

Does Share Repurchase Legalization Really Harm Corporate Investments?

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September 2025

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Abstract

We examine the investment effects of stock-repurchase legalization on public firms, using staggered adoptions in 17 countries (1985-2010). Legalization raises investment by 8-10% of the sample mean. Our findings are consistent with the hypothesis that buyback legalization improves equity-capital access across public markets. The positive investment effects are driven entirely by younger, high-growth, cash-constrained non-repurchasers; capital structures shift from debt toward equity; profitability and valuation improve; and effects are strongest where capital-access frictions are more severe and where post-legalization buyback volumes were highest. Taken together, the results imply that blanket restrictions on buybacks could inadvertently impede capital-market functioning.

Keywords: Stock buyback, Share repurchases, Investments, Financial Regulation

JEL Classifications: G18, G34, G38, G41, L51, M14, M52

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1 Introduction

The allocation of corporate resources between investments and shareholder payouts is a fundamental issue in corporate finance. How managers deploy cash flows has profound implications for firm value, economic growth, and societal welfare. On one hand, reinvesting cash flows into productive assets can drive innovation, create jobs, and generate long-term value. On the other hand, returning excess cash to shareholders through dividends or share repurchases can mitigate agency problems, provide shareholder discipline, and allow capital to flow to its highest-value uses in the economy. Understanding this tradeoff is crucial for evaluating corporate governance mechanisms, managerial incentives, and financial regulations.

Share repurchases have emerged as a particularly important and controversial form of shareholder payout. Unlike dividends, which tend to be sticky and signal long-term commitment, repurchases offer managers greater flexibility in timing and magnitude ([Bonaimé, Hankins, and Harford, 2014](#)). This flexibility can be valuable for managing temporary cash flow shocks or signaling undervaluation ([Jagannathan, Stephens, and Weisbach, 2000](#); [Gruillon and Michaely, 2004](#)). However, substantial concerns remain about the potential misuse of repurchases. Critics argue that the growing prevalence of repurchases may incentivize managers to sacrifice long-term investments in favor of short-term stock price appreciation ([Almeida, Fos, and Kronlund, 2016](#); [Lazonick, 2014](#)).

As the volume of stock buybacks grew over the last decade, reaching a record \$1.2 trillion in 2022, the debate about stock buybacks spilled over to the public arena. Asset managers ([Fink, 2015](#)), leading corporate lawyers ([Lipton, 2015](#)), and senior politicians ([Biden, 2016](#)) have raised concerns about the extent to which repurchases deprive firms of the capital needed for long-term investment, innovation, and employee compensation.

These concerns have spurred U.S. legislative proposals from both sides of the aisle aimed at limiting the practice of open market repurchases. In 2018, Senator Tammy Baldwin (D-WI) introduced the *Reward Work Act* which proposed banning open market repurchases outright.¹ Senator Marco Rubio (R-FL) called for eliminating stock buybacks’ tax advantage over dividends (Rubio, 2019). These regulatory efforts culminated in the passage of the Inflation Reduction Act in 2022, which included a 1% excise tax on repurchases (“the Biden excise tax”). In 2023, Senator Sherrod Brown’s (D-OH) *Stock Buyback Accountability Act* proposed increasing the excise tax to 4%, while the *Reward Work Act* was reintroduced by Representatives Jesús García (IL-04), Ro Khanna (CA-17), and Val Hoyle (OR-04) to ban open market repurchases.² Interest in regulating stock buybacks has also gained prominence in other major economies, such as the United Kingdom (Department for Business, 2017; PWC, 2019), the European Union (Ernst & Young, 2020; Roe, Spamann, Fried, and Wang, 2021; Fried and Wang, 2021), and Japan (Nonomiya and Reidy, 2021).

Central to these policy debates is the fear that repurchases divert resources from productive investments, potentially harming long-term economic growth and innovation. This contentious issue has important implications for corporate governance, managerial incentives, and financial regulation. However, there is limited and mixed academic evidence to inform policymakers about the impact of repurchases on corporate investments (see Section 2). In a recent review of the literature on share repurchases, Bonaimé and Kahle (2024) emphasize that many significant research questions remain, highlighting the need to explore the extent to which firms use available funds for repurchases instead of investing in long-term projects or increasing employment.

In this backdrop, we exploit the staggered legalization of share repurchases across 17 coun-

¹See, e.g., <https://shorturl.at/PVkNZ>.

²See, e.g., <https://shorturl.at/1Ckk5>.

tries between 1985 and 2010, similar to [Wang, Yin, and Yu \(2021\)](#) (henceforth “WYY21”), to study how share repurchase legalization affects corporate investment across all public firms in countries that legalized repurchases. Our analysis reveals a striking finding: legalizing share repurchases is followed by a statistically and economically meaningful *increase* in investment (i.e., *CAPEX* and *R&D*). Our effect estimates imply an increase of 8% to 9.8% in investments after repurchase legalization. This positive effect is driven entirely by non-repurchasing firms—those that do not immediately engage in repurchases after legalization, which constitute approximately 93.5% of public firms in these markets—and is robust to various econometric specifications that address potential biases from the “bad comparisons” problem in staggered adoption settings.

We further demonstrate that these investment effects are consistent with the equity-capital access hypothesis: that repurchase legalization improved access to equity capital across public companies in the affected markets. We test five predictions of this hypothesis. If share repurchase legalization eased equity-capital access: (1) investment increases should be concentrated among firms that stand to benefit most from external equity financing; (2) public companies’ capital structures will shift from debt to equity; (3) such improved access to capital could result in more positive NPV projects being realized, leading to improved operating performance and valuation; (4) the investment effects should be more pronounced in countries with greater frictions on capital access, where improvements in equity market functioning would have larger marginal effects; and (5) the investment effects should be stronger where the legalization had greater impact, as measured by subsequent buyback volumes.

Consistent with these predictions, we find that the positive investment effects are driven entirely by firms that do not engage in repurchases within two years of legalization (“non-

repurchasing firms”), which constitute approximately 93.5% of public firms in these markets and tend to be younger, smaller, higher-growth, and hold less cash. Moreover, share repurchase legalization is associated with a decline in debt issuances and overall debt, and an increase in equity issuances and overall equity, in companies’ capital structures. Legalization is also associated with improvements in sales growth, ROE, ROA, Tobin’s Q, and buy-and-hold stock returns, consistent with companies realizing more positive NPV opportunities. Finally, cross-sectional analyses show that the positive effects of repurchase legalization on investment are more pronounced—by at least a factor of 2—in countries with greater frictions on capital access, such as those with capital controls, high cash concentration among a subset of firms, or a high degree of equity market segmentation. Moreover, investment effects are stronger in countries where legalization had greater practical impact, as measured by post-legalization buyback volumes, consistent with the hypothesis that where the reform more substantially changed market dynamics, the improvements in equity capital access were correspondingly larger.

Overall, our evidence supports the conjecture that repurchase legalization is especially beneficial in environments where capital allocation inefficiencies are more severe, enabling a more flexible flow of capital from cash-rich mature firms to cash-needy firms with greater growth opportunities. These findings do not support the claim that legalizing repurchases systematically harm corporate investments across the board.

This paper makes several contributions to the literature in corporate finance and to ongoing public policy debates. First, we contribute to the growing body of research on the real effects of share repurchases by providing new causal evidence on their impact on corporate investment. Leveraging recent advances in econometric methods for staggered difference-in-differences designs, our study offers more robust estimates of the effects of repurchase

legalization on CapEx and R&D spending. Our findings cast doubt on the claim that legalizing repurchases systematically reduce public firm long-term investment, particularly when considering the broader set of public firms beyond just those that engage in buybacks. This nuanced perspective adds valuable insight to the ongoing debate about the tradeoffs between shareholder payouts and corporate investment.

Second, our work adds to the understanding of the impact of share repurchase legalization around the world. Our findings contrast sharply with recent work by [Wang et al. \(2021\)](#) (“WYY21”), who documented negative investment effects from repurchase legalization. However, their analysis focused exclusively on firms that engaged in repurchases within two years of legalization (“repurchasing firms”)—only about 6.5% of public firms. These repurchasing firms, we show, tend to be older, larger, possess fewer growth opportunities, hold more cash, and pay out more dividends. More importantly, this small minority of firms exhibited substantially different effects relative to those of the full public firm sample, which is of first-order policy interest.

Third, our work highlights the importance of considering the economy-wide effects of financial regulations. By examining how repurchase legalization affects both repurchasing and non-repurchasing firms, we shed light on potential capital reallocation effects that previous studies have largely overlooked. This broader view is crucial for understanding the full economic implications of share repurchases and can inform more comprehensive policy evaluations. Our finding that, by easing equity capital access, repurchase legalization may actually increase investment among smaller, younger, and less profitable firms suggests that the effects of buybacks on economic growth and innovation may be more nuanced than previously thought.

Finally, our paper has important implications for public policy and corporate governance.

Ongoing legislative efforts to restrict share repurchases are largely predicated on the belief that buybacks harm corporate investment. Our findings suggest that these concerns are overstated, consistent with [Fried and Wang \(2018, 2019, 2021\)](#), [Asness, Hazelkorn, and Richardson \(2018\)](#), [DeAngelo \(2023\)](#), and [Guest, Kothari, and Venkat \(2023\)](#). By providing more robust and comprehensive causal evidence on the relationship between repurchase legalization and investment, our work helps policymakers make more informed decisions about the regulation of corporate payout policies. Moreover, our findings underscore the need for a careful and empirically-grounded approach to corporate governance reforms.

2 Background

The debate over the economic effects of share repurchases has intensified in recent years, fueled by the dramatic increase in buyback activity. In the United States alone, S&P 500 companies spent a record \$1.2 trillion on share repurchases in 2022, surpassing the previous high of \$806 billion in 2018 (S&P Dow Jones Indices, 2023). This surge in buybacks has sparked concerns among policymakers, academics, and corporate governance experts about their potential impact on long-term corporate investment and economic growth.

Proponents of share repurchases argue that they are an efficient means of returning excess capital to shareholders when firms lack attractive investment opportunities ([Grullon and Michaely, 2004](#)). By this view, buybacks help mitigate agency problems associated with free cash flow ([Jensen, 1986](#)) and allow capital to flow to its highest-value uses in the economy. Moreover, the flexibility of repurchases compared to dividends can be valuable for managing temporary cash flow shocks or signaling undervaluation ([Jagannathan et al., 2000](#)).

Critics, however, contend that the growing prevalence of repurchases may incentivize managers to sacrifice long-term investments in favor of short-term stock price appreciation (Lazonick, 2014; Almeida et al., 2016). They argue that buybacks divert resources from productive investments in research and development, capital expenditures, and human capital, potentially hampering innovation and long-term growth. This view has gained traction among policymakers, leading to proposals for restricting or taxing share repurchases.

The empirical evidence on the relationship between share repurchases and corporate investment has been mixed. Survey evidence suggest that CFOs make investment decisions first and then use leftover cash for repurchases, rather than the other way around, so that repurchases do not necessarily crowd out investment (Brav, Graham, Harvey, and Michael, 2005). Nevertheless, several studies find a negative association between buybacks and investment. Some studies argue this relation is due to repurchases being motivated by over-investment and excess cash (e.g., Grullon and Michael, 2004; Boudry, Kallberg, and Liu, 2013), while others suggest that managers trade off valuable investments and employment for stock repurchases that allow them to meet short-term EPS targets (Almeida et al., 2016).

Establishing a causal relationship between repurchases and investment has been challenging due to the endogenous nature of repurchase decisions (Bonaimé and Kahle, 2024). One exception is the study by Almeida et al. (2016), which employs a regression discontinuity design based on analyst consensus EPS forecasts. Yet, even if the evidence documented in Almeida et al. (2016) are taken at face value, they are insufficient for broad policymaking. For example, the documented effects are unlikely to be representative, as the empirical methodology hones in on a specific subset of firms—close to meeting or just missing analyst EPS forecasts. This represents a relatively small fraction of all repurchasing firms, which may engage in repurchases for other (non-EPS related) reasons, such as mitigating dilution from

employee stock compensation or facilitating debt recapitalization ([Kahle, 2002](#); [Bens, Nagar, Skinner, and Wong, 2003](#); [Bonaimé, Harford, and Moore, 2020](#); [Farre-Mensa, Michaely, and Schmalz, 2024](#)). Moreover, the evidence speaks to short-term effects (in the year after the repurchase) of share repurchases.

