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Roni Michaely and Jillian Popadak and Christopher Vincent

Duke University, Cornell University, IDC

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# The Deleveraging of U.S. Firms and Institutional Investors' Role \*

RONI MICHAELY  
Cornell University and IDC

JILLIAN POPADAK  
Duke University

CHRISTOPHER VINCENT  
Securities and Exchange Commission

## ABSTRACT

Corporate leverage has decreased markedly in the U.S. since 1992. In contrast to press coverage of hedge funds increasing debt, increases in institutional investments, primarily by mutual funds, account for part of this deleveraging. We use implied mutual fund trades constructed from individual-investor flows as exogenous variation in institutional ownership for estimation. Supporting the hypothesis institutions contributed to deleveraging, our estimates increase significantly after regulatory reforms incentivized stronger institutional governance. Firms deleverage by reducing debt and transitioning to debt associated with enhanced monitoring and efficiency. Counterfactual simulations indicate aggregate leverage would have been eight percentage points higher without institutions' influence.

**JEL classification:** G3, G32, G31, C23, E44

**Keywords:** Finance, Financial Stability, Corporate Leverage, Institutional Investors, Mutual Funds, Hedge Funds, Corporate Governance, Agency Costs, Capital Structure, Debt Structure

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\*Corresponding author: Popadak, Fuqua School of Business, Duke University (e-mail: jillian.popadak@duke.edu). Co-authors: Michaely, Johnson School of Management, Cornell University and the IDC (e-mail: rm34@cornell.edu); Vincent, Securities and Exchange Commission (email: vincentc@sec.gov). We greatly appreciate the comments of Viral Acharya, Robert Bartlett, Philip Bond, Alon Brav, Jonathan Cohn, Julian Franks, Itay Goldstein, John Graham, Owen Lamont, Michelle Lowry, Manju Puri, and Jamie Zender as well as audience feedback at NBER Summer Institute, WFA, SunTrust Finance Conference, and JLFA Conference. The Securities and Exchange Commission disclaims responsibility for any private publication or statement by any of its employees. This study expresses the authors' views and does not necessarily reflect those of the Commission, the Commissioners or other members of the staff.

# 1 Introduction

Economists have long called attention to the dangers of excessive corporate leverage because of the critical role the financial stability of the U.S. corporate sector plays in the persistence and initiation of business cycles (Bernanke and Gertler (1989)). The aggregate leverage of nonfinancial U.S. firms rose from 11% to 47% by the early 1990s, but in stark contrast, leverage sharply declined since then. What changes in financial market conditions or practices brought about this deleveraging trend? Leading explanations such as changes in tax rates, bankruptcy costs, and leveraged buyout rates cannot fully account for the deleveraging (Graham, Leary, and Roberts (2014)). In this paper, we show institutional investors' evolving role in firm governance explains an economically meaningful portion of individual firm capital structure changes as well as the aggregate deleveraging trend.

While the rising power of institutional investors is well-documented (e.g., Aghion, Van Reenen, and Zingales (2013)), the notion that they use their power to reduce leverage seems surprising at first. Savvy institutional investors such as hedge funds and private equity funds are often glamorized in the business press for exerting their influence over management to increase corporate debt levels and thereby leverage their returns. Yet in contrast to hedge funds and private equity firms' modus operandi with respect to debt, the leverage of non-financial U.S. firms has steadily declined for the last two decades. To reconcile this contradiction, we study a broader set of institutional investors and find increases in ownership by other institutional investors such as mutual funds and pension funds account for a significant portion of the decline in leverage over time.

To understand how institutional ownership and leverage vary on average within firms, we analyze institutional investors' effect on leverage. To empirically establish credible point estimates, we must overcome the challenge that neither leverage nor institutional ownership is randomly assigned.<sup>1</sup> To overcome this bias and other empirical challenges, we employ an instrumental variable strategy that uses *implied* mutual fund trades generated from idiosyncratic, individual-investor flows as

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<sup>1</sup>As an example, suppose a firm's latent investment opportunities improved. If institutional investors predicted such an improvement which led them to increase their holdings in that firm, and subsequently, the firm issued equity to fund the latent investment opportunities, we would observe both a decrease in leverage and an increase in institutional holdings. Yet in this hypothetical setting, any claims that increases in institutional ownership lead to decreases in leverage would be spurious, because the observed negative interrelation is through latent investment opportunities rather than through a direct effect.

an instrument for institutional holdings. Using the disclosed stock holdings of mutual funds and individual-investors' flows to and from the mutual funds, we construct implied annual mutual fund trades for all equities. Implied mutual fund trades represent a relevant instrument for institutional holdings because individual-investor flows lead mutual funds to buy or sell stock holdings on behalf of the individuals (Coval and Stafford (2007); Edmans, Goldstein, and Jiang (2012)).

Variation attributable to the portion of institutions' trades driven by the idiosyncratic part of individual-investors flows such as liquidity needs would likely satisfy the exclusion restriction because those trades do not have any direct or indirect relationship with management's capital structure decisions. Because not all individual-investor flows are for idiosyncratic reasons, we use a two-step procedure to produce our instrument. First, we remove individual-investors' flows associated with chasing returns and portfolio-management skills (Chevalier and Ellison (1997); Berk and Green (2004)) and only use the residual portion of individual-investors flows to project the implied changes in mutual fund equity holdings. Second, we assume that mutual fund managers only change their current year stock holdings by an amount proportional to the idiosyncratic individual-investor flows. The key logic underlying the instrument is that while actual mutual fund trades based on individual-investor flows may be "selected" and thereby not satisfy the exclusion restriction, by projecting proportionate (and thereby non-selected) trades from the idiosyncratic portion of individual-investor flows, indirect correlations with other determinants of firm's leverage are plausibly eliminated.

Our analyses provide evidence for four new stylized facts. First, we find strong statistical support for a negative relationship between leverage and institutional holdings. Our evidence from the instrumental variable strategy suggests a one standard deviation increase in institutional ownership is associated with a 0.72 standard deviation decrease in leverage. This suggests a 10 percentage point increase in institutional ownership leads to a 4.9 percentage point reduction in leverage from an average leverage of 23.1%. This marginal effect on leverage is significantly larger than that of other firm characteristics, such as profitability, tangibility, and the market-to-book ratio but smaller than the marginal effect attributable to firm size.

Second, we relate our evidence examining the interrelation between institutional ownership

and leverage to the sharp decline in aggregate leverage that began in 1992. Applying the same instrumental variable strategy to different time periods and testing for structural breaks, we find strong support for a strengthening of the negative interrelation following regulatory reforms enacted between 1989 and 1992. We find the average treatment effect of institutional holdings on leverage is much greater after the reforms than before. Residual variance tests reveal the post-reform influence of institutional investors explains 11% of the deleveraging since 1992. Importantly, the results are not driven by highly levered firms because quantile instrumental variable tests indicate institutional ownership affects firms across the leverage distribution. To understand what would have happened had the cross-section of firms not been affected by the reforms, we simulate each firm's leverage evolution had the reforms not occurred and find that aggregate leverage would have been eight percentage points higher by 2013.

Third, we examine why institutional ownership and leverage developed a negative interrelation after regulatory reforms incentivized stronger institutional governance. We find increases in institutional ownership enabled firms to use less debt because firms could maintain operational efficiency at lower levels of debt through an alternative debt structure. Evidence from three empirical tests are consistent with this mechanism. First, we rule out an information mechanism. Because we find firms deleverage by reducing debt and transitioning to debt associated with enhanced monitoring and efficiency, we conclude institutions are not reducing informational frictions associated with equity issuance or credible credit worthiness. Second, we provide supporting evidence for an agency mechanism. Using multiple definitions of firms prone to high agency costs, we consistently find institutional ownership leads to more deleveraging in these firms. Third, using comprehensive data on bank loans, we show deleveraging firms also see their loan covenants tighten. Improvements in operational outcomes and equity valuation are achieved through tighter bank contracts (Nini, Smith, Sufi (2012)). Combining this set of tests with evidence from the prior literature suggests institutions fulfil their mandated governance role and materially benefit from facilitating firms to attain greater operational efficiency at lower levels of debt via this alternative debt structure.

Fourth, we compare the approach taken by mutual funds with respect to leverage to that of hedge funds. Using data on hedge fund activism, we find hedge fund activism leads to an increase

in leverage after hedge funds acquire 5% of a firm's equity. When comparing economic magnitudes, the deleveraging influence of mutual funds dominates any influence by hedge funds for managers' to lever up. Even when mutual funds can free-ride off of the hedge funds' tactics with respect to leverage, we find the economic magnitude of such actions are only a small fraction of the size of the institutional investors' tactic of using their influence to encourage deleveraging.

To place our results in the context of other findings, we investigate several alternative explanations for our findings. We show our results are robust to controls for the composition of firms and in particular, the arrival of new tech firms in the 1990s, variations in supply and demand for corporate debt, and booms and busts in the leveraged buyout (LBO) market. We study alternative mechanisms such as crowding out of activist investors by index mutual funds and institutional investment strategies related to valuation and beta anomalies. Such mechanisms do not explain our results. We consider the magnitude of reverse causality in explaining the deleveraging by estimating the extent of institutional sorting on leverage using a semi-natural experiment involving the introduction of credit default swaps (CDS) as a natural shock to leverage clienteles. Our evidence from the difference-in-differences estimation strategy involving CDS suggests leverage clienteles exist, but the economic magnitude is so small that it cannot contribute to the deleveraging trend.

In conclusion, our study relates to and contributes to several strands of literature. Our paper shows the interaction between institutional holdings and firm policy extends to capital structure and plays a meaningful role in explaining the deleveraging of U.S. firms since the 1990s. While no prior literature has not focused on the relationship between capital structure and institutional investors in the U.S., our findings complement a broader literature considering the legal, financial, and institutional systems across the world, and their effect on capital structure decisions (Demirgüç-Kunt and Maksimovic (1998)). Our findings also support the growing literature showing that institutions affect corporate decisions. Specifically, institutional investors play a role in executive turnover (Huson, Parrino, and Starks (2001)), payout policy (Grinstein and Michaely (2005)), and innovation (Aghion, Van Reenen, and Zingales (2013)).

In addition, our paper speaks to capital structure research by Lemmon, Roberts, and Zender (2008), who find traditional determinants of leverage account for relatively little of the cross-

sectional variation in capital structure, and [Graham, Leary, and Roberts \(2014\)](#), who find these determinants cannot account for trends in the aggregate leverage. Our paper provides an economic interpretation of these puzzles. Institutional investors and the SEC reforms that consolidated institutions' power generated structural breaks in the relative importance of leverage determinants. In this sense, our research also informs the broader macroeconomic literature that studies the role of the corporate sectors' financial policy, and in particular asks if corporate leverage is excessive, for economic and financial stability (e.g., [Bernanke and Gertler \(1990\)](#); [Gertler and Hubbard \(1993\)](#)).

Finally, our study informs corporate governance research. Our analysis of why and how the deleveraging occurs augments recent research on interactions between shareholder and creditor governance ([Ivashina et al. \(2008\)](#); [Nini, Smith, Sufi \(2012\)](#)). Our evidence that hedge funds differ from mutual funds in their tactics with respect to leverage and minimizing agency costs supports research suggesting corporate governance is not one-size fits all but a nuanced trade-off between short and long-term value creation ([Coles, Daniel, and Naveen \(2008\)](#); [Popadak \(2014\)](#)).

## 2 Data and Summary Statistics

Our data are drawn from the annual Compustat-CRSP database over the period extending from 1980 through 2013. We start the sample period in 1980 to coincide with the availability of institutional holdings data from SEC 13F filings. We define institutional holdings as the percentage of shares outstanding held by institutional investment managers and the source for this data is SEC 13F filings. Institutional investment managers that exercise investment discretion over \$100 million or more are required to file 13F filings, which detail all equity holdings of more than \$200,000 or 10,000 shares. An institutional investment manager is an entity that either invests in, or buys and sells, securities for its own account. For example, mutual funds, insurance companies, pension funds, banks, and other broker/dealers are institutional investment managers must file form 13F. We exclude banks (Standard Industrial Classification (SIC) codes 6000 – 6999) and utilities (SIC codes 4900 – 4999) to avoid capital structures governed by regulation. In line with previous capital structure studies, we winsorize the upper and lower 1% of each variable used in the analysis to mitigate the impact of data errors and outliers. Further, we require a firm to be in the sample for

at least two years. The final sample consists of 106,171 firm-year observations, with non-missing data for all of the variables used in our analysis. All variables are formally defined in [Appendix A](#).

Institutional shareholder power evolved considerably beginning in the late 1980s ([Gillan and Starks \(2007\)](#)). The first key event in the new era of institutional shareholder power is the issuance of the Avon Letter in 1988. The letter put private pension plan trustees on notice that proxy voting rights must be a diligently exercised aspect of fiduciary duty. Additional posturing by federal regulators and later the SEC meant that institutional investors could no longer rubber stamp the passage of management-supported proposals; as a consequence, CalPERS and other prominent pension funds began to submit proxy proposals and actively critique managerial practices. The second key event in the new era of institutional shareholder power occurred when the SEC formalized this evolution toward stronger institutional activism by reforming shareholder proxy rules. The new SEC rules substantially reduced the barriers to communication between institutional investors and managers, which subsequently allowed institutional investors to express their views on proxy solicitations, organize public campaigns to pressure management, and to mount pressure against members of the board of directors.

A key part of our analyses is to distinguish the changing role of institutional investors over these decades in order to understand the interrelation between leverage and institutional holdings. [Figure 1](#) plots our two primary variables of interest – corporate leverage and institutional holdings – over time. First, we use aggregate leverage data compiled by [Graham, Leary, and Roberts \(2014\)](#) from 1920 through 2010. The upper plot shows aggregate leverage increased in the post-war period, peaked in 1992, and subsequently decreased. Second, in the lower plot, we use Compustat data matched to SEC 13F filings of institutional equity ownership to highlight the years around the 1992 peak. Comparing the leverage trend to the institutional holdings trends in that time period begins to suggest a pattern. There appears to be either positive correlation or little correlation between leverage and institutional holdings in the period prior to the onset of institutional activism in the late 1980s, but then, there is a sharp drop in leverage when institutional activism began, which is highlighted in the area with gray shading. Finally, the negative correlation between leverage and institutional ownership persists throughout the decades after the onset of increased shareholder



power. While the figure provides suggestive evidence that the correlations shift over time, we rely on our regression analyses when making inferences.

Next, we move from plots of levels to first-differences, because our statistical analyses focus on within-firm variation over time. [Figure 2](#) plots the annual within-firm variation in our two primary variables of interest – book leverage and institutional holdings. The left-hand side plot shows the 25th, 50th, and 75th percentile for annual within-firm changes in leverage over time. The right-hand plot shows 25th, 50th, and 75th percentile for annual within-firm changes in institutional ownership over time. What is striking is how much first-difference variation there is in these variables over time. On average, 35% of firms have a greater than 5 percentage point swing in leverage in a given year while 37% of firms have a greater than 5 percentage point swing in institutional ownership. While the median firm persists with a negligible change in leverage and institutional holdings over time, examining the full distribution of first-differences reveals substantial variation in the upper and lower quartiles. Like the main findings in [DeAngelo and Roll \(2015\)](#), our figures suggest capital structure instability is prevalent. To see that such instability also extends to the percentage of shares held by institutions is suggestive of a potentially important interrelation.

[Table I](#) complements the visual evidence from [Figure 1](#) and [Figure 2](#) by presenting summary statistics on a range of key variables for the entire sample period as well as for the pre- and post-SEC reform periods. Like the observed aggregate trends, the simple mean of the firm-specific leverage and institutional holdings measures also change substantially over time. Average holdings doubled from the first time period to the second time period (21.2% to 43.9%). The holdings of the largest institutional investors in an equity also increased sharply (12.3% to 21.9%). The increases in holdings were not limited to only the largest institutional investors as evidenced by the tripling of the average number of institutional investors from 36 to 90. Another measure that captures this balance across sizes of institutional investors is the ownership concentration Herfindahl-Hirschman Index (HHI), which takes into account the relative size distribution of the investors in a given equity. The data show the HHI decreased from 0.312 to 0.196. The remaining summary statistics reveal other important determinants of leverage such as profitability, firm size, payout and sales growth are consistent with prior studies (e.g., [Lemmon, Roberts, and Zender \(2008\)](#)).

