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

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ABSTRACT

Corporate leverage is important for economic stability and decreased markedly in the U.S. since 1992. In contrast to press coverage of hedge funds increasing corporate debt, we find increases in institutional ownership, primarily by mutual funds, account for part of this deleveraging. We use implied mutual fund trades constructed from individual-investor flows as exogenous variation when estimating the interrelation. Supporting the hypothesis institutions contributed to the deleveraging, our estimates increase significantly after regulatory reforms incentivized stronger governance, and an agency mechanism explains the interrelation. Counterfactual simulations indicate aggregate leverage would have been eight percentage points higher today without institutions' influence.

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

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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)). For example, corporate debt accumulations of the 1980s are often cited as a cause of the U.S. recession from 1990-1991. Following that recession, the aggregate leverage of nonfinancial U.S. firms sharply declined which stands in stark contrast to the rise in leverage from 11% to 47% by the early 1990s. 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 from passive to active figures in influencing managerial decisions explains an economically meaningful portion of firm's capital structure decisions as well as the aggregate deleveraging trend since 1992.

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. Without random assignment, omitted variables produce biased coefficient estimates in naive regressions.[1] To overcome this bias and other empirical challenges, we use an instrumental

[1] 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

variable strategy to identify our primary effect of interest.

We examine the effect of institutional holdings on capital structure by employing an instrumental variable strategy that uses *implied* mutual fund trades generated from idiosyncratic, individual-investor flows as 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

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.

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 strong negative interrelation after the regulatory reforms and find evidence in support of an agency mechanism. Using three definitions of firms prone to high agency costs, two of which are novel and meant to capture nuances in the ways agency conflicts arise, we find institutions encourage deleveraging more in high agency cost firms. A 10 percentage point increase in institutional ownership reduces leverage by 6.9 percentage points in high agency cost firm, which is higher than the 4.9 percentage point decline in the average firm. The agency mechanism is further supported by additional tests examining the structure of the deleveraging as well as the governance role of creditors. Specifically, we use a comprehensive database on types of debt employed from Capital IQ and find increased institutional ownership leads firms to retire their arm's-length debt (bonds, notes, and commercial paper) while keeping their closely-monitored bank debt. We find no change in equity. Examining the contrac-

tual terms of the bank debt, we find that bank's strengthen the covenants on the firm's term loans following an increase in institutional ownership. Stronger contractual terms point to a reason why mutual funds' would want lower leverage and debt owned by bank creditors. Roberts and Sufi (2009) find new covenant restrictions influence firm behavior and Nini, Smith, Sufi (2012) show creditors through covenant monitoring play an important governance role that leads firm operating performance and equity valuation to improve.

Fourth, we compare the approach taken by mutual funds to influence managerial decision-making with respect to leverage to that of hedge funds. Using data on hedge fund activism, we find evidence consistent with Brav et al. (2008) that 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.

In conducting our analyses, we investigate and rule out several alternative explanations for the observed interrelation and deleveraging trend. For example, we examine the extent to which institutional sorting, as opposed to institutional influence post investment, contributes to an interrelation between leverage and institutional ownership. To estimate the effect from this reverse causality hypothesis, we use 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 (i.e., 1/10th of that stemming from institutional investors to leverage) that it cannot explain the deleveraging trend. Similarly, we examine and show our results are robust to controls for the arrival of new tech firms in the 1990s, the emergence of the junk bond market, and the expansion of financial assets resulting from overfunded pensions in the 1990s.

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 (Demirguc-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)), investment (Cronqvist and Fahlenbrach (2009)), 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 general economic and financial stability (e.g., Bernanke and Gertler (1990); Gertler and Hubbard (1993)).

Finally, our research informs the literature on shareholder activism. Heterogeneous effects across types of institutional investors is consistent with investment-horizon mattering for firm outcomes (Chen, Harford, and Li (2007)). The contrast between potentially short-horizon hedge funds and other long-horizon institutions such as mutual funds provides an explanation for the often contradictory findings about the overall effect of increased shareholder power on firm value; they range from negative (Comment and Schwert (1995)) to positive (Cunat, Gine, and Guadalupe (2012)) to a nuanced trade-off between short and long-term value creation (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 change statistically 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. These results which suggest capital structure instability is prevalent are

consistent with the main findings in DeAngelo and Roll (2015). To learn that these variation over time also extend to institutional ownership 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 Pfliederer (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. A final potential concern lessened with the construction of our instrument is indirect correlation with unobservable firm or market conditions. If markets exhibit informational efficiency, even on a quarterly delayed frequency as that is our projection frequency, which seems likely given evidence for informational efficiency at much shorter frequencies and the prevalence of arbitrage traders (e.g., see Ljungqvist and Qian (2014)), then implied trades contain no information content, even when an individual-investor's trade contained some information or sentiment (Frazzini and Lamont (2008)). This is consistent with empirical evidence that suggests individual-investors' trades do not 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:

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

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, δ_{jt} is an industry-by-year fixed-effect, and ϵ_{ijt} is the unobservable error component.