Thus, although the literature on share repurchases is maturing, many significant research questions about repurchases remain. [Bonaimé and Kahle \(2024\)](#) highlight the need to better understand the extent to which firms use available funds for repurchases instead of investing in valuable long-term projects or increasing employment, to what extent myopic behavior exists at the margins or is widespread, and whether the deleterious effects of repurchases are temporary or long lasting.

In this context, WYY21 contributed timely and important evidence on the causal impact of share repurchases. The study exploits the staggered legalization of share repurchases across 17 countries between the 1980s to the 2000s as plausibly exogenous shocks and examines the changes in the behaviors and outcomes of repurchasing firms, or those firms that engaged in repurchases within two years after legalization. Their SDID analysis suggested that legalizing buybacks led to significant reductions in capital expenditures and R&D spending among repurchasing firms.

WYY21’s findings are particularly relevant to the policy debate for several reasons. First, by amassing a broad sample, both in the cross section and time series, WYY21’s estimates can speak to more representative and long-term effects of repurchases. Second, by leveraging cross-country variation in repurchase legalization, the study offers insights that go beyond the U.S. context, where most previous research has focused. Third, the study’s use of a quasi-experimental design addresses some of the endogeneity concerns that have plagued earlier studies. Finally, the finding of a negative causal effect on investment provides ammunition

for those advocating for stricter regulation of share repurchases.

However, the policy implications of their study hinge on the robustness and generalizability of their results. If the negative effects on investment are confined to a subset of repurchasing firms or are offset by positive effects elsewhere in the economy, the case for broad restrictions on buybacks may be weakened. Moreover, recent advances in econometric methods for SDID designs have raised questions about the validity of traditional TWFE estimators in such settings (e.g., [de Chaisemartin and D’Haultfœuille, 2020](#); [Callaway and Sant’Anna, 2021](#); [Goodman-Bacon, 2021](#); [Sun and Abraham, 2021](#); [Borusyak, Jaravel, and Spiess, 2024](#)).

Given the high stakes of the policy debate surrounding share repurchases, a thorough re-examination of the evidence presented by WYY21 is warranted. While their study made significant strides in addressing endogeneity concerns and providing cross-country evidence, questions remain about the robustness of their findings to more robust estimators for SDID designs (e.g., [de Chaisemartin and D’Haultfœuille, 2020](#); [Callaway and Sant’Anna, 2021](#); [Goodman-Bacon, 2021](#); [Sun and Abraham, 2021](#); [Borusyak et al., 2024](#)) and their generalizability beyond the subset of firms that quickly implemented repurchases after legalization. Moreover, the broader economic implications of repurchase legalization, including potential effects on other public firms, have yet to be fully explored. By addressing these gaps in the literature, a re-evaluation can provide valuable insights for policymakers grappling with how to regulate corporate payout policies in a way that balances shareholder rights with the promotion of long-term investment and economic growth. This study aims to fill these gaps by re-examining and extending WYY21’s analysis using state-of-the-art econometric methods and considering a more comprehensive set of outcomes and firms.

3 Empirical Analyses

This section details our empirical analyses. We begin by demonstrating that we are able to reconstruct WYY21’s sample and expand this sample for the cross section of listed firms in the legalization countries. We describe differences in the repurchasing versus non-repurchasing firms. We then report estimates of the effect of legalizing repurchases for *all* public firms in the legalizing countries.

3.1 Data

Our main analyses requires the collection of market and accounting information for all public companies in the legalizing countries. As WYY21’s sample is limited to firms that engaged in share repurchases within two years of legalization (“repurchasing firms”) in each of the 17 jurisdictions studied, it cannot accommodate an analysis of effects beyond repurchasing firms. As such, we construct our own dataset to allow for a more comprehensive examination of repurchase legalization effects across the entire population of public firms in these jurisdictions. Although we cannot employ WYY21’s sample for our main analysis, it serves as a useful benchmark for validating our reconstructed data.³

In creating our own dataset, we adhere to the sample restrictions used in WYY21. We obtain Worldscope data from 1980 to 2018 for the 17 countries that legalized repurchases between 1985 and 2010, as included in WYY21’s sample. These countries span from Canada, which legalized repurchases in 1985, to Kuwait, which did so in 2010. The sample excludes countries that legalized repurchases before 1985, such as the United States, due to insufficient pre-period data in Worldscope. Additionally, some countries are omitted due to data quality

³We are grateful to WYY21’s authors for providing their final regression dataset and the code accompanying their main regression tables to the *Journal of Financial Economics*.

issues identified in WYY21.⁴

Our approach differs from WYY21 in the measurement of repurchases. While WYY21 uses SDC Platinum, we utilize Worldscope’s repurchase variable (ITEM04751). This difference in data sources may result in slight differences in the set of repurchasing firms and the repurchased amounts in our sample compared to WYY21. However, as we show below, these differences do not appear material in the regression analysis.

Our main analyses focuses on four dependent variables: *Repurchase*, *Cash*, and *Investment*, which is the sum of *CapEx*, and *R&D*. Following WYY21, we replace missing R&D values with 0 and we scale all dependent variables, except for *Repurchase*, by the sum of total assets and repurchases. *Repurchase* is scaled only by assets. When including covariate controls, we employ the same set as WYY21, all sourced from Worldscope. Detailed descriptions of all variables are provided in [Appendix A](#).

3.2 Validating Sample Construction

We check the quality of this reconstruction by comparing some of the main results in WYY21 estimated using the paper’s sample versus our sample. Our ability to replicate the original findings of WYY21 by applying their estimation methods to our independently constructed sample provides confidence in the quality of our reconstruction.

[Table 1](#) reports the replication results. We employ their basic pooled two-way fixed-effects

⁴The following countries are included in WYY21’s analysis, in chronological order (with legalization year reported in parentheses): Canada (1985), Spain (1989), Netherlands (1992), Switzerland (1992), New Zealand (1994), South Korea (1994), Japan (1995), Russia (1995), Germany (1998), Singapore (1998), Israel (1999), South Africa (2000), Taiwan (2000), Greece (2003), China (2005), Turkey (2009), and Kuwait (2010). For a detailed discussion of the countries and the sample selection process, see WYY21 Section 3 and Table 1.

regression specification:

$$Y_{ijt} = \beta_0 + \beta_1 \text{Legalization}_{jt} + \gamma'_k \mathbf{X}_{ijt} + FE_i + FE_t + \epsilon_{ijt}, \quad (1)$$

where for firm i in country j and year t , Y_{ijt} is the dependent variable of interest (e.g., *Repurchase*, *Cash*, or *Investment*), $\mathbf{X}_{ijt}$ refers to the firm-level control variables, FE_i are firm-fixed effects, and FE_t are year-fixed effects.

Only firms that repurchases within two years of legalization are included in the WYY21 sample. The maintained identification assumption is that all of the sample’s firms have the same counterfactual intent to repurchase, but only the post-legalization firms can actually repurchase. Additionally, the treated and control firms must have the same counterfactual trends in *CapEx* and *R&D* had legalization never occurred.

Panel A, [Table 1](#), reproduces the effects of share repurchase legalization on *Repurchase*, *Cash*, and *Investment* using WYY21’s regression sample. Panel B presents the results from estimating the same specifications as Panel A but using our independently collected data.

We present both covariate-free (odd columns) and covariate-inclusive (even columns) specifications.⁵ Our inclusion of covariate-free specifications is motivated by recent econometric literature highlighting potential biases in TWFE DiD estimates when including time-varying covariates (e.g., [Pearl, 2013](#); [Imbens, 2020](#); [Sant’Anna and Zhao, 2020](#)). The inclusion of “bad” controls can potentially induce collider bias ([Pearl, 2013](#); [Imbens, 2020](#)), and [Sant’Anna and Zhao \(2020\)](#) notes that TWFE DiD regressions with time-varying controls are valid estimators for the average treatment effect on the treated (ATT) only under additional, potentially more stringent assumptions beyond the standard “parallel-trends” and

⁵The covariate-inclusive specification is WYY21’s “long” regression specifications with the full set of covariates. Untabulated results confirm our ability to replicate their “short” specifications with fewer controls.

“no-anticipation” requirements.

As shown, we successfully reproduce Panel A’s estimates using our independently constructed data, with comparable coefficients on all key variables. The legalization effect on repurchases (0.38-0.55 in our sample, compared to 0.39-0.59 in WYY21), cash holdings (-3.64 to -1.11, compared to -3.39 to -1.30), and investment (-1.11 to -0.75, compared to -1.07 to -0.68) are all consistent with the original study’s conclusions. Moreover, the regression samples in each specification are of similar size.⁶ While including controls doesn’t substantially alter statistical inferences in our replication, it does reduce the sample size by approximately 40%, a point we revisit in subsequent analyses.

Overall, the close correspondence between our results and WYY21’s suggests that our sample construction is robust and provides a reliable foundation for extending the analysis to the broader set of public firms in repurchase-legalizing countries.

3.3 Sample Summary Statistics

Table 2 presents summary statistics for our full sample, which includes all listed firms in the 17 markets that legalized share repurchases. Our sample comprises approximately 287,000 firm-year observations with non-missing *Repurchase*, substantially larger than WYY21’s sample of repurchasing firms. Share repurchases average 0.23% of assets across the full sample, considerably lower than the 0.46-0.57% reported in WYY21’s repurchasing-firm sample, reflecting the fact that most public firms do not engage in repurchases even after legalization. Cash holdings average 18.8% of total assets, with substantial variation (standard deviation

⁶During our replication process, we identified and addressed a technical issue with the winsorization approach in WYY21 that resulted in some observations being converted to missing values. Our replication employs a standard winsorization at the top and bottom 1% of each country’s distribution, which recovers these observations. See the Online Appendix for detailed discussion of this methodological adjustment and comparative summary statistics across the original and replication samples.

of 18.9%). Total investment, including both capital expenditures (*CapEx*) and research and development (*R&D*), averages 7% of assets, with CapEx accounting for 5.38% and R&D for about 1.55%.

3.4 Differences Between Repurchasing and Non-repurchasing Firms

To motivate our analysis of all public firms in the repurchase-legalizing countries, our empirical analysis begins by better understanding how repurchasing firms differ from non-repurchasing firms. We estimate a linear probability model predicting which firms engage in share repurchases within two years of legalization. [Table 3](#) presents these results, with increasingly comprehensive specifications that incorporate incremental covariates.

The estimates reveal several distinct characteristics of repurchasing firms compared to their non-repurchasing counterparts. First, repurchasing firms tend to be more mature in their business life cycle, as evidenced by the positive and significant coefficients on firm age and size (*Log Assets*) across all specifications. Second, repurchasing firms have lower growth opportunities, as indicated by the positive coefficient on *Log Book to Market* and the negative (albeit often insignificant) coefficient on *Sales Growth*. Third, repurchasing firms hold significantly more cash (coefficient of 0.074-0.073 on *Cash/Assets*) and pay more dividends (coefficient of 4.24-4.32 on *Dividends/Assets*), consistent with their having more excess cash. Finally, repurchasing firms generally have less insider ownership, as shown by the negative coefficient (-0.044) on *Insider Shares* in column 4.

These findings suggest that repurchasing firms are typically larger, more mature companies with fewer growth opportunities and more excess cash than their non-repurchasing counterparts. This systematic selection into repurchasing activity implies that research focusing solely on repurchasing firms may offer an incomplete picture of the economy-wide

effects of repurchase legalization. Indeed, non-repurchasing firms—which constitute approximately 93.5% of public firms in these markets—may experience different, and potentially offsetting, effects.

3.5 The Effect of Repurchase Legalization on All Public Firms

We contend that, from a policy perspective, a more natural starting point for analyzing the impact of legalizing repurchases is to assess their effects on all public firms. Since capital returned to shareholders by repurchasing firms can be reinvested in other companies in the economy, non-repurchasing firms in legalizing countries may also experience significant effects. Therefore, limiting analysis to only repurchasing firms excludes an important component of the overall treatment effect.