Finally, to characterize the correlations between institutional holdings and leverage, [Table II](#) presents the partial correlations from simple multivariate regressions, where we control for other firm characteristics that affect leverage and are commonly used in the capital structure literature: firm size, tangibility, intangibility, collateral, profitability, losses, dividend paying firm, payout, lifecycle stage, market-to-book, investment, sales growth, asset growth, firm risk, market risk, adjusted firm returns, Amihud’s illiquidity measure, Altman’s Z-score, and insider ownership. To account for the strong trends, we also include firm and industry-by-year fixed effects. Panel A of [Table II](#) focuses on the partial correlations for book leverage, Panel B focuses on the partial correlations for leverage including leases, and Panel C for leverage in terms of book value of capital ([Welch \(2011\)](#)), where cash on the balance sheet is deducted from the value of debt. In each case, the partial correlation between leverage and institutional holding is strong and negative. The 95% confidence interval for the mean of the partial correlation between leverage and institutional holdings is -0.15 to -0.19. The negative partial correlation between leverage and institutional holdings also extends across alternative definitions of institutional holdings. For example, we find negative partial correlations of -0.038 or -0.067 when institutional holdings is redefined as the percentage of shares held by the 5 or 10 largest institutional positions in that equity, respectively.

### 3 Identification Strategies and Empirical Results

In this section, we use an instrumental variable design to statistically test if the negative relationship between institutional holdings and leverage documented in our summary statistics is more than a correlation. Our research design and empirical findings for institutional influence are presented in [Section 3.1](#). while [Section 3.2](#). describes a set of empirical tests and findings that examine how time-varying institutional influence contributes to the corporate deleveraging trend that began in 1992. The evolution of shareholder power over the last three decades suggests institutional investors’ influence strengthened significantly over this time period, so understanding the extent to which this phenomenon statistically affects corporate leverage enlightens our understanding of firm financial policy as well as economic and regulatory policy aimed at the stability of the corporate sector.

## 3.1 The Effect of Institutional Holdings on Leverage

**3.1.1 Identification Strategy.** The summary statistics show institutional holdings and leverage are negatively correlated, yet theory is ambiguous in its predictions of the dominant direction for the relationship. On one hand, institutional holdings and debt can have a negative interrelation. If organizational inefficiencies must be controlled, threatening to sell shares (Admati and Pfleiderer (2009)) or fighting management through a proxy battle (Gillan and Starks (2000)) may be as effective at reducing agency costs as committing managers to pledge funds to creditors (Jensen (1986)). Similarly, because institutions gather information and make trades based on their findings, they may reduce informational frictions associated with equity issuance (Greenwood, Hanson, and Stein (2010)). On the other hand, institutional holdings and leverage can have a positive interrelation. If institutional investors enable outside shareholders to implement devices such as debt that limit management discretion (La Porta et al. (2000)), leverage will rise when institutional ownership increases.

To estimate if changes in institutional ownership affect corporate leverage, we use implied mutual fund trades induced by idiosyncratic individual-investor flows as an instrument for institutional holdings. The idea follows from work by Coval and Stafford (2007) and Edmans, Goldstein, and Jiang (2012), who document that individual-investor outflows lead mutual funds to sell a portion of their holdings to repay these investors. To understand the logic for why our instrument, *implied* mutual fund trades, generates plausibly exogenous variation in institutional holdings, it is important to understand why the necessary exclusion restriction for using *actual* individual-investor induced trades as an instrument is not satisfied. Such a discussion should help to clarify the rationale behind the construction of our instrument as well.

The critical argument for satisfying the exclusion restriction when using actual trades is individual-investors' decisions to trade mutual fund shares are not directly or indirectly correlated with the leverage of the firms held by the mutual funds. The direct exclusion is likely satisfied, because an investor who wishes to speculate on an individual firm's capital structure, or relatedly the firm's potential bankruptcy, will trade the stock of the firm rather than a mutual fund share that only exposes the investor to a small fraction of the firm. In contrast, the indirect exclusion may not be

satisfied, because individual-investors' trades may be related to the leverage of the firms held by the mutual funds through another factor unobservable to the econometrician. For example, if a mutual fund manager is trading in response to the individual-investors' flows because the manager has private information about one of his portfolio firm's imminent capital structure change, or if a firm's management takes into consideration mutual fund share turnover to time the market with equity issuances or repurchases, then actual trades would be indirectly correlated with leverage.

Therefore, instead of using actual trades, we use mutual funds' hypothetical (or implied) trades induced by individual-investor flows. [Figure 3](#) helps to explain the construction of our instrument and how such construction plausibly eliminates the previous two arguments for indirect correlation that invalidated actual trades as an instrument. The data used to construct the instrument includes: mutual fund trades, mutual fund stock holdings, stock returns, mutual fund assets under management, and individual-investor flows. In our first step, we cleanse the individual-investor flows of variation attributable to these investors' chasing managerial skill and reputation ([Berk and Green \(2004\)](#)) as well as chasing fund performance ([Chevalier and Ellison \(1997\)](#)). Specifically, we regress quarterly individual-investor flows on a flexible form of mutual fund returns and mutual fund fixed effects. We label the residual from these regressions as the idiosyncratic individual-investor flow because they are meant to represent variation in individual-investors' flows attributable to phenomenon such as liquidity needs that have no direct or indirect relationship with corporate leverage. In our second step, we use the actual stock holdings of the mutual fund in a given quarter to project proportionate changes in each of the fund's stock holdings in the next quarter based on the magnitude of the idiosyncratic individual-investor inflows/outflows in a given quarter. The final step aggregates all of our projected changes in stock holdings to an individual stock-year frequency.

By using implied mutual fund trades as an instrument, we eliminate the previous two arguments for indirect correlation that invalidated actual flows as an instrument (i.e., when mutual funds traded based on leverage and when managers timed mutual fund trades with equity issuances or repurchases). Another potential concern alleviated by using implied trades as an instrument is correlation with recent mutual fund performance. This concern is addressed by Step 2 in [Figure 3](#) where we remove flows from chasing mutual fund returns. A final potential concern lessened with

the construction of our instrument is correlation with unobservable market conditions. Because we project trades one period in the future, if markets exhibit informational efficiency on an annual basis, which seems likely given evidence for informational efficiency at much shorter frequencies (e.g., see [Ljungqvist and Qian \(2014\)](#)), then implied trades are not correlated with the future information that moves markets. This means even when an individual-investor’s flow contains some information or sentiment ([Frazzini and Lamont \(2008\)](#)), the constructed instrument does not. The notion that an instrument based on individual-investor flows is uncorrelated with future market conditions is further supported by empirical evidence suggesting individual-investors’ trades fail to systematically predict the market ([Barber and Odean \(2000\)](#)). Taken together, the logic presented thus far suggests implied mutual fund trades based on the idiosyncratic portion of individual-investor inflows/outflows plausibly should have no relationship to executives’ capital structure choices.

We believe our construction of mutual funds’ implied trades and the aforementioned economic arguments suggest our instrument creates plausibly exogenous variation in institutional holdings in the following regression specification:

$$Leverage_{ijt} = \alpha + \beta Holdings_{ijt} + \Gamma X_{ijt} + f_i + \delta_{jt} + \epsilon_{ijt} \quad (1)$$

where observations are at the firm-year level,  $Leverage_{ijt}$  represents the book leverage ratio for firm  $i$  in industry  $j$  in year  $t$ ,  $Holdings_{ijt}$  captures the percentage of shares outstanding held by institutional investors,  $X_{ijt}$  is a vector of observable firm-specific covariates,  $f_i$  is a firm fixed-effect,  $\delta_{jt}$  is an industry-by-year fixed-effect, and  $\epsilon_{ijt}$  is the unobservable error component. By including firm and industry-by-year fixed-effects, we are controlling for unobservables such as firm-specific production capabilities and industry-year-specific sentiment that otherwise may bias our estimates.

**3.1.2 Empirical Results.** [Table III](#) presents our test of the effect of institutional holdings on leverage. We find institutional holdings are a significant determinant of firms’ capital structures; Column (1) shows a one standard deviation change in institutional holdings leads to a -0.72 standard deviation change in leverage, on average. The estimate is significant at the 1% level. Comparisons across the standardized coefficients reveal that this marginal effect on capital structure is signifi-

cantly larger than that of other firm characteristics, such as profitability, payout, or investment, while the marginal effect is smaller than that of firm size. These marginal effects translate into economically meaningful effects as well. The point estimate suggests an average leverage decrease of 0.49 percentage points for every percentage point increase in institutional ownership, all else equal.

Additional tests displayed in [Table III](#) indicate we should have confidence in our inferences. The test statistic on the instrument from the first stage regression is statistically significant at the 1% level and the F-statistic from the first stage regression is 231.9. Both of these measures are well above recommended weak instrument thresholds and suggest the instrument is relevant. Column (2) and Column (3) of [Table III](#) show that the primary finding is robust to alternative definitions of leverage such as including off-balance-sheet items as well as adjusting leverage for cash. Further, these findings are robust to a myriad of controls, which include firm size, tangibility, intangibility, collateral, profitability, losses, dividend payments, repurchases, lifecycle stage, market-to-book, investment, sales growth, asset growth, firm risk, market risk, adjusted firm returns, Amihud's illiquidity measure, Altman's Z-score, and insider ownership.

Alternative specifications that account for other known leverage determinants such as analyst coverage, credit ratings, external-finance-weighted growth options ([Baker and Wurgler \(2002\)](#)), R&D-intensity, tax sensitivity, and geographic concentration do not qualitatively change our findings. Limiting the sample to firms with longer performance histories such as at least ten or at least twenty years of financial data does not change our results. Similarly, variations in the definition of institutional ownership such as using only the holdings by the 5 institutions with the largest ownership stakes do not change our conclusions. Alternative constructions of the instrument such as using a parametric functional form for mutual fund returns or focusing on idiosyncratic individual-investor inflows rather than both inflows and outflows do not qualitatively change our conclusions. We consider an alternative timing that allows for institutional investors impact on leverage to occur in the year after they acquire the shares, and we continue to find a statistically significant negative interrelation. [Appendix Table BI](#) shows the minor variations in point estimates from twenty-one different robustness checks of this sort.

The economic magnitude of the instrumented point estimate seems sensible. The point estimate indicates a 0.71 standard deviation decrease in leverage, for a one standard deviation increase in institutional ownership. The magnitude of this change is consequential economically. For example, it suggests a 10 percentage point increase in institutional ownership leads to a 4.9 percentage point reduction in leverage from an average leverage of 23.1%. While the average firm-year observation in our sample experiences only small positive changes in institutional ownership year-over-year, the average change in institutional ownership for the 40% of observations in the upper and lower quintiles is a 10 percentage point change in institutional ownership. [Appendix Table BII](#) repeats the instrumental variable regression analyses for firms-year observations according to the quantile of the size of change in institutional ownership. Consistent with intuition, the firms experiencing large changes in institutional ownership in a given year are driving the instrumental variable point estimate for the full sample. For the two quintiles where the average change in institutional ownership is less than 1.1%, the point estimate is statistically indistinguishable from zero but for the quintiles where the point estimate is larger than 3.3% , the point estimates are statistically significant.

As a final check of the robustness of our identification strategy, we implement a placebo test that checks for spurious correlation. Although the exclusion restriction cannot be tested in a strict statistical sense, our placebo test helps to assuage concerns about information inefficiency and/or unobservable firm performance persistency driving our results. Specifically, we run a placebo test, in which we project changes in mutual funds' stock holdings back in time rather than forward. Intuitively, if our instrument was spuriously correlated with some form of persistency, then, using the subsequent portfolios should produce similar point estimates. Our results from the placebo test suggest our instrument is valid. When examining changes in leverage that pre-date the placebo instrument, we find no effect. The null result from the placebo test implies our construction of implied mutual trades extracts the desired idiosyncratic variation stemming from individual-investor flows and hence, that our instrument is likely uncorrelated with the error term in our main specification. [Appendix Table BIII](#) shows the results from the placebo test analysis.

Taken together, all of these tests indicate our instrumental variable analyses are internally valid,

but it is worthwhile to discuss if our analyses are potentially externally valid. The explanatory power of the first stage is high as evidenced by the  $R^2$  of 50% in [Table III](#) and the statistical significance of the instrument in predicting institutional holdings. Because the instrument is broadly predictive, it appears reasonable to conclude the estimates are not stemming from a special subsample of firms. Furthermore, because individual-investors' flows occur at various points in time across various mutual funds, it appears reasonable to conclude the estimates are not driven by a unique year or fund. Such arguments suggest our results generalize, and therefore, are likely to be externally valid. It is important, however, to recognize that the point estimate and inferences provide guidance about an average effect over the sample period which spans from 1980 through 2013. Changing the time period may alter the average effect. In the next subsection, we examine the extent to which the evolution of institutional shareholder power may generate heterogeneity in the effect over time.

### **3.2 The Effect of Institutional Holdings on Aggregate Leverage Trends**

Evidence from the previous subsection demonstrates increases in institutional ownership lead to decreases in leverage, on average within a firm over time. While this finding informs our understanding of the interrelation between institutional investors and leverage, additional analyses are necessary to learn about the dynamics of the interrelation over time and its contribution to trends in aggregate leverage. Knowing these details are important for understanding economic and regulatory policy. For example, if the SEC's regulatory reforms that strengthened the potential for institutional activism represent a regime shift where activism substitutes for debt, this externality should also be taken into consideration in debates about the costs and benefits of regulating institutional activists.

In this section, we use the within-firm evidence to shed light on the observed trends in aggregate leverage. Specifically, we examine if changes in institutional shareholder activism explain part of the heterogeneity in the effect of institutional ownership on leverage over time. Recall from [Figure 1](#) that the leverage of unregulated U.S. firms exhibit an inverted-V shape. Leverage steadily increased until peaking in 1992, and subsequently, it sharply decreased.

We test whether the unprecedented rise in institutional ownership, and importantly, the power



of institutions to influence management, can account for a significant part of the abrupt decline in aggregate leverage. While institutional ownership began its ascent before 1992, the role for institutions in influencing managerial decisions was limited until that time period. Beginning in the late 1980s, institutional shareholder power evolved considerably and two key events helped to strengthen the potential role for institutions to influence managerial decisions (Gillan and Starks (2007)). The first key event is the issuance of the Avon Letter that occurred in 1988 and the second key event is the change in SEC regulations governing the proxy process that occurred in 1992.

To test the hypothesis that the increased power of institutional shareholders explains part of the downtrend in aggregate leverage, we implement an extension of the instrumental variable identification strategy described above. If the two key events that strengthened institutions' power are influencing the aggregate leverage trend, we would expect to see stronger treatment effects after the events. We split the full sample period which spans from 1980 through 2013 into three subsamples, 1980 through 1989, 1989 through 1992, and 1992 through 2013. We estimate Eq. (1) for each subsample. This subsample estimation produces valid estimates of the conditional effects of the other variables at these different values but cannot in itself show the time periods are different. To test if the time periods are different, we perform Chow's test, which determines whether the coefficients estimated over one group of the data are equal to the coefficients estimated over another.

Table IV provides evidence to suggest the strengthening of shareholder power accounts for a substantial portion of the heterogeneity in the effect of institutional holdings on leverage. Table IV shows that in the time period prior to the key events, the effect of institutional holdings on leverage is statistically indistinguishable from 0, yet in the time periods when the potential for institutional shareholder activism is stronger, the point estimates are larger and statistically significant. After the second key event – the SEC reform in 1992 – a standard deviation increase in institutional holdings leads to 1.15 standard deviation decrease in leverage. During the time period between the first and second key event (1989 through 1992), the coefficient estimate is statistically insignificant but this is the first time that the point estimate shifts from a positive to a negative sign. That the post-reform era point estimate is so much stronger than the pre-period is striking. Importantly,

the Chow test further confirms that the coefficients estimated over the two groups are not equal to one another. This suggests while there may have been some gray area between 1989 and 1992 in terms of institutions' full impact, by 1992 the consolidation of institutional power appears to be fully felt.

Because [Table IV](#) suggests there is significant heterogeneity in the effect of institutional ownership on leverage within a firm over time, our next aim is to quantify the economic significance of this heterogeneity and relate it to the macroeconomic trends. We do this by performing two tests. First, we generate a counterfactual aggregate leverage simulation and second, we analyze the full distribution of starting leverages to understand where institutions exert the most influence.

