumption for causal identification of the average treatment effect on the treated. The assumption requires that,

$$E[\Delta.Y_{it}^0 | Z_{it}, US = 1] = E[\Delta.Y_{it}^0 | Z_{it}, US = 0] \tag{3}$$

where $E[\Delta.Y_t^0 | Z_{it}, US = 1]$ is the potential change in outcomes for the US firms had the treatment not occurred conditional on Z_{it} (firm characteristics in the baseline period).[20] Put differently, the assumption states that the trends for US and matched OECD firms would have followed a similar path in the absence of 2003 US dividend tax cut. $\hat{\theta}$ is thus a consis-

[20]For simplicity, I focus on the average treatment effect and drop the i subscripts. However, this framework can be extended to allow for heterogeneous effects.

tent estimator for the true θ, the effect of the US dividend tax cuts on the US firms.

Empirical Decisions

I do not include firm-level fixed effects in any of the specifications. This is done since the inclusion of firm-level fixed effects would require the introduction of an additional assumption, namely that the treatment effect is homogenous across treated observations. Theoretically, this assumption does not hold because standard models (Gourio & Miao 2010, Chetty & Saez 2010) predict cash-poor treated firms to increase their investments and payouts relative to cash-rich firms. Sant'Anna & Zhao (2020) present Monte Carlo simulation evidence on the performance of fixed effects model in a difference-in-difference setting. They find that the fixed effects model biases the results regardless of the specification of the propensity model and the outcome regression. In fact, it performs poorer than all the other estimation techniques (including the IPW estimation adopted in this paper).

I also do not include time-varying firm characteristics as control variables in the outcome regression model to avoid the inclusion of variables that may be influenced by the tax cut. For example, including cash holdings of the firm in the current year (t) as a regressor, might be selecting on the effect of the treatment thereby biasing the estimate of θ.

In relation to the empirical modelling of the OECD's (control group) tax system, I adopted a baseline practice and multiple robustness checks. Note that most OECD countries included in the sample did not have major dividend tax reforms that affected their marginal tax rate during the period. In fact, more than 90% of non-US OECD firms in the sample did not experience a change in the marginal dividends tax rate paid by their shareholders of more than 1% point over the entire sample period (compared to the 23.4 percentage point tax cut on the marginal rate in the US). Nevertheless, I deal with this potential concern in multiple ways. Firstly, as detailed in Table 2, I include a time varying regressor equal to the change in the after-tax distributed profit that accrues to investors in the home country of firm i relative to pre-tax profit distribution. I also run the main model sequentially removing domicile countries in the control group. Finally, I also consider a specification that focuses on the immediate aftermath of the introduction of the US dividend tax cut. France is the only other OECD economy that had a dividend tax cut in 2003 of a mild 1.41%. Despite this, I include French firms in the control group given the

much larger tax cut in the US. Results from these alternative specifications are presented in the appendix. A note of caution is worthwhile highlighting. If we believe that tax decisions are independent across time and countries, then dividend tax policy of other OECD nations is strongly exogenous. In that case, we can include it as a control variable in the outcome regression. However, if we believe that the 2003 JGTRRA tax cuts in the US may have influenced other countries to cut their dividend tax rates, for reasons discussed in the next subsection, then controlling for the dividend taxes of other countries could introduce a bias to our estimate of the effect. This is because we would be selecting on one of the effects of the 2003 JGTRRA. This concern turns out not to be important in the exercise. This is because on comparing specifications with and without the tax controls the results in the paper are unaffected.

Finally, in line with Yagan (2015), I define the elasticity for outcome Y as[21]

$$\begin{aligned}\epsilon_Y &= \frac{\Delta Y_{it}}{\hat{Y}_{pre-03}} . \frac{(1-\tau_d)}{\Delta(1-\tau_d)} \\ &= \frac{\hat{\theta}}{\hat{Y}_{pre-03}} . \frac{(1-\tau_d)}{\Delta(1-\tau_d)}\end{aligned}$$

4.2 Assessing Causality

In this subsection, I discuss the validity of the conditional parallel trends assumption (3) underlying the results in this paper.

Firstly, US and OECD firms are comparable. The international nature of these firms implies that their organisational structures are similar. In fact, many US and OECD firms in this sample compete in the same markets. US and OECD firms share a substantial common support in the summary statistics presented in Table 1 and Figure 3. Additionally as shown in Appendix B, US and OECD firms share substantial common support in propensity scores based on firm characteristics. This overlap in support is necessary for the matching algorithm to find comparable units.

[21] $\hat{Y}_{pre-2003}$ is the mean of the outcome variable pre-2003 for the US firms. $\frac{(1-\tau_d)}{\Delta(1-\tau_d)}$ is calculated using the OECD net-of-tax distributed profits. One can define alternative elasticities using other definitions of the tax wedge. I report the average treatment effect for the treated to allow the reader to calculate their preferred elasticity but report the elasticity defined in the literature.

Secondly, in regards to the violation of stable unit treatment value assumption, it may be argued that contemporaneously the US dividend tax cuts led to an indirect impact on OECD firms. OECD firms' managers may have been compelled to pay higher dividends to attract US investors who suddenly had a US equity advantage. Portfolio rebalancing is a documented adjustment mechanism to the tax change (Kawano 2014). However, all current studies assessing the impact of shareholder taxation on firm payouts suffer from this general equilibrium effect (Chetty & Saez 2005, Yagan 2015, Alstadsæter et al. 2017). It is unlikely that the total effect of the policy will be mitigated by this general equilibrium adjustment mechanism. This is especially true given that in many of the internally valid studies (comparing samples of firms within countries), the literature finds a positive causal effect of dividend tax cuts on dividends paid by the treated group of firms (Yagan 2015, Alstadsæter et al. 2017, Boissel & Matray 2022). If it were the case that other untreated firms increased their payouts and changed their investment behaviour due to portfolio rebalancing concerns, previous studies would not have found evidence of an effect on dividend payouts. Additionally, this concern is likely to be more salient if the untreated firms are also US firms. This is because it is presumably easier to adjust portfolios within one country compared to across countries due to the home country bias in equity portfolios. I thus conclude that this concern is unlikely to be relevant for my paper.

More specific to the paper, two channels that directly affect the stable unit treatment value assumption are relevant. These relates to the tax residence of the owners of both US and non-US firms. US owners of non-US domiciled firms benefit from the US dividend tax cut in 2003. A US-owned firm that is domiciled outside the US might in principle respond to the tax cut by changing its investment and financial policy since its owners have reduced tax liability. This introduces potential contamination in the control group. I call this channel: the international ownership spillover channel of the JGTRRA on non-US domiciled firms. Additionally, non-US owners of US domiciled firms are not subject to the dividend tax cuts in 2003. Hence, in principle, a non-US owned firm domiciled inside the US might not respond to the tax cut because its owners are not affected by the tax cuts. I call this channel: the dividend tax irrelevance channel for foreign owners of US firms. This introduces further heterogeneity in the treatment effect. While there is reason to believe that these channels

are not important due to the home bias in equity markets, I systematically deal with these worries in a variety of ways.[22] To deal with the international ownership spillover channel on the control group, using unique data on firm ownership in March 2003 from Factset, I remove non-US firms in the control group with more than 5% US ownership as a robustness check. To account for both channels, I symmetrically treat US ownership in US and non-US domiciled firms in the Appendix. In this exercise, the fraction of US ownership is the treatment definition as opposed to firm domicile. Finally, I explore heterogeneity in my headline effect by the extent to which firms are domestically-owned. US domiciled domestically owned firms are likely to respond most relative to non-US domiciled domestically owned firms.

Importantly, both domicile- and ownership-based treatment measures are interesting and answer slightly different questions. A null effect based on the domicile-based treatment (adopted in the headline results in the paper) suggests that US firms did not respond to the tax cut. Hence, US policymakers interested in increasing US investment and employment growth (like the stated aim of JGTRRA) have not achieved the required aim. This null result might be driven by the irrelevance of the dividend tax cuts on non-US owners of US domiciled firms. A null effect based on the ownership-based treatment suggests that, even conditional on the relevance of the dividend tax cut, firms are not responding for other reasons like firm financing decisions (Korinek & Stiglitz 2009) or a wedge between the interests of managers and owners (Chetty & Saez 2010). These issues are discussed further in the discussion section.

Another criticism is that the temporary accelerated depreciation provisions were expanded in the US, and the capital gains tax rate was reduced, at the same time as 2003 dividend tax cuts. Accelerated depreciation was introduced in 2002 to provide an incentive to US firms to invest. Tests of the parallel trends assumption using the pre-reform period would register a violation of the assumption if this is likely a problem, assuming the parallel trends test is sufficiently powered. Although the consensus estimates based on Zwick & Mahon (2017) suggest that the introduction of bonus depreciation did stimulate investment, their estimates show that the lowest response is that of the average US COMPUSTAT firm. Additionally, this policy implies that for investment the average treatment effect on

[22]Refer to Coeurdacier & Rey (2013) for a recent survey of the literature on home bias.

the treated (ATT) that I estimate would be biased upwards. Since my ATT is insignificant economically and statistically, I conclude that it is unlikely that either policies mattered for investment decisions.

Two additional benefits of the research design in this paper over previous work are as follows. Firstly, there is no contemporaneous policy change that affected the entirety of the control group. This is for example the case in (Yagan 2015) where the control group actually faced an earlier tax reduction in 2001. Secondly, identification based on legal status assumes that the static selection in firm characteristics does not matter for the dynamic evolution of the outcomes. However, this assumption may not be true because difference in legal status are strongly associated with differences in firm size and ownership characteristics. Put simply as an example, firms with a smaller number of owners may behave differently during the dotcom bubble and in the run-up to the financial crisis relative to firms with more owners. Hence, it is hard to interpret evidence for smaller firms as causal for bigger firms. On the other hand, my identification strategy runs into a similar concern if we believe that the emergence from the dotcom bubble and the run-up to the financial crisis was more salient for US firms relative to non-US firms.

However, a final defence of this research design rests on the cyclical behaviour of the outcome variables analysed. As shown Section 3.1, investment and payout behaviour are extremely cyclical around the tax reform. This pattern also extends to non-US OECD firms. In the results section, I show that I cannot reject the null of no differential pre-trends in the pre-reform period between US and non-US firms. Based on this, I believe that had the US tax cuts not taken place, US corporate investment and payout behaviour would have conditionally trended similarly to OECD corporate investment and payouts (albeit at different levels) due to common global factors.

Table 2: Main Results Table.

	Total Payout (USD per lagged Assets) Top Winsorised at 5%			Investment (USD per lagged Assets) Top Winsorised at 5%		
	Unbalanced		Balanced	Unbalanced		Balanced
Panel A	(1)	(2)	(3)	(4)	(5)	(6)
US*Post-2003	-.00217	-.00241	-.00301	-.00208	-.00233	-.00153
	(.00202)	(.0021)	(.00232)	(.00523)	(.00534)	(.00482)
Matching	Y	Y	Y	Y	Y	Y
Controls	N	Y	Y	N	Y	Y
Observations (firm-years)	41929	40809	32433	61079	59769	48864
Clusters (firms)	5421	5272	3938	7059	6899	5322
R^2	0.0294	0.0388	0.0367	0.0424	0.0468	0.0472
US firms mean	0.0206	0.0206	0.0225	0.0659	0.0660	0.0666
US firms SD	0.0324	0.0324	0.0332	0.0650	0.0650	0.0647
ϵ wrt $(1-\tau_d)$	-0.244	-0.272	-0.310	-0.0729	-0.0818	-0.0531
ϵ SD	0.226	0.236	0.239	0.184	0.187	0.168
p-val. pretrends test	0.000784	0.000115	6.35e-05	0.935	0.771	0.687

	Any Dividend Payout Not Winsorised			Dividends (USD per lagged Assets) Top Winsorised at 5%		
	Unbalanced		Balanced	Unbalanced		Balanced
Panel B	(7)	(8)	(9)	(10)	(11)	(12)
US*Post-2003	.016	.0169	.0193	-.00286***	-.00289***	-.00273***
	(.0206)	(.0203)	(.0166)	(.000856)	(.000864)	(.000833)
Matching	Y	Y	Y	Y	Y	Y
Controls	N	Y	Y	N	Y	Y
Observations (firm-years)	72250	70715	57567	53553	52348	43170
Clusters (firms)	7085	6918	5330	6697	6544	5165
R^2	0.0760	0.148	0.155	0.0582	0.0870	0.0900
US firms mean	0.343	0.340	0.368	0.00506	0.00505	0.00561
US firms SD	0.475	0.474	0.482	0.00946	0.00946	0.00980
ϵ wrt $(1-\tau_d)$	0.108	0.115	0.122	-1.305	-1.324	-1.126
ϵ SD	0.139	0.138	0.104	0.393	0.398	0.345
p-val. pretrends test	0.112	0.260	0.994	0.262	0.354	0.750

Notes. Table 2 reports difference-in-difference estimates of the effect of the 2003 tax cuts on US firms (the average treatment effect for the treated) on investments and payout behaviour. All columns display the coefficient on the interaction between being incorporated in the US and a dummy for 2003 or later from a regression of the outcome on this interaction, a US-incorporation dummy, and a set of year fixed effects. The regressions are weighted using an inverse probability weighing algorthim (described by Abadie (2005)). This is to account for baseline differences between the treatment and control groups that can influence the dynamic evolution of the outcome variables. In the first year of the sample, I construct the propensity scores for the weights using a regression of the probability of being US incorporated against firm characteristics including deciles total assets in 1997, deciles of sales in 1997, deciles of age since IPO, deciles of employment, and deciles of profitability and SIC 2-digit sectors with an additional layer of 3-digit sectors for manufacturing. Controls indicate the inclusion of change in the total marginal tax rate faced by firm owners including corporation tax and dividend tax. The balanced panel restricts the sample to firms that have consolidated financial reports on COMPUSTAT for the duration of the sample period. Outcome variables are scaled by lagged total assets then winsorised at the top 5%. Elasticity equals the reported coefficient divided by the pre-2003 US firms mean, divided by the percent change in $1-\tau_d$. Standard errors are clustered by firm. Appendix A gives an overview of the quality of the matching algorithm. Appendices B-D report alternative scaling and winsorising specifications.

5 US Listed Firms did not Respond to JGTRRA

5.1 Main Results

Table 2 reports the main result of the paper: US listed firms neither paid out more dividends to their shareholders nor increased their investments in response to the 2003 dividend tax cuts. Each panel reports results for two separate outcome variables. The first row details the variable name, the second explains the level at which the variable has been winsorised. Then, for each variable, three regression specifications are reported. The first specification (columns (1), (4), (7), & (10) in Table 2) reports $\hat{\theta}$ from the main specification as described in equation 2. I then report the estimated elasticity and its standard error.[23] In the final row of each panel, I report the probability value of a test of no differential linear pre-trends on the outcome. The second specification (columns (2), (5), (8), & (11)) controls for additional firm characteristics (sales growth, cash holdings, short-term investment, R&D investment, and acquisitions) and the tax variable as described earlier. Finally, the third specification (columns (3), (6), (9), & (12)) reports the results from the second specification after re-running the matching algorithm and the outcome regression using a balanced panel of firms with accounts reported between 1997 and 2006.