While the traditional pooled TWFE approach employed by WYY21 is a standard method for difference-in-differences analysis, recent econometric literature has highlighted potential biases in such estimators, particularly in staggered adoption settings like ours. These biases arise from what has been termed the “bad comparisons” or the “negative weights” problem highlighted in [de Chaisemartin and D’Haultfœuille \(2020\)](#) and [Goodman-Bacon \(2021\)](#), where already-treated units serve as controls for newly-treated units in later periods. These issues can lead to substantially biased treatment effect estimates, even when treatment assignment is random and parallel trends assumptions hold (see, also, [Callaway and Sant’Anna, 2021](#); [Sun and Abraham, 2021](#); [Borusyak et al., 2024](#)). The biases are particularly concerning when treatment effects are heterogeneous over time or across units, which is plausible in our setting given the dynamic nature of investment decisions ([Bloom, 2007](#); [Bloom, Bond, and Van Reenen, 2007](#); [Bloom, 2009](#)).

To address these concerns, we employ a stacked difference-in-differences design, following

(Gormley and Matsa, 2011; Cengiz, Dube, Lindner, and Zipperer, 2019; Deshpande and Li, 2019). This approach creates separate datasets for each treatment-year cohort (i.e., based on legalization year), each comprising the treated units and all not-yet-treated units as controls. These datasets are then “stacked” and analyzed together, with stack-specific two-way fixed effects to ensure that comparisons are made only between the newly-treated units and valid control units within each stack, then applies OLS variance weighting to generate an overall treatment effect. This is an attractive approach for addressing the issues associated with traditional two-way fixed effects models in staggered adoption settings and is suitable for our cross-country study of repurchase legalizations. As Baker, Larcker, and Wang (2022) note, the stacked regression approach is “likely the most easily implementable solution for researchers interested in producing aggregated treatment effect estimates via OLS while circumventing the problems introduced by staggered treatment timing and treatment effect heterogeneity.”

Table 4 presents the results of our stacked difference-in-differences analysis analysis the full sample of public firms. The table presents results for the three main dependent variables—*Repurchase* (columns 1-3), *Cash* (columns 4-6), and *Investment* (columns 7-9)—while Panel B breaks down *Investment* into *CapEx* and *R&D*. Across all specifications, we include stack-firm and stack-year fixed effects, with standard errors clustered at the firm-stack level.

For each dependent variable, three model specifications are provided, progressively adding sample restrictions or controls: in the first specification, we estimate a stacked TWFE regression without firm-level controls; in the second specification, we estimate the same covariate-free stacked TWFE regression but on a sample of firms that have non-missing values for all control variables; in the third specification, we estimate the stacked TWFE regression

with firm-level controls included. This progression enables us to understand whether the differences between the covariate-free and covariate-inclusive specifications are due to sample selection or due to the inclusion of control variables.

The results reveal a strikingly different pattern. While repurchase legalization does increase share repurchases (columns 1-3) and reduce cash holdings (columns 4-6) across all firms, consistent with WYY21, the effect on investment is markedly positive and significant (columns 7-9). The coefficient on legalization in column 9, which includes the full set of controls, indicates that repurchase legalization is associated with a 0.57 percentage point increase in investment relative to total assets, representing an approximate 8.1% increase relative to the sample mean investment of 7%. This result directly contradicts WYY21's conclusions that legalizing repurchases harm investments.

Panel B further decomposes the investment effect into capital expenditures and RD. The positive investment effect is primarily driven by increased capital expenditures, with a coefficient of 0.58 (column 3), representing a 10.8% increase relative to the sample mean. The effect on R&D is sensitive to sample selection. For the baseline specification that does not require non-missing covariates, we also observe a positive and statistically significant effect on R&D (column 4). However, when we restrict the sample to firms with non-missing controls (column 5), this effect becomes statistically insignificant and economically small, even before controls are included. This suggests that the differences in R&D results are driven by the selection of firms with non-missing controls—which tend to be older, larger, and more profitable—rather than by the inclusion of controls themselves.

To examine the dynamic pattern of these effects, Figure 1 plots the estimated coefficients from an event-study specification of our stacked difference-in-differences model. The figure shows that the investment effects gradually increase over time, becoming economically and

statistically significant around three years after legalization and continuing to grow thereafter. This pattern suggests that the positive investment effects are not merely a short-term reaction but reflect a structural shift in corporate investment behavior following repurchase legalization.

To examine the dynamic pattern of these effects, [Figure 1](#) plots the estimated coefficients from an event-study specification of our stacked difference-in-differences model. The figure shows that the investment effects gradually increase over time, becoming economically and statistically significant around three years after legalization and continuing to grow thereafter. This pattern suggests that the positive investment effects are not merely a short-term reaction but reflect a structural shift in corporate investment behavior following repurchase legalization, consistent with our expectations that investments adjust dynamically to changes in the economic environment.

3.6 Robustness to Alternative Estimators

To further validate our findings and address potential limitations of the stacked regression approach, we also examine overall effect estimates based on two estimators proposed by [Callaway and Sant’Anna \(2021\)](#): an outcome regression DiD estimator based on ordinary least squares (“OR”) and a doubly-robust DiD estimator (“DRIPW”) based on inverse probability of tilting and weighted least squares ([Sant’Anna and Zhao, 2020](#)).

These estimators offer stronger theoretical justification and greater flexibility than stacked regression, but they are more complex to implement. For example, the researcher must first estimate cohort-specific effect estimates before aggregating them to construct overall effect estimates. Following [Callaway and Sant’Anna \(2021\)](#), Equation 3.11, we aggregate cohort-specific estimates across all time periods and then average these effects across groups

to summarize the overall average effect of participating in the treatment. This summary treatment effect estimate is interpreted similarly to an ATT in the standard DiD setup.

Table 5 presents results using these alternative estimators using different matching variables. The first row presents OR estimates without matching. Rows 2-4 match on variable values at different pre-treatment periods (1, 3, and 5 years prior to legalization), while rows 5-6 match on the 3-year and 5-year averages of those variables, respectively. For columns 1-2, 5-6, and 9-10, matching is based on the pre-treatment values of the same control variables used in our main specifications (*Log Assets*, *Log Net Sales*, *Log Net Income*, *Leverage*, *ROA*, *Sales Growth*, *EBIT to Sales*, *PPE to Sales*, *Quick Ratio*, and *Market Share*), following WYY21. For columns 3-4, 7-8, and 11-12, matching is based on the pre-treatment values of the outcome variable itself. The odd-numbered columns use the standard outcome regression estimator, while even-numbered columns use the doubly-robust inverse probability weighted estimator.

The results consistently confirm our main findings. Across all matching approaches and estimation methods, we observe positive and significant repurchase effects, negative and significant cash effects, and positive and significant investment effects. The magnitudes of the investment effect range from 0.9 to 2.9 percentage points, generally larger than our baseline stacked difference-in-differences estimates. (The difference in magnitudes is largely due to the differences in the weighting of individual cohort-specific treatment effects between OLS and these alternative approaches.) The effects are particularly strong when matching on T-5 values (row 4), with coefficients of 2.95 and 1.53 percentage points for the OR and DRIPW estimators, respectively, when matching on covariates (columns 9-10). Similarly robust positive effects are observed when matching on 3-year and 5-year average values (rows 5-6).

Table 5, reports the overall effect estimates for each of the four outcome variables using each of the CS estimators we consider. The results for *Repurchase* are very similar to those from stacked regression estimates: all CS estimators show a positive effect of legalization on repurchases, with similar magnitudes to those in Panel A. The CS estimates for *Cash* are mixed and statistically weak. Regarding the investment variables—*CapEx* and *R&D*—the CS estimates also diverge from the stacked regression estimates in terms of statistical significance and economic magnitudes. However, they yield a similar conclusion: the CS estimates do not provide strong empirical evidence supporting the argument that repurchases repurchasing firms’ investment.

3.7 Testing the Equity-Capital Access Hypothesis

Our analysis of all public firms supports the hypothesis that legalizing share repurchases improves access to equity capital across public companies in the affected markets. This “equity-capital access” hypothesis posits that legalizing repurchases increases the availability of equity capital. To the extent so, we may expect such an effect to redirect resources from cash-rich, mature firms with limited growth opportunities to firms with more productive investment prospects.

Testing the equity-capital access hypothesis directly is challenging, as we cannot directly observe or trace the movement of capital.⁷ However, we can test five key predictions that would be consistent with this mechanism.

⁷One empirical exception is Lin (2024). Drawing on U.S. flow-of-funds accounts, county-level IRS dividend data, FDIC branch-level deposit records, and bank call reports, Lin traces how corporate equity payouts are deposited by households and then recycled through the banking system into loans, thereby providing rare direct evidence of a “cash-to-deposits-to-lending” channel of capital recirculation.

3.7.1 Firm-Level Investment Effect Heterogeneity: Non-Repurchasing vs. Repurchasing Firms

First, if repurchase legalization improved equity capital access, we would expect the investment effects to be concentrated among firms with greater need for external financing—namely, the non-repurchasing firms that tend to be younger, smaller, and more cash-constrained. [Table 6](#) presents the results of our stacked difference-in-differences analysis separately for these two groups. Panel A shows the effects for non-repurchasing firms, which constitute the vast majority of our sample. For these firms, repurchase legalization leads to a significant increase in investment of 0.78-0.98 percentage points (columns 7-9), equivalent to an 11.1-14% increase relative to the sample mean. This positive effect is robust across all specifications and is statistically significant at the 1% level. Non-repurchasing firms also experience increases in repurchases after legalization, though the magnitude (0.10-0.14 percentage points) is smaller than for repurchasing firms. These findings likely capture the eventual repurchases, beyond two years after legalization, that “non-repurchasing” firms engage in. Cash holdings also decrease modestly for these firms, with effects becoming statistically significant only in the specification with controls (column 6).

In contrast, Panel B shows the effects for repurchasing firms, which align closely with WYY21’s findings. For this subset of firms, repurchase legalization is associated with a decrease in investment of 0.59-0.78 percentage points (columns 7-9), though the statistical significance varies across specifications. These firms experience much larger increases in repurchases (0.59-0.62 percentage points) and more substantial reductions in cash holdings (2.22-3.22 percentage points) compared to non-repurchasing firms, consistent with their classification as firms that quickly implement buybacks after legalization.

These contrasting findings provide support for the equity-capital access hypothesis: while

repurchasing firms—which are older, larger, and more cash-rich—reduce investment and return cash to shareholders, non-repurchasing firms—which are younger, smaller, and more cash-constrained—significantly increase investment.

3.7.2 Capital Structure Effects

Second, if repurchase legalization increased the availability of equity capital, we should observe shifts in firms’ capital structures from debt toward equity. [Table 7](#) examines this prediction by analyzing the effect of repurchase legalization on *Debt*, *Debt Issuance*, *Equity*, and *Equity Issuance*. Consistent with the equity-capital access hypothesis, we find that legalization is associated with significant decreases in *Debt* (columns 1-3) and *Debt Issuance* (columns 4-6), coupled with significant increases in *Equity* (columns 7-9) and *Equity Issuance* (columns 10-12). The economic magnitudes are substantial: legalization reduces *Debt* by approximately 0.93 percentage points (column 3), representing a 3.5% decrease relative to the sample mean, while increasing *Equity* by about 0.60 percentage points (column 9). The effect on *Equity Issuance* is particularly strong, with coefficients ranging from 2.83 to 3.67 percentage points across specifications, equivalent to a 23-30% increase relative to the sample mean.

3.7.3 Firm Performance and Valuation

Third, if repurchase legalization improves access to equity capital, firms should be able to fund more positive NPV projects, leading to improvements in overall firm performance and valuation. [Table 8](#) examines this prediction by analyzing the effect of legalization on profitability (*ROA* and *ROE*), growth (*Sales Growth*), and market valuation (*Tobin’s Q* and *Buy-and-Hold Returns*). The results strongly support this prediction. Legalization is

associated with significant improvements in ROA (2.29 percentage points, column 3), ROE (4.76 percentage points, column 6), and Sales Growth (6.98 percentage points, column 9). Market-based performance measures also improve substantially, with Tobin’s Q increasing by 5.68 points (column 3, Panel B) and buy-and-hold returns increasing by approximately 28.8 percentage points (column 6, Panel B).