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 significantly 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 as well as the construction of the instrument do not change our conclusions. Appendix Table BI shows the results from 14 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 a 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 quantiles 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, τ.

$$Q(Leverage_{ijt}|\tau) = \alpha(\tau) + \beta(\tau)Holdings_{ijt} + \gamma(\tau)Z_{ijt} + f_i(\tau) + \delta_{jt}(\tau) + \epsilon_{ijt}(\tau) \tag{2}$$

Observations are at the firm-year level. $Leverage_{ijt}$ represents 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, δ_{jt} is an industry-by-time fixed-effect, and ϵ_{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 τ 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 mangers' 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?

In this section, we propose an alternative mechanism whereby a segment of institutional investors, primarily mutual funds but also pension funds, and insurance companies, rationally want to reduce corporate leverage and concentrate outstanding debt in banks as a way to enhance operational performance. The intuition for the mechanism is as follows: if institutional investors can influence firms' financial policy such as capital structure and dividend decisions but not operational decisions, these institutions may encourage firms to reduce arm's-length debt while maintaining bank debt as they push for overall lower levels of debt. The overall lower levels of debt reduce the type of agency problems associated with managerial risk aversion and career concerns (Aghion, Van Reenen, and Zingales (2013); Gormley and Matsa (2015)) while concentrating debt in banks helps to improve operational performance. As Nini, Smith, Sufi (2012) show, banks play an important monitoring role that improves firm's operating performance and equity valuation. By concentrating the debt in banks, institutional investors assume the other creditors are exerting behind-the-scenes pressure on managers that extends beyond financial policy to investment and operating policies (Roberts and Sufi (2009); Nini, Smith, Sufi (2009)).

Institutional investors such as mutual and pension funds require a long-term approach to investing which is likely incompatible for the tactics used by hedge funds with respect to corporate leverage, yet because these institutional investors have mandated fiduciary duties, they cannot ignore executives' capital structure decisions. By adopting the tactic proposed above, the mutual funds can satisfy their legal responsibilities and generate greater 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)).

To test the veracity of the proposed mechanism, we provide supportive evidence from three empirical tests. 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? Third, we examine the relationship between institutional ownership and creditor governance. Do creditors strengthen their governance after institutional ownership increases? Finally, we use our empirical setting to understand which institutional approach dominates that of hedge funds or mutual funds. 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, inSection 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 section, we investigate the nature of the deleveraging by examining how firms change their capital structure in response to an increase in institutional ownership. If the deleveraging occurs through a reduction in debt, this would be consistent with the agency cost hypothesis. In contrast, if the deleveraging occurs through equity issuance, this would support an alternative mechanism such as reduced information asymmetry.

Beyond the debt vs. equity analysis, we examine the number and types of creditors as well as type of debt instruments used by the borrowers. A key trade-off highlighted by the theoretical literature on debt structure is the choice of arm's-length vs. closely-monitored debt (Diamond (1991)). If institutional investors' changing role post regulatory reform led them to serve as an exact substitute for the disciplinary role of debt, the empirical prediction is a reduction in highly-

monitored bank debt. If institutional investors through their increased power post reform facilitate improvements in the reputation of the firm to other creditors, the empirical prediction is a movement from more expensive bank debt to cheaper public debt (Rajan (1992)). Finally, if institutional investors' role as active monitors led them to encourage management to only use highly-monitored debt, the empirical prediction is a reduction in arm's-length debt.

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:

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

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, δ_{jt} is an industry-by-year fixed-effect, and ϵ_{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.

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 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 the firm's debt is more concentrated in arm's length-debt that would be consistent with an asymmetric information story as well if the institutions are enhancing firms' reputations. 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. Overall, the specific debt components suggest an agency mechanism. Any agency mechanism, however, does not appear to be as straightforward as institutional investors' role as active monitors directly substituting for the disci-

plinary role of debt. Instead, the concentration of debt among bank debt suggests a more nuanced agency mechanism such as the one proposed above. Because our analysis of the debt components suggests the rationale behind the deleveraging is institutional investors reducing agency costs and delegating monitoring to those with even greater capabilities of affecting operating policies, the next two subsections probe this hypothesis.