Panel A reports the effect of the dividend tax cuts on total payouts and investment. The estimated average treatment effect is statistically insignificant for all specifications for both variables. The result in column (2) suggests that the average treatment effect of the reduction of the top statutory rate of dividend taxation by 23.6 percentage points on total payouts is a statistically insignificant decrease of 0.24 cent per dollar of lagged total assets. This translates to an elasticity of -0.27. The upper bound on this estimate of the elasticity is less than half of the reported elasticities in the literature (Chetty & Saez (2005) report an elasticity of 0.5). However, the p-value from a linear test of pre-trends is violated. This is because there is a trend break 2 periods exactly before the reform due to share repurchases. A similar concern exists in Yagan (2015). Corporate investment of publicly-traded firms also does not respond to dividend taxation. Column (5) suggests that the ATT is a statistically

[23] To estimate the elasticity, the ATT reported coefficient is divided by the pre-2003 US firms mean and by the percentage change in the US top marginal tax rate on dividends.

insignificant reduction of 0.23 cent per dollar of lagged total assets. This translates to an elasticity of -0.08. This result is in line with evidence from Yagan (2015) using a sample of smaller firms and a different research design. However, it contradicts other findings from papers on publicly-traded firms that usually report a positive effect on investment (Gourio & Miao 2010, Campbell et al. 2013).[24]

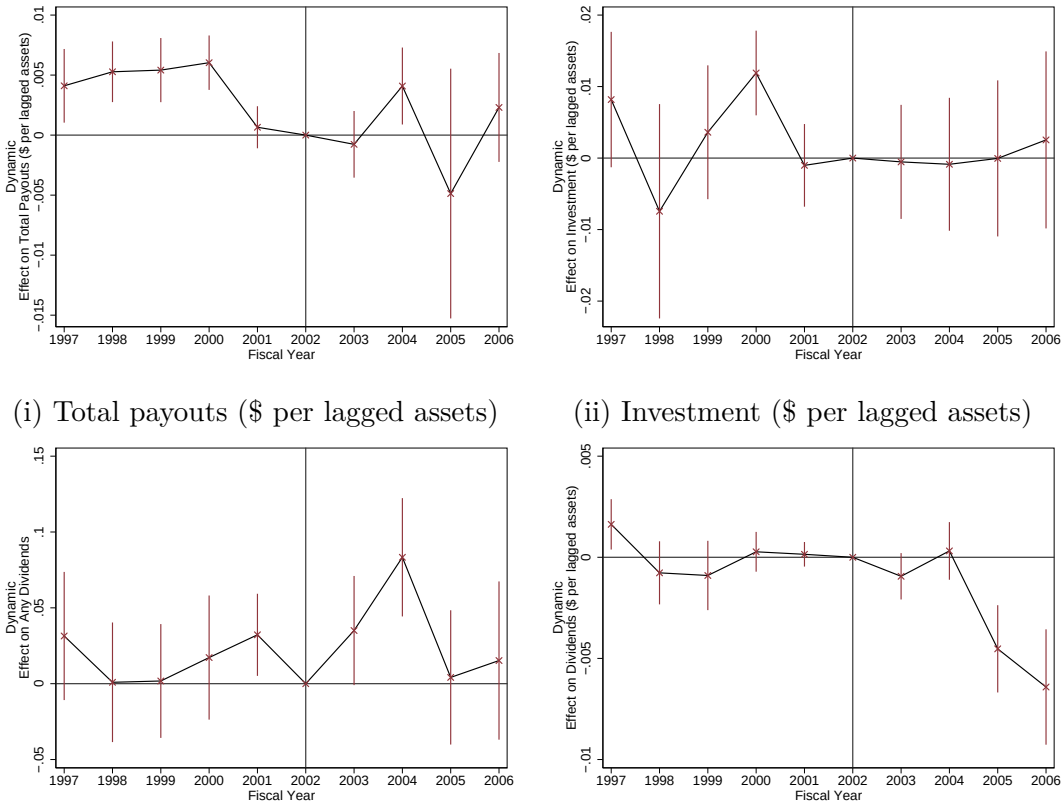
The null total payout and investment responses are the main findings of this paper. These null responses are interesting in that they contradict previously established facts on the responses to the 2003 dividend tax cuts by US publicly-traded corporations (Chetty & Saez 2005, Campbell et al. 2013). This highlights the importance of using a well-defined control group.

Panel B reports the effect of the JGTRRA on whether a firm pays any dividends (the extensive margin of payouts) and the total amount of cash dividends paid out. Similarly, the effects are insignificant for all of the specifications of the extensive margin of dividend payouts. And the test of differential linear pre-trends is satisfied. The estimated effect is consistent with a reduction in dividend payouts along with a small increase in share-buybacks. There has been a noticeable shift in the US equity market over the last 40 years of relying on share-repurchases as opposed to dividends as a mechanism of paying out individuals (Grullon & Michaely 2002). Fundamentally, this documented long-run trend seems unrelated to the tax policy on profit distributions.

These null effects are despite the large trend reversal in the time series of US corporate payouts and investment around the 2003 dividend tax cuts that was first documented by Chetty & Saez (2005) and replicated here for completeness in Appendix A.

[24]Interestingly, conditional on a true null effect, standard assumptions on the error process in panel data settings leads to a downward bias on the standard errors. This leads to "fictitious non-zero effects" Bertrand et al. (2004).

5.2 Dynamics of the Effect



(i) Total payouts ($ per lagged assets)

(ii) Investment ($ per lagged assets)

(iii) Effect on any dividend payout.

(iv) (Dividends $ per lagged assets)

Figure 6: Dynamic effect of the 2003 JGTRRA on US firms' financial and investment behaviour.

Figure 29 reports a generalisation of the results from the second specification in Table 2 that allows for lags and leads in the effect of the reform to study the dynamics of the effect of the reform. The figure reports the θ_l coefficients from $Y_{it} = \sum_{\substack{l \neq 2002 \\ l>1997}} \theta_l . DD_l + \alpha_t + Z_{it} + \mu_{it}$ with inverse probability weights, where DD_l is the variable identifying the treatment group with suitable lags and leads.

Firstly, the plots show that, aside from the trend break in total payouts as a result of share repurchases, there is no statistically significant pre-trends. Additionally, where there is a linear pre-trend, it would seem to bias our estimated effect upwards. Hence, the null estimated effect is even more believable.

There is a limited increase in total payouts and the extensive margin for dividend payouts

in 2004 relative to 2002. This increase quickly disappears after 2004 as opposed to prior literature on total payouts documented by Yagan (2015) where payouts continue increasing until 2008. This muted rise is completely reversed after the reform for the dividends margin.

For investment, there is no effect of the dividend tax reduction on corporate investment at any time during the period.

5.3 Robustness

Alternative winsorising levels and choice of scale

Tables 4-9 in the appendices report the same results for Table 2 under different winsorisation percentages and using different scaling methods. Appendix B reports on robustness to winsorisation method. Appendix C reports robustness to scaling method. Winsorisation does not seem to be influencing the average treatment effect on the treated for all the variables. The same can be said for the scaling choice except for scaling with lagged common equity. This variable is cyclical. On the whole, the finding that corporate payouts and investment by publicly-traded US firms are unresponsive to the US dividend tax rate is corroborated.

Different Matching Methods

In Table 10, I re-estimate the specifications reported in Table 2 using nearest neighbour matching. In Table 11, I rerun the matching exercise using a larger set of pre-treatment variables. The results from both exercises are similar.

Given that firm age and employment include many missing values, I rerun Table 2 without including the firm age variable in Table 12 and additionally matching using multiple pre-reform years in Table 13. In those tables, I find tighter estimated null effects around 0, although the pre-trends test is violated for some of the outcome variables. My results are robust to these choices, given that the estimated elasticities in practice are very close to the headline value.

Announcement Robustness

In Figure 26, I re-estimate the regression summarised in Figure 29 but assign firm accounts reported after January 2003 as treated firms, given that the policy was first publicly announced in January. The results reported earlier are robust to allowing for an announcement effect.

Robustness to control countries

I re-estimate the average treatment effect excluding each control domicile country one-by-one. Figure 27 reports the treatment effects from this exercise. The results documented in the previous subsection are robust to the removal of firms domiciled in each of the control countries. This strengthens our belief that the documented null effect is not determined by any one country's institutional settings or, indeed, changes. For example, Japan had a dividend tax cut around the same period but also has a unique corporate governance structure where there is a large amount of cross-ownership between firms. Removing Japanese domiciled firms has a limited effect on the estimated parameters, even though Japanese firms account for a large fraction of control firms as seen in Figure 3.

6 Why were listed firms unresponive to JGTRRA?

Immediately after the reform, Poterba (2004) argued that the tax cut may pay for itself by estimating a long-run elasticity of 3.3 based on US time series data over the century. A similarly large elasticity was reached later by Chetty & Saez (2005) using data on the 2003 reform. In this paper, I argue that these estimates are clouded by the cyclical nature of dividend payouts. Why then did listed US firms not respond? The answer to this question is even more important given that we know smaller firms increase their dividends in response to dividend tax cuts (Yagan 2015, Boissel & Matray 2022). Hence, it is surprising that larger firms do not respond to dividend tax cuts. I provide evidence on two main arguments: a financing argument furthered in the literature and a foreign ownership argument, which might be even more salient today relative to 20 years ago.

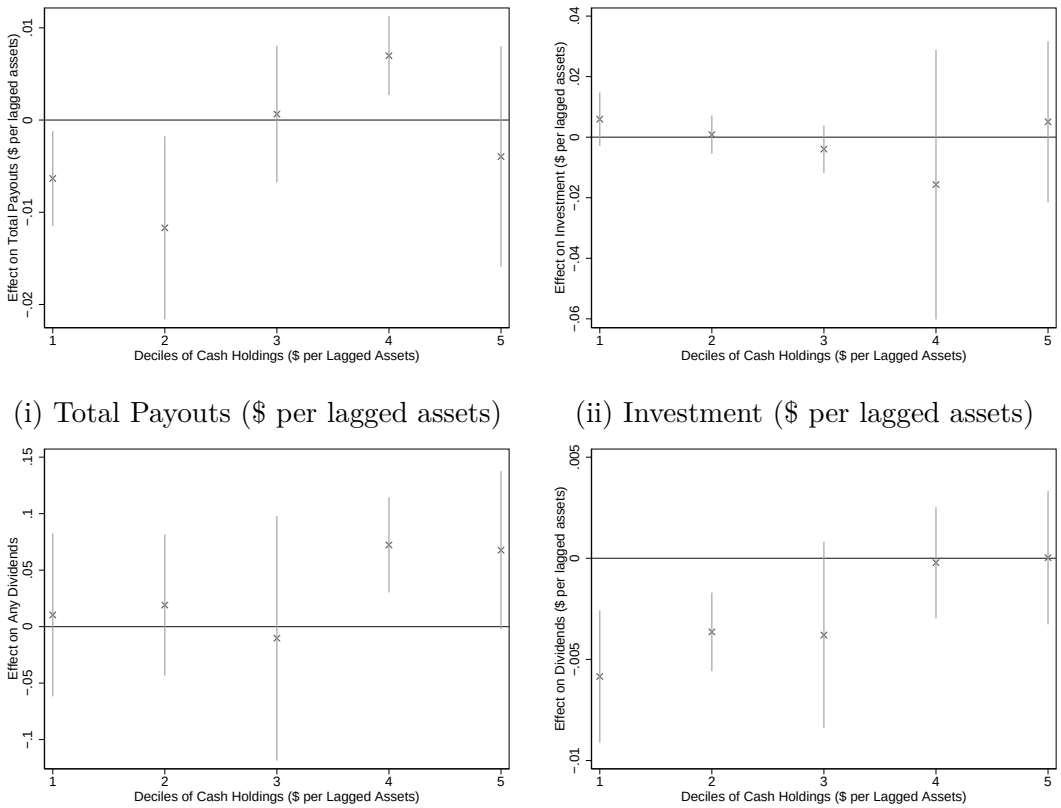
6.1 Financing argument: New vs Old View of the Firm

Further to our previous results, I find no pattern in the average treatment effect on the treated when the estimation procedure is repeated by deciles of US firm cash-holdings. There are multiple ways to rationalise these results; chiefly that the traditional view of the firm does not seem to be of first-order importance for US listed firms. US listed corporations do not seem to be responding to the US dividend tax policy. This result indicates that most

firms in this sample are not cash-constrained. There are multiple candidate explanations for the null response.

Another way to rationalise this result is that there are firm-level variations that account for differences in the cut-off cash-holdings necessary to change payout policy. Cash-poor firms theoretically ought to be investing and paying out more after a dividend tax change of this size relative to cash rich firms. This is not the pattern that I observe. More generally, firms that finance investment externally as opposed to internally ought to respond most. Appendix G reports the results by quintiles of the KZ index of equity dependence (Baker et al. 2003).[25] This broadly confirms the previous finding that even the most equity dependent firms are not responding to the tax cut. Hence, we are likely dealing with "new view" firms or other (potentially ownership-related) explanations for the null response.

[25]There is a worry in the literature as to whether the KZ-index actually measures equity dependence (Hadlock & Pierce 2010). Appendix G shows that in this sample the KZ-index is associated negatively with share repurchases.



(i) Total Payouts ($ per lagged assets)

(ii) Investment ($ per lagged assets)

(iii) Effect on any dividend payout.

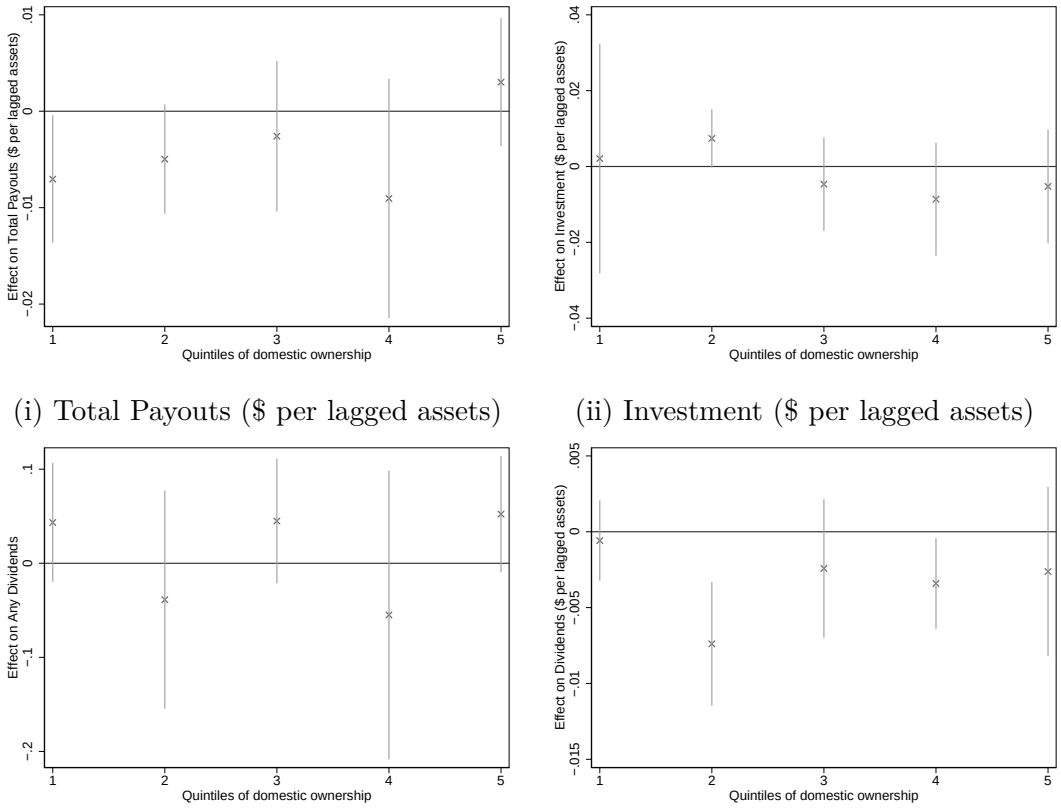
(iv) Dividends ($ per lagged assets)

Figure 7: Heterogeneity of the effect along the distribution of US firm cash holdings in 2002.
Notes. Firms are divided into deciles of US firm cash holdings. The coefficient reported is of the outcome regression estimated by the deciles of cash holdings of US firms.

6.2 Ownership of US Listed Firms: Domestic vs Foreign

Figure 8 reports the heterogeneity of the treatment effect with respect to the extent to which a firm is domestically-held. The US dividend tax code is more salient to domestically-owned US domiciled firms relative to domestically-owned French or British domiciled firms. This is because shareholders of these US firms are US tax residents and are more likely to benefit from the US dividend tax cuts. I find that domestically owned US domiciled firms are not likely to react to dividend tax cuts when compared to domestically owned OECD firms. This result is robust to the choice of the measure of domestic ownership used as seen in Figure 19 in Appendix G.

Separately, in Figures 20-23, I rerun the entire difference-in-difference analysis using the degree of US ownership as the treatment intensity variable, with 5 quintiles of US ownership. This alternative identification strategy confirms the main results in the paper that the US dividend tax cuts had no effect on payouts and investment decisions of US firms. The null result here indicates that, regardless of the international ownership structure of US listed firms, listed firms that face a relevant dividend tax cut do not change their financial or investment decisions as a result of the tax cuts.