[Figure 4](#) plots our counterfactual simulation as well as actual aggregate leverage from 1980 through 2013. To generate this figure, we simulate leverage evolutions for each individual firm and then, we sum across all of the individual firm simulations to create an aggregate counterfactual series. We make the following assumptions when creating the counterfactual simulations. First, we assume the strengthening of institutional power did not occur, so the marginal effect of institutional holdings is set to what it was prior to 1992. Second, we assume the average marginal effects for other determinants of leverage such as firm size, profitability, and growth options were unaffected by the change in strength of institutional power. This second assumption of no spillovers seems reasonable because the coefficients did not change substantially on the additional covariates across the subsamples. And third, we assume firms exhibit the same degree of unexplained idiosyncratic behavior over the period from 1992 to 2013 that we actually observe in the data. This assumption means we use the actual firm-year specific residuals in our simulations.

The results from the simulation exercise depicted in [Figure 4](#) suggest the aggregate leverage of unregulated U.S. firms would have been eight percentage points higher by 2013 without the influence of institutions on managements' capital structure decisions. The actual aggregate leverage in 2013 for unregulated U.S. firms is 21% whereas in the counterfactual simulation aggregate leverage is 29%. Our simulations suggest leverage would have continued on an upward trend but with a smaller slope had it not been for the evolution of activism. Our examination of the residual unexplained variance in leverage after 1992 echoes the counterfactual simulation. It suggests that the influence

of institutional investors accounts for 11% of the residual variation for the time period after 1992. One way to interpret this result is that institutional power is an important determinant of the deleveraging trend but other factors such as taxes may be an even more important determinant.

Finally, to test the hypothesis that institutional holdings are only important for leverage when leverage is in the high range of the distribution, we explore the entire distribution of effects over time using an instrumental variables quantile regression framework. This design is a methodological improvement over studies that employ traditional quantile regressions, because when the identifying assumptions hold, inferences are more credible. For the inferences from the quantile instrumental variable regression to be credible, an additional rank similarity assumption is necessary. The rank similarity condition means that each firm’s rank in the conditional outcome distribution is invariant in expectation, regardless of the status of the change in institutional holdings. There are no formal tests available to validate an assumption of rank similarity, but controlling for observable covariates helps to achieve rank similarity. The quantile regression specification is the same as in Equation (1) but varies across quantiles,  $\tau$ .

$$Q(\text{Leverage}_{ijt}|\tau) = \alpha(\tau) + \beta(\tau)\text{Holdings}_{ijt} + \gamma(\tau)Z_{ijt} + f_i(\tau) + \delta_{jt}(\tau) + \epsilon_{ijt}(\tau) \quad (2)$$

Observations are at the firm-year level.  $\text{Leverage}_{ijt}$  represents the leverage ratio for firm  $i$  in industry  $j$  in year  $t$ ,  $\text{Holdings}_{ijt}$  captures the percentage of shares outstanding held by institutional investors,  $Z_{ijt}$  is a vector of observable firm-specific covariates,  $f_i$  is a firm fixed-effect,  $\delta_{jt}$  is an industry-by-time fixed-effect, and  $\epsilon_{ijt}$  is the unobservable error component. The vector of observable covariates is the same as in previous specifications and includes firm size, tangibility, intangibility, collateral, profitability, losses, dividend paying firm, payout, lifecycle stage, market-to-book, investment, sales growth, asset growth, firm risk, market risk, adjusted firm returns, Amihud’s illiquidity measure, Altman’s Z-score, and insider ownership.

**Table V** presents the quantile instrumental variable estimates for the effect of institutional holdings on leverage. Columns (1) and (2) of Panel A show the quantile and the associated coefficient estimate for  $\beta(\tau)$ . Column (3) shows the leverage associated with quantile  $\tau$  while Column (4) shows our inference for the new leverage after a 10 percentage point increase in leverage, all else

equal, respectively. The instrumental variables quantile regression estimates suggest the importance of institutional investors varies over the leverage distribution. Consider the 33rd percentile, which represents a book leverage of only 9.4%; for this quantile, a standard deviation increase in institutional ownership is associated with a statistically significant 0.511 standard deviation decrease in leverage. In contrast, for the 66th percentile, which represents a book leverage of 29.2%, a standard deviation increase in institutional ownership is associated with a statistically significant 0.595 standard deviation decrease in leverage. The 10th and 25th percentiles, which encompass leverage levels below 5% show no significance, yet high leverage levels show an event more meaningful change brought about by institutions. The 95th percentile is more than double the estimate at the median.

## 4 The Role of Institutions in Affecting Leverage Change

The empirical evidence from the previous sections indicates institutional ownership and corporate leverage have a negative interrelation that became economically meaningful after a change in regulatory conditions incentivized institutional investors to monitor firms more. But why would increases in institutional ownership lead to a decrease in leverage? The popular press often details how hedge funds and private equity funds increase their ownership stakes in firms to influence firm executives to increase debt levels. By increasing debt levels, executives are thought to improve operational efficiency while hedge funds and private equity funds lever their returns. Theoretically, the hedge funds use their power to facilitate additional debt that disciplines managers' use of free cash flow (La Porta et al. (2000)). Yet we observe the exact opposite effect empirically, so what mechanism could produce the opposite effect and which institutional tactic dominates that of hedge funds or mutual funds?

While it is very difficult to tease out a precise mechanism, in this section, we present evidence from three empirical tests that begin to explain how and why the deleveraging occurs. First, we examine the composition of the firms' deleveraging. Is the deleveraging coming from changes in debt or equity? If it is coming from debt, what kind of debt? Second, we examine the characteristics of the firms where the deleveraging effect is strongest. Do these firms display a high potential for

agency conflicts or informational frictions? Third, we examine the relationship between institutional ownership and creditor governance. Is the debt that remains in the firm’s capital structure after deleveraging the type of debt that benefits institutional investors? Finally, we use our empirical setting to understand which institutional approach dominates – that of hedge funds or mutual funds.

The empirical evidence is consistent with a nuanced agency mechanism. In particular, we find increases in institutional ownership enabled firms to use less debt because the firms could maintain operational efficiency at lower levels of debt through an alternative debt structure. The rationale supporting this finding is as follows. Institutions want managers to reduce arm’s-length, public debt and transition toward monitoring-intensive, bank debt. By concentrating the debt structure in bank debt, which leads to improved operational performance and equity valuation when agency conflicts arise (Roberts and Sufi (2009); Nini, Smith, Sufi (2009); Nini, Smith, Sufi (2012)), the institutional investors are satisfying their mandated fiduciary duties and positioning their portfolios for superior returns.

Examples of mutual funds following such a tactic can be found on their websites as well as in publicly disclosed court documents. Specifically, Fidelity, one of the largest mutual fund providers, offers two “Leveraged Company Stock Funds” to their retail clients. Fidelity markets these funds in its’ highest risk category, suggesting that institutional investors do not view increased leverage in as favorable terms as hedge funds do. Similarly, while it is hard to know what happens at behind-the-scenes, one-on-one meetings between institutional investors and management, documents uncovered through the legal discovery process shed some light. For example, records from a court case involving HCA reveal its expert financial advisor told the CFO when he was contemplating increasing his firm’s leverage that such actions were not warranted. HCA’s financial advisor argued with greater leverage, HCA’s valuation as a public company would be pressured due to HCA’s long-term institutional investors’ aversion to the increased risk inherent in a levered situation (Merrill Lynch (2006)).

While our evidence on the mechanism is mostly suggestive, it points to a direction for future research – to understand the interaction between shareholder and creditor governance and its

implications for economic outcomes. The details of our empirical specifications and findings are presented in [Section 4.1](#), which examines the exact debt and equity instruments used to accomplish the deleveraging, in [Section 4.2](#), which examines firm characteristics associated with deleveraging, in [Section 4.3](#), which illustrates the relationship between institutional influence and creditor governance, and in [Section 4.4](#) which compares hedge fund and mutual funds' tactics.

## 4.1 Composition of the Deleveraging

In this subsection, we investigate the nature of the deleveraging by examining how firms change their capital structure in response to an increase in institutional ownership. First, we want to determine if the deleveraging occurs through changes in debt, equity, or both. Second, we seek to understand if the deleveraging involves changes in the types of creditors and/or debt instruments used by the borrowers. Examining the composition of the deleveraging in such ways helps to shed light on the potential for an agency cost mechanism or an informational frictions mechanism.

The theoretical literature predicts that when informational frictions are alleviated firms are more likely to choose seasoned equity or public debt issuances as sources of external financing. Because managers know more about the true condition of the firm, when informational frictions are high investors believe that managers are taking advantage of over-valuation when issuing equity. As a result, investors discount the new equity issuance, so if institutional investors through their trades and dissemination of information can alleviate such frictions, the empirical prediction is more equity issuance and lower leverage ([Myers and Majluf \(1984\)](#)). A key trade-off highlighted by the theoretical literature on debt structure is the choice of arm's-length public debt vs. closely-monitored bank debt ([Diamond \(1991\)](#)). If institutional investors through their increased power post reform alleviate reputational frictions the firm faces for other creditors, the empirical prediction is a movement from bank debt to public debt ([Rajan \(1992\)](#)). In contrast, if institutional investors' role as active monitors led firms to specialize in the type of debt shown to enhance operational efficiency, then, the empirical prediction is a movement from arm's-length debt in favor of monitoring-intensive bank debt ([Nini, Smith, Sufi \(2012\)](#)).

To study the average change in the underlying debt and equity structure, we use an instrumental

variable framework similar to what we used for leverage, but we replaces leverage with the specific debt or equity component:

$$LeverageComponent_{ijt} = \alpha + \beta Holdings_{ijt} + \Gamma X_{ijt} + f_i + \delta_{jt} + \epsilon_{ijt} \quad (3)$$

where observations are at the firm-year level,  $LeverageComponent_{ijt}$  represents a component of the leverage ratio for firm  $i$  in industry  $j$  in year  $t$ ,  $Holdings_{ijt}$  captures the percentage of shares outstanding held by institutional investors,  $Z_{ijt}$  is a vector of observable firm-specific covariates,  $f_i$  is a firm fixed-effect,  $\delta_{jt}$  is an industry-by-year fixed-effect, and  $\epsilon_{ijt}$  is the unobservable error component.

To examine debt and equity changes, we define debt and equity issuance in accordance with [Lemmon, Roberts, and Zender \(2008\)](#). Equity issuance is the product of the split-adjusted change in shares outstanding and the split-adjusted average stock price, normalized by total assets in the previous year. Consequentially, a firm is an equity issuer if  $\Delta Equity > 1\%$ , and an equity repurchaser if  $\Delta Equity < -1\%$ . Debt issuance is defined similarly and includes long-term plus short-term debt, normalized by total assets in the previous year. This procedure adjusts for new issuances that result from rolled-over debt. This procedure also distinguishes between equity issuance related to the exercise of employee stock options and actual seasoned equity offerings.

**Table VII** Panel A provides estimates of the composition of the debt vs. equity change. The main finding is that the deleveraging primarily occurs through debt changes rather than equity changes. Specifically, we find a statistically significant but economically small reduction in equity repurchases, and no meaningful change in equity issuance. In contrast, we find firms change their debt in an economically meaningful way across the board. We find statistically significant reductions in long-term debt, reductions in new debt issuance, and reductions in debt through early retirement. In each case, our estimate is significant at the 1% level. For example, the -0.62 for debt issuances is interpreted to mean a one standard deviation change in institutional holdings leads to a -0.62 standard deviation change in new debt issuances, on average. The additional statistics reported in the table suggest we should be confident in these inferences. The  $t$ -statistic on the instrument from the first stage regression is statistically significant at the 1% level and the  $F$ -statistic from the

first stage regression is 43.3. Both of these measures are well above recommended weak instrument thresholds and suggest the instrument is relevant. In addition, these findings are robust to a myriad of controls, which include firm size, tangibility, intangibility, collateral, profitability, losses, dividend payments, repurchases, lifecycle stage, market-to-book, investment, sales growth, asset growth, firm risk, market risk, adjusted firm returns, Amihud's illiquidity measure, Altman's Z-score, and insider ownership.

The finding that firms deleverage primarily through decreased debt begins to rule out an information asymmetry mechanism driving the negative interrelation between leverage and institutional ownership, but further testing is needed. If, however, the firm's debt is more concentrated in arm's length-debt that would also be consistent with an asymmetric information story assuming the institutions are reducing frictions associated with the firms' reputations (Rajan (1992)). As such, we examine the details of the debt composition. Firm-level debt structure variables are gathered from Capital IQ, which pulls their data from a variety of sources including firms' 10-Ks, 10-Qs, Annual Reports, 424 and S-3 prospectuses. Capital IQ decomposes total debt into mutual exclusive debt types, which we aggregate into five categories. Our categories include commercial paper, term loans, bonds and notes, capital leases, and other borrowings. Appendix A details the exact formulas we use to classify the debt types. For example, drawn credit lines are a part of the other borrowing category.

Table VII Panel B provides estimates of the composition of the debt change. We find institutional investors exert their influence through reductions in bonds, notes, and commercial paper. Capital leases, term loans, and other borrowings, which include drawn credit lines, do not meaningfully change. Because heavily-monitored bank debt is not reduced while arm's length debt is reduced, we can rule out an asymmetric information mechanism. As Rajan (1992) demonstrates, an asymmetric information mechanism predicts that institutions ability to convey information about the firm's credit worthiness leads to more arm's-length debt, and we do not find this. Overall, the specific debt components suggest a mechanism that would require more diligent monitoring facilitated by bank debt such as an agency mechanism. Any agency mechanism, however, does not appear to be as straightforward as institutional investors' role as active monitors directly substitut-



ing for the disciplinary role of debt. Instead, the concentration of debt among bank debt suggests the potential for an agency mechanism. As such, the next two subsections probe this hypothesis.

## 4.2 Potential for Agency Conflicts

In this subsection, we analyze firm characteristics that capture heterogeneity in potential agency costs within our sample of firms over time. Potential agency costs associated with excessive managerial risk aversion and career concerns may be exacerbated by high leverage. In these cases, institutional ownership coupled with a credible threat of intervention may be as or more effective than committing managers to pledge funds to creditors as a way of promoting efficiency and financially sound investments and acquisitions.

To measure agency costs, we consider several definitions. First, we define a high agency cost firm as a large market capitalization firm with few growth opportunities. Our second definition of a high agency cost firm examines low sales growth but high SG&A expense firms. Empirically this measure is a new empirical measure, but the intuition comes from the accounting literature that demonstrates SG&A expenses increase more rapidly when demand increases than they decline when demand decreases. This definition captures the idea that economically inefficient managers are likely to increase SG&A costs too rapidly by adding employees and awarding bonuses too quickly when sales go up and to decrease SG&A costs too slowly by delaying deductions to payroll, travel, and entertainment expenses when sales go down. Our third definition of high agency cost firms looks at acquisitions, which evidence indicates may be a way managers grow out of trouble and protect their careers ([Gormley and Matsa \(2011\)](#)). We define high agency cost firms as those with two consecutive years of non-core acquisition expenses, which may approximate these undesirable managerial tendencies.

To test if such an agency mechanism underlies the negative relation between institutional ownership and leverage, we extend the instrumental variable framework to include interactions with various firm and investor characteristics. In particular, we focus on the extent to which the negative relationship is driven by the mechanisms most affected by the SEC reforms such as institutional

activism and monitoring. The exact specification is as follows:

$$Leverage_{ijt} = \beta_1 Char_{ijt} \times Holdings_{ijt} + \beta_2 Char_{ijt} + \beta_3 Holdings_{ijt} + \Gamma X_{ijt} + f_i + \delta_{jt} + \epsilon_{ijt} \quad (4)$$

where the specification is the same as Eq. (1) except for the inclusion of  $Char_{ijt}$ , which is a dummy variable indicating a firm displays the high potential for agency conflicts characteristic, and  $Char_{ijt} \times Holdings_{ijt}$ , which is the associated interaction term. When present, the controls and fixed effects are interacted with the  $Char_{ijt}$  as well. Because  $Char_{ijt} \times Holdings_{ijt}$  is endogenous, it is instrumented for with our original instrument, mutual funds' implied trades, interacted with  $Char_{ijt}$ . Because of the interaction terms, the regression coefficients no longer indicate the change in mean response with a unit increase in the covariates, all else equal. Instead, the mean response is dependent on the given level of the characteristic under study.

Columns (1) through (3) of [Table VII](#) reveal the extent to which an agency mechanism drives the substitution between leverage and institutional ownership. Across each definition of agency costs, the effect of institutional ownership on leverage is stronger economically in high agency cost firms. To put the statistics in perspective, recall the average point estimate across all firms suggests a 10 percentage point increase in institutional ownership leads to a 4.9 percentage point reduction in leverage, all else equal. For high agency cost firms, a 10 percentage point increase in institutional ownership reduces leverage between 5.0 and 5.7 percentage points. Because the importance of agency costs remains robust across the three alternative definitions, each of which captures a nuance of managerial potential for agency conflicts, we are confident our conclusion that an agency mechanism is economically meaningful explanation for the substitution is not fragile to a single definitional assumption. The remaining statistics presented in the columns further support that our inferences are valid. The statistics reveal both instruments are relevant, as evidenced by a high first-stage  $t$ -statistics and non-trivial adjusted  $R^2$  statistics; the instruments are statistically strong as evidenced by the first-stage  $F$ -statistics greater than 10.