As an alternative way to demonstrate the stock return impact of repurchase legalization, [Figure 2](#) illustrates the cumulative returns from investing \$1 following repurchase legalization, tracked over firms’ fiscal years (rather than calendar time). For each treatment cohort, we calculate equal-weighted returns among the treated and control firms, where the “investment period” begins at each firm’s fiscal year end following legalization. We compare firms in countries that have legalized repurchases (post-legalization) against those in countries that have not yet done so (pre-legalization). We cumulate the average returns in each group across fiscal years, with control firms exiting the pre-legalization group when their country legalizes repurchases.

To aggregate results across the 14 treatment cohorts, we compute weighted-average returns for each event year relative to legalization, with weights proportional to the number of firms in each cohort-fiscal year. We take this weighted average approach for the treatment firms and the control firms separately, and summarize the returns across event years in [Figure 2](#). The figure reveals that post-legalization firms substantially outperform pre-legalization firms, with the return differential widening notably after the fifth fiscal year and continuing to expand through the tenth fiscal year. By the end of the observation period, the post-legalization firms achieve approximately double the cumulative average return of the pre-legalization firms’. This performance gap across fiscal years provides additional support for the equity-capital access hypothesis, suggesting that improved access to equity capital

following repurchase legalization enables firms to pursue more value-enhancing investment opportunities over time.

3.7.4 Country-Level Investment Effect Heterogeneity: Capital Access Frictions

Fourth, if the equity-capital access hypothesis explains our findings, the positive investment effects should be more pronounced in environments where pre-existing frictions to capital access are more severe. [Table 9](#) tests this prediction by examining the heterogeneity in investment effects across countries with varying degrees of frictions in capital access. We use three proxies for capital allocation frictions: capital controls (columns 1-2), cash resource imbalance in the market (columns 3-4), and equity market segmentation (columns 5-6).

Columns 1 and 2 examine the differences in investment responses of firms in countries with and without capital controls.⁸ Capital controls, which restrict the flow of capital in and out of a country, could make it more challenging for companies to raise cash and potentially lead cash-rich companies to hoard capital. Consequently, we expect a greater investment effect in countries with capital controls compared to those without. The results strongly support this prediction: in countries with high capital controls (column 1), the investment effect is 1.45 percentage points, compared to just 0.39 percentage points in countries with low capital controls (column 2)—a difference of 1.06 percentage points that is statistically significant at the 1% level.

Similarly, we explore how the concentration of cash in a subset of firms creates frictions in capital access. The rationale is that cash hoarding by some public firms may reduce capital availability for others, creating an imbalance in access to financial resources. To operationalize this concept, we compute the interquartile range of cash holding intensity

⁸We identify countries' capital control policies in the year prior to share repurchase legalization. Capital control countries include South Korea, China, Kuwait, Russia, Taiwan, South Africa, and Japan.

(the ratio of cash to assets)—termed *Cash Spread*—and compare high-cash-spread (above median) to low-cash-spread countries.⁹ In high-cash-spread countries, we expect repurchase legalization and the subsequent improvement in public firms’ overall equity capital access to produce a larger impact on investments. The results confirm this prediction: in countries with high *Cash Spread* (column 3), the investment effect is 0.98 percentage points, compared to 0.45 percentage points in countries with low *Cash Spread* (column 4). This difference of 0.53 percentage points is significant at the 5% level.

Finally, we examine whether the investment effects of legalizing repurchases vary with equity market segmentation across countries. Market segmentation can create frictions in capital mobility and lead to inefficient capital allocation. Following [Jiao, Karolyi, and Ng \(2024\)](#), who study international mutual funds’ ability to navigate changing global investment environments, we classify countries into high versus low segmentation based on market competition measures.¹⁰

In countries with high market segmentation (column 5), where capital markets are less integrated and competitive, the investment effect is 0.94 percentage points, compared to 0.57 percentage points in countries with low market segmentation (column 6)—a difference of 0.37 percentage points that is significant at the 10% level. This result indicates that the positive investment effects of repurchase legalization are more pronounced in countries with greater frictions in capital mobility, providing additional support for the equity-capital access hypothesis.

Additionally, we examine whether investment effects vary based on the practical impact

⁹We identify countries’ cash spread in the year prior to share repurchase legalization. High-cash-spread countries include China, Germany, Greece, Israel, Japan, Kuwait, Singapore, South Africa, Taiwan, and Turkey.

¹⁰We thank David Ng (Cornell) for generously sharing their market segmentation data for this analysis. High segmentation countries in our sample are Germany, Greece, Kuwait, Netherlands, Russia, South Korea and Turkey.

of legalization, as measured by post-legalization buyback volumes. We split countries into high versus low buyback volume based on the median level of repurchase activity in the two years following legalization. Where legalization had greater practical impact on market behavior, as evidenced by higher buyback activity, we expect the corresponding improvements in capital access—and corresponding investment effects—to be larger. Columns 7 and 8 confirm this prediction: in countries with high buyback volumes, the investment effect is 1.32 percentage points, compared to 0.39 percentage points in countries with low buyback volumes—a difference of 0.93 percentage points that is statistically significant at the 1% level.

Collectively, our tests of the five predictions provide strong support for the equity-capital access hypothesis. These improvements manifest in shifts from debt to equity financing, better firm performance and valuation, and particularly strong investment effects in contexts with greater capital market frictions and where the reform had the greatest practical impact. These findings help explain why we observe positive investment effects for the broader set of public firms, even as repurchasing firms themselves reduce investment following legalization. These findings have important implications for ongoing policy debates about regulating share repurchases, suggesting that blanket restrictions on repurchases could have unintended consequences by hampering efficient capital reallocation across firms in the economy.

4 Discussion

We provide some caveats about the paper’s empirical results and their interpretation. Our identification strategy builds directly on the staggered legalization design WYY21. Like them, we treat the staggered adoption of repurchase legalization across countries as plausibly

exogenous shocks to corporate payout flexibility. However, in contrast to WYY21’s reliance on pooled two-way fixed effects estimators, we employ more modern econometric methods—stacked difference-in-differences and [Callaway and Sant’Anna \(2021\)](#) estimators—that avoid “bad comparison” and negative-weight problems, ensuring that treated firms are compared only to not-yet-treated firms.

WYY21 provide statistical evidence supporting the identification strategy, showing that legalization timing is uncorrelated with pre-existing country characteristics and that parallel trends assumptions are not violated. Our event-study estimates confirm no differential pre-trends in investment.

Even if the timing of legalization is not driven by contemporaneous firm-level shocks, it is important to recognize that repurchase legalization rarely occurred in isolation. In most jurisdictions legalization was part of a broader package of reforms aimed at enhancing the attractiveness of local capital markets for non-bank investors. These reforms often tightened board duties, strengthened disclosure rules, imposed solvency tests, or bolstered minority-shareholder protections. In this broader context, repurchase legalization was one element of wider corporate-governance and capital-market reforms aimed at strengthening shareholder accountability and shifting financing from bank-based to market-based channels.

This context suggests an alternative interpretation of our findings. Rather than attributing the observed rise in investment solely to the legalization of share repurchases *per se*, one could interpret our results as reflecting the combined effects of repurchase legalization and the complementary reforms that often accompanied it. Put differently, the positive investment effects may stem from a broader strengthening of equity markets, improved shareholder protection, and enhanced access to external finance. While our heterogeneity tests support the equity-capital access hypothesis, they should be read as capturing the impact of repurchase

legalization in tandem with the institutional reforms that frequently accompanied it.

Whether the observed effects are attributed narrowly to the legalization of repurchases or more broadly to the package of reforms with which legalization was often bundled, the evidence points to a consistent conclusion: regulatory changes that expand payout flexibility while strengthening shareholder accountability are associated with improved equity-capital access and higher corporate investment, particularly among firms most constrained in external financing. Both interpretations of our findings caution against policy proposals to dismantle mechanisms like share repurchases through blanket bans or severe restrictions. Our evidence suggests that, despite enabling some firms to return capital to shareholders, these mechanisms appear to facilitate broader improvements in capital market efficiency and access.

5 Conclusion

Our study provides new evidence on the impact of share repurchase legalization on corporate investment by examining the effects across all public firms in countries that legalized repurchases. Unlike previous research that focused exclusively on repurchasing firms, our comprehensive analysis reveals that legalizing repurchases leads to a statistically and economically significant increase in investment among public companies overall. This positive investment effect is substantial, representing an 8-10% increase relative to the sample mean, and grows stronger over time.

The contrasting findings between repurchasing firms (which experience investment decreases) and non-repurchasing firms (which experience investment increases) highlight the importance of considering the market-wide effects of financial regulations rather than focus-

ing solely on a subset of entities that are most responsive to the regulations. Our evidence supports the equity-capital access hypothesis: legalizing repurchases improves market access to equity capital and facilitates their redistribution from mature, cash-rich firms with limited growth opportunities to younger, smaller firms with more productive investment prospects.

Several additional findings strengthen this interpretation. Following repurchase legalization, firms shift their capital structure from debt toward equity, experience improved operating performance and market valuation, and show particularly pronounced investment effects in countries with greater frictions in capital access. These patterns are consistent with repurchases helping to alleviate capital allocation inefficiencies in public markets.

Our results have important implications for both academic research and policy discussions. They suggest that blanket restrictions on share repurchases, motivated by concerns about reduced corporate investment, may be misguided and potentially counterproductive. Instead, our findings indicate that repurchases might serve as an effective mechanism for capital reallocation, challenging the notion that they universally detract from productive investment. These insights highlight the importance of considering market-wide effects when evaluating financial regulations, rather than focusing solely on the firms most active in the regulated activity.

We also contribute to the corporate finance literature by providing more robust causal evidence on the relationship between share repurchases and investment and highlighting the importance of considering market-wide effects, areas that represent significant gaps in the literature on stock buybacks ([Bonaimé and Kahle, 2024](#)). We also enhance the understanding of how financial market regulations affect real economic activity across different institutional contexts.

While our study provides novel evidence on the effects of repurchase legalization, it is

important to acknowledge its limitations. First, this study does not rule out the possibility that some public companies abuse stock buybacks to enrich executives or to the detriment of long-term shareholder value. However, it does call into question how widespread such practices are among public companies and their net effects on the economy. Second, while our study’s findings using the full sample of public firms suggests that legalizing repurchases help to improve the efficiency of capital flow, we do not directly observe the reallocation of capital from repurchasing firms to non-repurchasing firms. Third, our analysis is based on historical data from the 1980s to 2000s, and the economic landscape has evolved since then.

Future research might explore the longer-term productivity and innovation outcomes stemming from this capital reallocation, investigate how different corporate governance environments mediate these effects, or study how specific regulatory frameworks surrounding repurchases impact the efficiency of capital redistribution. Additionally, examining to what extent myopic share repurchases exist at the margins or are widespread would provide a more comprehensive understanding of potential abuses and their implications.

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Appendix A Description of Variables

This table defines variables used in our analyses. The construction of variables based on data obtained from the Thomson/Refinitiv Worldscope database, as described in Section 2. All variables are winsorized the top and bottom 1% of the country-level distribution.