(i) Total Payouts ($ per lagged assets)

(ii) Investment ($ per lagged assets)

(iii) Effect on any dividend payout.

(iv) Dividends ($ per lagged assets)

Figure 8: Heterogeneity of the effect based on the distribution of domestic firm ownership in March 2003.
Notes. Firms are divided into quintiles of domestic firm ownership. US domiciled firms that are held mostly by domestic owners (quintile 5) should be more likely to respond relative to domestically owned firms in the OECD.

7 Implications for Models of Firm Behaviour

I have shown that US listed firms do not respond to US dividend taxation both in terms of total payouts and investment. The effect was statistically insignificant. This result is in contrast to multiple studies on listed US firms' behaviour in the aftermath of the 2003 tax cut. The main reason for this difference in results is that this paper constructs a different, arguably more valid control group for listed US firms. Even though as a time series, payouts and investment by listed US firms showed a marked trend reversal around the cut in US dividend tax rates, it is clear that these cyclical trends were similar for listed firms in other OECD countries. Dynamically, there is a statistically significant positive effect on total payout for one year immediately after the reform. This contrasts with an observed increase in dividend payouts in (Yagan 2015) for 5 years after the reform for a sample of smaller US firms. This may indicate that, on the re-election of the Bush administration, bigger firms were able to update their beliefs about the permanency of the dividend tax cuts as opposed to the smaller US firms studied in Yagan (2015).

There are multiple models of the firm that can be reassessed in light of these results:

Traditional view of the firm. Firms, according to this view, have a sudden reduction in their cost of capital, as a result of the reduced cost of equity. This view assumes that firms finance their investments through issuance of new equity (Poterba & Summers 1984). Equity-dependent firms raise equity (in expectation, their shareholders have to pay taxes on subsequent dividends from this raised equity). Hence, the dividend tax distorts their decision to invest and a permanent cut in dividend taxes reduces this distortion. Thus, we expect these firms to increase their investments in response to a dividend tax cut, especially should they believe the tax cut to have been permanent given the re-election of President Bush in 2004. There is limited evidence that US listed firms changed their payout or investment behaviour along multiple measures of investment and payouts. This result is robust to multiple specifications, announcement effects, and control countries. One argument for the limited reaction of US listed firms when compared with non-US listed firms is that both types of firms are too internationally owned for the US tax code to matter. Using unique ownership data, I show that even the most domestically-owned US listed firms do not respond to one

of the largest dividend tax cuts in US history. My results suggest that it is unlikely the case that US listed firms are equity dependent "traditional" view firms along (Poterba & Summers 1984). Taken together with evidence on smaller unlisted firms, we can conclude that no investment response occurred in the US by any firms. The increase in dividend payouts in unlisted firms documented by Yagan (2015) is unlikely to have been channelled as productive investments to other firms as some papers held (**?**).

New View of the Firm. Economically, there is a reason to justify why capital elasticity is zero at this margin and why firms do not increase pay-outs in response to a tax cut on dividends. Under the 'new' view of the firm, cash-rich firms finance their investments from retained earnings (Auerbach 1979, Stiglitz 1973, King 1977). A permanent change in dividend taxation faced by the owners of the firms does not affect the inter-temporal decision to payout dividends today vs tomorrow as long as there is no room for tax arbitrage. Even though the tax cut announced here was temporary, it seems that there was no effect caused by the policy. This null effect is documented even for firms whose owners are US residents. This additional null result implies that, even for firms whose owners are likely to face a more relevant tax cut for its owners, there was no change in investment or payout policy, implying that these firms are best approximated by the "new view."

Dividend Tax Relevance. One important feature of the analysis presented in this paper is to ask whether there is a dividend tax irrelevance problem. International owners of US listed firms are unaffected by the dividend tax cuts. If they are the marginal owners targetted by US listed corporate boards, then it is unlikely that US listed firms will change their behaviour in response to a domestic dividend tax cut. This feature seems to have been previously ignored by the literature. This international ownership feature probably explains part of the null response of dividend payouts documented in this paper, compared to the rise in dividend payouts in smaller domestically-owned firms in Yagan (2015). Two important caveats to note are that, firstly, it is well documented that in times of heightened uncertainty, investment is not responsive to policy changes (Bloom et al. 2007). Although globally the political tension were heightened around the 2003 tax cuts, this mechanism might have been more at play in the run-up to the Iraq war in the US relative to other OECD nations. This may have dampened the ATT for both payouts and subsequently investment responses.

Secondly, corporate boards, due to a misalignment of interests with shareholders, might not respond at all the shareholders payout taxes. These two channels could also explain the headline null result in this paper.

8 Conclusion

In this paper, I revisit the impact of the 2003 US dividend tax cuts on the investment and financial behaviour of the publicly-traded US firms using a matched difference-in-difference design around the period of the reform. Despite promising to incentivise growth and investment, the tax cut actually led to no change in both payouts and investment of listed US firms compared to observationally similar listed non-US firms. This paper closes an important open gap in the literature on the response of large publicly-traded firms to the 2003 US dividend tax cut (Yagan 2015). My results are at odds with previous work on the response of US listed firms to the dividend tax cuts. This work was used to build and discipline a line of general equilibrium models that need to be revised in light of the new evidence. The results are consistent with a "new view" firm that finances its investment through retained earnings. Additionally, the results are also consistent with a domestic dividend tax irrelevance view which emphasises the liquid and international nature of the US stock market, in which US taxpayers compete with individuals and institutions from all around the world for ownership of US stocks. With the rising degree of globalisation, domestic dividend taxes are likely to be even less relevant for listed firms decisions. Hence, their use as a policy lever to increase "jobs and growth" is likely to remain limited.

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Zwick, E. & Mahon, J. (2017), 'Tax policy and heterogeneous investment behavior', *American Economic Review* **107**(1), 217–48.
URL: *https://www.aeaweb.org/articles?id=10.1257/aer.20140855*

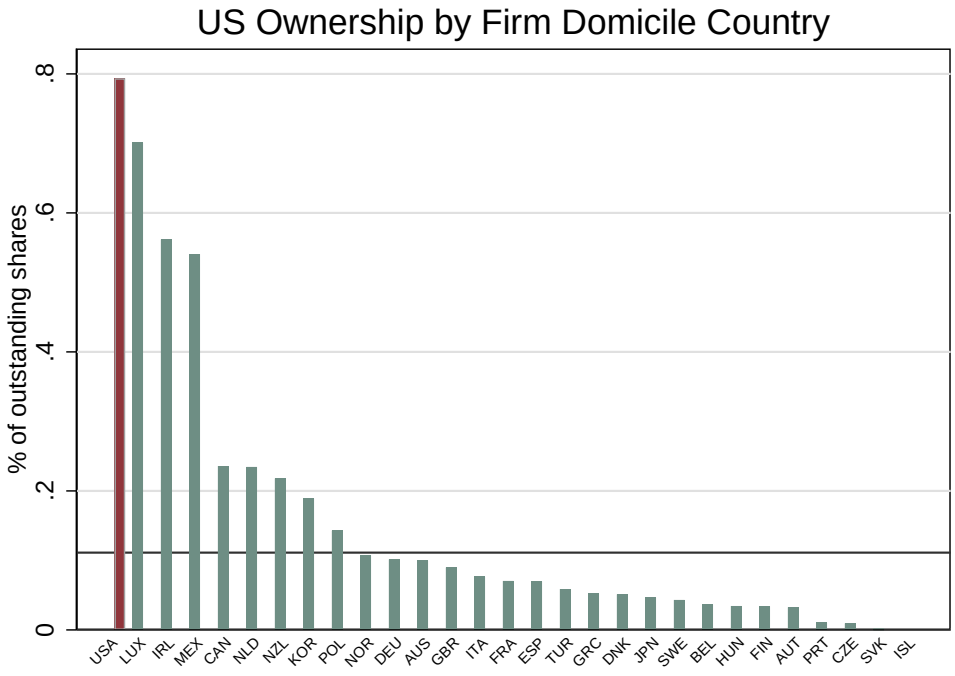


Figure 9: Reported US Ownership by Firm Country of Domicile relative to total reported ownership.
Source. COMPUSTAT, Factset Ownership, and Author's calculations (Board of Governors of the Federal Reserve System (US) 1946). Horizontal line indicates OECD average. This figure includes firms where data on ownership of more than 20% of outstanding shares data is available.

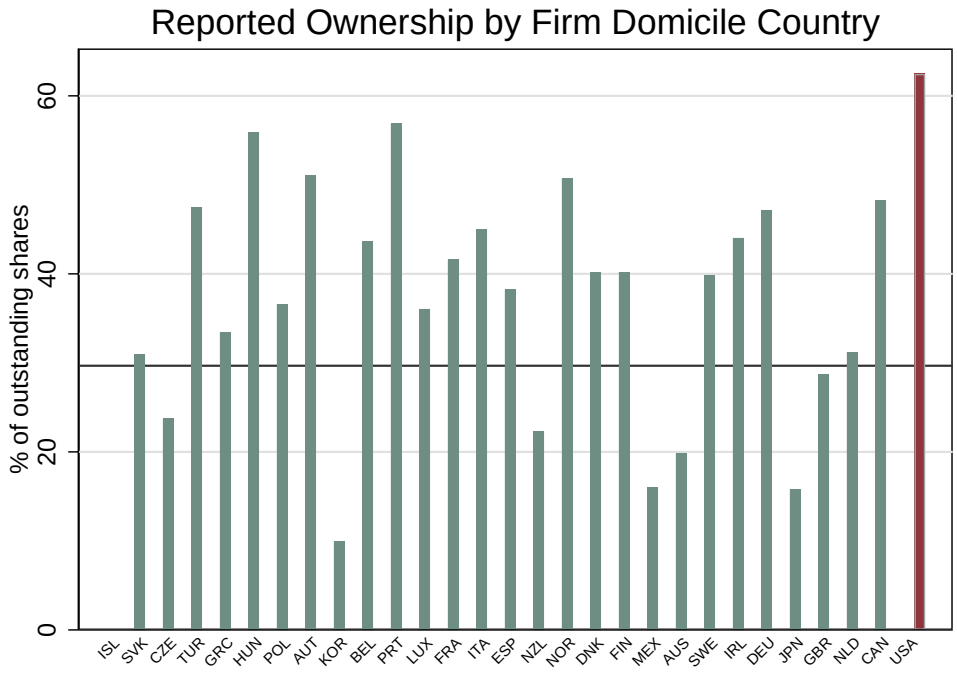


Figure 10: Reported Ownership by Firm Country of Domicile relative to total reported ownership.
Source. COMPUSTAT, Factset Ownership, and Author's calculations (Board of Governors of the Federal Reserve System (US) 1946). Horizontal line indicates OECD average. Firms with ownership greater than 100% are assigned a value of 100%.

Appendix A: Event Study Evidence Chetty & Saez (2005)

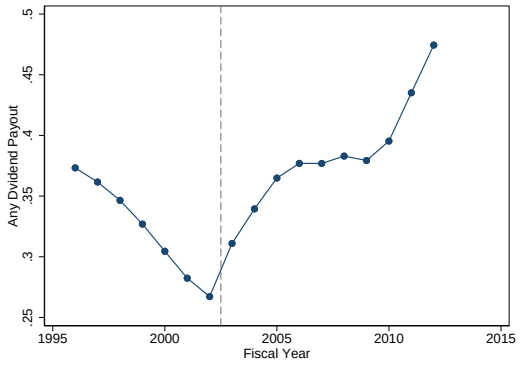
This appendix highlights the marked trend reversal in dividend payouts and investment in the US after the 2003 dividend tax cuts as seen in Figure 11. However, this reversal is also found in the data for publicly-traded firms in other OECD countries. In this exercise, no matching is done. Absent any comparison to the OECD firms, I observe the trend reversal in dividend payouts and investment in the US in this sample of firms. This is in line with evidence presented earlier as causal for the increase in US dividend payouts and investment (Chetty & Saez 2005, Blouin et al. 2011, Campbell et al. 2013).

The regression used to estimate the post-2003 coefficients presented in subfigures (ii), (iv), and (vi) of Figure 11 is:

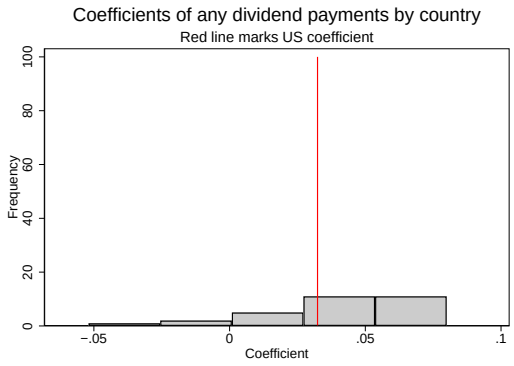
$$Y_{it} = \alpha_c + \beta_c.T + \gamma_c.D_{post-03} + \delta_c.T \times D_{post-03} + \mu_{it}, \qquad \forall c = 1, ..., C \tag{4}$$

where Y_{it} is the outcome variable, $D_{post-03}$ is a dummy for post-2003, and T is a linear time trend. The δ_c coefficient thus estimates the break in a linear trend after 2003. I then plot a histogram summarising the estimated δ_c coefficients. The large δ_{US} was used in previous work to suggest that the impact of US dividend taxation on US firm behaviour is large. These studies include Chetty & Saez (2005), Gourio & Miao (2010), Chetty & Saez (2010). However I observe that relative to the rest of the distribution of δ_c, the coefficient estimated for the US is not substantially different.

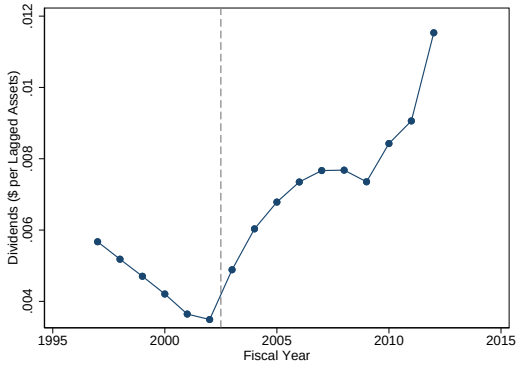
Thus, in this Appendix, we can see that, although listed firms experience the previously documented large trend reversal in dividend payouts and investment around 2003, this pattern is observed in other non-US domiciled firms. This indicates that there is a cyclical trend that potentially underlies this trend reversal as opposed to a US tax policy reform.



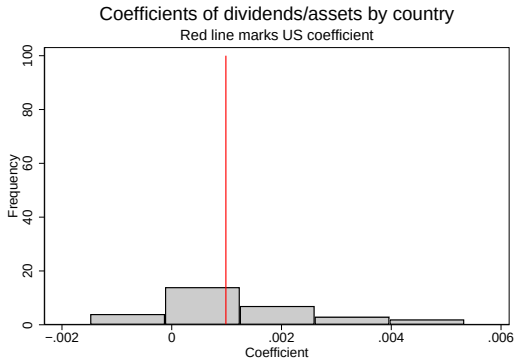
(i) US firms: Extensive margin of dividends.



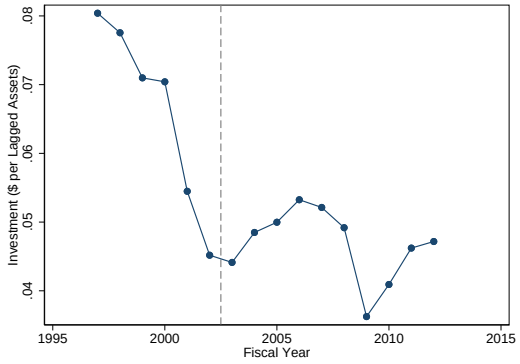
(ii) Post-03 Coefficient: Extensive margin.



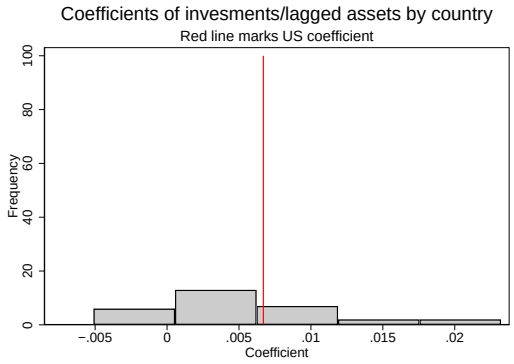
(iii) US firms: Dividends ($ per lagged assets).