### 4.3 The Relationship between Institutional Ownership and Creditor Governance

While the previous two subsections provide evidence consistent with the deleveraging occurring more in firms with high potential for agency conflicts and primarily through a reduction in arm's length-debt in favor of bank debt, to rationalize this tactic, we would hope to observe active monitoring by banks in the firms with increased institutional ownership. From prior research, we know active monitoring by banks is associated with improved equity valuations and greater operational efficiency (Nini, Smith, Sufi (2012)). We test the hypothesis that bank creditors are engaging in greater governance, even when firms are deleveraging, by analyzing the covenants banks place in their contracts after a firm experiences an increase in institutional ownership.

Specifically, we use Thomson Reuter's Dealscan data to examine the contractual use of net worth covenants, financial covenants, and total covenants used in bank loans and other creditor facilities. Our primary specification is the same instrumental variable approach used throughout the paper where institutional ownership is instrumented for using implied mutual fund trades induced by idiosyncratic individual-investor flows. The only difference is that covenant usage rather than leverage is the dependent variable in the specification.

Columns (2) through (4) of Table VIII reveal the extent to which covenant usage changes following an increase in institutional ownership. Across the board, the effect of increases institutional ownership is statistically significant increases in covenant usage. For example, a one standard deviation increase in institutional ownership leads to a 0.44 standard deviation increase in total covenants. While this means a firm would need at least a 10 percentage point increase in institutional ownership for a new covenant to be added given the variation in the data; the economic magnitude also seems plausible given that the firms with 10 percentage point changes in institutional ownership are the ones experiencing the greatest deleveraging and likely renegotiating the terms of their debt. This is important because it seems unreasonable to expect banks to write stronger contracts in response to institutional investors rather what seems plausible is that institutions through their influence to encourage both the use of bank debt and the acceptance of stronger contractual terms.

The remaining statistics presented in the columns of Table VIII provide evidence that the inferences about covenant usage are valid. The statistics suggest the instrument is relevant, as evidenced by a high first-stage  $t$ -statistics and non-trivial adjusted  $R^2$  statistics; similarly, the instruments are statistically strong as evidenced by the first-stage  $F$ -statistics greater than 10. Finally, the results are robust to limiting the sample to only firms in the Thompson Reuters' Dealscan database. The limited sample results are reported in Panel B and remain statistically similar to those for the whole sample. An additional take-away from Panel B is that the Dealscan set of firms are similar to the whole sample of firms as Column (1) reveals the point estimate for the change in leverage is within the 95% confidence interval of that for the full sample.

#### 4.4 Comparing Hedge Fund and Other Institutional Investors' Tactics

The empirical evidence presented in the previous sections suggests increased institutional ownership, primarily by mutual funds and pension funds, leads to a decrease in corporate leverage through a reduction in arm's-length debt and that institutions, motivated by SEC mandates, are influencing leverage decisions in order to reduce potential agency conflicts and enhance equity valuations. In contrast to the evidence presented, prior research on hedge fund activism suggests hedge funds follow the exact opposite tactic with respect to corporate leverage (Brav et al. (2008)). In this section, we compare hedge funds tactics with that of other institutional investors, primarily mutual funds, to understand which institutional tactic dominates.

To understand the interaction between hedge fund and mutual fund tactics, we use a sample of 1,139 hedge fund activism events. We examine the firm-year where a hedge fund first files a 13D form. A 13D form indicates a hedge fund increased its size to exceed the 5% disclosure threshold, but also that the fund intends to use the stake to intervene in the way the target firm is run (since the fund filed a 13D and not a 13G form). The 13D filings data are borrowed from Brav et al. (2008). We also examine the course of a hedge fund activism campaign using the date the hedge fund sells its equity stake in the firm. Our primary specification is the same instrumental variable approach used throughout the paper where institutional ownership is instrumented for using implied mutual fund trades induced by idiosyncratic individual-investor flows. The main

addition to the specification is to include the hedge fund activism event as well as an interaction term with institutional holdings.

**Table IX** Panel A provides empirical evidence that hedge funds follow an alternative tactic with regards to leverage than that of our institutional investors. We find a statistically significant positive interrelation between hedge fund activism and corporate leverage. The inclusion of hedge fund activism events in our analyses does not meaningfully change our point estimates for the institutional investors influence over leverage. On average, a one standard deviation increase in institutional ownership still leads to a 0.72 standard deviation decrease in leverage while the effect of a hedge fund activism event is much smaller in economic magnitude.

Examining the interaction between hedge fund tactics and that of other institutional investors, we find some other institutional investors appear to free-ride off the hedge funds' strategy, but the magnitude of this interaction suggests free-riding is 1/10th of the size of the deleveraging tactic. In combination, these results suggest that hedge fund activism may attenuate the influence of other institutional investors at times but the strategy of encouraging greater deleveraging dominates for most institutional investors. Panel B of Table IX shows that these results are robust to alternative sample definitions as well as an alternative definition of hedge fund activism.

## 5 Discussion

In the previous sections, we provide evidence suggesting a causal relationship between institutional holdings and leverage contributed to the corporate deleveraging trend in the U.S. that began in 1992. We further show our instrumental variable estimation approach is robust to alternative statistical specifications and placebo tests. In this section, we discuss our results in the context of contemporaneous trends and potential explanations for the deleveraging. First, we examine and show our results are not attributable to the composition of firms and in particular, the arrival of new tech firms in the 1990s, to booms or busts in the LBO market, and to variations in supply and demand for corporate debt. Second, we consider alternative mechanisms for our findings such as crowding out of activist investors by index mutual funds and institutional investment strategies related to valuation and beta anomalies. We find they cannot fully explain our findings. Third, we

examine the extent to which institutional sorting contributes to an interrelation between leverage and institutional ownership, and we find it cannot account for the deleveraging.

There are large variations in the number of publicly listed firms because of waves of new IPOs during some periods such as in the dot-com boom and waves of de-listing in some other periods such as during LBO booms and stock market crashes. As [Fama and French \(2001\)](#) point out, the large variation over time in the number of firms raises important issues for the study of firm financial policy. For example, newly listed firms have lower profitability but more growth opportunities which may affect aggregate leverage trends. If the composition of firms or a change in firm characteristics were driving our results, we would expect our estimated effect to change when using a constant number of firms sample or when using only firms with many years of financial data. Panel E of [Appendix Table BI](#) shows that our results do not change when limiting our analysis to only firms that were alive in our sample for more than 10 or 20 years or when using a constant sample size. This suggests the arrival of new tech firms in the 1990s and booms and busts in the LBO market are not driving our results.

The deleveraging trend associated with institutional ownership may be the artefact of concurrent variation in *supply* of other debt such as government bonds or mortgage debt leading to the fluctuations observed over time. As [Graham, Leary, and Roberts \(2014\)](#) document, U.S. federal government debt issuance affects corporate financial policies through an impact on investors' portfolio allocations, and government debt is negatively correlated with corporate debt. We test whether this is a contemporaneous effect or if it is directly related to the effect we find via institutional owners' portfolio choice. We would not expect a direct effect because our instrument isolates idiosyncratic variation in individual-investors flows to institutions. Nevertheless, to test this alternative, we follow [Graham, Leary, and Roberts \(2014\)](#) and limit our sample to years when government debt issuance is low. Consistent with our baseline instrumental variable results, we find when the potential for government debt crowding out corporate debt is at its lowest, greater institutional ownership statistically significant decreases leverage. This result is contained in Panel E of [Appendix Table BI](#). This suggests a government debt crowding out effect is independent of the effect we isolate.

The deleveraging trend associated with institutional ownership may be the artefact of simultaneous variation in *demand* for corporate debt through a risk-preference or debt-vs-equity channel. As Baker and Wurgler (2002) illustrate, when investor demand for corporate debt is low, a catering hypothesis suggests managers will cater to investors demand and issue more equity. Because we find firms deleverage by reducing debt rather than issuing equity, this suggests the interrelation we observe is distinct from a catering hypothesis. Consistent with this distinction, when we include in our analysis the measure of investor-demand-induced variation in external financing costs defined by Baker and Wurgler (2002), our point estimates remain similar and statistically significant. This result is contained in Panel A of [Appendix Table BI](#).

An alternative hypothesis for the mechanism driving the negative interrelation between institutional ownership and leverage is that the influx of passive institutional investors is crowding out activist hedge fund investors that prefer firms take on higher leverage. The evidence, however, does not support this mechanism. First, the timing is off. Our results suggest the deleveraging trend did not begin until 1992 when SEC regulatory reforms were enacted that incentivized greater institutional governance, yet Vanguard launched its first index mutual fund in 1976. Further, there were no noteworthy changes in assets pledged to index funds in the early 1990s. Second, the hypothesis suggests management should have more slack today than in prior decades. In contrast, research on CEO tenure and style suggest managerial slack is less today than in prior decades (Bertrand and Schoar (2003); Kaplan and Minton (2006)). Third, following the procedure outlined by Cremers and Petajisto (2009), we identify that our effect still holds when examining stocks with large shares of active mutual fund investors. These results are reported in Panel E of [Appendix Table BI](#). This suggests our findings are not a story of passive investors crowding out smart hedge funds.

A second alternative hypothesis for the mechanism driving the negative interrelation between institutional ownership and leverage is institutional investment strategies related to valuation and beta anomalies. Because mutual fund investors tend to chase returns over time and across funds, fund managers are thought to care more about outperforming during expansionary business cycles than underperforming during recessionary periods. This response will increase demand for high-beta stocks and reduce their required returns. As a test for high-beta effects, we limit our sample

to recessionary market years. If a high-beta effect were driving our results, we would not expect to observe the negative interrelation in bear market years, yet as reported in Panel E of [Appendix Table BI](#) we still find a significant negative interrelation during bear market years. As a test for valuation effects, we consider an alternative construction of our instrument that excludes individual-investor redemptions. If large outflows by individual investors that shifted stock prices away from fundamental value were driving our estimates, then relying on only individual-investor inflows would eliminate this confounding dynamic. Yet, as is reported in Panel E of [Appendix Table BI](#), our results remain statistically significant and similar. Hence, these tests suggest our findings are not driven by valuation or beta anomalies.

While our instrumental variable approach mitigates endogeneity attributable to reverse causality, one may still be interested in knowing the extent to which reverse causality could contribute to the deleveraging trend. To test the reverse causality argument and in particular if institutional shareholders vary in their preferences for firms with high or low leverage, we analyze the response of institutional investors to a natural break in leverage clienteles that occurs when CDS contracts first trade on a firm. Instead of only investing in firm's with a particular capital structure, CDS contracts allow institutional investors to hedge the risk-return trade-off attributable to capital structure from investing in the firm. [Appendix C](#) explains this test in more detail and presents the results from implementing it. The take-away from the test is there is some evidence for leverage clienteles, but the economic magnitude of the clienteles is so small (i.e., 1/10th of that stemming from institutional investors to leverage) that it cannot meaningfully explain even a small portion of the deleveraging trend.

## 6 Conclusion

In this paper, we address three important and related questions. The first question examines the interrelation between institutional ownership and capital structure decisions. The second question investigates whether a series of regulatory changes that incentivized institutional investors to actively engage in firm governance affected this interrelation. The third question assesses what mechanism drives such an interrelation. Because corporate leverage plays a critical role in the



stability of the economy, our answers to these questions provide important stylized facts relevant to academic research and policymaking.

First, we find increases in institutional ownership, primarily by mutual funds, account for within-firm declines in leverage over time. We reach this conclusion using implied mutual fund trades induced by idiosyncratic individual-investor flows as an instrument for institutional holdings. Our point estimates suggest a 10 percentage point increase in institutional ownership leads to a 4.9 percentage point decline in leverage, on average. Second, we find the magnitude of institutional investors' effect on corporate leverage varies over time. Supporting the hypothesis that institutional investors contributed to the deleveraging trend that began in 1992, our estimates increase significantly after regulatory reforms incentivized stronger institutional governance. To quantify the economic importance of institutional investors' influence post the reforms, we simulate counterfactual leverage paths for all firms and find aggregate corporate leverage would have been eight percentage points higher today without institutional investors' influential role.

The notion that institutional investors such as mutual funds apply pressure to reduce leverage may seem surprising at first. Hedge funds and private equity funds are often glamorized in the business press for exerting their influence over executives to increase corporate debt levels and thereby leverage their funds' returns. We find, however, that increases in ownership by other institutional investors such as mutual funds and pension funds is the dominant force in the deleveraging trend. Even when institutions can free-ride off of hedge funds strategies with respect to leverage, few do.

Our third set of findings suggest why these institutional investors, such as mutual funds, may rationally prefer firms to deleverage. Our evidence indicates the deleveraging occurs through debt not equity and through arm's-length debt not closely-monitored bank debt. Because these institutional investors can influence firms' capital structure but not operational decisions, they encourage firms to concentrate their debt in the type of debt most likely to reduce agency costs, improve operational performance, and enhance equity valuation. Supporting this notion, we find the deleveraging effect is stronger in firms with high potential for agency problems, and increased institutional ownership is associated with active monitoring of loans by banks. Because banks exert the behind-the-scenes pressure necessary to improve firm operational and investment policies, these institutional investors

realize improved equity valuation and fulfill their mandated fiduciary duties through overall lower leverage.

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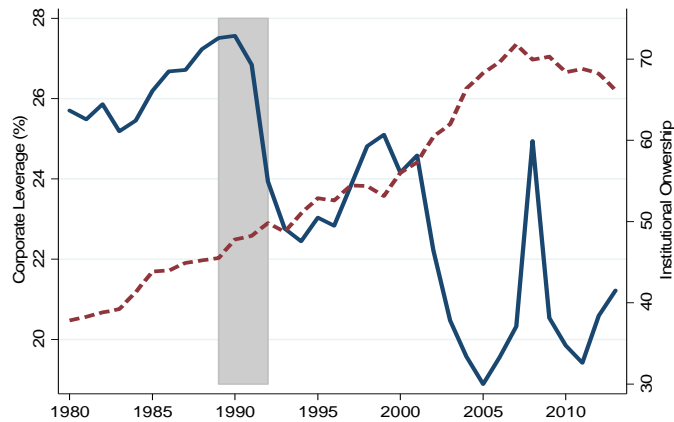
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## Figures and Tables

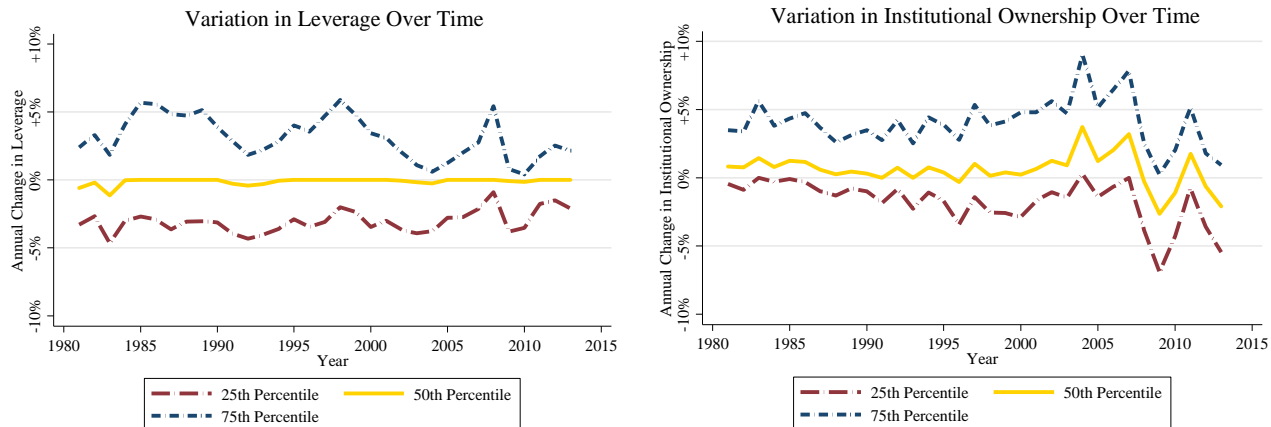


Source: CRSP firms covered in Compustat or Moody's Industrial Manuals from 1920 through 2010.