Variable	Description	Computation
<i>Legalization</i>	Firm-year variable. An indicator equal to 1 for all firm-years after the country legalizes repurchases. Equal to 0 otherwise.	If the firm's country legalized repurchases in year t , then the indicator is 1 for all firm-years greater than or equal to t . Legalization years retrieved from WYY.
<i>First Repurchase</i>	Firm-year variable. An indicator equal to 1 for firm years during and after the firm's first time repurchasing after legalization. It is 0 until the firm repurchases for the first time following legalization.	If the firm repurchases in year t following the country's legalization, then it is 1 for the firm in all years greater than or equal to t .
<i>Repurchase</i>	Firm-year variable. The ratio of share repurchase to total assets (in percentage). Winsorized at 1% and 99% levels.	Calculated as [Common/Preferred Redeemed, Retired, Converted (Worldscope item 04751) / Total Assets (Worldscope item 02999)] * 100.
<i>Cash</i>	Firm-year variable. The ratio of cash to total assets (in percentage) at the end of the year. End-of-year total assets (denominator) are adjusted by adding back the share repurchase item. Winsorized at 1% and 99% levels.	Calculated as [Cash & Short Term Investments (Worldscope item 02001) / (Total Assets (Worldscope item 02999) + <i>Repurchase</i>)] * 100.
<i>CapEx</i>	Firm-year variable. The ratio of capital expenditure to total assets (in percentage). End-of-year total assets (denominator) are adjusted by adding back the share repurchase item. Winsorized at 1% and 99% levels.	Calculated as [Capital Expenditure (Worldscope item 04601) / (Total Assets (Worldscope item 02999) + <i>Repurchase</i>)] * 100.
<i>R&D</i>	Firm-year variable. The ratio of R&D expense to total assets (in percentage). End-of-year total assets (denominator) are adjusted by adding back the share repurchase item. Winsorized at 1% and 99% levels.	Calculated as [Research And Development Expense (Worldscope item 01201) / (Total Assets (Worldscope item 02999) + <i>Repurchase</i>)] * 100.
<i>Age</i>	Firm-year variable. Represents the length of time the firm has been publicly traded.	Number of years since firm had first observation in Worldscope.
<i>Log Assets</i>	Firm-year variable. Represents total assets of the company converted to U.S. dollars using the fiscal year-end exchange rate.	Natural logarithm of [1 + Raw Total Assets (Worldscope item 07230)].
<i>ROA</i>	Firm-year variable. The ratio of net income to total assets. Winsorized at 1% and 99% levels.	Calculated as Net Income (Worldscope item 01651) / [Total Assets (Worldscope item 02999) + Com/Pfd Purchased, Retired, Converted, Redeemed (Worldscope item 04751)].

Variable	Description	Computation
<i>Log Book to Market</i>	Firm-year variable. The ratio of the book value of equity to the market value of equity.	Calculated as $[\log(\text{Book Value (Worldscope item 03501)} / [\text{Market Value (Worldscope item 08001)}])]$.
<i>Sales Growth (%)</i>	Firm-year variable. The growth rate of firm's net sales (in percentage). Winsorized at 1% and 99% levels.	Worldscope item 08631. Calculated as $(\text{Current Year's Net Sales or Revenues} / \text{Last Year's Total Net Sales or Revenues} - 1) * 100$.
<i>Dividends</i>	Firm-year variable. The ratio of dividend to total assets (in percentage). End-of-year total assets (denominator) are adjusted by adding back the share repurchase item.	Calculated as $[\text{Dividends Per Share (Worldscope item 05101)} * \text{Common Shares Outstanding (Worldscope item 05301)} / (\text{Total Assets (Worldscope item 02999)} + \text{Repurchase})] * 100$.
<i>Leverage</i>	Firm-year variable. The ratio of total debt to total assets. Total assets (denominator) is adjusted by adding back the share repurchase item. Winsorized at 1% and 99% levels.	Calculated as $[\text{Total Debt (Worldscope item 03255)} / \text{Total Assets (Worldscope item 02999)} + \text{Com/Pfd Purchased, Retired, Converted, Redeemed (Worldscope item 04751)}] * 100$.
<i>Insider Shares</i>	Firm-year variable. The fraction of ownership by insiders.	Calculated as $[\text{Insider Control (Worldscope item 08027)} / 100]$.
<i>Log Net Income</i>	Firm-year variable. Represents net income of the company converted to U.S. dollars using the fiscal year-end exchange rate.	Natural logarithm of $[1 + \text{Raw Net Income (Worldscope item 07250)}]$.
<i>Log Net Sales</i>	Firm-year variable. Represents net sales or revenues of the company converted to U.S. dollars using the fiscal year-end exchange rate.	Natural logarithm of $[1 + \text{Raw Net Sales or Revenues (Worldscope item 07240)}]$.
<i>EBIT to Sales</i>	Firm-year variable. The ratio of earnings before interest and taxes to net sales (in percentage). Winsorized at 1% and 99% levels.	Calculated as $[\text{Earnings Before Interest and Taxes (Worldscope item 18191)} / \text{Net Sales (Worldscope item 01001)}] * 100$.
<i>Market Share</i>	Firm-year variable. Winsorized at 1% and 99% levels.	Firm's percentage share of sales by all public firms in the same SIC industry and the same country.
<i>PPE to Sales</i>	Firm-year variable. The ratio of property, plant and equipment to net sales (in percentage). Winsorized at 1% and 99% levels.	Calculated as $[\text{Property, Plant and Equipment (Worldscope item 02301)} / \text{Net Sales (Worldscope item 01001)}] * 100$.
<i>Quick Ratio</i>	Firm-year variable. Winsorized at 1% and 99% levels.	Worldscope item 08101. Calculated as $(\text{Cash \& Equivalents} + \text{Receivables (Net)}) / \text{Current Liabilities}$.
<i>Capital Controls</i>	Country level variable. Equal to 1 if the country restricted capital flows in and out of the country at the time of repurchase legalization.	A country either has <i>Capital Controls</i> or <i>No Capital Controls</i> .
<i>Cash Spread</i>	Country level variable. Equal to 1 if the interquartile range of <i>Cash</i> within a country at the time of legalization is above the median for all countries.	A country either has <i>High Cash Spread</i> or <i>Low Cash Spread</i> depending on whether the cash spread is above or below the median cash spread at the time of legalization.

Fig. 1. Dynamic Effects of Share Repurchase Legalization's Impact on All Firms

This figure illustrates the dynamic investment effect estimates for the effect of repurchase legalization on all firms' investment decisions. All event time indicators are included in the regressions, with the coefficients for the 5 years before and 10 years after treatment depicted in the figure. The figure shows the regressions with stacked TWFE with controls. Control variables are *Log Assets*, *Log Net Sales*, *Log Net Income*, *Leverage*, *ROA*, *Sales Growth*, *EBIT to Sales*, *PPE to Sales*, *Quick Ratio* and *Market Share*. All event time indicators are included in the regressions, with the coefficients for the 5 years before and 10 years after treatment depicted. Error bars represent the 95% confidence intervals.

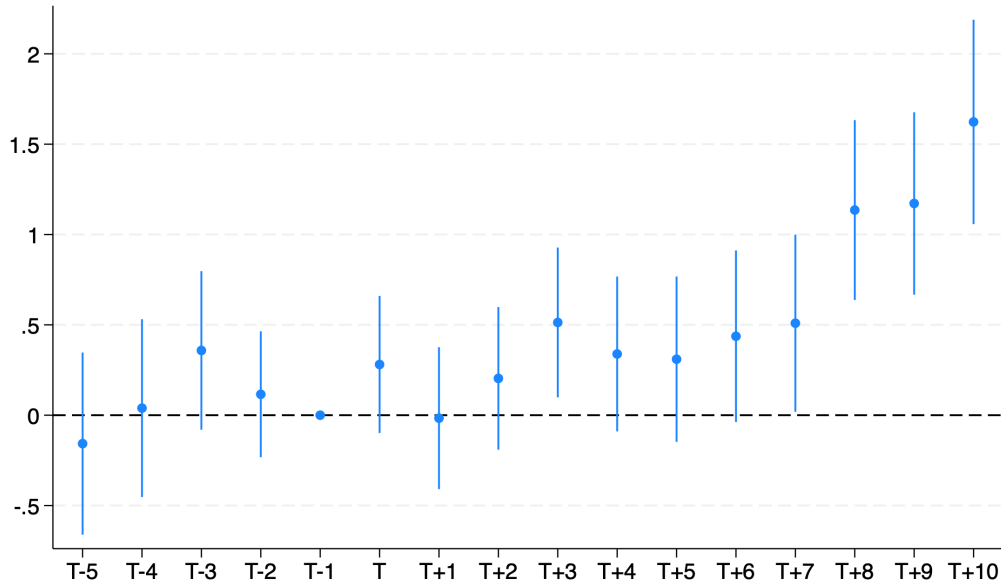


Fig. 2. Cumulative Average Return by Event Year: Post-Legalization vs. Pre-Legalization Firms

This figure illustrates the cumulative returns from investing \$1 following share repurchase legalization across 14 treatment cohorts. Returns are tracked over firms' fiscal years. For each cohort, we calculate equal-weighted average returns for firms in countries that have legalized repurchases (post-legalization, blue line) and firms in countries that have not yet done so (pre-legalization, red line). Control firms exit the pre-legalization group when their country legalizes repurchases. Returns are aggregated across cohorts using weights proportional to the number of firms in each cohort-fiscal year. The horizontal axis represents event years relative to legalization, with year 0 indicating the legalization year.

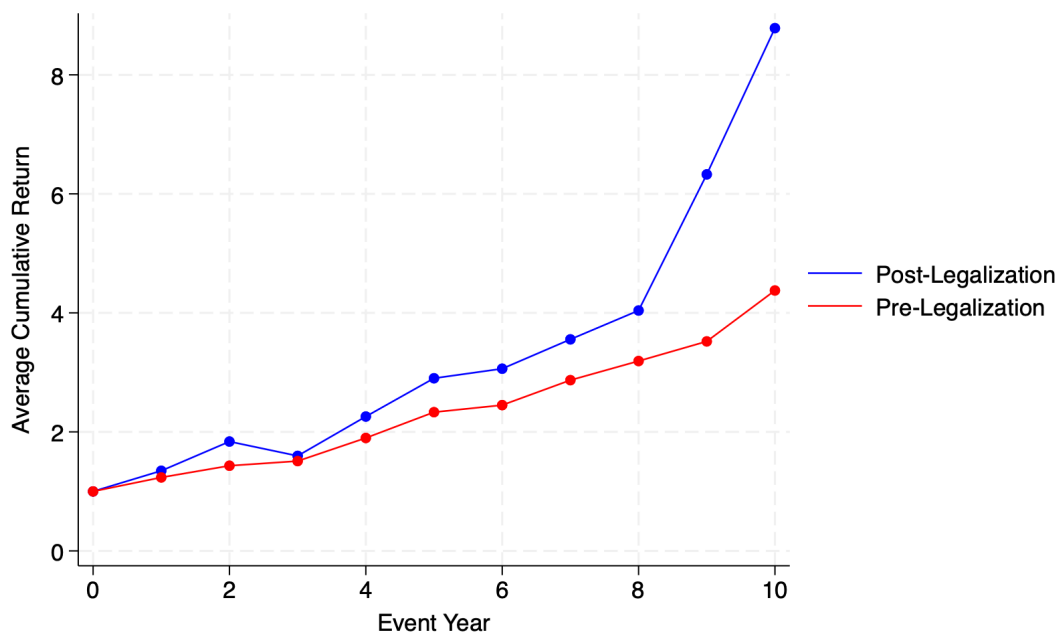


Table 1. Replication of WYY - Only Repurchasing Firms and Pooled TWFE

This table replicates the repurchase, cash balance and investment results from Wang, Yin and Yu (2021) (“WYY”). In both panels, firms are only included if they repurchase within two years of their country’s legalization (“repurchasing firms”). Panel A uses data that WYY released online while Panel B reports the results using our data. Both panels use the same pooled TWFE identification strategy used in WYY. For both panels, the dependent variable is *Repurchases* in columns 1-2, *Cash* in columns 3-4, *CapEx* in columns 5-6, and *R&D* in columns 7-8. For each dependent variable, the first column has no controls, while the second column includes the controls used in WYY. Control variables are *Log Assets*, *Log Net Sales*, *Log Net Income*, *Leverage*, *ROA*, *Sales Growth*, *EBIT to Sales*, *PPE to Sales*, *Quick Ratio* and *Market Share*. Variables are defined in [Appendix A](#). Standard errors, clustered at the firm-stack levels are reported in parentheses. Significance levels are indicated by *, **, and *** for 10%, 5%, and 1% respectively.