(iv) Post-03 Coefficient: Dividends ($ per lagged assets).



(v) US firms: Investment ($ per lagged assets).



(vi) Post-03 Coefficient: Investment ($ per lagged assets).

Figure 11: Time series behaviour of US dividend payouts and investment on the LHS. This trend is contrasted with the behaviour in the rest of the OECD nations on the RHS.
Notes. The samples used to produce these graphs do not use any matching algorithm.

Appendix B: Match Quality

Baseline Match

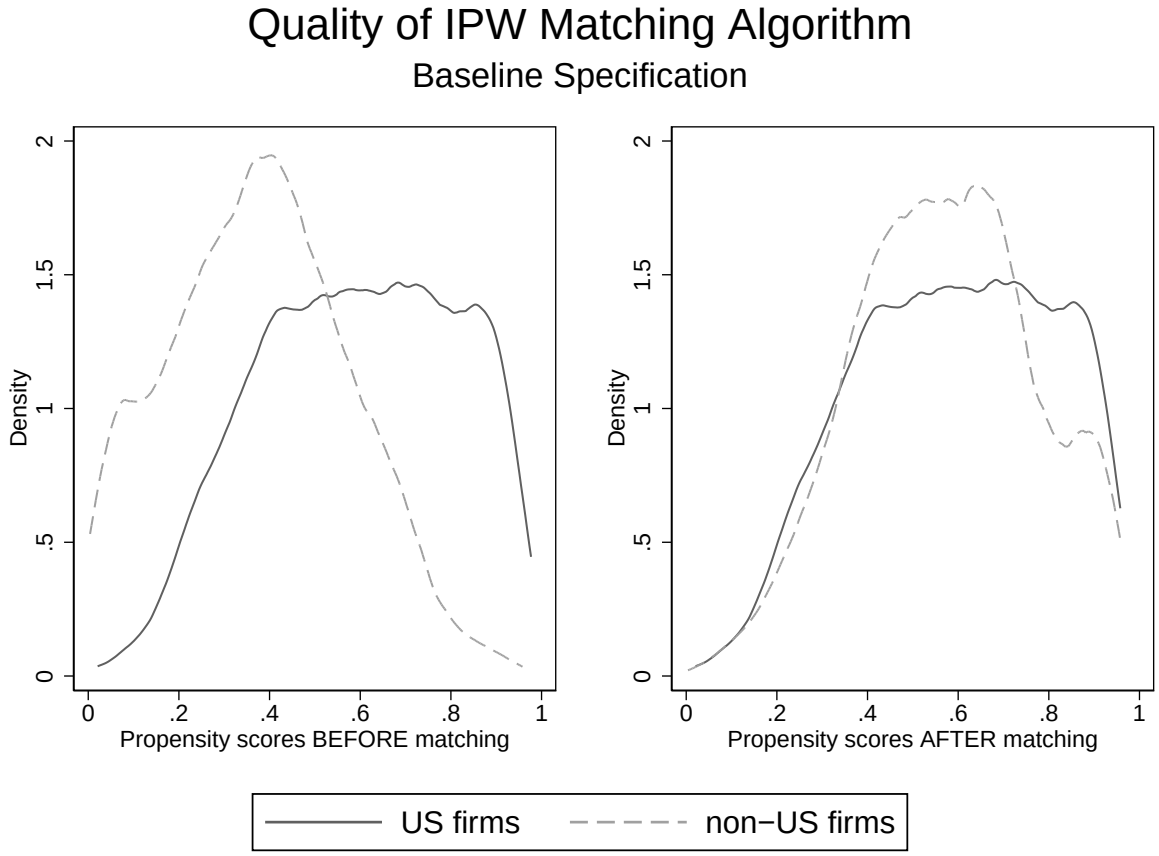


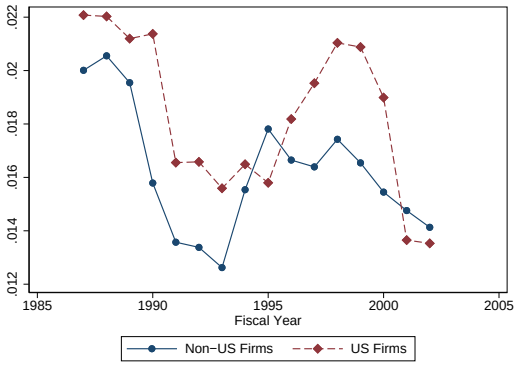
Figure 12: Performance of Inverse Propensity Weighting (IPW) in the baseline specification used for the main graphs and tables in the paper.

Table 3: Tests of the performance of the IPW matching.

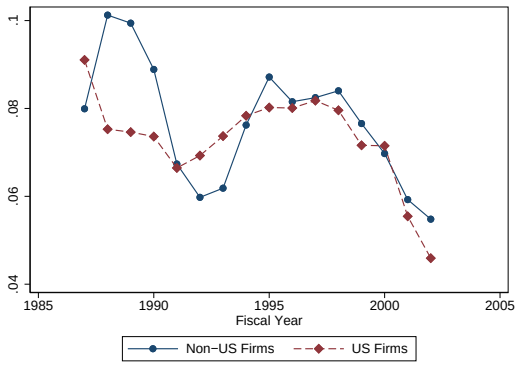
	Pseudo R^2	Prob>χ^2	Mean Bias	Median Bias	Rubin's B	Rubin's R
Unmatched Sample	0.18	0.00	12.32	9.49	103.81	1.02
Matched Sample	0.01	0.13	2.30	1.61	21.04	1.59

Notes. Diagnostic tests on the performance of the baseline specification of the IPW matching exercise. This exercise uses a probit regression p(US=1|X) with controls (X) as deciles of assets, deciles of sales, deciles of firm age since initial public offering, and industry dummies based on year 1997.

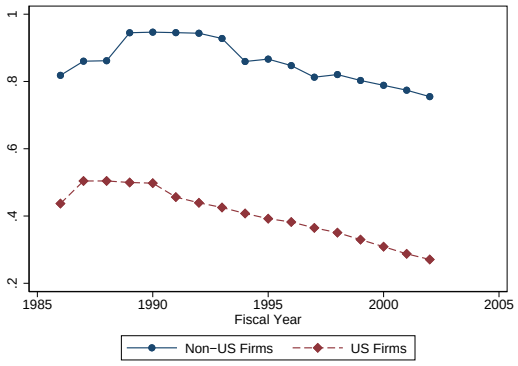
Long run Parallel Trends?



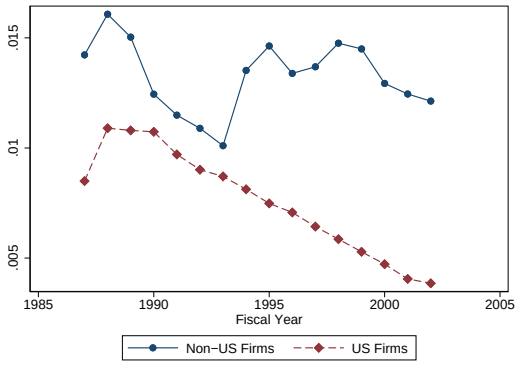
(i) Total Payouts ($ per lagged assets)



(ii) Investment ($ per lagged assets)



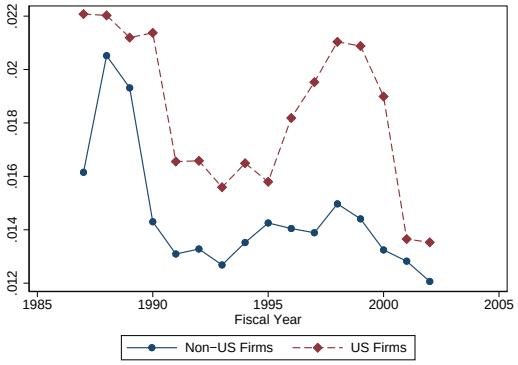
(iii) Any dividend payout.



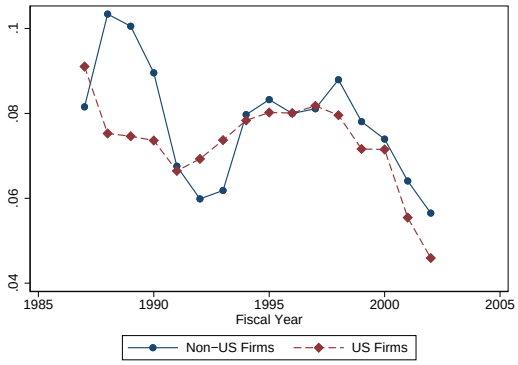
(iv) Dividends ($ per lagged assets)

Figure 13: Long-run parallel trends.

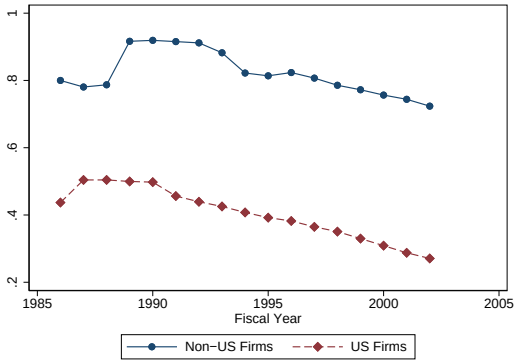
Notes. Long-run parallel trends for firms in the subsample of countries with longer time horizon using the main sample of firms in the analysis. No controls or matching applied.



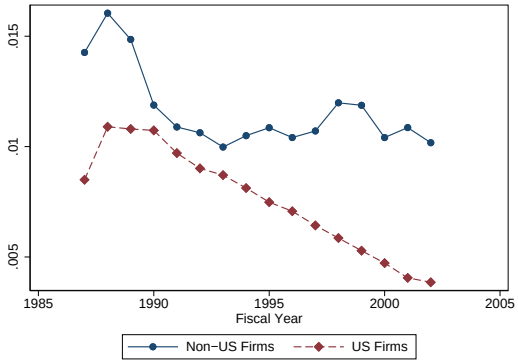
(i) Total Payouts ($ per lagged assets)



(ii) Investment ($ per lagged assets)



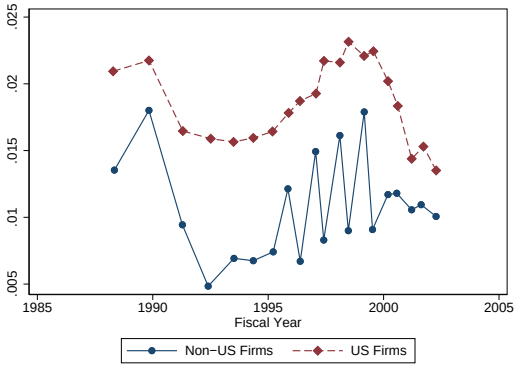
(iii) Any dividend payout.



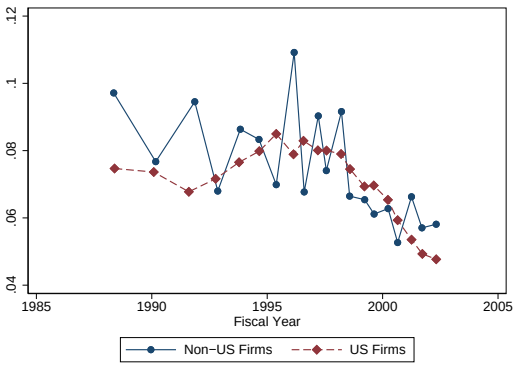
(iv) Dividends ($ per lagged assets)

Figure 14: Long-run parallel trends.

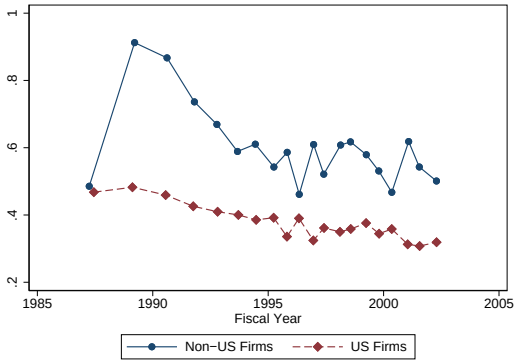
Notes. Long-run parallel trends for firms in the subsample of countries using the main sample of firms in the analysis. No controls or matching applied.



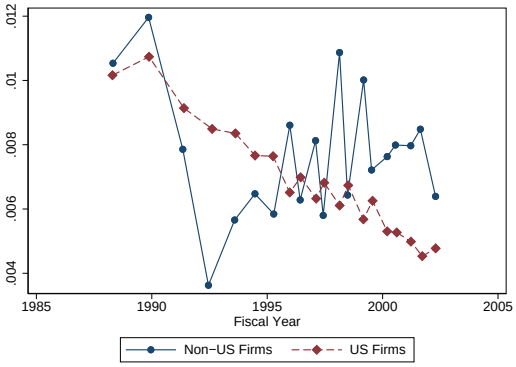
(i) Total Payouts ($ per lagged assets)



(ii) Investment ($ per lagged assets)



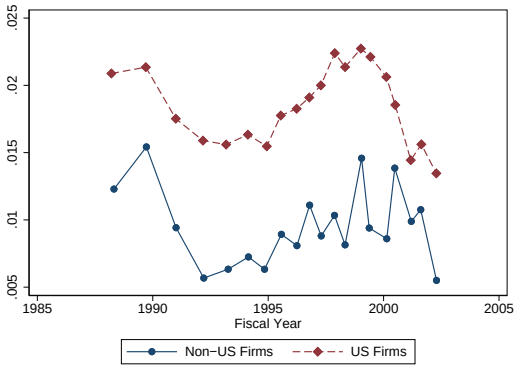
(iii) Any dividend payout.



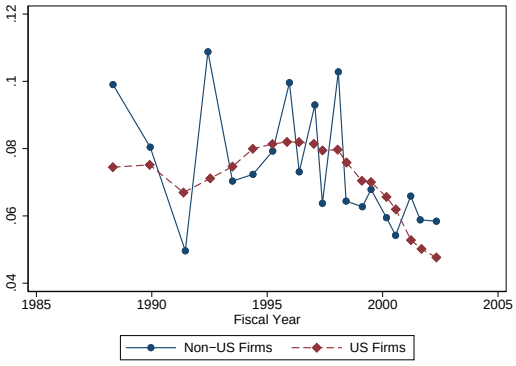
(iv) Dividends ($ per lagged assets)

Figure 15: Long-run parallel trends.

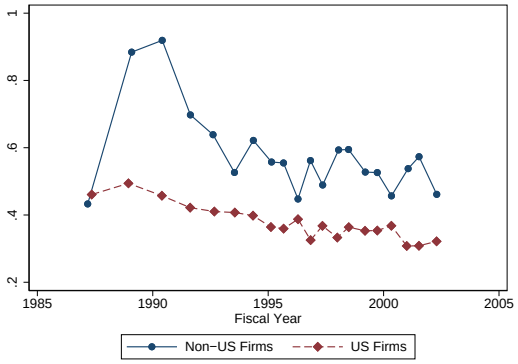
Notes. Long-run parallel trends for firms in the subsample of countries with longer time horizon using the main sample of firms in the analysis. Controls and matching.



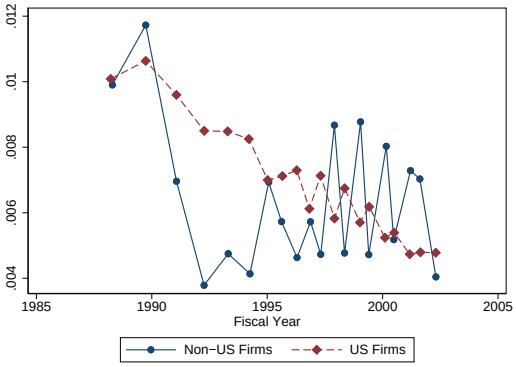
(i) Total Payouts ($ per lagged assets)



(ii) Investment ($ per lagged assets)



(iii) Any dividend payout.



(iv) Dividends ($ per lagged assets)

Figure 16: Long-run parallel trends.

Notes. Long-run parallel trends for firms in the subsample of countries using the main sample of firms in the analysis. Controls and matching.

Appendix C: Robustness to winsorising method

Table 4: Alternative winsorising for Total Payouts.