Source: Compustat firms matched to SEC 13-F Filings from 1980 to 2013.

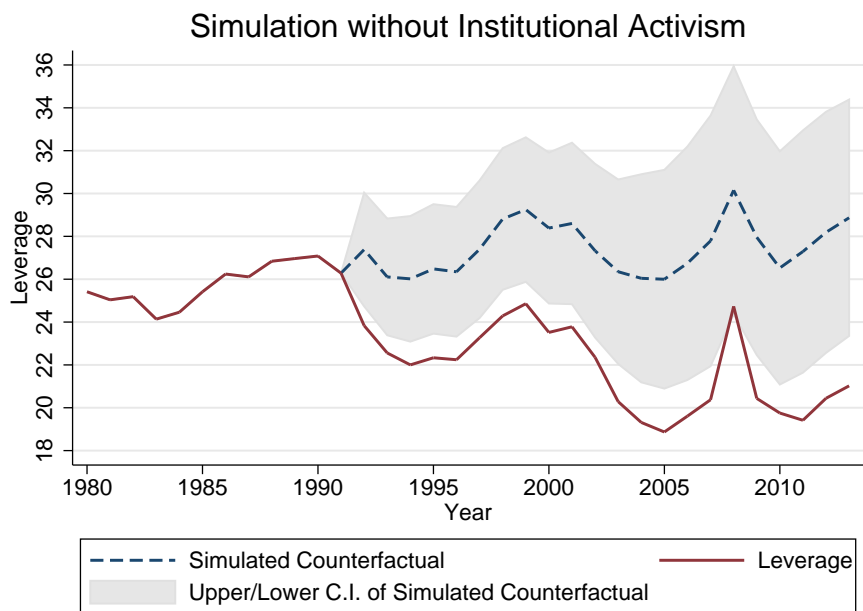
**Figure 1. Capital Structure and Changes in Equity Ownership:** The upper plot shows annual aggregate leverage as a black, solid line for all firms in the CRSP database that are also covered either in Compustat or Moody's Industrial Manuals from 1920 through 2010. The onset of institutional shareholder activism is shaded in gray. Financial firms and utilities are excluded. The annual leverage ratio is defined as aggregate total debt to aggregate financial capital per [Graham, Leary, and Roberts \(2014\)](#). The lower plot focuses in on the period post 1980. The lower plot depicts leverage as a navy, solid line for all firms in the Compustat database, institutional ownership as a red, dashed line, and the onset of institutional shareholder activism is shaded in gray. Financial firms and utilities are excluded. The annual leverage ratio is defined as total debt to total assets per common convention. Institutional shareholder activism began with the submission of governance related proxy proposals in the late 1980s and extended to firm performance and managerial actions by 1992 when the SEC formally adopted rules giving institutional shareholders and activists broad freedom to communicate with each other, express their views on proxy solicitations publicly, and put together organized campaigns to bring pressure to bear on corporate targets.



**Figure 2. Interquartile Range of Annual Changes in Capital Structure and Equity Ownership:** The left-hand side plot shows the 25th, 50th, and 75th percentile for annual within-firm changes in leverage over time. The right-hand plot shows 25th, 50th, and 75th percentile for annual within-firm changes in institutional ownership over time. Because of the first-difference definition, 5% should be interpreted as a 5 percentage point change over the year. The time period begins in 1980 to coincide with the years in which SEC 13-F filings of institutional ownership can be matched to Compustat data.

	Firm A	Firm B	Firm C
<b>Given: Mutual fund's holdings at <math>t</math></b>	\$2M	\$3M	\$5M
<b>Receives \$1M inflow from individual investors at <math>t</math></b>			
<b>Step 1: Isolate idiosyncratic part of individual-investor flows</b>			
Investors chasing returns		Remove flows from fund returns as in Chevalier and Ellison (1997)	
Investors chasing reputation/skill		Remove flows from fund fixed effects as in Berk and Green (2004)	
Focused-funds correlated with leverage		Drop funds that hold firms in one industry	
<b>Result: Estimate \$500,000 of \$1M inflow is idiosyncratic</b>			
<b>Step 2: Project "implied" <math>t + 1</math> trades from idiosyncratic flow at <math>t</math></b>			
\$1M inflow (\$500,000 idiosyncratic)	\$0.1M	\$0.15M	\$0.25M
<b>Then: Aggregate implied <math>t + 1</math> trades across all funds to firm-level</b>			

**Figure 3. Implied flow-induced Trading Example:** Assume a mutual fund holds equity in Firm A, Firm B, and Firm C at time  $t$  equal to \$2 million, \$3 million, and \$5 million, respectively, and has \$10 million total in assets under management at time  $t$ . Next, assume the mutual fund receives an inflow from individual investors of \$1 million. For our instrument to be plausibly exogenous, we need the implied  $t + 1$  trades to be correlated with actual trades but uncorrelated with Firm A, Firm B, or Firm C's next period leverage. By assuming equal proportion mutual fund trades, we bypass the potential confounding effect of mutual fund managers allocating more (or less) of the investors inflows to a Firm that is likely to have higher (or lower) leverage in the future. Because large positive or negative fund flows are not allocated randomly to mutual funds, a first step is needed to bypass the potentially confounding effects of smart money. To isolate the portion of the mutual funds' trades driven by idiosyncratic reasons such as the individual investors' liquidity needs or lumpy inflows from bonuses and tax returns, we remove the proportion of the variation in flows that can be explained by mutual fund returns as well as the proportion of the variation in flows that can be explained by mutual portfolio management skills (Berk and Green (2004)). To accomplish this, we estimate a non-linear relationship between actual flows and fund returns as in Chevalier and Ellison (1997) and include mutual fund fixed effects to control for management skills. Using the fitted values from these regressions, we calculate the residual unexplained variance in actual flows. We use this residual flow (i.e., the idiosyncratic individual-investor flows) to project the implied trade at time  $t + 1$ . The final rows of the figure shows that instead of the \$1 million inflow from the top of the figure, if 50% of the individual-investor flow variation is explained by performance and skills, we only project the mutual fund trades attributable to the remaining \$500,000 in unexplained, idiosyncratic individual-investor flows. Finally, we aggregate all the implied mutual funds trades from all mutual funds to the firm-year level.



**Figure 4. Simulated Counterfactual:** The plot depicts actual leverage as a red, solid line for nonfinancial public firms and simulated leverage for nonfinancial public firms as a blue, dashed line. The simulation sets the influence of institutional investors on leverage to levels observed before the 1992 SEC proxy reform. The grey shaded area represents the 90% confidence interval for the simulated counterfactual.



**Table I Summary Statistics**

This table reports means and standard deviations (SD) for a number of firm-specific covariates from 1980 through 2013 (Full Sample), 1980 through 1991 (Pre-SEC Proxy Reform Sample), and 1992 through 2013 (Post-SEC Proxy Reform Sample). Each sample consists of firm-year observations for all U.S. incorporated, non-regulated firms (i.e., excluding financial firms and utilities) with a positive market capitalization, positive assets, and non-missing values for all covariates. Accounting data are obtained from the Compustat-CRSP merged database, and institutional holdings data are obtained from Thomson-Reuters via 13F SEC filings. [Appendix A](#) provides precise definitions for the variables.

Summary Statistics	Full Sample		1980 to 1991		1992 to 2013	
	Mean	SD	Mean	SD	Mean	SD
Institutional Holdings	36.5%	29.3%	21.2%	19.9%	43.9%	30.2%
Block Holdings	11.7%	12.9%	5.8%	8.7%	14.6%	13.6%
Number of Institutional Owners	72.8	112.2	35.9	69.4	90.4	123.9
Number of Blockholders	1.4	1.4	0.7	0.9	1.7	1.5
Holdings, Top 1 Institution	7.4%	5.5%	5.4%	5.2%	8.3%	5.5%
Holdings, Top 5 Institutions	18.8%	12.9%	12.3%	10.2%	21.9%	12.8%
Holdings, Top 10 Institutions	24.7%	17.5%	15.5%	13.1%	29.1%	17.6%
Ownership Concentration HHI	0.234	0.258	0.312	0.311	0.196	0.219
Leverage	23.1%	21.0%	26.2%	20.2%	21.6%	21.2%
Leverage including Leases	33.2%	25.9%	35.5%	25.0%	32.0%	26.3%
Market Leverage	23.7%	23.8%	29.3%	24.4%	21.0%	23.0%
Leverage in terms of Book Value of Capital	32.0%	29.6%	35.5%	27.7%	30.3%	30.3%
Firm Size	5.41	2.01	5.05	2.01	5.58	1.99
Market-to-book	1.58	1.52	1.31	1.29	1.71	1.60
Lifecycle Stage	-0.32	1.63	-0.01	1.02	-0.47	1.83
Profitability	6.3%	21.5%	8.8%	17.8%	5.2%	22.9%
Losses	19.6%	39.7%	16.7%	37.3%	21.0%	40.7%
Dividend Paying Firm	34.6%	47.6%	46.1%	49.8%	29.1%	45.4%
Payout	1.6%	3.6%	1.0%	2.2%	1.9%	4.0%
Tangibility	28.5%	22.2%	33.4%	21.3%	26.1%	22.2%
Collateral	44.2%	24.3%	53.3%	20.9%	39.8%	24.6%
Intangibility	9.5%	15.3%	3.5%	8.1%	12.4%	17.0%
Investment	31.1%	38.3%	28.7%	37.0%	32.3%	38.9%
Sales Growth	15.2%	47.3%	14.4%	45.8%	15.6%	48.0%
Asset Growth	13.0%	40.1%	12.1%	36.4%	13.5%	41.7%
Firm Beta	0.83	0.63	0.82	0.61	0.83	0.64
Market Risk	5.3%	5.9%	4.3%	4.3%	5.8%	6.5%
Adjusted Firm Returns	-6.0%	10.7%	-12.0%	6.5%	-3.0%	11.0%
Altman's Z-Score	1.34	2.43	1.86	2.00	1.09	2.57
Amihud Liquidity	10.83	45.33	24.50	71.25	4.29	21.78
Observations	106,171		34,389		71,782	

**Table II Partial Correlations**

Panel A reports partial correlations from multivariate regressions relating book leverage and institutional holdings, Panel B reports for leverage including off-balance sheet operational leases, and Panel C for leverage defined as the ratio of financial debt to the book value of capital. The sample consists of firm-year observations for all U.S. incorporated, non-regulated firms. The vector of firm controls include firm size, tangibility, intangibility, collateral, profitability, losses, dividend paying firm, payout, lifecycle stage, market-to-book, investment, sales growth, asset growth, firm risk, market risk, adjusted firm returns, Amihud's illiquidity measure, Altman's Z-score, and insider ownership. [Appendix A](#) provides precise definitions for the variables.

<b>Panel A. Partial Correlations with Leverage</b>					
Institutional Holdings	-0.170***				
	(0.011)				
Holdings, Top 5 Institutions		-0.038***			
		(0.008)			
Holdings, Top 10 Institutions			-0.067***		
			(0.009)		
Block Holdings				-0.002	
				(0.006)	
Ownership Concentration HHI					0.073***
					(0.005)
Adjusted $R^2$	67%	66%	66%	66%	66%
<b>Panel B. Partial Correlations with Leverage including Leases</b>					
Institutional Holdings	-0.148***				
	(0.011)				
Holdings, Top 5 Institutions		-0.036***			
		(0.007)			
Holdings, Top 10 Institutions			-0.060***		
			(0.008)		
Block Holdings				-0.005	
				(0.006)	
Ownership Concentration HHI					0.057***
					(0.005)
Adjusted $R^2$	70%	69%	70%	69%	70%
<b>Panel C. Partial Correlations with Leverage in terms of Book Value of Capital</b>					
Institutional Holdings	-0.173***				
	(0.012)				
Holdings, Top 5 Institutions		-0.037***			
		(0.008)			
Holdings, Top 10 Institutions			-0.067***		
			(0.009)		
Block Holdings				0.002	
				(0.006)	
Ownership Concentration HHI					0.078***
					(0.006)
Adjusted $R^2$	61%	60%	60%	60%	60%
Firm-level Controls	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effect	Yes	Yes	Yes	Yes	Yes
Industry Fixed Effect	Yes	Yes	Yes	Yes	Yes
Year Fixed Effect	Yes	Yes	Yes	Yes	Yes
Observations	106,171	106,171	106,171	106,171	106,171
Unique Firms	10,383	10,383	10,383	10,383	10,383

**Table III The Effect of Institutional Holdings on Leverage**

This table explores institutional investors' role in aggregate leverage trends using the instrumental variable specification outlined in Eq. (1). The instrument for institutional holdings is mutual funds' implied trades. The vector of firm controls include firm size, tangibility, intangibility, collateral, profitability, losses, dividend paying firm, payout, lifecycle stage, market-to-book, investment, sales growth, asset growth, firm risk, market risk, adjusted firm returns, Amihud's illiquidity measure, Altman's Z-score, and insider ownership. \*\*\*, \*\*, and \* represent statistical significance at the 1%, 5%, and 10% level, respectively. Robust standard errors, clustered at the firm level, are reported in parentheses.

Dependent Variable =	IV	IV Robustness (Alt. Leverage Measures)	
	(1)	(2)	(3)
	Leverage	Leverage including Leases	Leverage in terms of Book Value of Capital
Institutional Holdings	-0.718*** (0.144)	-0.543*** (0.123)	-0.659*** (0.145)
Firm Size	0.821*** (0.090)	0.507*** (0.077)	0.706*** (0.090)
Tangibility	0.077*** (0.021)	0.028 (0.020)	0.004 (0.021)
Intangibility	0.099*** (0.009)	0.084*** (0.008)	0.073*** (0.009)
Collateral	0.146*** (0.020)	0.197*** (0.019)	0.175*** (0.020)
Profitability	-0.021** (0.010)	-0.059*** (0.010)	-0.006 (0.012)
Losses	-0.014*** (0.005)	-0.015*** (0.005)	0.002 (0.006)
Dividend Paying Firm	-0.091*** (0.008)	-0.080*** (0.007)	-0.111*** (0.009)
Payout	0.017*** (0.004)	0.011*** (0.004)	0.022*** (0.005)
Market-to-book	0.034*** (0.013)	0.031*** (0.011)	0.02 (0.013)
Additional Firm Controls	Yes	Yes	Yes
Firm Fixed Effect	Yes	Yes	Yes
Industry-by-Year Fixed Effects	Yes	Yes	Yes
First-stage F-stat	231.9	231.9	231.9
T-stat on Instrument in First Stage	9.76	9.76	9.76
First-Stage $R^2$	49%	49%	49%
Adjusted $R^2$	68%	72%	63%
Observations	106,171	106,171	106,171
Unique Firms	10,383	10,383	10,383

**Table IV Institutional Investors' Role in Aggregate Leverage Trends**

This table explores institutional investors' role in aggregate leverage trends by testing if SEC regulatory changes which enhanced institutions' power strengthened the relationship from institutional holdings to leverage. Panel A reports standardized coefficient estimates from instrumental variable regressions of leverage on institutional holdings for time periods before and after the SEC reform as outlined in Eq. (1). The various subsamples relate to time periods near the SEC reform. Institutional shareholder activism began with the submission of governance related proxy proposals in the late 1980s and extended to firm performance and managerial actions by 1992 when the SEC formally adopted rules giving institutional shareholders and activists broad freedom to communicate with each other, express their views on proxy solicitations publicly, and put together organized campaigns to bring pressure to bear on corporate targets. The pre-SEC proxy reform period includes 1980 through 1991; the post-SEC proxy reform period includes 1992 through 2013. The vector of firm controls include firm size, tangibility, intangibility, collateral, profitability, losses, dividend paying firm, payout, lifecycle stage, market-to-book, investment, sales growth, asset growth, firm risk, market risk, adjusted firm returns, Amihud's illiquidity measure, Altman's Z-score, and insider ownership. \*\*\*, \*\*, and \* represent statistical significance at the 1%, 5%, and 10% level, respectively. Robust standard errors, clustered at the firm level, are reported in parentheses.