Panel A: WYY Data						
	<i>Repurchase</i>		<i>Cash</i>		<i>Investment</i>	
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Legalization</i>	0.3919*** (0.086)	0.5878*** (0.106)	-3.3855*** (0.566)	-1.3047** (0.533)	-1.0744*** (0.210)	-0.6790** (0.280)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm Controls	No	Yes	No	Yes	No	Yes
Clustering	Yes	Yes	Yes	Yes	Yes	Yes
Observations	16,093	10,023	17,573	11,604	18,196	11,290
Adj R^2	0.1652	0.1668	0.6112	0.7004	0.5529	0.5300

Panel B: TW (This Paper’s) Data						
	<i>Repurchase</i>		<i>Cash</i>		<i>Investment</i>	
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Legalization</i>	0.3796*** (0.060)	0.5454*** (0.075)	-3.6395*** (0.611)	-1.1107** (0.532)	-1.1117*** (0.217)	-0.7507** (0.293)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm Controls	No	Yes	No	Yes	No	Yes
Clustering	Yes	Yes	Yes	Yes	Yes	Yes
Observations	17,728	10,942	16,379	10,942	17,081	10,681
Adj R^2	0.1510	0.1677	0.6033	0.7053	0.4807	0.4944

Table 2. Summary Statistics

This table reports summary statistics for firm-year observations in our repurchase legalization analysis. All balance sheet variables, except repurchases, are scaled by total assets plus repurchases. [Appendix A](#) defines the variables.

<i>Variable</i>	<i>N</i>	<i>Mean</i>	<i>SD</i>	<i>p10</i>	<i>p25</i>	<i>p50</i>	<i>p75</i>	<i>p90</i>
<i>Repurchase</i>	287,079	0.23	1.16	0.00	0.00	0.00	0.00	0.18
<i>Cash</i>	273,970	18.80	18.89	1.63	5.50	13.10	25.35	43.86
<i>Investment</i>	261,039	7.00	9.09	0.23	1.48	4.51	9.19	15.92
<i>CapEx</i>	261,039	5.38	7.21	0.17	0.95	3.11	6.93	12.89
<i>R&D</i>	287,085	1.55	5.76	0.00	0.00	0.00	1.04	3.90
<i>Debt</i>	283,463	26.86	85.46	0.00	4.20	18.55	35.25	51.12
<i>debt_issuance_LC</i>	377,257	1.61	20.22	-7.53	-2.00	0.00	4.29	12.51
<i>Equity</i>	287,031	24.76	350.99	10.03	27.60	46.38	65.70	81.73
<i>equity_issuance_tar_LC</i>	210,108	12.30	83.76	-0.01	0.00	0.02	3.01	19.18
<i>ROA</i>	416,526	-17.31	216.40	-15.70	-0.47	2.19	5.85	11.04
<i>ROE</i>	414,921	1.02	61.23	-23.71	0.40	6.40	13.84	25.49
<i>Sales Growth</i>	380,109	17.69	80.43	-17.59	-3.33	4.77	18.95	46.79
<i>Tobin's Q</i>	359,421	166.55	778.75	15.30	31.46	63.42	129.18	267.92
<i>Buy-and-Hold Return</i>	310,929	26.25	129.44	-44.13	-21.19	3.50	36.36	93.86

Table 3. Selection into Repurchasing Firms

This table reports a linear probability model describing the determinants for being a firm that repurchased within two years of share repurchase legalization (“repurchasing firms”). The dependent variable is an indicator equal to 1 only if the firm-year observation is a repurchasing firm. All other observations are 0 (“non-repurchasing firms”). Country and year fixed effects are used in all specifications. Column 1 includes “maturity” variables (*Age*, *Log Assets*, *ROA*). Column 2 adds growth opportunities (*Log Book to Market* and *Sales Growth*). Column 3 adds balance sheet quality and payout policy characteristics (*Cash/Assets*, *Dividends/Assets* and *Leverage*). Finally, column 4 adds governance characteristics (*Insider Shares*). All variables are defined in [Appendix A](#). Standard errors, clustered at the firm level, are reported in parentheses. Significance levels are indicated by *, **, and *** for 10%, 5%, and 1% respectively.

	(1)	(2)	(3)	(4)
<i>Age</i>	0.0037*** (0.001)	0.0044*** (0.000)	0.0041*** (0.000)	0.0036*** (0.000)
<i>Log Assets</i>	0.0112*** (0.001)	0.0127*** (0.001)	0.0133*** (0.001)	0.0126*** (0.001)
<i>ROA</i>	-0.0019*** (0.000)	-0.0025* (0.001)	-0.0024 (0.002)	-0.0031* (0.002)
<i>Log Book to Market</i>		0.0048*** (0.002)	0.0059*** (0.002)	0.0057*** (0.002)
<i>Sales Growth</i>		-0.0021** (0.001)	-0.0011 (0.001)	-0.0008 (0.001)
<i>Cash/Assets</i>			0.0744*** (0.010)	0.0727*** (0.011)
<i>Dividends/Assets</i>			4.2396*** (1.078)	4.3198*** (1.106)
<i>Leverage</i>			0.0000 (0.000)	0.0000 (0.000)
<i>Insider Shares</i>				-0.0442*** (0.007)
Country FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	286,622	231,194	204,849	174,119
Adjusted R^2	0.1252	0.1373	0.1362	0.1332

Table 4. Share Repurchase Legalization and Investment - Stacked DiD Specifications

This table reports the impact of repurchase legalization on all firms' repurchases, cash balances and investments. We use stacked, two-way fixed effects DiD regressions. Panel A reports results for *Repurchases* in columns 1-3, *Cash* in columns 4-6 and *Investment* in columns 7-9. Panel B splits investment into *CapEx* in columns 1-3 and *R&D* in columns 4-6. For each dependent variable, the first column has no controls. The second column includes only observations that have non-missing controls. The third column includes control variables. Control variables are *Log Assets*, *Log Net Sales*, *Log Net Income*, *Leverage*, *ROA*, *Sales Growth*, *EBIT to Sales*, *PPE to Sales*, *Quick Ratio* and *Market Share*. Variables are defined in [Appendix A](#). Standard errors, clustered at the firm-stack levels are reported in parentheses. Significance levels are indicated by *, **, and *** for 10%, 5%, and 1% respectively.

Panel A: Repurchases, Cash and Investment									
	<i>Repurchase</i>			<i>Cash</i>			<i>Investment</i>		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
<i>Legalization</i>	0.2185*** (0.0142)	0.2284*** (0.0162)	0.2054*** (0.0165)	-0.6260** (0.2484)	-0.4674** (0.2311)	-0.9660*** (0.2175)	0.6864*** (0.1167)	0.6758*** (0.1480)	0.5672*** (0.1467)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Controls	No	No	Yes	No	No	Yes	No	No	Yes
Clustering	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	604,497	339,664	339,664	567,214	339,661	339,661	483,800	284,150	284,150
Adj R^2	0.2968	0.1794	0.1821	0.6472	0.6854	0.7151	0.5140	0.5104	0.5163

Panel B: Components of Investment – <i>Capex</i> and <i>R&D</i>						
	<i>Capex</i>			<i>R&D</i>		
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Legalization</i>	0.5980*** (0.1114)	0.6628*** (0.1438)	0.5781*** (0.1428)	0.1116*** (0.0291)	0.0077 (0.0276)	-0.0251 (0.0282)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm Controls	No	No	Yes	No	No	Yes
Clustering	Yes	Yes	Yes	Yes	Yes	Yes
Observations	483,800	284,150	284,150	604,505	339,664	339,664
Adj R^2	0.4591	0.4884	0.4950	0.6917	0.8261	0.8281

Table 5. Robustness of Share Repurchase Legalization's Effect - Callaway and Sant'Anna Estimators

This table reports the impact of repurchase legalization on all firms' repurchases (columns 1-4), cash balances (columns 5-8) and investments (columns 9-12) using estimators based on [Callaway and Sant'Anna \(2021\)](#). Row 1 reports results using no matching. Rows 2,3 and 4 match based on variable values in 1, 3 and 5 years prior to legalization. Rows 5 and 6 use the 3 and 5 year average. In columns that match on covariates (1-2, 5-6, 9-10), matching is based on the pre-treatment values of the covariates used in the main specification (*Log Assets*, *Log Net Sales*, *Log Net Income*, *Leverage*, *ROA*, *Sales Growth*, *EBIT to Sales*, *PPE to Sales*, *Quick Ratio* and *Market Share*). In columns that match on pre-values (3-4, 7-8, 11-12), matching is based on the pre-treatment values of the outcome variable. Odd columns use the standard outcome estimator, while even columns use the DRIPW estimator. Variables are defined in [Appendix A](#). Significance levels are indicated by *, **, and *** for 10%, 5%, and 1% respectively.

	<i>Repurchase</i>				<i>Cash</i>				<i>Investment</i>			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
No Matching	0.2755*** (0.0151)				-0.7655 (0.4996)				1.4591*** (0.3151)			
Match on T-1 Values	0.2887*** (0.0197)	0.2826*** (0.0212)	0.2809*** (0.0153)	0.2809*** (0.0148)	-3.5061*** (0.8986)	-3.9356*** (0.9613)	-1.4138*** (0.5187)	-3.8494*** (1.0235)	1.6072*** (0.5264)	2.0326*** (0.6190)	1.4087*** (0.4398)	1.3815*** (0.4480)
Match on T-3 Values	0.2599*** (0.0192)	0.2646*** (0.0182)	0.2747*** (0.0141)	0.2727*** (0.0138)	-0.4779 (2.9487)	2.5037 (1.9944)	-1.6505** (0.6435)	-2.0216*** (0.7365)	1.3219** (0.5232)	0.9645** (0.4904)	1.9074*** (0.3677)	1.9424*** (0.3438)
Match on T-5 Values	0.2796*** (0.0173)	0.2602*** (0.0257)	0.2677*** (0.0134)	0.2663*** (0.0130)	-3.2995** (1.6278)	-4.0682* (2.2802)	-1.9619** (0.9877)	-5.7614*** (1.7205)	2.9520*** (0.8159)	1.5267** (0.7149)	1.7813*** (0.4364)	0.9745* (0.5495)
Match on 3 Year Avg.	0.2937*** (0.0220)	0.2923*** (0.0240)	0.2798*** (0.0144)	0.2757*** (0.0134)	-4.4421** (1.9756)	-7.1577*** (1.6352)	-2.0898*** (0.7582)	-3.4561*** (1.0680)	1.1631* (0.6609)	1.0008 (0.8008)	1.7609*** (0.3995)	1.7826*** (0.4001)
Match on 5 Year Avg.	0.2633*** (0.0283)	0.2548*** (0.0317)	0.2772*** (0.0134)	0.2767*** (0.0125)	-7.7692*** (2.6456)	-8.8367*** (2.5643)	-2.0494** (0.8945)	-2.1921** (1.0800)	2.1295*** (0.6457)	0.9221 (0.7201)	1.8139*** (0.4855)	1.6189*** (0.5290)
Matching On	Cov	Cov	Pre	Pre	Cov	Cov	Pre	Pre	Cov	Cov	Pre	Pre
Estimator	Outcome	DRIPW	Outcome	DRIPW	Outcome	DRIPW	Outcome	DRIPW	Outcome	DRIPW	Outcome	DRIPW

Table 6. Share Repurchase Legalization and Investment: Repurchasing and Non-Repurchasing Firms

[Table 6](#) reports the impact of repurchase legalization on firms' repurchases, cash balances and investments, split by whether the firm is a repurchasing firm. We use stacked, two-way fixed effects DiD regressions. Panel A reports the results for “non-repurchasing firms,” who are firms who do not repurchase within two years of legalization. Panel B reports the results for “repurchasing firms,” who are firms who repurchase within two years of legalization. For both panels, the dependent variable is *Repurchases* in columns 1-3, *Cash* in columns 4-6 and *Investment* in columns 7-9. For each dependent variable, the first column has no controls. The second column includes only observations that have non-missing controls. The third column includes control variables. Control variables are *Log Assets*, *Log Net Sales*, *Log Net Income*, *Leverage*, *ROA*, *Sales Growth*, *EBIT to Sales*, *PPE to Sales*, *Quick Ratio* and *Market Share*. Variables are defined in [Appendix A](#). Standard errors, clustered at the firm-stack levels are reported in parentheses. Significance levels are indicated by *, **, and *** for 10%, 5%, and 1% respectively.