	Total Payout (USD per lagged Assets) Top Winsorised at 5%			Total Payouts (USD per lagged Assets) Top Winsorised at 1%		
	Unbalanced		Balanced	Unbalanced		Balanced
Panel A	(1)	(2)	(3)	(4)	(5)	(6)
US*Post-2003	-.00217	-.00241	-.00301	-.00292	-.00352	-.00462
	(.00202)	(.0021)	(.00232)	(.00266)	(.00275)	(.00325)
Matching	Y	Y	Y	Y	Y	Y
Controls	N	Y	Y	N	Y	Y
Observations (firm-years)	41929	40809	32433	41929	40809	32433
Clusters (firms)	5421	5272	3938	5421	5272	3938
R^2	0.0294	0.0388	0.0367	0.0208	0.0260	0.0250
US firms mean	0.0206	0.0206	0.0225	0.0254	0.0253	0.0274
US firms SD	0.0324	0.0324	0.0332	0.0497	0.0496	0.0504
ϵ wrt $(1-\tau_d)$	-0.244	-0.272	-0.310	-0.266	-0.322	-0.391
ϵ SD	0.226	0.236	0.239	0.243	0.251	0.274
p-val. pretrends test	0.000784	0.000115	6.35e-05	0.168	0.0259	0.0226
	Total Payouts (USD per lagged Assets) Double-sided Winsorised at 5%			Total Payouts (USD per lagged Assets) Double-sided Winsorised at 1%		
	Unbalanced		Balanced	Unbalanced		Balanced
Panel B	(7)	(8)	(9)	(10)	(11)	(12)
US*Post-2003	-.00259	-.00294	-.00383	-.0035	-.00424	-.00543
	(.00235)	(.00244)	(.00281)	(.00293)	(.00302)	(.00363)
Matching	Y	Y	Y	Y	Y	Y
Controls	N	Y	Y	N	Y	Y
Observations (firm-years)	41929	40809	32433	41929	40809	32433
Clusters (firms)	5421	5272	3938	5421	5272	3938
R^2	0.0254	0.0323	0.0306	0.0188	0.0230	0.0225
US firms mean	0.0233	0.0232	0.0253	0.0264	0.0263	0.0283
US firms SD	0.0408	0.0408	0.0418	0.0563	0.0562	0.0562
ϵ wrt $(1-\tau_d)$	-0.257	-0.293	-0.350	-0.307	-0.373	-0.444
ϵ SD	0.233	0.242	0.256	0.256	0.265	0.296
p-val. pretrends test	0.0176	0.00274	0.00164	0.218	0.0274	0.0313

Notes.

Table 5: Alternative winsorising for Investment.

	Investment (USD per lagged Assets) Top Winsorised at 5%			Investment (USD per lagged Assets) Top Winsorised at 1%		
	Unbalanced		Balanced	Unbalanced		Balanced
Panel A	(1)	(2)	(3)	(4)	(5)	(6)
US*Post-2003	-.00208	-.00233	-.00153	-.00251	-.00265	-.0013
	(.00523)	(.00534)	(.00482)	(.00894)	(.00918)	(.00801)
Matching	Y	Y	Y	Y	Y	Y
Controls	N	Y	Y	N	Y	Y
Observations (firm-years)	61079	59769	48864	61079	59769	48864
Clusters (firms)	7059	6899	5322	7059	6899	5322
R^2	0.0424	0.0468	0.0472	0.0272	0.0318	0.0313
US firms mean	0.0659	0.0660	0.0666	0.0713	0.0714	0.0717
US firms SD	0.0650	0.0650	0.0647	0.0880	0.0881	0.0865
ϵ wrt $(1-\tau_d)$	-0.0729	-0.0818	-0.0531	-0.0814	-0.0859	-0.0419
ϵ SD	0.184	0.187	0.168	0.290	0.297	0.259
p-val. pretrends test	0.935	0.771	0.687	0.797	0.992	0.483

	Total Payouts (USD per lagged Assets) Double-sided Winsorised at 5%			Total Payouts (USD per lagged Assets) Double-sided Winsorised at 1%		
	Unbalanced		Balanced	Unbalanced		Balanced
Panel B	(7)	(8)	(9)	(10)	(11)	(12)
US*Post-2003	-.00226	-.00244	-.00129	-.00375	-.0039	-.00241
	(.00739)	(.00758)	(.00669)	(.0103)	(.0106)	(.00919)
Matching	Y	Y	Y	Y	Y	Y
Controls	N	Y	Y	N	Y	Y
Observations (firm-years)	61079	59769	48864	61079	59769	48864
Clusters (firms)	7059	6899	5322	7059	6899	5322
R^2	0.0353	0.0397	0.0397	0.0218	0.0263	0.0253
US firms mean	0.0692	0.0693	0.0698	0.0723	0.0724	0.0725
US firms SD	0.0770	0.0771	0.0763	0.0972	0.0972	0.0944
ϵ wrt $(1-\tau_d)$	-0.0754	-0.0815	-0.0428	-0.120	-0.125	-0.0768
ϵ SD	0.247	0.253	0.222	0.331	0.339	0.293
p-val. pretrends test	0.826	0.998	0.532	0.864	0.929	0.545

Notes.

Table 6: Alternative winsorising for Cash Dividends.

	Dividends (USD per lagged Assets) Top Winsorised at 5%			Dividends (USD per lagged Assets) Top Winsorised at 1%		
	Unbalanced		Balanced	Unbalanced		Balanced
Panel A	(1)	(2)	(3)	(4)	(5)	(6)
US*Post-2003	-.00286***	-.00289***	-.00273***	-.00229**	-.00234**	-.00205*
	(.000856)	(.000864)	(.000833)	(.001)	(.00102)	(.00106)
Matching	Y	Y	Y	Y	Y	Y
Controls	N	Y	Y	N	Y	Y
Observations (firm-years)	53553	52348	43170	53553	52348	43170
Clusters (firms)	6697	6544	5165	6697	6544	5165
R^2	0.0582	0.0870	0.0900	0.0277	0.0456	0.0487
US firms mean	0.00506	0.00505	0.00561	0.00672	0.00672	0.00728
US firms SD	0.00946	0.00946	0.00980	0.0156	0.0156	0.0157
ϵ wrt $(1-\tau_d)$	-1.305	-1.324	-1.126	-0.787	-0.805	-0.652
ϵ SD	0.393	0.398	0.345	0.345	0.350	0.337
p-val. pretrends test	0.262	0.354	0.750	0.699	0.786	0.565

	Total Payouts (USD per lagged Assets) Double-sided Winsorised at 5%			Total Payouts (USD per lagged Assets) Double-sided Winsorised at 1%		
	Unbalanced		Balanced	Unbalanced		Balanced
Panel B	(7)	(8)	(9)	(10)	(11)	(12)
US*Post-2003	-.00278***	-.00281***	-.00259***	-.0022**	-.00227**	-.002*
	(.000906)	(.000916)	(.000904)	(.00108)	(.00109)	(.00117)
Matching	Y	Y	Y	Y	Y	Y
Controls	N	Y	Y	N	Y	Y
Observations (firm-years)	53553	52348	43170	53553	52348	43170
Clusters (firms)	6697	6544	5165	6697	6544	5165
R^2	0.0411	0.0651	0.0678	0.0222	0.0370	0.0405
US firms mean	0.00596	0.00596	0.00656	0.00709	0.00709	0.00760
US firms SD	0.0123	0.0123	0.0126	0.0180	0.0180	0.0177
ϵ wrt $(1-\tau_d)$	-1.079	-1.094	-0.915	-0.720	-0.741	-0.609
ϵ SD	0.353	0.357	0.320	0.351	0.356	0.355
p-val. pretrends test	0.321	0.406	0.994	0.701	0.779	0.552

Notes.

Appendix D: Robustness to scaling method

Table 7: Alternative scaling for Total Payouts.

	Total Payout (USD per lagged Assets) Top Winsorised at 5%			Total Payout (USD per lagged Revenue) Top Winsorised at 5%		
	Unbalanced		Balanced	Unbalanced		Balanced
Panel A	(1)	(2)	(3)	(4)	(5)	(6)
US*Post-2003	-.00217	-.00241	-.00301	-.00475***	-.00454***	-.00515***
	(.00202)	(.0021)	(.00232)	(.00156)	(.00158)	(.0017)
Matching	Y	Y	Y	Y	Y	Y
Controls	N	Y	Y	N	Y	Y
Observations (firm-years)	41929	40809	32433	41834	40745	32390
Clusters (firms)	5421	5272	3938	5416	5272	3938
R^2	0.0294	0.0388	0.0367	0.0447	0.0488	0.0475
US firms mean	0.0206	0.0206	0.0225	0.0226	0.0225	0.0242
US firms SD	0.0324	0.0324	0.0332	0.0380	0.0379	0.0384
ϵ wrt $(1-\tau_d)$	-0.244	-0.272	-0.310	-0.486	-0.468	-0.492
ϵ SD	0.226	0.236	0.239	0.159	0.162	0.162
p-val. pretrends test	0.000784	0.000115	6.35e-05	0	5.87e-11	5.98e-11

	Total Payout (USD per lagged Common Equity) Top Winsorised at 5%			Total Payout (USD per lagged Employees) Top Winsorised at 5%		
	Unbalanced		Balanced	Unbalanced		Balanced
Panel B	(7)	(8)	(9)	(10)	(11)	(12)
US*Post-2003	.00556	.00642	.00504	-.887	-.887	-1.3
	(.0277)	(.0289)	(.0356)	(.695)	(.701)	(.79)
Matching	Y	Y	Y	Y	Y	Y
Controls	N	Y	Y	N	Y	Y
Observations (firm-years)	41902	40785	32415	38472	37616	29996
Clusters (firms)	5421	5272	3938	5202	5071	3789
R^2	0.000577	0.000919	0.00131	0.0332	0.0373	0.0371
US firms mean	0.0312	0.0314	0.0342	4.786	4.746	5.173
US firms SD	0.416	0.416	0.469	8.642	8.624	8.815
ϵ wrt $(1-\tau_d)$	0.412	0.473	0.341	-0.429	-0.433	-0.580
ϵ SD	2.056	2.139	2.417	0.337	0.342	0.355
p-val. pretrends test	0.526	0.552	0.545	0.358	0.149	0.109

Notes.

Table 8: Alternative scaling for Investment.

	Investment (USD per lagged Assets) Top Winsorised at 5%			Investment (USD per lagged Revenue) Top Winsorised at 5%		
	Unbalanced		Balanced	Unbalanced		Balanced
Panel A	(1)	(2)	(3)	(4)	(5)	(6)
US*Post-2003	-.00208	-.00233	-.00153	.00487	.00586	.00962
	(.00523)	(.00534)	(.00482)	(.0128)	(.0133)	(.0123)
Matching	Y	Y	Y	Y	Y	Y
Controls	N	Y	Y	N	Y	Y
Observations (firm-years)	61079	59769	48864	60945	59678	48798
Clusters (firms)	7059	6899	5322	7055	6900	5323
R^2	0.0424	0.0468	0.0472	0.0282	0.0661	0.0765
US firms mean	0.0659	0.0660	0.0666	0.0973	0.0972	0.0968
US firms SD	0.0650	0.0650	0.0647	0.159	0.158	0.156
ϵ wrt $(1-\tau_d)$	-0.0729	-0.0818	-0.0531	0.116	0.139	0.230
ϵ SD	0.184	0.187	0.168	0.304	0.316	0.295
p-val. pretrends test	0.935	0.771	0.687	0.0517	0.0904	0.0530

	Investment (USD per lagged Common Equity) Top Winsorised at 5%			Investment (USD per lagged Employees) Top Winsorised at 5%		
	Unbalanced		Balanced	Unbalanced		Balanced
Panel B	(7)	(8)	(9)	(10)	(11)	(12)
US*Post-2003	-.0167	-.0105	.00039	-3.31	-3.12	-2.69
	(.0301)	(.0298)	(.0318)	(2.31)	(2.37)	(2.06)
Matching	Y	Y	Y	Y	Y	Y
Controls	N	Y	Y	N	Y	Y
Observations (firm-years)	61048	59743	48847	53091	52139	42293
Clusters (firms)	7059	6899	5322	6645	6507	5004
R^2	0.000798	0.00104	0.00159	0.0100	0.0213	0.0247
US firms mean	0.102	0.101	0.108	15.16	15.13	15.19
US firms SD	0.967	0.974	0.934	23.05	22.99	22.50
ϵ wrt $(1-\tau_d)$	-0.379	-0.241	0.00839	-0.504	-0.478	-0.409
ϵ SD	0.677	0.677	0.684	0.352	0.362	0.313
p-val. pretrends test	0.0857	0.0681	0.255	0.870	0.916	0.712

Notes.

Table 9: Alternative scaling for Cash Dividends.

	Dividends (USD per lagged Assets) Top Winsorised at 5%			Dividends (USD per lagged Revenue) Top Winsorised at 5%		
	Unbalanced		Balanced	Unbalanced		Balanced
Panel A	(1)	(2)	(3)	(4)	(5)	(6)
US*Post-2003	-.00286***	-.00289***	-.00273***	-.00307**	-.00318**	-.00252
	(.000856)	(.000864)	(.000833)	(.0014)	(.00143)	(.00167)
Matching	Y	Y	Y	Y	Y	Y
Controls	N	Y	Y	N	Y	Y
Observations (firm-years)	53553	52348	43170	53450	52277	43120
Clusters (firms)	6697	6544	5165	6691	6544	5165
R^2	0.0582	0.0870	0.0900	0.0547	0.0748	0.0769
US firms mean	0.00506	0.00505	0.00561	0.00520	0.00515	0.00570
US firms SD	0.00946	0.00946	0.00980	0.0101	0.0101	0.0104
ϵ wrt $(1-\tau_d)$	-1.305	-1.324	-1.126	-1.365	-1.426	-1.022
ϵ SD	0.393	0.398	0.345	0.625	0.646	0.680
p-val. pretrends test	0.262	0.354	0.750	0.692	0.662	0.600

	Dividends (USD per lagged Common Equity) Top Winsorised at 5%			Dividends (USD per lagged Employees) Top Winsorised at 5%		
	Unbalanced		Balanced	Unbalanced		Balanced
Panel B	(7)	(8)	(9)	(10)	(11)	(12)
US*Post-2003	-.0135	-.0138	-.0167	-.936***	-.917**	-.94**
	(.0111)	(.0114)	(.0144)	(.36)	(.359)	(.394)
Matching	Y	Y	Y	Y	Y	Y
Controls	N	Y	Y	N	Y	Y
Observations (firm-years)	53526	52324	43152	47271	46355	38036
Clusters (firms)	6697	6544	5165	6262	6130	4809
R^2	0.000721	0.00303	0.00501	0.0566	0.0771	0.0792
US firms mean	0.00770	0.00763	0.00972	1.051	1.040	1.163
US firms SD	0.145	0.146	0.134	2.169	2.161	2.248
ϵ wrt $(1-\tau_d)$	-4.058	-4.175	-3.964	-2.062	-2.042	-1.870
ϵ SD	3.339	3.469	3.435	0.800	0.805	0.791
p-val. pretrends test	0.334	0.347	0.319	0.561	0.388	0.187

Notes.

Appendix E: Robustness to Matching Method

Table 10: Matching based on the year 2002.