4.1.1 Potential for Agency Conflicts. To further test the veracity of the proposed mechanism, 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, some of which have not been used in the prior literature. 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)). While not all acquisitions represent misuse of cash, we believe our novel empirical definition approximates this phenomenon. We define high agency cost firms as those with two consecutive years of non-core acquisition expenses, which we believe 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 $Char_{jit}$. 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.

4.1.2 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 stronger governance or more active monitoring by banks in the firms with increased institutional ownership. From prior research, we know more active monitoring by banks is associated with improved equity valuations and greater operational efficiency (Nini, Smith, Sufi (2012)). We test the hypothesis that creditors engage in greater governance by analyzing the covenants banks place in their contracts after a firm experiences an increase in institutional ownership. If we observe increases in covenant usage, this suggests banks are monitoring firms more after 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.

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 our 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.

4.1.3 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

While our evidence from the above tests suggest a robust negative association between leverage and institutional holdings, we want to investigate and rule out alternative explanations for the deleveraging trend that are also associated with institutional investors. The first alternative explanation we consider is increased institutional sorting on leverage. In addition, we examine other contemporaneous phenomenon which could have influenced institutional investors beyond the regulatory changes such as the arrival of new tech firms in the 1990s, the emergence of the junk bond market, and the expansion of financial assets resulting from overfunded pensions in the 1990s.

To test this alternative hypothesis that institutional shareholders vary in their preferences for firms with high or low leverage and affirm confidence in the economic magnitude of our results, 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 explain the deleveraging trend in a meaningful way.

Beyond leverage clienteles, could there be some other contemporaneous phenomenon that is driving the deleveraging trend and also closely linked to institutional ownership. One potential explanation suggested by Graham, Leary, and Roberts (2014) is that variation in demand for safe bonds leads to fluctuations in the level of government, corporate, and mortgage debt observed over time. Under this logic, when institutional investors are rebalancing their portfolios from equity toward debt, there will be a decrease in institutional ownership in equity and a proportionate increase in demand for corporate bonds. When demand for corporate bonds is high, firms will cater to investors and issue more debt. When demand for bonds is low, the firms will issue more equity. While this hypothetical scenario would create a negative interrelation, it is not supported by our results from the debt composition change. As shown in Table VI, firms do not issue equity as a way of reducing leverage and instead retire corporate debt.

Another alternative explanation for the deleveraging trend is that firms respond to the interest tax deductibility. If institutional investors' tax liabilities are also changing as a part of a greater tax reform this could explain part of our observed interrelation between institutional investors and leverage. As larger portion of interest can be deducted for tax proposes, corporations will assume more and more debt (Gordon and Lee (2001)). Conversely, as the costs or likelihood of financial distress increases (e.g., because of economic downturns), firms will tend to reduce their debt issuances and outstanding bank debt. When we include controls for a firms marginal tax rate as well as when we examine the coefficients on our year fixed effects, the magnitude of our point estimate remains statistically significant and similar.

In a recession, corporate leverage becomes more important as solvent but illiquid firms are forced into bankruptcy from inability to roll over debt. Our observed phenomenon strengthened significantly after the 1990-1991 U.S. recession. Corporate debt accumulations of the 1980s are often cited as a cause of the U.S. recession from 1990-1991. If learning about the dangers of excessive corporate debt led to a change in norms among investors toward aggregate riskiness,

understanding why the norms changed, which may be hard to ascertain, is key. If the norms changed because of explicit regulation such as the SEC telling investors to be more active, then, this is completely consistent with the results presented. If the norms, however, changed for reasons such as compositional changes in types of firms, availability of funds for leveraged buyouts, and increased ownership by risk-averse foreign investors, this would suggest a plausible alternative mechanism through which institutional investors influenced leverage.

Our inclusion of firm-fixed effects should mitigate some of the concern about the composition of firms, but we do test more exclusively for this by showing that our results hold for a balanced panel of firms that were alive in our sample for more than 20 years as reported in Appendix Table BI. Controls for multinational activity, which may attract foreign investors, as well as controls for credit spreads (our preferred proxy for junk bond attractiveness) do not significantly change our point estimates. This leads us to believe this alternative norm channel is not as important as explicit norm change brought about by the SEC regulation.

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 governance. To quantify economic importance of institutional investors increased power 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 illustrate why these institutional investors rationally prefer the tactic of encouraging firm's to deleverage. Because these institutional investors can influence firms' capital structure but not operational decisions, they encourage firms to reduce arm's-length debt in favor of bank debt as they push for overall lower levels of debt. The overall lower levels of debt reduce the type of agency problems associated with managerial risk aversion and career concerns while concentrating debt in banks helps to improve operational performance and equity valuation.