Institutional Investors' Role in Aggregate Leverage Trend – Subsample Tests				
Dependent Variable = Leverage	All Years (1980 - 2013)	Pre Onset of Institutional Shareholder Activism		
		Pre SEC Reform (1989-1992)	Post Onset to Post SEC Reform (1992-2013)	
Institutional Holdings	-0.718*** (0.144)	0.290 (0.402)	-0.411 (0.374)	-1.146*** (0.310)
Chow Test F-stat		Pre vs. Post SEC Reform		30.443***
Sum of Squared Residuals	33,609.2	7,031.0		24,270.3
Firm Controls	Yes	Yes	Yes	Yes
Firm Fixed Effect	Yes	Yes	Yes	Yes
Industry-by-Year Fixed Effects	Yes	Yes	Yes	Yes
First-stage F-stat	231.9	54.9	12.2	43.3
T-stat on Instrument in First Stage	9.76	3.33	4.40	5.38
First-Stage $R^2$	49%	33%	14%	44%
Adjusted $R^2$	68%	76%	86%	71%
Observations	106,176	24,187	9,381	71,616
Unique Firms	10,383	4,373	3,286	7,816

**Table V Heterogeneity in Observed Treatment Effects**

This table reports standardized coefficient estimates across various quantiles from instrumental variable quantile regressions as is outlined in Eq. (2). In all specifications, the instrument for institutional holdings is mutual funds' implied trades. The vector of firm controls include firm size, tangibility, intangibility, collateral, profitability, losses, dividend paying firm, payout, lifecycle stage, market-to-book, investment, sales growth, asset growth, firm risk, market risk, adjusted firm returns, Amihud's illiquidity measure, Altman's Z-score, and insider ownership. \*\*\*, \*\*, and \* represent statistical significance at the 1%, 5%, and 10% level, respectively. Robust standard errors, clustered at the firm level, are reported in parentheses.

Panel A. Instrumental Variables Quantile Regressions			
Quantile ( $\tau$ )	Coefficient at $\tau$ on		Estimate of Leverage ( $\tau$ ) After 10% Increase in Institutional Holdings, All Else Equal
	Institutional Holdings	Leverage ( $\tau$ )	
0.95	-1.075***	63.1%	55.4%
0.90	-0.981***	51.4%	44.4%
0.85	-0.890***	44.5%	38.1%
0.80	-0.797***	39.5%	33.8%
0.75	-0.718***	35.4%	30.2%
0.66	-0.595***	29.2%	24.9%
0.50	-0.519***	19.8%	16.1%
0.33	-0.511***	9.4%	5.7%

Panel B. Tests for Heterogeneous Relationship		
	95% Test Statistic	Kolmogorov-Smirnov Test for Distributional Equality
Constant Effect ( $\tau < 0.25$ equals $\tau > 0.75$ )	(9.11)***	D = 0.947, Reject Equality
Constant Effect ( $\tau < 0.33$ equals $\tau > 0.66$ )	(12.31)***	D = 0.965, Reject Equality
Constant Effect ( $\tau < 0.5$ equals $\tau > 0.5$ )	(16.53)***	D = 0.954, Reject Equality

**Table VI The Structure of the Deleveraging**

This table explores how institutional investors affected the equity and debt structure of the unregulated firms following the SEC regulatory changes. Panel A reports standardized coefficient estimates from instrumental variable regressions of debt and equity changes on institutional holdings. Panel B presents an even more detailed analysis of the debt structure using Capital IQ's capital structure files. In all specifications, the instrument for institutional holdings is the hypothetical annual change in mutual fund holdings implied by previously disclosed holdings as is outlined in Eq. (3). The vector of firm controls include firm size, tangibility, intangibility, collateral, profitability, losses, dividend paying firm, payout, lifecycle stage, market-to-book, investment, sales growth, asset growth, firm risk, market risk, adjusted firm returns, Amihud's illiquidity measure, Altman's Z-score, and insider ownership. \*\*\*, \*\*, and \* represent statistical significance at the 1%, 5%, and 10% level, respectively. Robust standard errors, clustered at the firm level, are reported in parentheses.

The Composition of Deleveraging						
Panel A. Deleveraging through Debt and Equity Changes						
Dependent Variable =	Debt				Equity	
	Short-Term	Long-term	Issuance	Issuance (Net Retirements)	Buybacks	Issuance (Net Buybacks)
Institutional Holdings	-0.390 (0.277)	-1.554*** (0.398)	-0.616* (0.371)	-0.405** (0.185)	-0.266** (0.132)	0.213 (0.290)
Firm Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes
Industry-by-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
First-stage F-stat	43.3	43.3	43.3	43.3	43.3	43.3
T-stat on Instrument in First Stage	5.38	5.38	5.38	5.38	5.38	5.38
Adjusted $R^2$	47%	69%	42%	8%	23%	36%
Observations	71,616	71,616	71,616	71,616	71,616	71,616
Unique Firms	7,986	7,986	7,986	7,986	7,986	7,986
Panel B. Deleveraging through Debt Structure Changes in Detail						
Dependent Variable =	Debt					
	Bonds and Notes	Commercial Paper	Terms Loans	Capital Leases	Other Borrowings	
Institutional Holdings	-1.688*** (0.495)	-2.828*** (0.982)	0.378 (0.330)	-0.043 (0.375)	0.671 (0.515)	
Firm Controls	Yes	Yes	Yes	Yes	Yes	
Firm Fixed Effect	Yes	Yes	Yes	Yes	Yes	
Industry-by-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	
First-stage F-stat	20.7	20.7	20.7	20.7	20.7	
T-stat on Instrument in First Stage	4.55	4.55	4.55	4.55	4.55	
Adjusted $R^2$	61%	22%	51%	49%	32%	
Observations	40,398	40,398	40,398	40,398	40,398	
Unique Firms	5,939	5,939	5,939	5,939	5,939	

**Table VII Tests of Underlying Mechanisms**

This table shows which mechanisms relate institutional holdings and leverage. The exact specification is as in Eq. (4). In all variants of the specification, the instrument for institutional holdings is mutual funds' implied trades and instrument for the characteristic is the same instrument interacted with the characteristic of interest. \*\*\*, \*\*, and \* represent statistical significance at the 1%, 5%, and 10% level, respectively. Robust standard errors, clustered at the firm level, are reported in parentheses.

Dependent Variable = Leverage	High Potential for Agency Conflicts		
	Defn. 1.	Defn. 2.	Defn. 3.
Institutional Holdings	-0.698*** (0.146)	-0.698*** (0.144)	-0.706*** (0.147)
Institutional Holdings x Agency Conflicts	-0.151** (0.060)	-0.014 (0.071)	-0.114 (0.228)
Agency Conflicts	0.275*** (0.085)	0.061* (0.036)	0.093 (0.169)
Additional Firm Controls	Yes	Yes	Yes
Firm Fixed Effect	Yes	Yes	Yes
Industry-by-Year Fixed Effects	Yes	Yes	Yes
First-stage F-stat Instrument 1	263.3	187.0	216.5
First-stage $R^2$ Instrument 1	49%	49%	50%
T-stat on Instrument 1	8.14	9.77	9.70
First-stage F-stat Instrument 2	100.3	17.9	18.4
First-stage $R^2$ Instrument 2	68%	36%	55%
T-stat on Instrument 2	18.09	3.15	0.61
Adjusted $R^2$	68%	68%	68%
Observations	106,171	106,171	106,171
Unique Firms	10,383	10,383	10,383

**Table VIII Institutional Investors and the Governance Role of Creditors**

This table explores the hypothesis that institutional investors encourage the use of bank debt rather than arm's length debt because of the important governance role these creditors play. Panel A reports standardized coefficient estimates for all firms whereas Panel B reports estimates only for firms included in Thomson Reuters' Dealscan database. The exact specification is as in Eq. (1). In all variants of the specification, the instrument for institutional holdings is mutual funds' implied trades. \*\*\*, \*\*, and \* represent statistical significance at the 1%, 5%, and 10% level, respectively. Robust standard errors, clustered at the firm level, are reported in parentheses.

Panel A. All Firms				
Dependent Variable =	Leverage	Net Worth Covenants	Financial Covenants	Total Covenants
Institutional Holdings	-0.718*** (0.144)	0.640*** (0.235)	0.342* (0.184)	0.448** (0.189)
Additional Firm Controls	Yes	Yes	Yes	Yes
Firm Fixed Effect	Yes	Yes	Yes	Yes
Industry-by-Year Fixed Effects	Yes	Yes	Yes	Yes
First-stage F-stat	231.9	231.9	231.9	231.9
T-stat on Instrument in First Stage	9.76	9.76	9.76	9.76
First-Stage $R^2$	49%	49%	49%	49%
Adjusted $R^2$	68%	40%	57%	56%
Observations	106,171	106,171	106,171	106,171
Unique Firms	10,383	10,383	10,383	10,383
Panel B. Firms in Thomson Reuters' Dealscan Database				
Dependent Variable =	Leverage	Net Worth Covenants	Financial Covenants	Total Covenants
Institutional Holdings	-0.643*** (0.234)	1.220** (0.527)	1.354*** (0.399)	1.455*** (0.407)
Additional Firm Controls	Yes	Yes	Yes	Yes
Firm Fixed Effect	Yes	Yes	Yes	Yes
Industry-by-Year Fixed Effects	Yes	Yes	Yes	Yes
First-stage F-stat	35.1	35.1	35.1	35.1
T-stat on Instrument in First Stage	5.92	5.92	5.92	5.92
First-stage $R^2$	45%	45%	45%	45%
Adjusted $R^2$	75%	56%	67%	67%
Observations	39,987	39,987	39,987	39,987
Unique Firms	5,254	5,254	5,254	5,254



**Table IX Comparison of Institutional Investors and Hedge Funds**

This table explores if the institutional investors in our sample which include mutual funds, pension funds, and insurance companies have different viewpoints than hedge funds do with respect to what a firm's leverage ratio should be. Panel A reports standardized coefficient estimates for all firms whereas Panel B reports estimates only for firms that were ever targeted by an activist hedge fund. The exact specification is as in Eq. (1). In all variants of the specification, the instrument for institutional holdings is mutual funds' implied trades. \*\*\*, \*\*, and \* represent statistical significance at the 1%, 5%, and 10% level, respectively. Robust standard errors, clustered at the firm level, are reported in parentheses.

Comparison of Institutional Investors and Hedge Funds				
Panel A. All Firms	(1)	(2)	(3)	(4)
Dependent Variable =	Leverage	Leverage	Leverage	Leverage
Institutional Holdings	-0.718*** (0.144)	-0.715*** (0.144)	-0.720*** (0.145)	-0.716*** (0.144)
Hedge Fund Campaign		0.084*** (0.025)		
Hedge Fund Activist Crosses 5% Threshold			0.058** (0.023)	0.065*** (0.024)
Hedge Fund Activist x Institutional Holdings				0.088*** (0.017)
Additional Firm Controls	Yes	Yes	Yes	Yes
Firm Fixed Effect	Yes	Yes	Yes	Yes
Industry-by-Year Fixed Effects	Yes	Yes	Yes	Yes
First-stage F-stat	231.9	231.9	231.9	231.9
T-stat on Instrument in First Stage	9.76	9.76	9.76	9.76
First-Stage $R^2$	49%	49%	49%	49%
Adjusted $R^2$	68%	68%	68%	68%
Observations	106,171	106,171	106,171	106,171
Unique Firms	10,383	10,383	10,383	10,383
Panel B. Firms Targeted by Activist Hedge Funds	(1)	(2)	(3)	(4)
Dependent Variable =	Leverage	Leverage	Leverage	Leverage
Institutional Holdings	-0.687** (0.319)	-0.681** (0.317)	-0.691** (0.320)	-0.692** (0.322)
Hedge Fund Activism Campaign		0.042* (0.022)		
Hedge Fund Activist Crosses 5% Threshold			0.023 (0.020)	0.034 (0.022)
Hedge Fund Activist x Institutional Holdings				0.065*** (0.022)
Additional Firm Controls	Yes	Yes	Yes	Yes
Firm Fixed Effect	Yes	Yes	Yes	Yes
Industry-by-Year Fixed Effects	Yes	Yes	Yes	Yes
First-stage F-stat	463.9	463.9	463.9	463.9
T-stat on Instrument in First Stage	4.51	4.51	4.51	4.51
First-Stage $R^2$	57%	57%	57%	57%
Adjusted $R^2$	66%	66%	66%	66%
Observations	18,214	18,214	18,214	18,214
Unique Firms	1,139	1,139	1,139	1,139

## A Variable Definitions

**Institutional Holdings and Block Holdings** data comes from Thomson-Reuters via 13F SEC filings. All ownership percentages are derived based on the number of shares outstanding and correspond to calendar dates. A blockholder is defined as an institutional investor with more than 5% holdings as filed through 13D, 13F, or 13G filings. The ownership concentration Herfindahl-Hirschman Index (HHI) is defined to take into account the relative size distribution of the investors in a given equity. The HHI index approaches zero when the equity of a firm is occupied by a large number of investors of relatively equal size and reaches its maximum of one when it is controlled by a single investor.

$$\text{Leverage} = \frac{DLTT+DLC}{AT}$$

**Leverage including Leases** =  $\frac{DLTT+DLC+XRENT+\sum_{t=1}^5 \frac{MRC_t}{(1+K_d)^t}}{AT}$ , where  $K_d$  is the cost of debt capital, which we set equal to 10% following [Rauh and Sufi \(2010\)](#).

**Leverage in Terms of Book Value of Capital** =  $\frac{DLTT+DLC}{(DLTT+DLC+SEQ+MIB)}$  as discussed in [Welch \(2011\)](#).

**Market Equity** =  $MEQ = PRCC \times CSHO$ , where PRCC is calendar year in order to match calendar year institutional holdings reports.

$$\text{Market Leverage} = \frac{DLTT+DLC}{DLTT+DLC+MEQ}$$

$$\text{Firm Size} = \text{Log}(AT)$$

$$\text{Market Value of Assets (MVA)} = MEQ + DLC + DLTT + PSTK - TXDITC$$

$$\text{Market-to-book} = \frac{MVA}{AT}$$

$$\text{Lifecycle Stage} = \frac{RET}{AT}$$

$$\text{Profitability} = \frac{OIBDP}{AT}$$

**Losses** is a dummy variable taking the value of one if  $OIBDP$  is negative and zero otherwise.

**Dividend Paying Firm** is a dummy variable taking the value of one if  $DVC$  is positive and zero otherwise.

$$\text{Payout} = \frac{DVC+PRSTKC}{AT}$$

$$\text{Tangibility} = \frac{PPENT}{AT}$$

$$\text{Collateral} = \frac{INVT+PPENT}{AT}$$

$$\text{Intangibility} = \frac{INTAN}{AT}$$

$$\text{Investment} = \frac{((CAPX-SPPE)-(CAPX_{t-1}-SPPE_{t-1}))}{PPENT_{t-1}}$$

$$\text{Sales Growth} = \frac{(REVT-REVT_{t-1})}{REVT_{t-1}}$$

$$\text{Asset Growth} = \frac{(AT-AT_{t-1})}{AT_{t-1}}$$

**Firm Beta** is the calendar year beta computed from daily CRSP data where the betas are adjusted to reflect the spillover of returns that often occur around non-trading days.

**Market Risk** is the calendar year market risk computed from daily CRSP data, where market risk and risk to an individual industry are disaggregated.

**Adjusted Firm Returns** is each firm's annual return adjusted by the CAPM return using the firm's beta, the 10-year Treasury bond yield, the realized return on the S&P 500 index.

$$\text{Altman's Z-Score} = 3.3 \times \frac{PI}{AT} + .99 \times \frac{SALE}{AT} + 1.4 \times \frac{RE}{AT} + 1.2 \times \frac{(ACT-LCT)}{AT} + .6 \times \frac{MEQ}{LLT}$$

**Amihud's Illiquidity** is the monthly ratio of absolute stock return to its dollar volume average over the prior calendar year. ([Amihud \(2002\)](#))

**Insider Holdings** data comes from Thomson-Reuters cleansed non-derivative transaction and insider activity data extracted from SEC Forms 3, 4, 5, and 144. Note, this data is only available beginning in 1986.

**CDS** data comes from Markit and coverage begins in 2001. CDS introduction is the first year in which CDS contracts are introduced on a firm. High CDS depth is a dummy variable taking the value one if that equity has above median coverage quality in a calendar year, which Markit approximates using the number of unique daily reports on a name.