	Total Payout (USD per lagged Assets) Top Winsorised at 5%			Investment (USD per lagged Assets) Top Winsorised at 5%		
	Unbalanced		Balanced	Unbalanced		Balanced
Panel A	(1)	(2)	(3)	(4)	(5)	(6)
US*Post-2003	-.000993	-.0022*	-.00375***	-.00231	-.00245*	-.00283*
	(.00101)	(.00121)	(.00134)	(.00147)	(.00146)	(.00154)
Matching	Y	Y	Y	Y	Y	Y
Controls	N	Y	Y	N	Y	Y
Observations (firm-years)	50463	40540	32205	76961	61670	50774
Clusters (firms)	6898	5187	3867	9299	7081	5520
R^2	0.0244	0.0292	0.0258	0.0482	0.0563	0.0540
US firms mean	0.0193	0.0208	0.0227	0.0667	0.0670	0.0677
US firms SD	0.0316	0.0325	0.0333	0.0662	0.0655	0.0652
ϵ wrt $(1-\tau_d)$	-0.119	-0.245	-0.381	-0.0800	-0.0847	-0.0966
ϵ SD	0.121	0.134	0.136	0.0509	0.0501	0.0524
p-val. pretrends test	1.93e-09	6.07e-09	1.30e-09	0.148	0.592	0.610

	Any Dividend Payout Not Winsorised			Dividends (USD per lagged Assets) Top Winsorised at 5%		
	Unbalanced		Balanced	Unbalanced		Balanced
Panel B	(7)	(8)	(9)	(10)	(11)	(12)
US*Post-2003	-.000631	.0365***	.0206**	-.00234***	-.00276***	-.00351***
	(.0108)	(.0105)	(.0096)	(.000432)	(.000479)	(.000497)
Matching	Y	Y	Y	Y	Y	Y
Controls	N	Y	Y	N	Y	Y
Observations (firm-years)	91981	73332	60179	67105	53926	44764
Clusters (firms)	9329	7099	5527	8804	6717	5354
R^2	0.197	0.245	0.253	0.0824	0.0979	0.111
US firms mean	0.325	0.343	0.373	0.00458	0.00512	0.00570
US firms SD	0.468	0.475	0.484	0.00910	0.00951	0.00985
ϵ wrt $(1-\tau_d)$	-0.00449	0.246	0.128	-1.184	-1.250	-1.425
ϵ SD	0.0766	0.0721	0.0603	0.221	0.220	0.206
p-val. pretrends test	0.388	0.539	0.750	0.0113	0.0220	0.0835

Notes.

Table 11: Kernel Matching.

	Total Payout (USD per lagged Assets) Top Winsorised at 5%			Investment (USD per lagged Assets) Top Winsorised at 5%		
	Unbalanced		Balanced	Unbalanced		Balanced
Panel A	(1)	(2)	(3)	(4)	(5)	(6)
US*Post-2003	-.00217	-.00241	-.00301	-.00208	-.00233	-.00153
	(.00202)	(.0021)	(.00232)	(.00523)	(.00534)	(.00482)
Matching	Y	Y	Y	Y	Y	Y
Controls	N	Y	Y	N	Y	Y
Observations (firm-years)	41929	40809	32433	61079	59769	48864
Clusters (firms)	5421	5272	3938	7059	6899	5322
R^2	0.0294	0.0388	0.0367	0.0424	0.0468	0.0472
US firms mean	0.0206	0.0206	0.0225	0.0659	0.0660	0.0666
US firms SD	0.0324	0.0324	0.0332	0.0650	0.0650	0.0647
ϵ wrt $(1-\tau_d)$	-0.244	-0.272	-0.310	-0.0729	-0.0818	-0.0531
ϵ SD	0.226	0.236	0.239	0.184	0.187	0.168
p-val. pretrends test	0.000784	0.000115	6.35e-05	0.935	0.771	0.687

	Any Dividend Payout Not Winsorised			Dividends (USD per lagged Assets) Top Winsorised at 5%		
	Unbalanced		Balanced	Unbalanced		Balanced
Panel B	(7)	(8)	(9)	(10)	(11)	(12)
US*Post-2003	.016	.0169	.0193	-.00286***	-.00289***	-.00273***
	(.0206)	(.0203)	(.0166)	(.000856)	(.000864)	(.000833)
Matching	Y	Y	Y	Y	Y	Y
Controls	N	Y	Y	N	Y	Y
Observations (firm-years)	72250	70715	57567	53553	52348	43170
Clusters (firms)	7085	6918	5330	6697	6544	5165
R^2	0.0760	0.148	0.155	0.0582	0.0870	0.0900
US firms mean	0.343	0.340	0.368	0.00506	0.00505	0.00561
US firms SD	0.475	0.474	0.482	0.00946	0.00946	0.00980
ϵ wrt $(1-\tau_d)$	0.108	0.115	0.122	-1.305	-1.324	-1.126
ϵ SD	0.139	0.138	0.104	0.393	0.398	0.345
p-val. pretrends test	0.112	0.260	0.994	0.262	0.354	0.750

Notes.

Table 12: Removing deciles of firm age.

	Total Payout (USD per lagged Assets) Top Winsorised at 5%			Investment (USD per lagged Assets) Top Winsorised at 5%		
	Unbalanced		Balanced	Unbalanced		Balanced
Panel A	(1)	(2)	(3)	(4)	(5)	(6)
US*Post-2003	-.000662	-.00201	-.00335*	-.00424***	-.00481*	-.0055*
	(.000846)	(.00163)	(.00171)	(.00126)	(.00271)	(.00287)
Matching	Y	Y	Y	Y	Y	Y
Controls	N	Y	Y	N	Y	Y
Observations (firm-years)	42934	18843	15286	67414	21160	17208
Clusters (firms)	5594	2330	1786	7882	2358	1804
R^2	0.0225	0.0305	0.0297	0.0376	0.0539	0.0559
US firms mean	0.0205	0.0203	0.0221	0.0657	0.0593	0.0605
US firms SD	0.0323	0.0328	0.0337	0.0650	0.0576	0.0580
ϵ wrt $(1-\tau_d)$	-0.0748	-0.230	-0.351	-0.149	-0.188	-0.211
ϵ SD	0.0956	0.186	0.179	0.0438	0.105	0.109
p-val. pretrends test	2.43e-06	0.000785	0.0114	0.0490	0.0579	0.0875

	Any Dividend Payout Not Winsorised			Dividends (USD per lagged Assets) Top Winsorised at 5%		
	Unbalanced		Balanced	Unbalanced		Balanced
Panel B	(7)	(8)	(9)	(10)	(11)	(12)
US*Post-2003	.0157**	-.0457*	-.0465*	-.00282***	-.00366***	-.00409***
	(.00784)	(.0248)	(.0254)	(.000389)	(.00116)	(.00122)
Matching	Y	Y	Y	Y	Y	Y
Controls	N	Y	Y	N	Y	Y
Observations (firm-years)	81017	23825	19264	59120	20963	17061
Clusters (firms)	7908	2358	1804	7493	2356	1802
R^2	0.247	0.113	0.119	0.0787	0.0985	0.0968
US firms mean	0.340	0.279	0.306	0.00500	0.00429	0.00488
US firms SD	0.474	0.448	0.461	0.00941	0.00893	0.00938
ϵ wrt $(1-\tau_d)$	0.107	-0.379	-0.351	-1.307	-1.973	-1.940
ϵ SD	0.0538	0.205	0.191	0.185	0.632	0.585
p-val. pretrends test	0.709	0.890	0.460	8.98e-06	0.976	0.439

Notes.

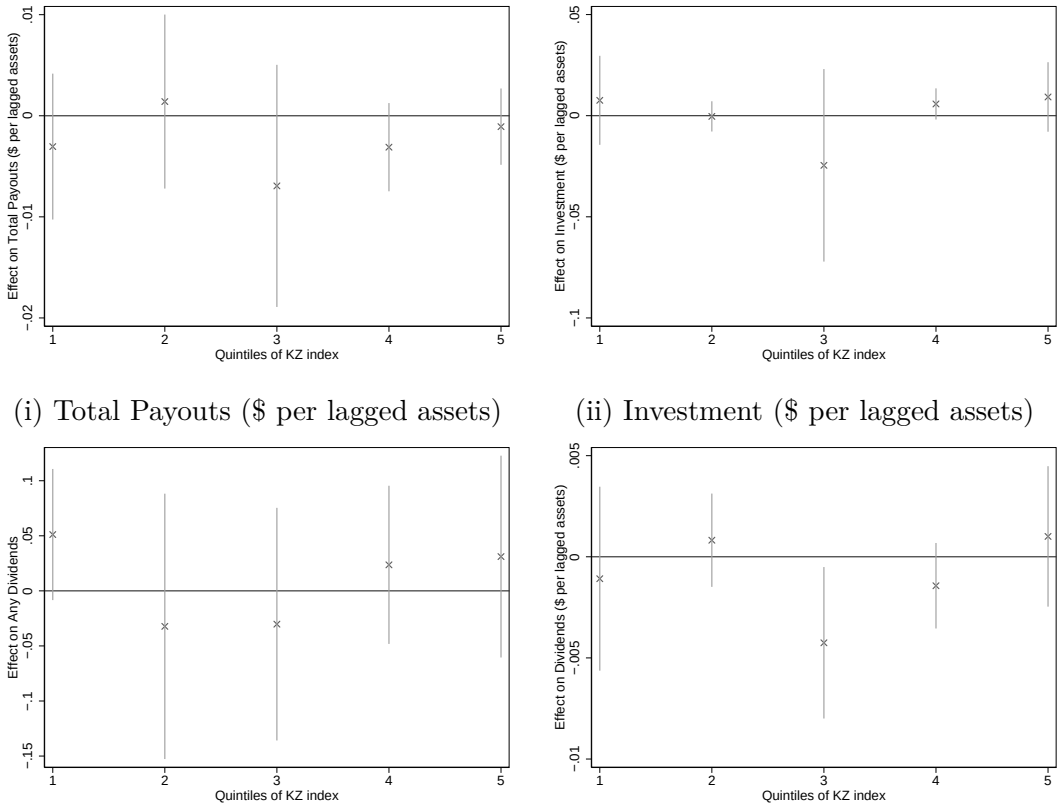
Table 13: Matching based on assets, sales, profitability (USD per lagged assets) in 1997.

	Total Payout (USD per lagged Assets) Top Winsorised at 5%			Investment (USD per lagged Assets) Top Winsorised at 5%		
	Unbalanced		Balanced	Unbalanced		Balanced
Panel A	(1)	(2)	(3)	(4)	(5)	(6)
US*Post-2003	-.0002	-.00226	-.00396***	-.00447***	-.00448*	-.00487**
	(.000708)	(.00147)	(.00153)	(.00102)	(.00232)	(.00244)
Matching	Y	Y	Y	Y	Y	Y
Controls	N	Y	Y	N	Y	Y
Observations (firm-years)	52524	19070	15464	82672	21440	17424
Clusters (firms)	7377	2384	1824	10241	2415	1845
R^2	0.0218	0.0285	0.0270	0.0398	0.0544	0.0576
US firms mean	0.0188	0.0202	0.0220	0.0657	0.0595	0.0605
US firms SD	0.0314	0.0328	0.0337	0.0662	0.0582	0.0584
ϵ wrt $(1-\tau_d)$	-0.0246	-0.260	-0.417	-0.157	-0.174	-0.186
ϵ SD	0.0870	0.169	0.161	0.0355	0.0898	0.0927
p-val. pretrends test	5.00e-11	0.000465	0.00679	0.0266	0.0270	0.0277

	Any Dividend Payout Not Winsorised			Dividends (USD per lagged Assets) Top Winsorised at 5%		
	Unbalanced		Balanced	Unbalanced		Balanced
Panel B	(7)	(8)	(9)	(10)	(11)	(12)
US*Post-2003	-.00888	-.031	-.0329	-.00314***	-.00345***	-.00399***
	(.00689)	(.0215)	(.0215)	(.000343)	(.00102)	(.00102)
Matching	Y	Y	Y	Y	Y	Y
Controls	N	Y	Y	N	Y	Y
Observations (firm-years)	99727	24147	19515	71972	21227	17264
Clusters (firms)	10286	2415	1845	9660	2411	1841
R^2	0.254	0.125	0.134	0.0907	0.109	0.109
US firms mean	0.319	0.277	0.304	0.00443	0.00425	0.00484
US firms SD	0.466	0.448	0.460	0.00899	0.00890	0.00935
ϵ wrt $(1-\tau_d)$	-0.0644	-0.259	-0.250	-1.642	-1.878	-1.907
ϵ SD	0.0497	0.179	0.163	0.186	0.563	0.496
p-val. pretrends test	0.00373	0.840	0.445	8.19e-06	0.952	0.567

Notes.

Appendix F: New vs Old View of the firm



(i) Total Payouts ($ per lagged assets)

(ii) Investment ($ per lagged assets)

(iii) Effect on any dividend payout.

(iv) Dividends ($ per lagged assets)

Figure 17: Heterogeneity of the effect by quintiles of the KZ-index of equity dependence.

Notes. Firms are divided into quintiles of KZ index (as defined by Baker et al. (2003)). The coefficient reported is of the outcome regression effect of the tax cuts for US firms in the specific quintile of KZ index with controls in an unbalanced sample of OECD firms.

I construct the KZ index of equity dependence based on Baker et al. (2003) as follows, $KZ_i = -1.002 cashflow_{it}/assets_{it-1} - 39.368 dividends_{it}/at_{it-1} - 1.315 cash_{it}/assets_{it-1} + 3.139 leverage_{it}$, with each of these independent variables winsorised at the 1% level. I then divide US firms into quintiles of the KZ index of equity dependence and compare them to non-US firms that fall in the same quintile as the US-defined quintile. I also show the association between this measure of equity dependence and my measure of share repurchases. As expected, additional share repurchases is associated with less equity dependence. This results from this exercise are reported in the following graph.

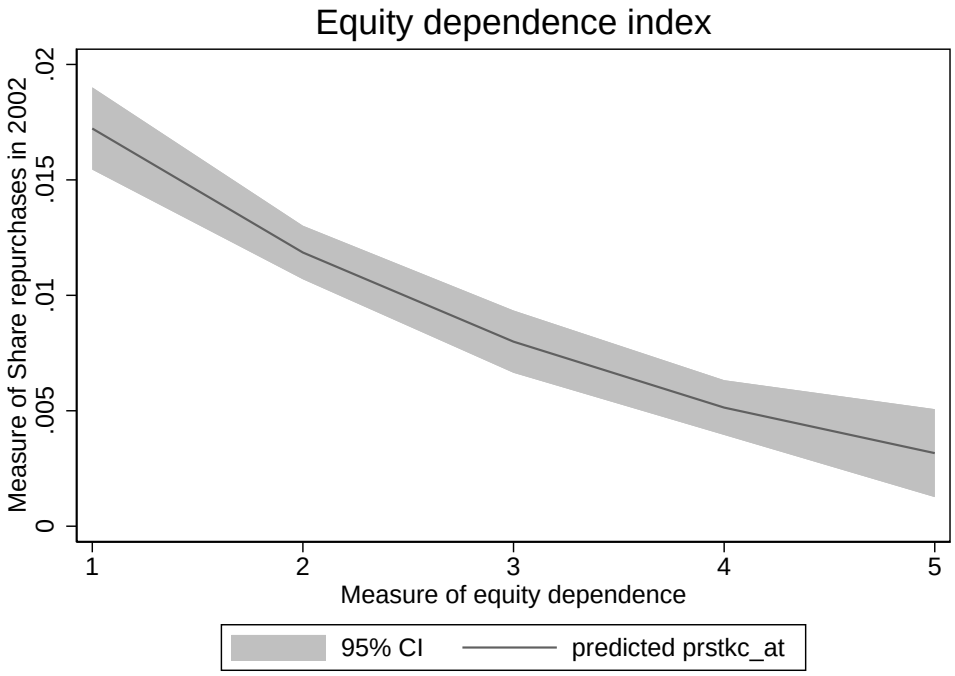
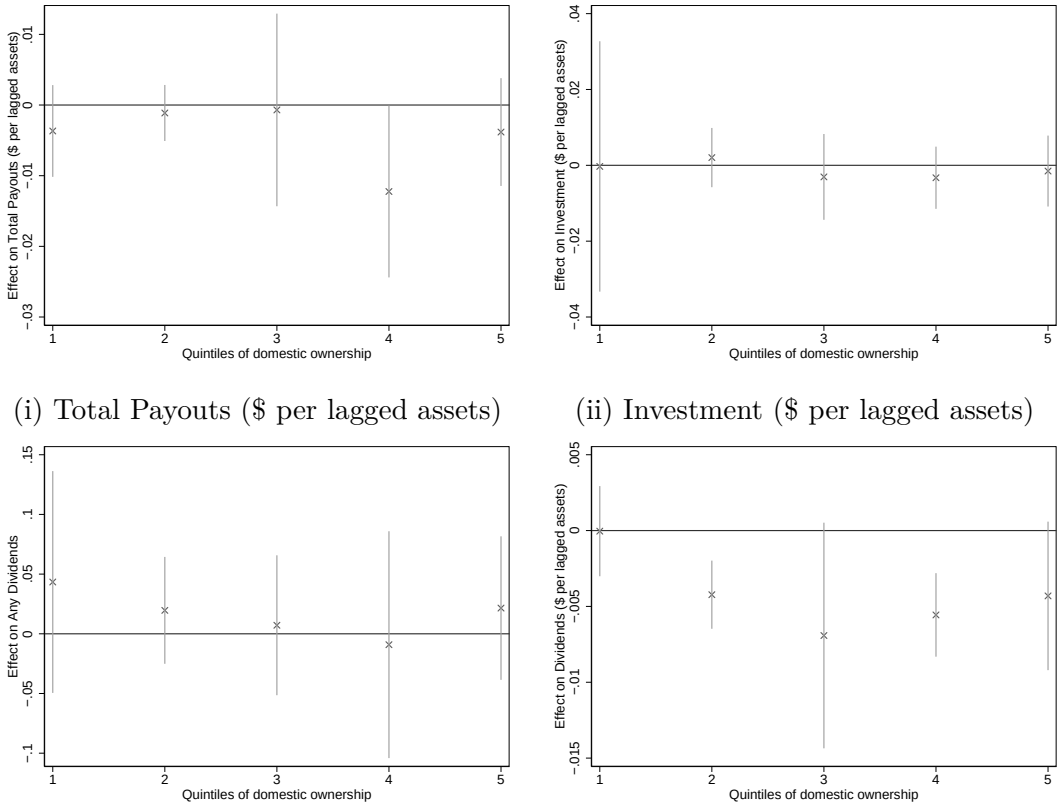


Figure 18: KZ index quintiles association with share-repurchases is negative. This indicates that at least in my sample, the KZ index does measure cash-constrainedness and equity dependence. This is because we shouldn't expect cash constrained firms to buyback shares at a time when they need cash to finance investments.