Panel A: Non-Repurchasing Firms									
	<i>Repurchase</i>			<i>Cash</i>			<i>Investment</i>		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
<i>Legalization</i>	0.1355*** (0.0144)	0.1268*** (0.0152)	0.0996*** (0.0152)	-0.5644** (0.2808)	-0.2157 (0.2566)	-0.6361*** (0.2417)	0.9831*** (0.1311)	0.9020*** (0.1662)	0.7997*** (0.1642)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Controls	No	No	Yes	No	No	Yes	No	No	Yes
Clustering	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	565,424	317,492	317,492	531,591	317,489	317,489	448,659	263,796	263,796
Adj R^2	0.3033	0.1871	0.1899	0.6459	0.6843	0.7148	0.5126	0.5091	0.5153

Panel B: Repurchasing Firms									
	<i>Repurchase</i>			<i>Cash</i>			<i>Investment</i>		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
<i>Legalization</i>	0.5858*** (0.0509)	0.6276*** (0.0648)	0.6220*** (0.0686)	-2.2218*** (0.6273)	-2.3884*** (0.7463)	-3.2164*** (0.6264)	-0.5924* (0.3303)	-0.6611* (0.3760)	-0.7775** (0.3740)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Controls	No	No	Yes	No	No	Yes	No	No	Yes
Clustering	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	39,065	22,157	22,157	35,614	22,157	22,157	35,127	20,337	20,337
Adj R^2	0.2602	0.1577	0.1610	0.6763	0.7002	0.7382	0.5421	0.5305	0.5365

Table 7. Share Repurchase Legalization and Capital Structure

This table reports the impact of repurchase legalization on repurchasing firms' capital structure and issuance decisions. We use stacked, two-way fixed effects DiD regressions. The dependent variable is debt in columns 1-3, debt issuance in columns 4-6, equity in columns 7-9 and equity issuance in columns 10-12. All dependent variables are scaled by total assets and repurchases. For each dependent variable, the first column has no controls. The second column includes only observations that have non-missing controls. The third column includes control variables. Variables are defined in [Appendix A](#). Standard errors, clustered at the firm-stack levels are reported in parentheses. Significance levels are indicated by *, **, and *** for 10%, 5%, and 1% respectively.

	<i>Debt</i>			<i>Debt Issuance</i>			<i>Equity</i>			<i>Equity Issuance</i>		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
<i>Legalization</i>	-2.4046*** (0.3751)	-1.9523*** (0.3685)	-0.9284** (0.3818)	-0.5919*** (0.1434)	-1.1101*** (0.1652)	-0.8831*** (0.1632)	4.0665*** (1.0425)	1.5372*** (0.4294)	0.6045 (0.6885)	2.8134*** (0.4188)	3.6745*** (0.3888)	2.5295*** (0.3879)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Controls	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes
Clustering	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	597,731	351,676	351,676	635,703	411,080	411,080	604,290	351,775	351,775	338,686	219,958	219,958
Adj R^2	0.4963	0.6844	0.6965	0.1085	0.0964	0.1168	0.4630	0.5712	0.5798	0.3853	0.2108	0.2338

Table 8. Share Repurchase Legalization and Firm Performance

This table reports the impact of repurchase legalization on repurchasing firms' performance. We use stacked, two-way fixed effects DiD regressions. Panel A reports earnings and balance sheet performance (*ROA* in columns 1-3, *ROE* in columns 4-6 and *Sales Growth* in columns 7-9). Panel B reports market based performance (*Tobin's Q* in columns 1-3 and *Buy and Hold Return* in columns 4-6). For each dependent variable, the first column has no controls. The second column includes only observations that have non-missing controls. The third column includes control variables. From the list of variables in previous tables, we remove variables that are mechanically correlated with the dependent variables. Control variables are *Log Net Income*, *EBIT to Sales*, *PPE to Sales* and *Market Share*. Variables are defined in [Appendix A](#). Standard errors, clustered at the firm-stack levels are reported in parentheses. Significance levels are indicated by *, **, and *** for 10%, 5%, and 1% respectively.

Panel A: Operating Performance									
	<i>Return on Assets</i>			<i>Return on Equity</i>			<i>Sales Growth</i>		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
<i>Legalization</i>	3.0388*** (0.2322)	2.1567*** (0.1025)	2.2941*** (0.0909)	4.2607*** (0.4568)	4.5122*** (0.2892)	4.7635*** (0.2700)	8.8051*** (1.0334)	8.6245*** (0.9532)	6.9769*** (0.9737)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Controls	No	No	Yes	No	No	Yes	No	No	Yes
Clustering	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	732,394	468,554	468,554	730,198	467,855	467,855	642,818	415,658	415,658
Adj R^2	0.3239	0.5445	0.6857	0.0609	0.3566	0.4128	0.1144	0.1997	0.2148

Panel B: Market Valuation						
	<i>Tobin's Q</i>			<i>Buy and Hold Return</i>		
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Legalization</i>	1.1684 (1.9377)	3.7252* (1.9418)	5.6751*** (1.9360)	27.8817*** (1.4407)	28.8843*** (1.8009)	28.7881*** (1.8122)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm Controls	No	No	Yes	No	No	Yes
Clustering	Yes	Yes	Yes	Yes	Yes	Yes
Observations	614,804	393,031	393,031	496,033	315,661	315,661
Adj R^2	0.4480	0.5724	0.5777	0.1233	0.1889	0.1951

Table 9. Testing the Equity-Capital Access Hypothesis - Cross-Sectional Stacked Regressions

This table reports the impact of repurchase legalization on all firms' investment separately for countries with higher (odd columns) vs. lower (even columns) frictions to capital access. We use three different proxies. In columns 1-2, the odd (even) column includes high (low) capital control countries where there are more (fewer) restrictions to transfer capital out of the country. In columns 3-4, the comparable proxy is cash spread, based on the interquartile range of *Cash* to *Assets* ratio. This measure captures the degree of cash concentration among a subset of firms. A high cash spread has an above median cash interquartile range at the time of legalization. Columns 5-6 report by high and low market segmentation in the odd and even column, where a high (low) market segmentation indicates the country has less (more) competition. Columns 7-8 split countries by post-legalization buyback volume, where high (low) buyback volume countries are those with above (below) median repurchase activity in the two years following legalization. We report stacked, two-way fixed effect regression estimates in the first row, with standard errors clustered at the firm-stack level and reported in parentheses. In the second row, we report the difference in the investment effects between the high vs. low groups for each proxy, where standard errors are bootstrapped using 1,000 resamples. Variables are defined in [Appendix A](#). Significance levels are indicated by *, **, and *** for 10%, 5%, and 1% respectively.

	<i>Capital Controls</i>		<i>Cash Spread</i>		<i>Market Segmentation</i>		<i>Buyback Volume</i>	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>Legalization</i>	1.4468*** (0.1795)	0.3876** (0.1652)	0.9832*** (0.1549)	0.4523*** (0.1752)	0.9415*** (0.1791)	0.5729*** (0.1516)	1.3176*** (0.1691)	0.3914** (0.1751)
Higher - Lower	1.0591*** (0.2485)		0.5309** (0.2130)		0.3685* (0.2229)		0.9262*** (0.2318)	
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Controls	No	No	No	No	No	No	No	No
Clustering	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	222,518	391,212	250,520	360,230	278,412	330,328	325,673	288,057
Adj R^2	0.5306	0.5267	0.5169	0.5195	0.4901	0.5281	0.5490	0.5128

Online Appendix

“Does Share Repurchase Legalization Really Harm Corporate Investment?”

Elliot Tobin and Charles C.Y. Wang

In this Online Appendix, we report the results from our replication of WYY21’s main results on the effect of repurchase legalization on repurchasing firms’ investments. Following WYY21, we define “repurchasing firms” as those implementing a share repurchase within two years of the legalization date.

Our replication begins by using WYY21’s sample and applying their exact regression models.^a We then re-construct their dataset ourselves and estimate their regression models on this replicated sample.

0.1 Reconstructing the Main Sample

Constructing our own regression dataset serves two purposes. First, it validates our ability to replicate WYY21’s investment results. Second, because WYY21’s dataset is limited to firms that engaged in share repurchases within two years of legalization in each of the 17 jurisdictions studied, it cannot accommodate an analysis of effects beyond repurchasing firms. Our self-constructed dataset enables the more comprehensive examination of repurchase legalization effects across the entire population of public firms in these jurisdictions.

In creating our own dataset, we adhere to the sample restrictions used in WYY21. We obtain Worldscope data from 1980 to 2018 for the 17 countries that legalized repurchases between 1985 and 2010, as included in WYY21’s sample. These countries span from Canada, which legalized repurchases in 1985, to Kuwait, which did so in 2010. The sample excludes countries that legalized repurchases before 1985, such as the United States, due to insufficient pre-period data in Worldscope. Furthermore, some countries are omitted due to data quality issues identified in WYY21.

Our approach differs from WYY21 in the measurement of repurchases. While WYY21 uses SDC Platinum, we utilize Worldscope’s repurchase variable (ITEM04751). This difference in data sources may result in slight differences in the set of repurchasing firms and the repurchased amounts in our sample compared to WYY21.

Our replication focuses on four dependent variables: *Repurchase*, *Cash*, *CapEx*, and *R&D*. Following WYY21, we scale all dependent variables, except for *Repurchase*, by the sum of total assets and repurchases. *Repurchase* is scaled only by assets. When including

^aWe are grateful to WYY21’s authors for providing their final regression dataset and the code accompanying their main regression tables to the *Journal of Financial Economics*.

covariate controls, we employ the same set as WYY21, all sourced from Worldscope. Detailed descriptions of all variables are provided in [Appendix A](#) of the main paper.

0.2 Investment Effect on Repurchasing Firms: Replication of WYY21

Our empirical analysis begins by replicating the effect of repurchase legalization on repurchasing firms' investments.

0.2.1 Replication Sample Summary Statistics

[Table OA.1](#) reports the distributional summary statistics for the main dependent variables across three samples used for replicating WYY21's results. Panel A is based on WYY21's provided data file. Upon scrutiny, we noticed that WYY21's winsorization of *Cash* and *R&D* converted some observations to missing values. Specifically, the winsorized versions contained 250 and 343 fewer non-missing observations, respectively, compared to their unwinsorized counterparts.^b

To address this empirical irregularity, we created an alternative sample (Panel B) based on WYY21's data file, applying our own winsorization to the dependent variables at the top and bottom 1% of each country's pooled distribution. This approach recovers the missing observations and impacts the distributional statistics. Panel C reports the summary statistics for the main dependent variables based on our independently constructed replication sample, using the same winsorization approach as in Panel B.

Across the three samples, summary statistics are largely comparable. The most notable differences occur in *Repurchase* and observation counts between our independently constructed replication sample (Panel C) and the other samples (Panel A and B), attributable to our use of Worldscope data for measuring repurchases, in contrast to WYY21's use of SDC Platinum.

In all samples, *Repurchase* has a mean between 0.46% and 0.57% of assets, with a median of 0, reflecting the absence of repurchases in the pre-period and sparse repurchasing activity even among the selected repurchasing firms post-legalization. Cash holdings average about 15% of total assets, with the median between 9.5% to 10% across samples. Investment variables are consistent across samples, with *CapEx* averaging slightly above 5% of assets, and *R&D* averaging around 1.1% but heavily skewed upwards.

Overall, these summary statistics suggest that our replication samples closely mirror WYY21's original data, with some improvements in data completeness due to our winsorization approach.

^bWe contacted the authors and inquired about these observations. While they responded, they had not provided an explanation as of this writing.

0.2.2 Replication Results

Table OA.2 reports the replication results. We employ their basic TWFE regression specification:

$$Y_{ijt} = \beta_0 + \beta_1 \text{Legalization}_{jt} + \gamma'_k \mathbf{X}_{ijt} + FE_i + FE_t + \epsilon_{ijt}, \quad (2)$$

where for firm i in country j and year t , Y_{ijt} is the dependent variable of interest (e.g., *Repurchase*, *Cash*, *CapEx*, or *R&D*), $\mathbf{X}_{ijt}$ refers to the firm-level control variables, FE_i are firm-fixed effects, and FE_t are year-fixed effects.