**Implied Mutual Fund Trades** is the firm-specific, calendar-year annual dollar change in mutual fund holdings implied by idiosyncratic individual-investor inflows and outflows. To construct this variable, we combine quarterly data on mutual fund holdings from Thomson Reuters and quarterly data on stock returns, assets under management, and fund flows from CRSP. First, we apply definitions from [Sirri and Tufano \(1998\)](#) to isolate individual investor inflows to and outflows from each mutual fund in a given quarter. Next, we cleanse the individual-investor flows of variation attributable to individual-investors' chasing managerial skill and reputation ([Berk and](#)

Green (2004)) as well as chasing fund performance (Chevalier and Ellison (1997)). Specifically, we regress quarterly individual-investor flows on a flexible form of mutual fund returns and mutual fund fixed effects. We label the residual from these regressions as the idiosyncratic individual-investor flow because they are meant to represent variation in individual-investors' flows attributable to phenomenon such as liquidity needs that have no direct or indirect relationship with corporate leverage. Then, we use the actual holdings of the mutual fund in a given quarter to project changes in individual stock holdings in the next quarter based on the magnitude of the idiosyncratic, individual-investor flows and trading volume for that stock in the given quarter. We limit the set of mutual funds projected to change their individual stock holdings to those that do not experience a greater than 200% change in assets under management, and we limit the set of mutual funds to those that do not specialize in specific industries. The industry exclusion, which is defined at the two-digit SIC level, mitigates concerns that investors' flows are correlated with industry-specific business cycles. Finally, we sum the projected changes in stock holdings from the mutual funds that remain in the set to an individual stock-year frequency. [Figure 3](#) illustrates this procedure.

To clarify our calculation, consider the formula for firm  $i$  in year  $y$  based on quarter  $q$  investor flows. Let  $ImpliedTrade_{i,y} = \sum_{q=1}^4 \left[ \sum_{j=1}^m \frac{F_{j,q} \times SHARES_{i,j,q-1} \times PRC_{i,q-1}}{MTA_{j,q-1} \times VOL_{i,q}} \right]$ , where  $F_{j,q}$  is the total idiosyncratic individual-investor flow (positive or negative) experienced by fund  $j$  in quarter  $q$ ,  $MTA_{j,q-1}$  is mutual fund  $j$ 's total assets at the end of the previous quarter,  $SHARES_{i,j,q-1} \times PRC_{i,q-1}$  is the dollar value of fund  $j$ 's holdings of stock  $i$ , and  $VOL_{i,q}$  is the total dollar trading volume of stock  $i$  in quarter  $q$ . The summation is over funds  $j$ . To mitigate the impact of any flow on a stock's liquidity, we scale by trading volume  $VOL_{i,q}$ .

Our construction of implied mutual fund trades from idiosyncratic individual-investor flows builds on recent research (Coval and Stafford (2007); Edmans, Goldstein, and Jiang (2012)). The use of implied rather than actual trades differs from Coval and Stafford (2007). The estimation of idiosyncratic flows rather than outflows greater than 5% differs from Edmans, Goldstein, and Jiang (2012). Like both of these papers, we seek an instrument that is unrelated to the firm's fundamentals or managerial decisions.

**High Agency Firm** is a dummy variable taking the value of one if a firm is large and has few

growth opportunities (i.e., market capitalization greater than the 80th percentile and market-to-book ratio less than the 20th percentile in a given calendar year).

**High Agency Firm (SG&A Definition)** is a dummy variable taking the value of one for firms with managers that tend to overspend on SG&A costs without legitimate economic reasons. We proxy for this form of agency costs using small firms with SG&A expenses greater than the 80th percentile and sales growth less than the 20th percentile in a given calendar year.

**High Agency Firm (M&A Definition)** is a dummy variable taking the value of one if a firm had positive acquisition expenses (Compustat variable AQC) in each of the previous two years and SDC M&A data indicates the acquisitions were not of firms in the same primary 3-digit SIC code as the acquirer. Hence, they were non-core acquisitions.

$$\text{Short-term Debt} = \frac{DLTT}{AT}$$

$$\text{Long-term Debt} = \frac{DC}{AT}$$

$$\text{Debt Issuance} = \frac{DLTIS}{AT}$$

$$\text{Debt Issuance (Net Retirements)} = \frac{DLTIS - DLTR_{t-1}}{AT}$$

$$\text{Equity Buybacks} = I \left[ \frac{SSTK - PRSTKC_{t-1}}{AT} < -0.025 \right]$$

$$\text{Equity Issuance (Net Buybacks)} = \frac{SSTK - PRSTKC_{t-1}}{AT}$$

**Capital IQ Debt Structure Variables** are constructed using the Capital IQ Capital Structure Debt quarterly dataset which is available beginning in 1989. We use the variable *capitalstructuresubtypeid* to identify the component of the debt structure and the variable *dataitemvalue* scaled by *unittypeid* to populate five different components of the debt structure. When there are multiple filings for the same debt structure item we select *filingflag\_company*==2 for last filing. When capital structure items are the same item but coded differently because of descriptive differences across 10Ks, annual reports, etc..., we select the *dataitemvalue* from the last chronological date among the items with *filingflag\_company* indicating a last filing. We then aggregate the unique, fine-grained components of the capital structure into the following categories at an annual frequency.

$$\text{BN} = (\text{capitalstructuresubtypeid} = 4) = \text{bonds and notes,}$$

$$\text{CP} = (\text{capitalstructuresubtypeid} = 1) = \text{commercial paper,}$$

$$\text{TL} = (\text{capitalstructuresubtypeid} = 3) = \text{term loans,}$$

**CL** = (*capitalstructuresubtypeid* = 5) = capital leases,

**OTR** = (*capitalstructuresubtypeid* = 2|6|7|9) = drawn credit line + trust securities + other borrowings + preferred securities.

**Hedge Fund Activism Campaign** is a dummy variable taking the value of one if an activist hedge fund campaign is in progress. If the hedge funds exit date is not reported to the press, it is assumed to exit with 24 months of filing its 13D form with the SEC. Brav et al. (2008) provided this data.

**Hedge Fund Activist Crosses 5% Threshold** is a dummy variable taking the value of one in the year when a hedge fund crosses the 5% threshold that requires the fund to report its position to the SEC in a 13D filing. Brav et al. (2008) provided this data.

**Recessionary Year** is a dummy variable that takes a value of 1 if the U.S. economy was officially in a recession for 2 or more quarters in that year according to the NBER's Business Cycle data. The data are available at <http://www.nber.org/cycles/recessions.html>.

**Low Government Debt Year** is a dummy variable that takes a value of 1 if the U.S. government debt issuance decreased from the previous year. The data are available at <http://www.treasury.gov/Pages/default.aspx>.

**Active Institutional Investor Firm** is a dummy variable taking the value of one if a firm's mutual fund investors are active investors as defined in Cremers and Petajisto (2009). Intuitively, for each mutual fund Petajisto creates an active share that reveals the percentage of the portfolio that differs from the passive benchmark index. Any fund with an active share greater than 60% is considered active. Any stock where the funds on average have an active share greater than 60% are active. The data are available at <http://www.petajisto.net/data.html>.

## B Additional Tables

**Table BI The Effect of Institutional Holdings on Leverage**

This table provides evidence from several robustness checks of the estimated effect of institutional holdings on leverage using the instrumental variable specification outlined in Eq. (1). The instrument for institutional holdings is mutual funds' implied trades. The vector of firm controls is the same as in the baseline specification in Table III. \*\*\*, \*\*, and \* represent statistical significance at the 1%, 5%, and 10% level, respectively. Robust standard errors, clustered at the firm level, are reported in parentheses.

	IV Estimate	Observations	First-stage F-stat	T-stat on Instrument in First Stage
<b>Panel A. Robustness Checks with Additional Covariates</b>				
Analyst Coverage	-0.706*** (0.150)	106,176	231.9	9.76
Variation in External Financing Costs	-0.677*** (0.166)	87,903	69.8	8.36
Credit Ratings	-0.733*** (0.144)	106,176	231.9	9.76
Tax Rates	-0.645*** (0.147)	101,835	59.2	9.46
Research & Development Activity	-0.713*** (0.145)	106,176	231.9	9.76
Domestic Only Operations	-0.719*** (0.145)	106,176	231.9	9.76
<b>Panel B. Robustness Checks with Alternative Definitions of Institutional Holdings</b>				
Percentage Held by Top 5 Institutions	-0.479*** (0.100)	106,176	92.87	10.18
Percentage Held by Top 10 Institutions	-0.508*** (0.104)	106,176	58.09	10.67
Percentage Held by Blockholders	-0.516*** (0.118)	106,176	65.47	7.61
<b>Panel C. Robustness Checks with Alternative Instrument Construction</b>				
Without Fund Fixed Effects	-0.710*** (0.140)	106,176	230.96	10.03
Using Linear Functional Form for Fund Returns	-0.693*** (0.125)	106,176	233.54	11.06
With Funds Experiencing >200% Change in AUM	-0.852*** (0.151)	106,176	339.83	10.03
Without Individual-investor Redemptions	-0.537*** (0.062)	106,172	89.70	20.28
<b>Panel D. Robustness Checks with Alternative Timing</b>				
Lagged Institutional Holdings	-0.619*** (0.145)	93,777	87.29	9.34
Lagged Institutional Holdings and All Covariates	-0.599*** (0.153)	93,777	82.07	9.06
<b>Panel E. Robustness Checks with Alternative Samples</b>				
Limiting Sample to Firms with 10 Years of Data	-0.798*** (0.176)	76,225	224.81	8.18
Limiting Sample to Firms with 20 Years of Data	-0.758*** (0.263)	37,677	28.02	5.29
Limiting Sample to Constant Number of Firms	-0.616*** (0.225)	52,559	37.04	6.09
Limiting Sample to Recessionary Years	-0.490* (0.289)	15,170	221.25	4.57
Limiting Sample to Low Government Debt Years	-0.421*** (0.126)	50,049	97.91	9.89
Limiting Sample to Firms with Active Mutual Funds	-0.956*** (0.198)	22,803	52.22	7.23

**Table BII Institutional Characteristics in Relation to Estimated Effects**

This table explores which characteristics of the institutional holders drive the estimated effect of institutional holdings on leverage. Panel A provides evidence from subsample regressions, where subsamples are defined by size quantiles for the percentage point change in institutional holdings. Each regression uses the instrumental variable specification outlined in Eq. (1), where instrument for institutional holdings is mutual funds' implied trades. The vector of firm controls is the same as in the baseline specification in Table III. In all variants of the specification, the instrument for institutional holdings is mutual funds' implied trades and instrument for the characteristic is the same instrument interacted with the characteristic of interest. \*\*\*, \*\*, and \* represent statistical significance at the 1%, 5%, and 10% level, respectively. Robust standard errors, clustered at the firm level, are reported in parentheses.

Average Treatment Effect Driven by Large Changes in Institutional Holdings					
	Quantile 1	Quantile 2	Quantile 3	Quantile 4	Quantile 5
Mean Change in Institutional Holdings in Quantile	-8.3%	-1.1%	0.6%	3.3%	12.0%
Median Change in Institutional Holdings in Quantile	-6.1%	-1.0%	0.5%	3.2%	9.5%
Dependent Variable = Leverage					
Institutional Holdings	-2.401*	-0.518	-1.749	-0.864**	-0.492**
	(1.382)	(0.744)	(1.243)	(0.389)	(0.225)
Additional Firm Controls	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effect	Yes	Yes	Yes	Yes	Yes
Industry-by-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
First-stage F-stat	456.0	427.1	180.6	182.2	473.1
T-stat on Instrument in First Stage	1.85	1.91	1.78	3.92	5.84
First-Stage $R^2$	48%	51%	50%	61%	57%
Adjusted $R^2$	71%	68%	70%	72%	70%
Observations	16,693	16,410	16,522	16,668	16,917
Unique Firms	6,856	6,815	6,677	6,388	6,383



**Table BIII Placebo Test of Institutional Holdings on Leverage**

This table explores institutional investors' role in aggregate leverage trends using a placebo instrumental variable specification. The regression specifications follows that outlined in Eq. (1) except rather than instrumenting for institutional holdings using mutual funds' implied forward-looking trades, we use mutual funds' implied backward-looking trades. This variation is a falsification test that the instrument is not picking up some underlying unobservable economic trend. The vector of firm controls is the same as in the baseline specification in Table III. \*\*\*, \*\*, and \* represent statistical significance at the 1%, 5%, and 10% level, respectively. Robust standard errors, clustered at the firm level, are reported in parentheses.

Dependent Variable =	Original IV	Placebo IV	Placebo IV Robustness (Alt. Defn.)	
	(1)	(2)	(3)	(4)
	Leverage	Leverage	Leverage including Leases	Leverage in terms of Book Value of
Institutional Holdings	-0.718*** (0.144)	-0.160 (0.699)	-0.177 (0.614)	-0.093 (0.731)
Firm Size	0.821*** (0.090)	0.487 (0.417)	0.288 (0.366)	0.368 (0.436)
Tangibility	0.077*** (0.021)	0.070*** (0.023)	0.023 (0.021)	-0.004 (0.023)
Intangibility	0.099*** (0.009)	0.105*** (0.011)	0.088*** (0.010)	0.079*** (0.012)
Collateral	0.146*** (0.020)	0.173*** (0.039)	0.215*** (0.035)	0.203*** (0.041)
Profitability	-0.021** (0.010)	-0.022** (0.010)	-0.059*** (0.010)	-0.007 (0.011)
Losses	-0.014*** (0.005)	-0.01 (0.008)	-0.012* (0.007)	0.006 (0.008)
Dividend Paying Firm	-0.091*** (0.008)	-0.098*** (0.011)	-0.085*** (0.010)	-0.118*** (0.012)
Payout	0.017*** (0.004)	0.011 (0.008)	0.007 (0.007)	0.016* (0.009)
Lifecycle Stage	-0.073*** (0.013)	-0.080*** (0.015)	-0.148*** (0.015)	-0.076*** (0.016)
Market-to-book	0.034*** (0.013)	-0.01 (0.056)	0.002 (0.049)	-0.025 (0.058)
Additional Firm Controls	Yes	Yes	Yes	Yes
Firm Fixed Effect	Yes	Yes	Yes	Yes
Industry-by-Year Fixed Effects	Yes	Yes	Yes	Yes
First-stage F-stat	231.9	169.3	169.3	169.3
T-stat on Instrument in First Stage	9.76	1.76	1.76	1.76
First-Stage $R^2$	49%	49%	49%	49%
Adjusted $R^2$	68%	72%	72%	63%
Observations	106,171	106,171	106,171	106,171
Unique Firms	10,383	10,383	10,383	10,383

## C Additional Discussion of Potential for Reverse Causality

In this appendix, we examine the alternative hypothesis that institutional shareholders vary in their preferences for firms with high or low leverage. We analyze the response of institutional investors to a break in the linkage between a firm's capital structure and investment in the firm's equity using a difference-in-differences strategy. This strategy allows for an analysis of the effect of restrictions on institutional investment that may arise from leverage clienteles. After the natural break, which occurs when CDS contracts first trade on a firm, institutional investors may hedge the risk-return trade-off attributable to capital structure from investing in the firm.

The introduction of CDS contracts removes the leverage constraints on an institution's investment approach by allowing them to pursue a "capital-structure agnostic" investment approach. The main idea is CDS contract introduction provides a semi-exogenous break in the relationship between leverage and institutional investors, because CDS contracts allow investors to synthetically create whatever leverage they desire for a company. For example, if a potential institutional investor is deterred by the risk-reward trade-off a highly leveraged firm offers, the introduction of CDS removes this deterrent and makes investment in the firm's equity more attractive. The CDS introduction is unique from a put option because it exclusively decouples the leverage risk. While institutional investors have long had the ability to hedge risk by trading options on a firm's equity, we are concerned only with the risk associated with leverage. Only a CDS contract can explicitly compensate the contract-owner during a default or credit event.

By buying or selling this over-the-counter contract, which offers a guarantee against the non-payment of a bond, institutional investors may hedge credit events. Only institutional investors, hedge funds, and investment banks trade in the CDS markets, because the lower bound on the size limit of such a contract is \$5 million, which effectively eliminates most other types of investors. Importantly, a buyer or seller of a swap does not need to own the underlying credit vehicle such as the bond. In fact, according to the Bank for International Settlements, the notional amount of CDS outstanding is 12 times that of the outstanding debt and there is significant ownership by institutions such as pension funds, hedge funds, and insurance companies.

The semi-natural experiment arising from CDS introduction should produce a clean estimate of the relationship between institutional holdings and leverage for several reasons. First, firms cannot elect to have CDS contracts, so the introduction of CDS contracts is independent of their capital structure decision. Second, because CDS introduction was staggered throughout the 2000s, economy-wide or industry-specific time trends as well as other policy changes are unlikely to confound estimates. Third, while CDS introduction is not completely random, by using a conservatively estimated set of control firms, we can account for potential concerns such as CDS contracts being introduced on firms closer to default, CDS only being introduced on firm's with high leverage, or liquidity increases after introduction encouraging institutional investors to change their equity positions (Longstaff, Mithal, and Neis (2005)). For these and similar reasons, when constructing the set of control firms, we control for illiquidity, probability of default, firm-specific beta, leverage, and a wide variety of firm factors.