Figure 17 confirms that there is no heterogeneity in the average treatment effect along this measure of equity dependence. As such, even the most equity dependent firms are not changing their payout and investment policy.

Appendix G: Ownership Argument

Heterogeneity of Difference-in-Difference by quintiles of fraction of reported domestic ownership relative to total reported outstanding shares.



(i) Total Payouts ($ per lagged assets)

(ii) Investment ($ per lagged assets)

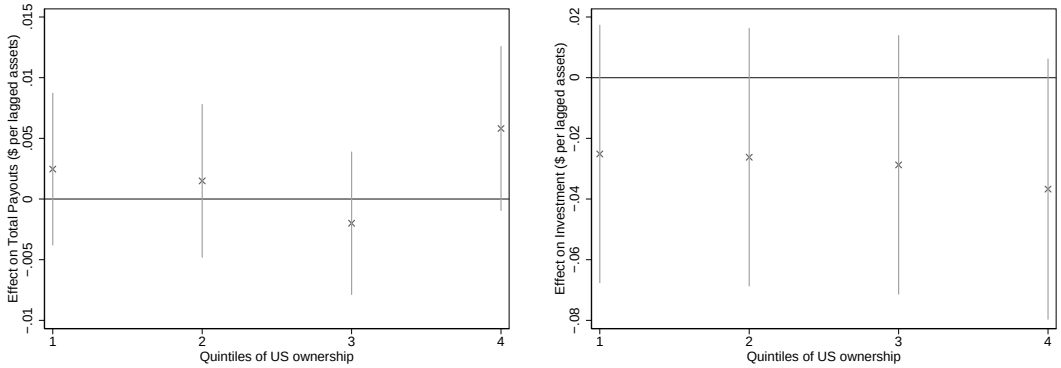
(iii) Effect on any dividend payout.

(iv) Dividends ($ per lagged assets)

Figure 19: Heterogeneity of the effect based on the distribution of domestic firm ownership (relative to percentage of reported outstanding shares) in March 2003.

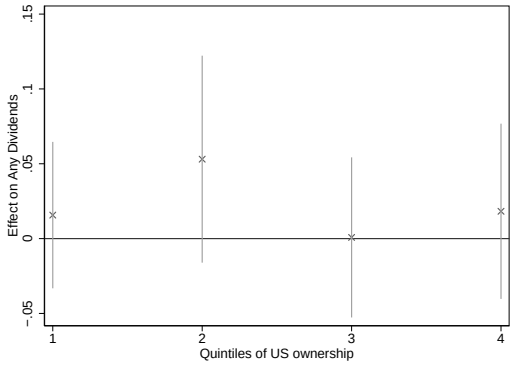
Notes. Firms are divided into quintiles of domestic firm ownership. US domiciled firms that are held mostly by domestic owners (quintile 5) should be more likely to respond relative to domestically owned firms in the OECD.

Difference-in-Difference effect based on reported fraction of US ownership regardless of firm domicile.

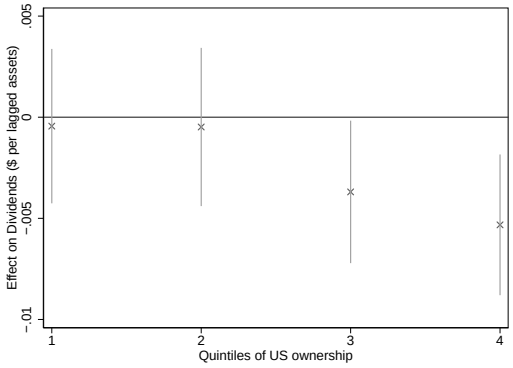


(i) Total Payouts ($ per lagged assets)

(ii) Investment ($ per lagged assets)



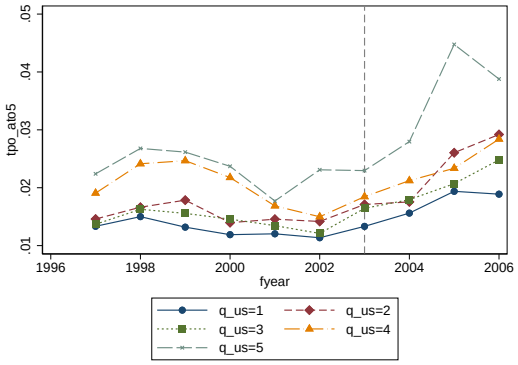
(iii) Effect on any dividend payout.



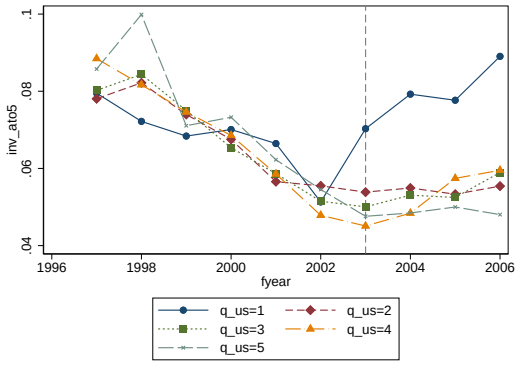
(iv) Dividends ($ per lagged assets)

Figure 20: Difference-in-Difference using quintiles of US Firm Ownership as the treatment variable.

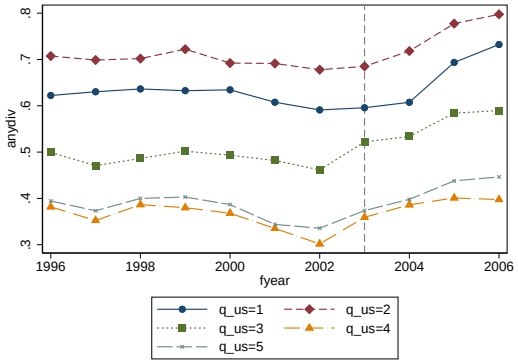
Notes. Firms are divided into quintiles of reported US ownership of firms (regardless of firm domicile). The coefficient reported is the change in the outcome variable after the reform relative to before for each quintile of ownership.



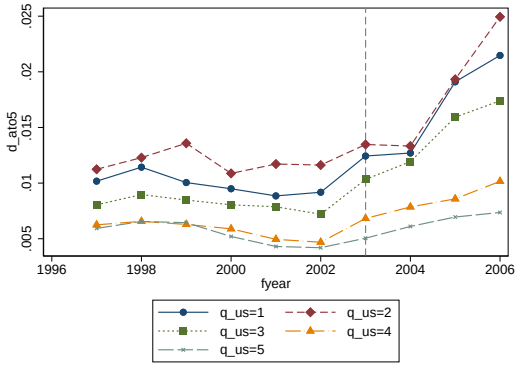
(i) Total Payouts ($ per lagged assets)



(ii) Investment ($ per lagged assets)



(iii) Effect on any dividend payout.

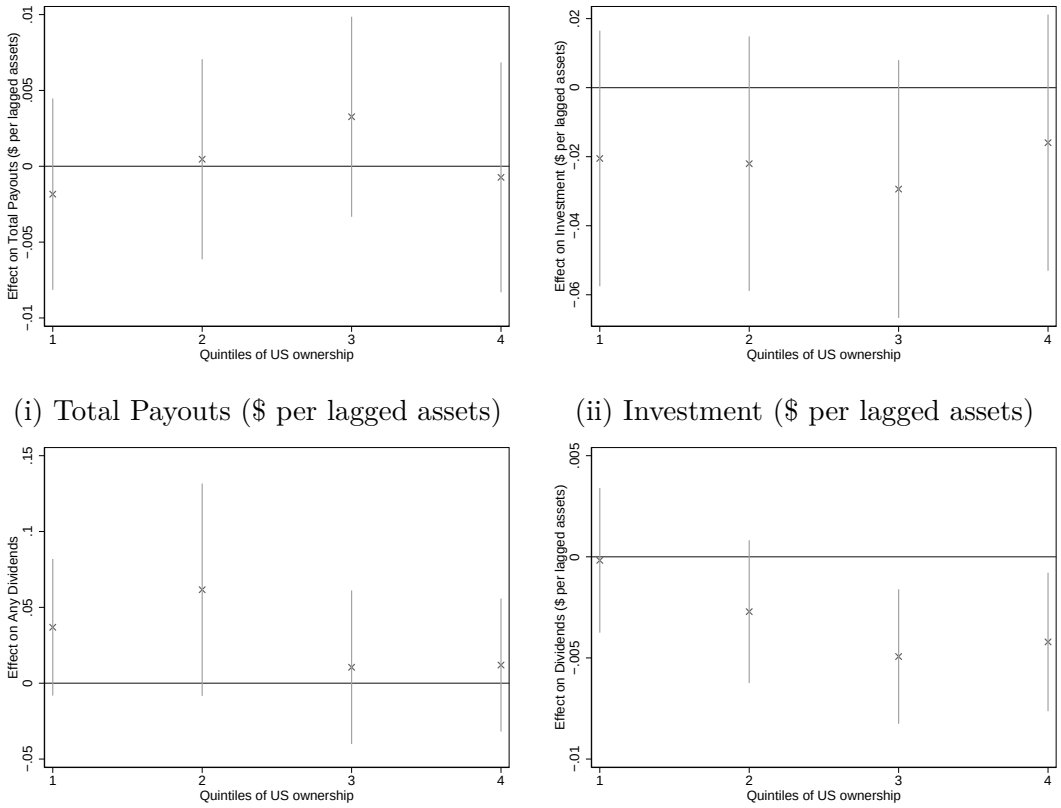


(iv) Dividends ($ per lagged assets)

Figure 21: Scatterplot over time by quintiles of US Firm Ownership as the treatment variable.

Notes. Firms are divided into quintiles of reported US ownership of firms (regardless of firm domicile). The coefficient reported is the change in the outcome variable after the reform relative to before for each quintile of ownership.

Difference-in-Difference effect based on reported fraction of US ownership (relative to total reported outstanding shares) regardless of firm domicile.



(i) Total Payouts ($ per lagged assets)

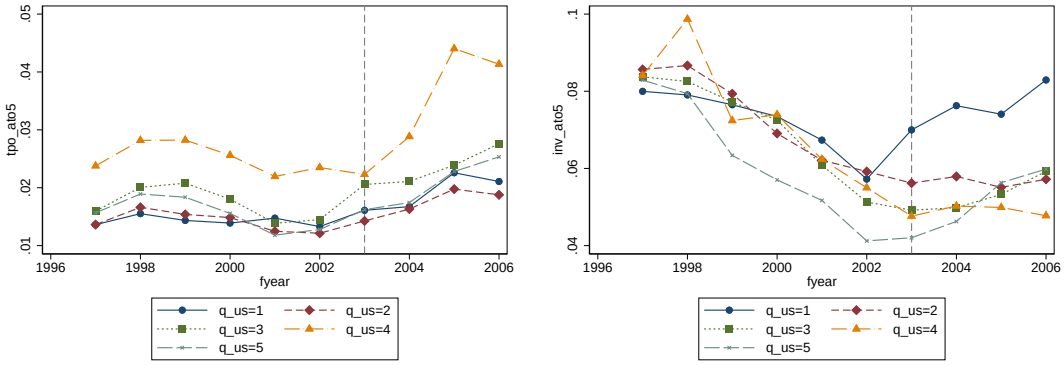
(ii) Investment ($ per lagged assets)

(iii) Effect on any dividend payout.

(iv) Dividends ($ per lagged assets)

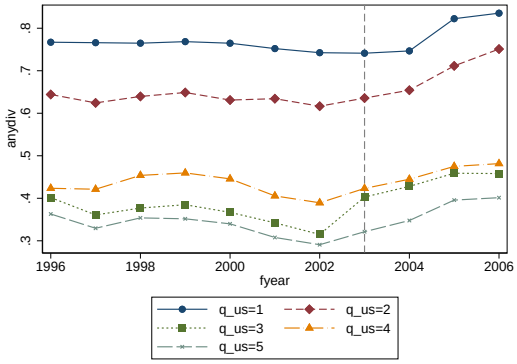
Figure 22: Difference using quintiles of US Firm Ownership as the treatment variable.

Notes. Firms are divided into quintiles of reported US ownership of firms as a fraction of reported outstanding shares (regardless of firm domicile). The coefficient plotted is the difference-in-difference effect relative to the base quintile with least US ownership (relative to reported outstanding shares). Firms predominantly owned by US owners do not respond to the reform when compared to a control group of non-US owned firms.

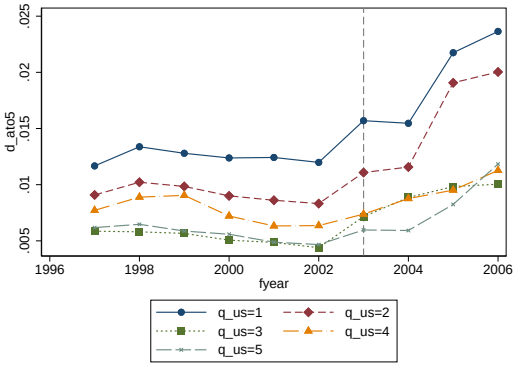


(i) Total Payouts ($ per lagged assets)

(ii) Investment ($ per lagged assets)



(iii) Effect on any dividend payout.



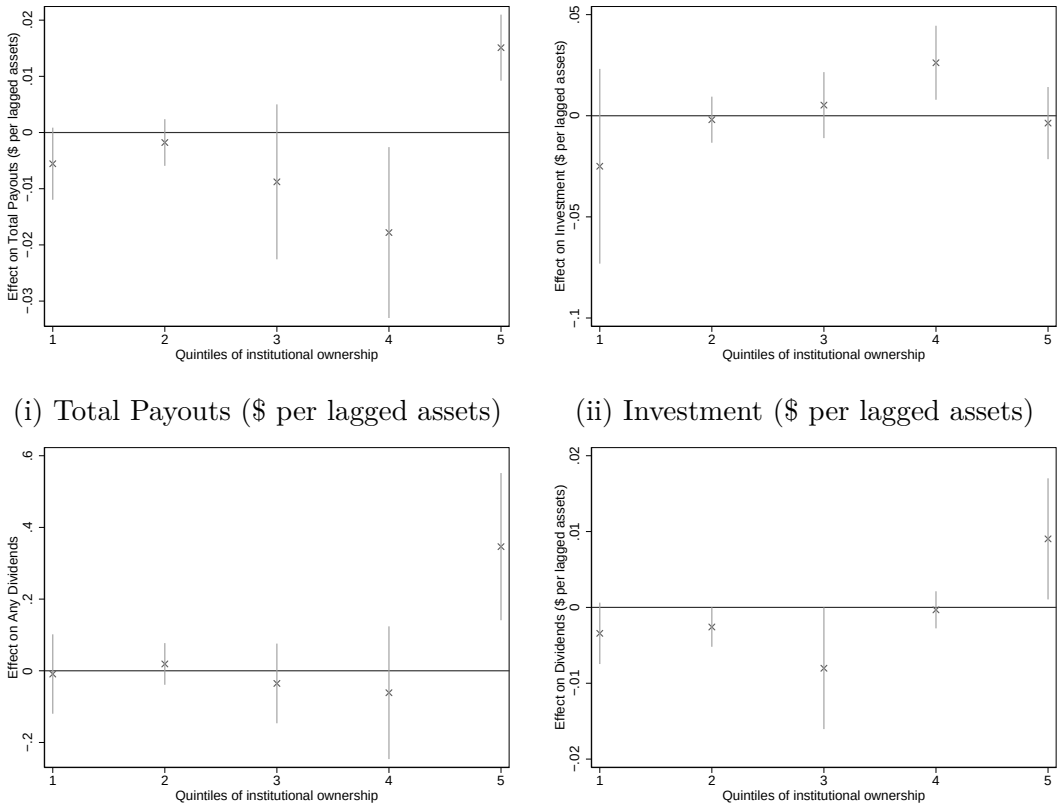
(iv) Dividends ($ per lagged assets)

Figure 23: Scatterplot over time by quintiles of US Firm Ownership as the treatment variable.