Only firms that repurchases within two years of legalization are included in the WYY21 sample. The maintained identification assumption is that all of the sample’s firms have the same counterfactual intent to repurchase, but only the post-legalization firms can actually repurchase. Additionally, the treated and control firms must have the same counterfactual trends in *CapEx* and *R&D* had legalization never occurred.

Recent econometric literature has highlighted potential biases in TWFE DiD estimates when including time-varying covariates (e.g., Pearl, 2013; Imbens, 2020; Sant’Anna and Zhao, 2020). For example, the inclusion of “bad” controls can potentially induce the “collider” bias in regressions (Pearl, 2013; Imbens, 2020). In the DiD literature, Sant’Anna and Zhao (2020) points out that TWFE DiD regressions that include time-varying controls are valid estimators for the average treatment effect on the treated (ATT) only under several (and plausibly more stringent) assumptions in addition to the usual “parallel-trends” and “no-anticipation” assumptions. Following the recommendations of Baker et al. (2022), we present both covariate-free (odd columns) and covariate-inclusive (even columns) specifications in Table OA.2 to assess the impact of control variables on the effect estimates.

Panel A of Table OA.2 demonstrates our successful replication of WYY21’s main findings. We precisely match their results for the full covariate specifications (columns 2 in their Table 4, and columns 4, 6, and 8 in their Table 5).^c While including controls doesn’t substantially alter statistical inferences, it does reduce the sample size by approximately 40%.

In Panel B, we re-estimate the same specifications using WYY21’s sample but apply our standard winsorization approach. Notably, this recovers about 300 R&D observations, which proves sufficient to render the previously negative and statistically significant effect on R&D both economically and statistically insignificant.

To be sure, we again estimate the same regression specifications but using our independently constructed sample. The results, reported in Panel C, are virtually identical to those of Panel B. In other words, we are able to reproduce WYY21’s results on *Repurchase*, *Cash*, and *CapEx* using our independently collected sample and applying a standard winsorization approach. However, unlike WYY21 and similar to the results reported in Panel B, we fail

^cWe focus on replicating WYY21’s “long” regression specifications with the full set of covariates. We are also able to replicate their “short” specifications with fewer controls.

to obtain an economically and statistically significant result for $R\&D$.

Panel C reports results using our independently constructed sample, applying the same winsorization approach as in Panel B. These results closely mirror those in Panel B. We successfully reproduce WYY21’s findings for *Repurchase*, *Cash*, and *CapEx*. However, consistent with Panel B and contrary to WYY21, we find no economically or statistically significant effect on $R\&D$.

The main takeaway from this replication exercise is that our independently constructed sample proves to be clean and successfully reproduces WYY21’s main results when we normalize winsorization methods.

0.2.3 Stacked Regression Results

Although WYY’s original TWFE results on R&D appeared sensitive to winsorization, we note that the negative investment effects among repurchasing firms persist in stacked regression estimation (see, e.g., Table 5 of the main paper). [Table OA.3](#) reports the stacked regression results for *CAPEX* and $R\&D$ separately.

We find that repurchase legalization has a strong positive effect on repurchases, with coefficients ranging from 0.59 to 0.63, all statistically significant at the 1% level. This confirms that the policy change indeed led to increased share repurchases. The legalization led to a significant decrease in cash holdings, with coefficients ranging from -2.22 to -3.22, all significant at the 1% level. This aligns with the expectation that firms reduce cash reserves when engaging in increased payouts. These results are largely consistent with WYY21’s findings and the results in [Table OA.2](#).

However, we fail to find a statistically significant effect on capital expenditures. The coefficients range from -0.44 to -0.57, about a half to a third of the magnitudes reported in WYY21, but are not statistically significant at the 10% level. This result contrasts with WYY21’s findings.

For research and development, we observe negative coefficients ranging from -0.14 to -0.22. In the specification without controls (column 10), the coefficient is least negative (-0.14) and not statistically significant at the 10% level. When we estimate the regression on a sample with non-missing firm-level controls (column 11), reducing the sample by 43%, the effect estimates are substantially larger (-0.20) and statistically significant at the 5% level. Including the control variables in the stacked regression further increases the estimated effect (-0.22), which remains statistically significant at the 5% level.

These results in columns 11 and 12 of [Table OA.3](#) suggest two important insights. First, the impact of share repurchase legalization on $R\&D$ depends on sample selection. Second, to the extent that legalizing share repurchases hampers investments in $R\&D$, such an effect is more likely to be concentrated in a specific subset of repurchasing firms. Specifically, we find these firms with non-missing values in all the firm-level controls tend to be larger, older, and less profitable. Thus, mature firms with potentially fewer growth opportunities may be

more likely to reduce *R&D* spending in favor of repurchases. This heterogeneity in effects underscores the importance of considering different firm types when evaluating the impact of repurchase legalization, motivating the analysis of all public firms in the main paper.

Table OA.1. Summary Statistics for Repurchasing Firm Analysis

This table reports summary statistics for firm-year observations in the repurchase legalization analysis: *Repurchases*, *Cash*, *Capital Expenditures* and *R&D*. The last three variables are scaled by assets plus repurchases, while *Repurchases* is scaled by assets. Panel A reports summary statistics of the data received by WYY’s authors directly, including their winsorized variables. Panel B uses WYY’s raw data, but uses our winsorization method. Panel C uses our data and winsorization method. “Our winsorization method” winsorizes the top and bottom 1% of each dependent variable within each country. This study’s authors have been unable to determine what the “WYY winsorization method” is using any consistent rule. [Appendix A](#) defines the variables.

<i>Variable</i>	<i>N</i>	<i>Mean</i>	<i>SD</i>	<i>p10</i>	<i>p25</i>	<i>p50</i>	<i>p75</i>	<i>p90</i>
<i>Panel A: WYY Sample with WYY Winsorization</i>								
<i>Repurchase</i>	16,107	0.50	1.35	0.00	0.00	0.00	0.16	1.53
<i>Cash</i>	17,326	14.79	16.23	0.96	3.59	9.56	19.98	34.90
<i>CapEx</i>	18,198	5.09	5.72	0.15	1.04	3.44	7.09	11.92
<i>R&D</i>	18,679	1.06	2.80	0.00	0.00	0.00	0.61	3.31
<i>Panel B: WYY Sample with Standard Winsorization</i>								
<i>Repurchase</i>	16,107	0.57	1.88	0.00	0.00	0.00	0.16	1.53
<i>Cash</i>	17,576	14.81	15.97	0.98	3.61	9.61	20.14	35.27
<i>CapEx</i>	18,198	5.02	5.39	0.15	1.04	3.44	7.09	11.92
<i>R&D</i>	19,022	1.11	3.26	0.00	0.00	0.00	0.65	3.40
<i>Panel C: TW Sample with Standard Winsorization</i>								
<i>Repurchase</i>	17,728	0.46	1.45	0.00	0.00	0.00	0.09	1.35
<i>Cash</i>	16,379	15.18	16.31	1.08	3.87	9.95	20.54	35.62
<i>CapEx</i>	17,081	5.10	5.65	0.15	1.06	3.47	7.15	11.98
<i>R&D</i>	17,728	1.14	3.51	0.00	0.00	0.00	0.75	3.52

Table OA.2. Share Legalization and Repurchasing Firm Investments: Re-Examining WYY21's Pooled TWFE Estimates

This table reports estimates of two-way fixed effects difference-in-difference regressions of the impact of repurchase legalization on a firm's repurchases, cash and investments. In Panel A, we use the winsorized data received directly from the authors of WYY. In Panel B, we also use data received by WYY; however, we winsorize the data ourselves. In Panel C, we use our data. The dependent variable is *Repurchases* in columns 1-2, *Cash* in columns 3-4, *CapEx* in columns 5-6, *R&D Expenditures* in columns 7-8 and *R&D Expenditures*. Within each dependent variable, the first column has no controls. The second column has the same controls as the most restricted regression in WYY; namely, the controls are *Log Assets*, *Log Net Sales*, *Log Net Income*, *Leverage*, *ROA*, *Sales Growth*, *EBIT to Sales*, *PPE to Sales*, *Quick Ratio* and *Market Share*. Variables are defined in [Appendix A](#) of the main paper. Standard errors, clustered at the firm level, are reported in parentheses. Significance levels are indicated by *, **, and *** for 10%, 5%, and 1% respectively.

	<i>Repurchase</i>		<i>Cash</i>		<i>CapEx</i>		<i>R&D</i>	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>Panel A: WYY Sample with WYY Winsorization</i>								
<i>Legalization</i>	0.3127*** (0.064)	0.4926*** (0.074)	-3.9612*** (0.582)	-1.9280*** (0.433)	-1.1963*** (0.207)	-0.8469*** (0.277)	-0.1454** (0.068)	-0.2357*** (0.075)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Controls	No	Yes	No	Yes	No	Yes	No	Yes
Clustering	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	16,093	10,023	17,323	11,353	18,196	11,290	18,678	11,260
Adj R^2	0.1631	0.1791	0.6028	0.7707	0.4608	0.4847	0.7522	0.8317
<i>Panel B: WYY Sample with TW Winsorization</i>								
<i>Legalization</i>	0.3919*** (0.086)	0.5878*** (0.106)	-3.3855*** (0.566)	-1.3047** (0.533)	-1.0948*** (0.196)	-0.7810*** (0.265)	-0.0048 (0.070)	0.0889 (0.078)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Controls	No	Yes	No	Yes	No	Yes	No	Yes
Clustering	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	16,093	10,023	17,573	11,604	18,196	11,290	19,021	11,604
Adj R^2	0.1652	0.1668	0.6112	0.7004	0.4906	0.5035	0.7524	0.8039
<i>Panel C: TW Sample with TW Winsorization</i>								
<i>Legalization</i>	0.3796*** (0.060)	0.5454*** (0.075)	-3.6395*** (0.611)	-1.1107** (0.532)	-1.1117*** (0.217)	-0.7507** (0.293)	-0.0081 (0.065)	0.0564 (0.082)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Controls	No	Yes	No	Yes	No	Yes	No	Yes
Clustering	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	17,728	10,942	16,379	10,942	17,081	10,681	17,728	10,942
Adj R^2	0.1510	0.1677	0.6033	0.7053	0.4807	0.4944	0.7451	0.7982

Table OA.3. Share Repurchase Legalization and Repurchasing Firm Investments: Stacked Regression

This table reports the impact of repurchase legalization on repurchasing firms' payout policies, cash and investments. The dependent variable is *Repurchases* in columns 1-3, *Cash* in columns 4-6, *CapEx* in columns 7-9, and *R&D* in columns 10-12. We report estimates of stacked, two-way fixed effects difference-in-difference regressions. Data used was collected by this paper's authors. For each dependent variable, the first column has no controls. The second column has no controls, but restricts the sample to the same as the third column, so that any firm-year observations with missing values for any of the controls are excluded. We include the same controls as the most restricted regression in WYY, namely, *Log Assets*, *Log Net Sales*, *Log Net Income*, *Leverage*, *ROA*, *Sales Growth*, *EBIT to Sales*, *PPE to Sales*, *Quick Ratio* and *Market Share*. Variables are defined in [Appendix A](#) of the main paper. Standard errors, clustered at the firm-stack levels, are reported in parentheses. Significance levels are indicated by *, **, and *** for 10%, 5%, and 1% respectively.

	<i>Repurchase</i>			<i>Cash</i>			<i>CapEx</i>			<i>R&D</i>		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
<i>Legalization</i>	0.5858*** (0.051)	0.6276*** (0.065)	0.6220*** (0.069)	-2.2218*** (0.627)	-2.3884*** (0.746)	-3.2164*** (0.626)	-0.4460 (0.300)	-0.4444 (0.363)	-0.5703 (0.361)	-0.1356 (0.104)	-0.2008** (0.093)	-0.2222** (0.097)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Controls	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes
Clustering	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	39,065	22,157	22,157	35,614	22,157	22,157	35,127	20,337	20,337	39,065	22,157	22,157
Adj R^2	0.2602	0.1577	0.1610	0.6763	0.7002	0.7382	0.4943	0.5037	0.5105	0.7413	0.7849	0.7875

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