Because our events occur at different points in time for different firms in our sample, we analyze only a small window around each event. In particular, we limit our analysis to the three years before and after treatment. We do not allow for overlap in treated and control firms; this implies a firm treated in 2006 cannot be a control for a firm that is treated in 2003. Across the universe of firms, multiple firm-specific treatment events occur in a given year, so we organize our data into cohorts. This cohort approach allows us to use a generalized difference-in-differences estimator for multiple events. Our exact specification is as follows:

$$Y_{ict} = \beta Treated \times Post_{ict} + \Gamma X_{ict} + f_{ic} + \delta_{tc} + \epsilon_{ict} \quad (\text{A.1})$$

where  $Y_{ict}$  is institutional holdings for firm  $i$  that is part of treatment cohort  $c$  in year  $t$ ,  $Treated \times Post_{ict}$  is an indicator equal to one for treatment having occurred in cohort  $c$  by year  $t$  and equals zero otherwise,  $X_{ict}$  is a vector of controls,  $f_{ic}$  is a firm-cohort fixed effect, and  $\delta_{tc}$  is a time-cohort fixed effect. Standard errors are clustered at the firm-cohort level. The coefficient of interest is  $\beta$ , which measures the change in  $Y_{ict}$  following a natural break in the restrictions on institutional investment that may arise from leverage clienteles relative to their control firms. Please note that in the above specification, individual  $Post$  and  $Treat$  dummies are purposefully excluded, because

they are collinear with the firm-cohort and time-cohort fixed effects.

The data used for the natural experiment comes from Markit, a provider of CDS quote data since 2001. [Figure C1](#) illustrates the two main variables we extract from the data: CDS introduction and CDS depth. The left-hand plot shows the frequency by year that firms first had CDS contracts actively quoted on their outstanding debt. From 2002-2012, the number of CDS introductions ranged from approximately 10 to 110 per year. The right-hand plot shows an equivalent diagram for CDS depth, which we define as the number of contributors providing daily CDS price quotes. Because there is no central clearing institution for these contracts, exact CDS trade data is not available. However, Markit collects quotes from major banks that provide pricing data from their record books and trading system feeds. Markit’s data has previously been used in academic research (see, e.g., [Feldhutter, Hotchkiss, and Karakas \(2014\)](#)), and our definition for CDS depth closely matches such definitions. From 2002-2012, the plot reveals the typical number of contributors is 4.

As further tests of the underlying identification assumptions, we estimate two triple-differences models. Our exact specification is as follows:

$$Y_{ict} = \beta_1 Treated \times Post \times High_{ict} + \beta_2 Post \times High_{ict} + \beta_3 Treated \times Post_{ict} + \Gamma X_{ict} + f_{ic} + \delta_{tc} + \epsilon_{ict} \quad (\text{A.2})$$

where  $Y_{ict}$  is institutional holdings for firm  $i$  that is part of treatment cohort  $c$  in year  $t$ ,  $Treated \times Post \times High_{ict}$  is an indicator equal to one for treatment having occurred in cohort  $c$  by year  $t$  for the firms with high exposure and zero otherwise. High exposure means a treated firm is in the top half of the treatment distribution in the year prior to treatment for a pre-determined covariate of interest; in this case, the first covariate of interest is CDS depth and the second covariate of interest is leverage.  $Treated \times Post_{ict}$  is an indicator equal to one for treatment having occurred in cohort  $c$  by year  $t$  and equals zero otherwise,  $Post \times High_{ict}$  is an indicator equal to one for the firm’s with high exposure in cohort  $c$  by year  $t$  and equals zero otherwise,  $X_{ict}$  is a vector of controls,  $f_{ic}$  is a firm-cohort fixed effect, and  $\delta_{tc}$  is a time-cohort fixed effect. Standard errors are clustered at the firm-cohort level. The coefficient of interest is  $\beta_1$ , which measures the change in  $Y_{ict}$  following a natural break in the restrictions on institutional investment that may arise from leverage clienteles

for high exposure firms relative to their control firms.

Panel A of [Table CI](#) presents our estimates of the change in institutional holdings following a break in the restrictions on investment that may arise from leverage clienteles. Column (1) reveals that institutional holdings increases by 4.5 percentage points, on average, for treated firms relative to control firms. The increase in institutional holdings after the introduction of CDS suggests that institutional sorting contributes to the negative interrelation between leverage and institutional ownership and that leverage clienteles exist. Column (2) shows that our results do not rely on the firm-level controls selected. In a difference-and-differences design adding controls may be inappropriate if the controls are also likely to be affected by treatment. The advantage of adding controls that are unaffected by treatment is that they improve precision of the estimate. When treatment is truly random, adding controls should not affect the actual estimate, rather they should only help to lower the standard errors. This is precisely what Column (2) reveals – rather than being significant at the 1% level, the estimate is significant at the 5% level in the regression without firm-level controls.

Although one can never directly test the underlying identification assumptions for a difference-in-differences strategy, we do perform several additional falsification tests to support our design's validity. First, we compare pre-treatment observables. Panel B shows that across several covariates our set of control and treatment firms are statistically indistinguishable prior to treatment. The covariates include: firm size, tangibility, intangibility, collateral, profitability, losses, dividend paying firm, payout, lifecycle stage, market-to-book, investment, sales growth, asset growth, firm risk, market risk, adjusted firm returns, Amihud's illiquidity measure, Altman's Z-score, and insider ownership. Second, we check that the observed change in institutional holdings coincides with the timing of the event. Column (3) of Panel A shows that the observed effect on institutional ownership is statistically indistinguishable from 0 in the years prior to treatment. Third, we check for treatment reversal. Column (4) of Panel A shows that the observed effect on institutional ownership does not depend solely on the year following treatment; instead, the effect remains statistically significant and strong in the second and third year after treatment. Such evidence is consistent with empirical evidence that shows the introduction of CDS contracts helped to alleviate frictions

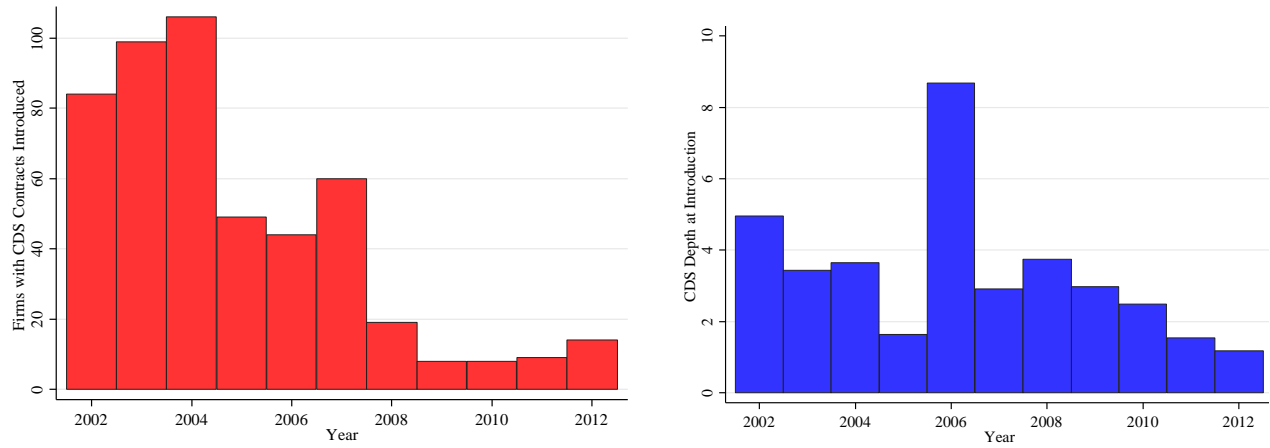
on the supply side of the market well after initial credit concerns (Saretto and Tookes (2013)).

The two triple-differences models split on cases where theory tells us treatment effects should be larger for one subset of observations. Specifically, treatment effects should be larger for equities with high CDS depth. As such, we divide treated firms into high and low CDS depth based on the median. Column (5) of Panel A shows the estimate of  $\beta_1$  is much stronger for equities with high CDS depth; institutional ownership increases by 9.8 percentage points, on average, for these firms. Our second triple-differences model splits firms into high and low leverage based on the median. If a potential institutional investor is deterred by leverage that is too high, the introduction of CDS removes this deterrent and makes the firm more attractive. For more levered firms, such an introduction is theoretically more beneficial. Column (6) shows the estimate of  $\beta_1$  is again stronger for equities with high leverage.

Figure C2 is the visual equivalent to Table CI; it illustrates our main results and provides a falsification test of the “parallel trends” assumption. The left-hand side plot shows the treated firms in red and control firms in blue. Both sets of firms follow parallel paths prior to the introduction of CDS contracts but diverge post-introduction. The observed “parallel trends” prior to treatment suggest difference-in-differences estimates are unbiased estimates of the average treatment effect for treated firms, because absent the treatment the change in institutional ownership likely would have been the same for the two sets of firms. The right-hand side plot illustrates an increase in institutional ownership following a shock to the linkage between leverage and institutional investment due to the introduction of CDS. In addition, 95% confidence intervals are plotted. From the plot, it is evident that prior to treatment, the institutional holdings of the two sets of firms were indistinguishable, but following treatment, institutional holdings increased by an amount statistically greater than 0 for the treated firms.

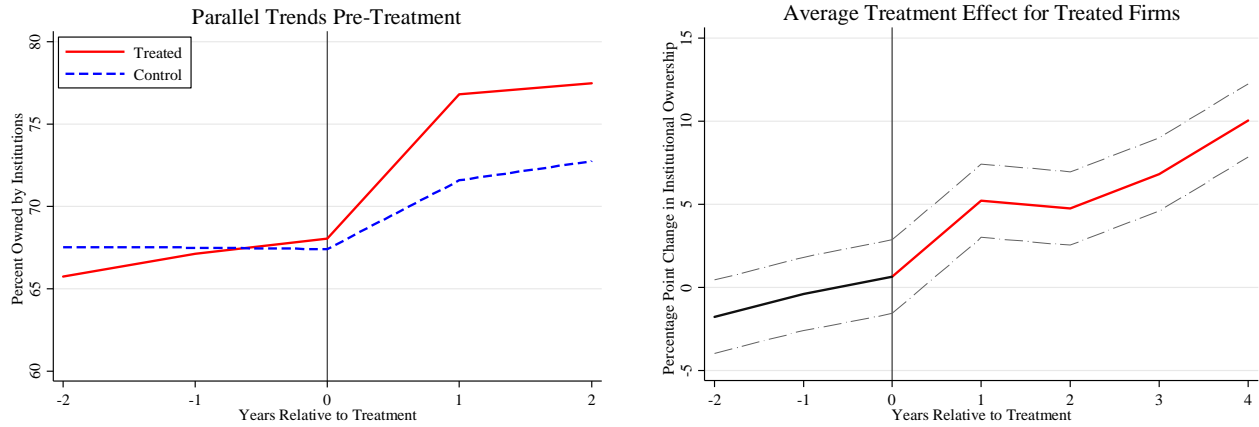
While the evidence suggests the semi-natural experiment, which allows us to test the response of institutional investors to a break in the linkage between a firm’s capital structure and investment in the firm’s equity is internally valid, external validity may still be an issue. The firms that we used in the experimental design have greater market capitalizations and institutional holdings than the average firm in the universe of public firms. Further, the time period under study is

concentrated in the 2000s, and the credit risks during this time period may have been perceived to be higher by investors. Despite the unique setting, we believe the underlying theory for why we observe what we observe – leverage clienteles – is likely to generalize to other settings and apply elsewhere. Our evidence to suggest generality stems from the fact that the main results hold when we focus specifically on the holdings of the top institutional investors. For both large and small firms, institutions are now, more often than not, the majority investor group. This implies that even these smallest firms have at least a few institutional investors, so our results would remain unchanged for those firms. Finally, while we motivate the introduction of CDS contracts as a way of breaking the direct clientele link because it allows institutional investors to synthetically create the capital structure they desire, CDS contracts also create an indirect break because they may alter institutional investors' monitoring incentives. For example, CDS contracts alter bondholders' incentives to monitor and reveal information about their credits, and sophisticated institutional investors may take advantage of this altered environment following the introduction of CDS by investing more.



**Figure C1. Introduction of CDS:** The left-hand side plot shows the frequency by year that firms first had CDS contracts trade on their outstanding debt. The right-hand side plot shows the CDS depth, which we define as the typical number of contributors providing daily CDS price quotes for the firms that first had CDS contracts trade on their outstanding debt. The data come from Markit and cover the period from 2002 through 2012.





**Figure C2. Evidence for Leverage Clienteles:** The left-hand side plot shows the treated firms (i.e., those with CDS contracts) as a red, solid line and the control firms as a blue, dashed line. Both sets of firms follow parallel paths prior to the introduction of CDS contracts but diverge post-introduction. The observed “parallel trends” prior to treatment suggest difference-in-differences estimates are unbiased estimates of the average treatment effect for treated firms, because absent the treatment the change in institutional ownership likely would have been the same for the two sets of firms. The right-hand side plot illustrates an increase in institutional ownership following a shock to the linkage between leverage and investment by institutions due to the introduction of CDS. In addition, 95% confidence intervals are plotted.

**Table CI Leverage Clientele Tests**

Panel A presents estimates of the effect of a semi-natural break in leverage clienteles on institutional holdings. The estimates stem from a difference-in-differences specification outlined in Eq. (A.1), where  $Post \times Treat_{ict}$  is an indicator equal to one for treatment having occurred in cohort  $c$  by year  $t$  and equals zero otherwise. Columns (5) and (6) present estimates from a triple-difference specification outlined in Eq. (A.2), where  $Post \times Treat \times High_{ict}$  is an indicator equal to one for treatment having occurred in cohort  $c$  by year  $t$  for the firm's with high exposure and zero otherwise. High exposure means a treated firm is in the top half of the treatment distribution in the year prior to treatment for a pre-determined covariate of interest; in this case, the first covariate of interest is CDS depth and the second covariate of interest is leverage. Robust standard errors, clustered at the firm-cohort level, are reported in parentheses. Panel B of this table presents the summary statistics, prior to treatment for a set of firm characteristics. Firms for which CDS contracts were introduced are the treated group, and the control group includes firms matched based on propensity-score design estimated from a logit regression with one-year lags of all of the covariates listed in Panel B. \*\*\*, \*\*, and \* represent statistical significance at the 1%, 5%, and 10% level, respectively.

Panel A. Dep. Var. = Institutional Holdings	(1)	(2)	(3)	(4)	(5)	(6)
Treated x Post	0.045*** (0.017)	0.043** (0.017)				
Treated x Year = $t-1$			0.006 (0.009)			
Treated x Year > $t$				0.036*** (0.013)		
Treated x Post x High CDS Depth					0.093** (0.039)	
Treated x Post x High Leverage						0.036 (0.024)
Firm-level Controls	Yes	No	Yes	Yes	Yes	Yes
Firm-Cohort Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes
Time-Cohort Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted $R^2$	66.1%	66.1%	65.9%	66.0%	66.1%	66.0%
Observations	3670	3670	3670	3670	3670	3670

Panel B. Falsification Test, Pre-Treatment Statistics	Controls		Treated		Difference T-statistic
	Mean	SD	Mean	SD	
Institutional Holdings	67.8%	24.9%	67.9%	24.9%	(0.06)
Leverage	32.8%	22.5%	33.6%	20.9%	(0.43)
Profitability	0.140	0.105	0.136	0.098	(0.39)
Market-to-book	1.573	1.207	1.581	1.268	(0.08)
Dividend Paying Firm	0.491	0.501	0.457	0.499	(0.83)
Capital Expenditures	0.058	0.045	0.058	0.052	(0.02)
Tangibility	35.4%	25.1%	34.5%	23.2%	(0.45)
Payout	0.034	0.056	0.032	0.053	(0.23)
Collateral	0.445	0.253	0.437	0.229	(0.39)
Intangibility	20.7%	19.8%	21.4%	18.6%	(0.40)
Losses	0.041	0.199	0.034	0.182	(0.43)
Insider Holdings	3.6%	14.2%	2.7%	5.9%	(0.90)
Beta	1.098	0.588	1.112	0.535	(0.32)
Altman's Z-Score	1.321	2.266	1.363	1.783	(0.25)
Amihud Liquidity	0.086	0.440	0.087	0.539	(0.02)

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