Notes. Firms are divided into quintiles of reported US ownership of firms (regardless of firm domicile). The coefficient reported is the change in the outcome variable after the reform relative to before for each quintile of ownership.

Appendix H: Other Heterogeneity

Institutional Ownership



(i) Total Payouts ($ per lagged assets)

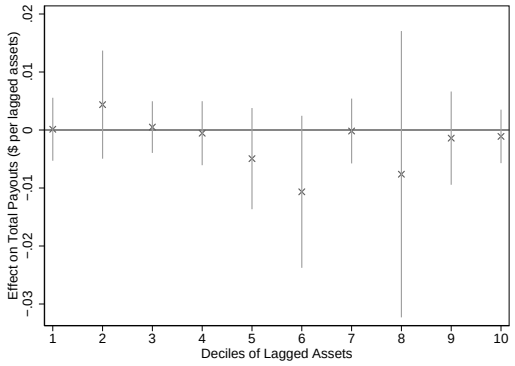
(ii) Investment ($ per lagged assets)

(iii) Effect on any dividend payout.

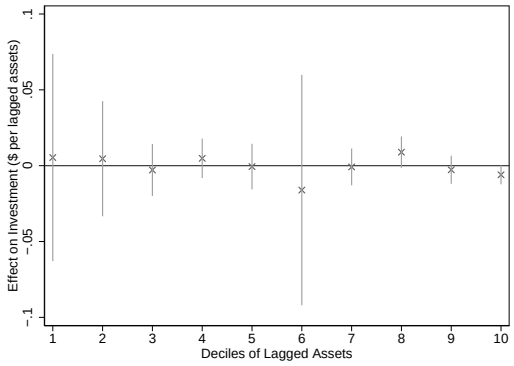
(iv) Dividends ($ per lagged assets)

Figure 24: Average Treatment Effect Heterogeneity along quintiles of US Firm Institutional Ownership.
Notes. Firms are divided into quintiles of US Firm Institutional Ownership. The coefficient reported is the change in the outcome variable after the reform relative to before for each quintile of ownership.

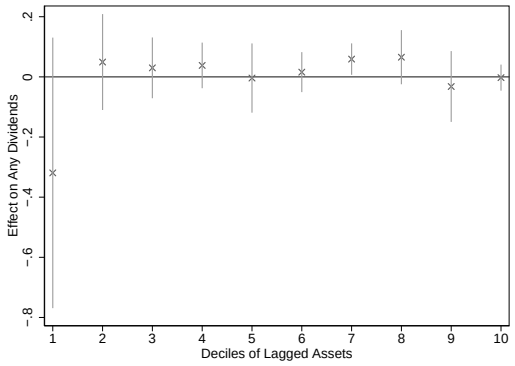
Asset Deciles



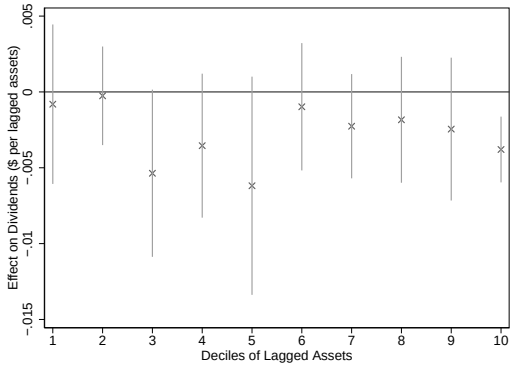
(i) Total payouts ($ per lagged assets)



(ii) Investment ($ per lagged assets)



(iii) Effect on any dividend payout.



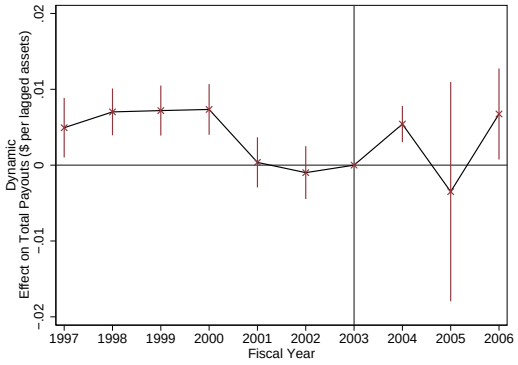
(iv) Dividends ($ per lagged assets)

Figure 25: Heterogeneity of the effect by deciles of total lagged asset holdings of the US firms' distribution.

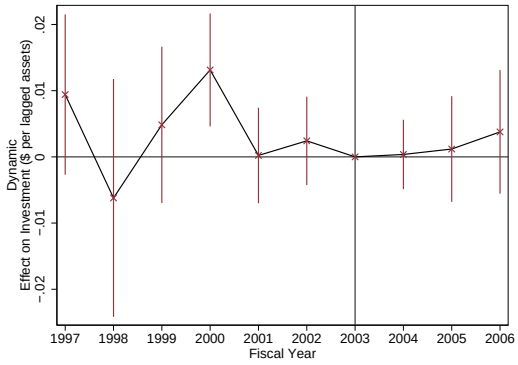
Notes. Firms are divided into deciles of US firm total lagged assets. The coefficient reported is of the outcome regression effect of the tax cuts for US firms in the specific decile of cash holdings with controls in an unbalanced sample of OECD firms.

Appendix J: Other Robustness Checks

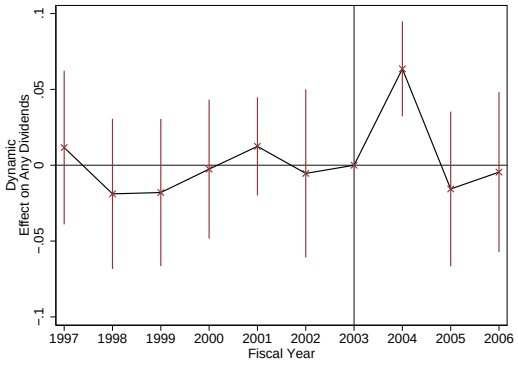
Announcement Robustness



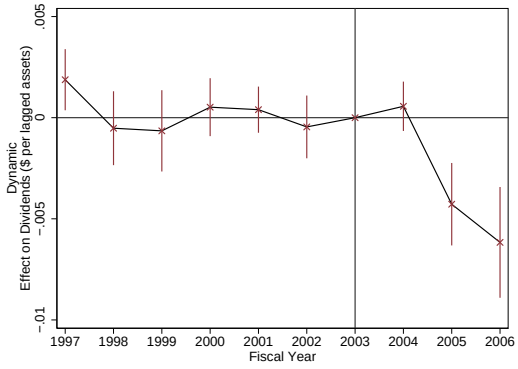
(i) Total payouts ($ per lagged assets)



(ii) Investment ($ per lagged assets)



(iii) Effect on any dividend payout.



(iv) (Dividends $ per lagged assets)

Figure 26: Dynamic effect of the 2003 JGTRRA on US firms' financial and investment behaviour. This figure treats all post-January 2003 firm accounts as part of the treatment group.

Country Robustness

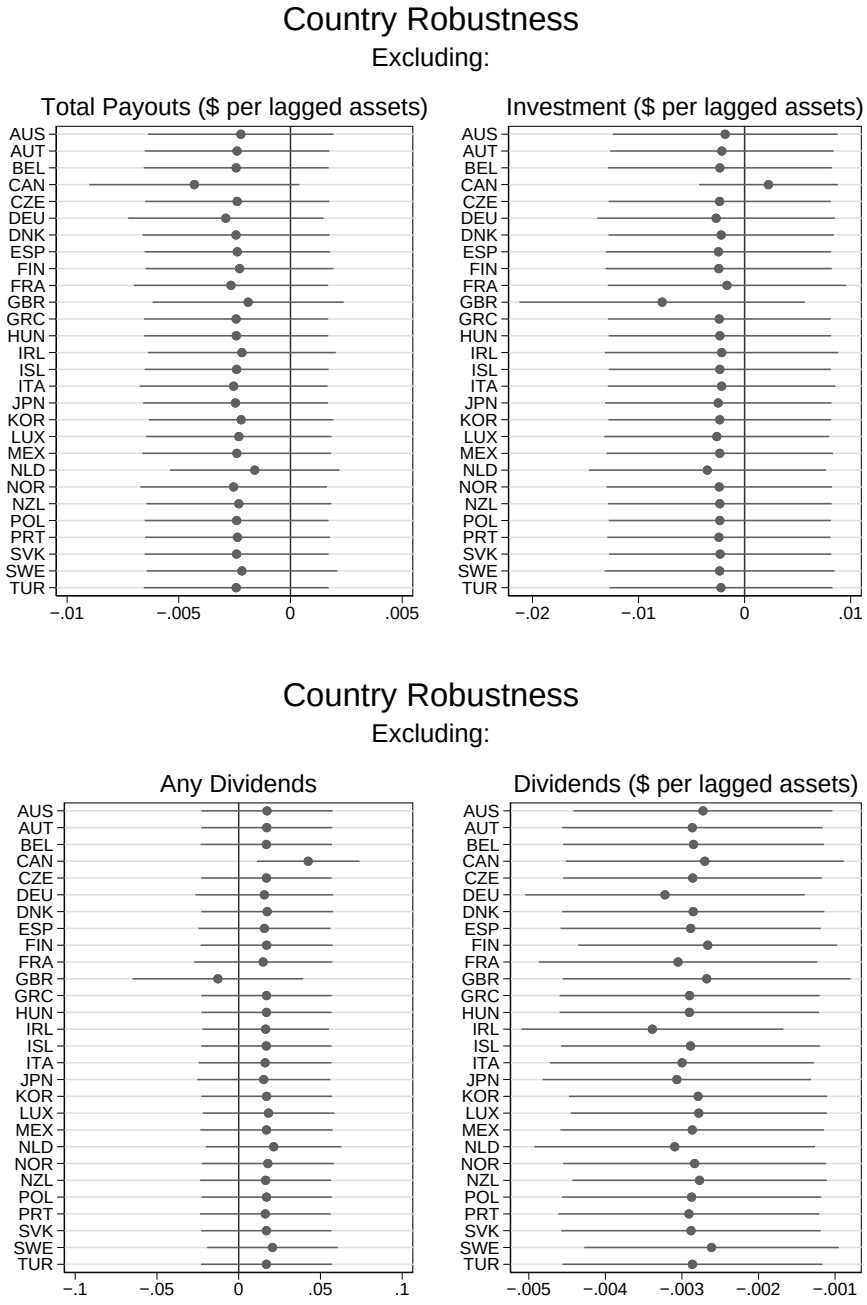
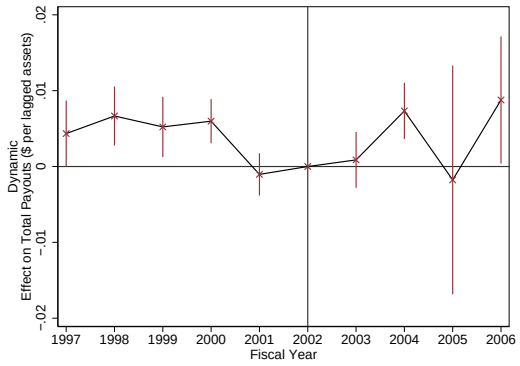
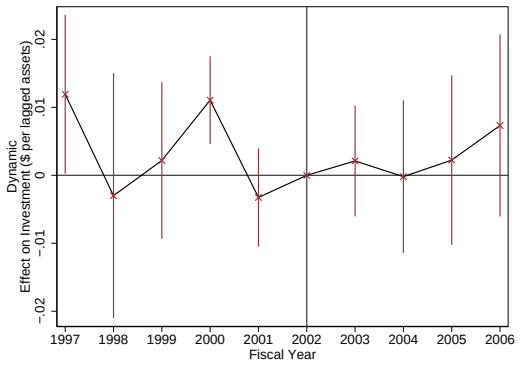


Figure 27: Sequentially removing control countries.

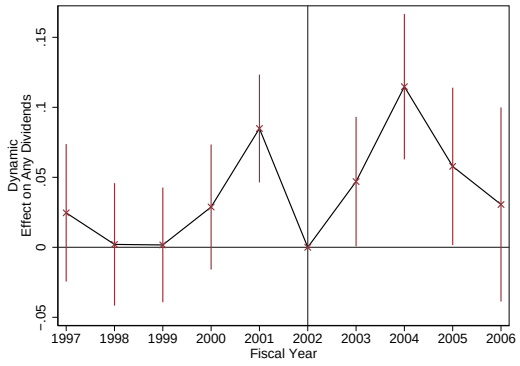
Smaller Sample of Countries



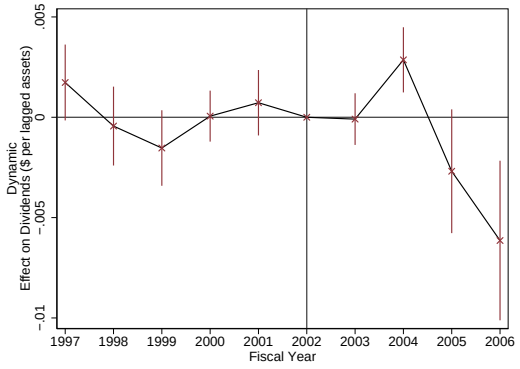
(i) Total payouts ($ per lagged assets)



(ii) Investment ($ per lagged assets)



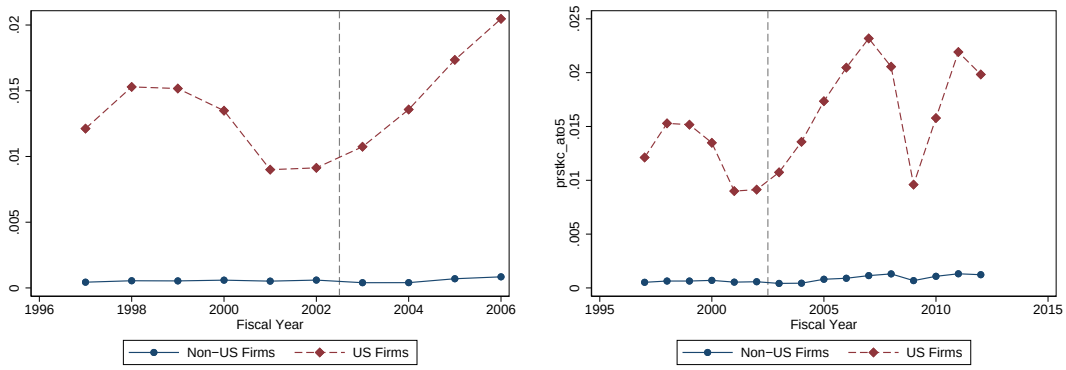
(iii) Effect on any dividend payout.



(iv) (Dividends $ per lagged assets)

Figure 28: Dynamic effect of the 2003 JGTRRA on US firms' financial and investment behaviour in the subsample of countries.

Share Repurchases Outside the EU



(i) Total payouts ($ per lagged assets)

(ii) Investment ($ per lagged assets)

Figure 29: Percentiles of the raw measure of share repurchases.