Supporting this alternative tactic, our evidence indicates the deleveraging occurs through debt not equity and through arm's-length debt not closely-monitored bank debt. The deleveraging effect is stronger in firms with high potential for agency problems, and increased institutional ownership is significantly associated with increased covenant usage in bank loans. Because banks exert the behind-the-scenes pressure necessary to improve firm operational and investment policies, these institutional investors realize improved equity valuation even with lower overall leverage.

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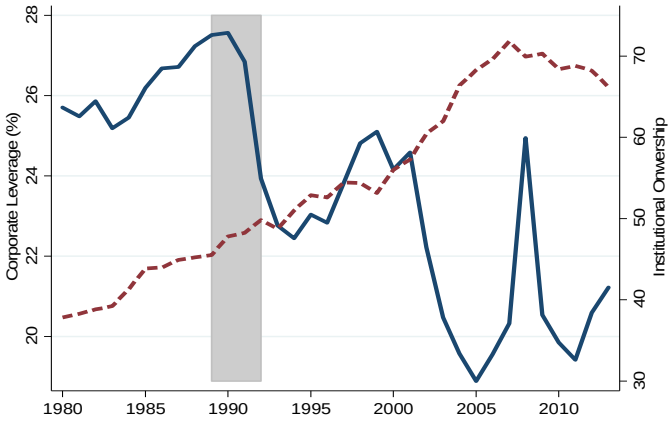
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Figures and Tables



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.

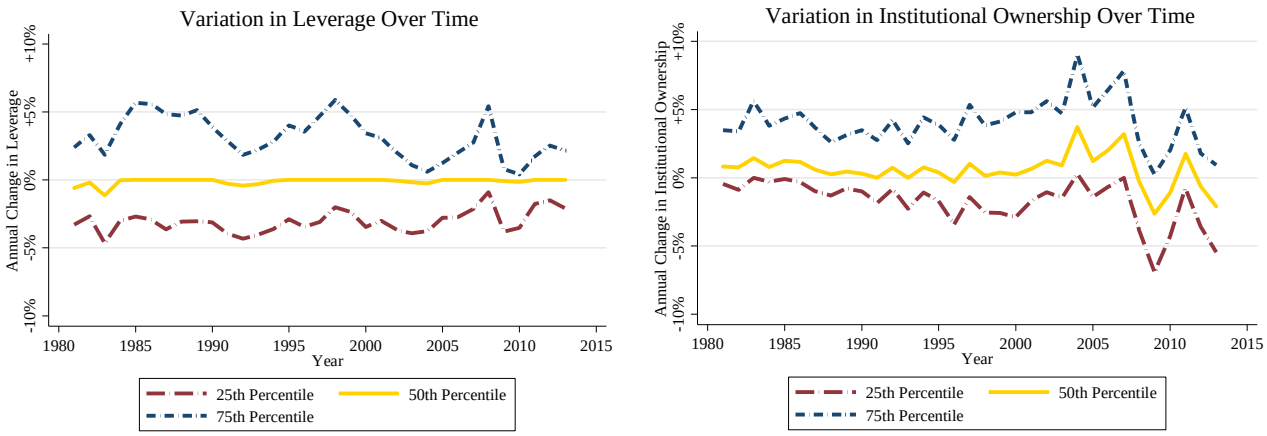


Figure 2. Interquartile Range of Annual Changes in Capital Structure and Equity Ownership: The left-hand side plot shows the 25th, 50th, and 75th percentile for annual within-firm changes in leverage over time. The right-hand plot shows 25th, 50th, and 75th percentile for annual within-firm changes in 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
Given:	Mutual fund's holdings at *t*	\$2M	\$3M	\$5M
Example:	Implied *t + 1* trades from			
	\$1M inflow	\$0.2M	\$0.3M	\$0.5M
	\$1M outflow	-\$0.2M	-\$0.3M	-\$0.5M
Step 1:	Isolate idiosyncratic part of implied *t + 1* trades			
	Investors chasing returns	Remove variation explained by fund returns (Chevalier and Ellison 1997)		
	Investors chasing reputation/skill	Remove variation explained by fund fixed effects (Berk and Green 2004)		
	Focused-funds correlated with specific leverage	Drop funds that only hold firms in the same industry		
Step 2:	Implied *t + 1* trades from idiosyncratic investor-flows			
	\$1M inflow of which \$500,000 is idiosyncratic	\$0.1M	\$0.15M	\$0.25M
	\$1M outflow of which \$500,000 is idiosyncratic	-\$0.1M	-\$0.15M	-\$0.25M

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. This suggests the mutual fund has \$10 million in assets under management at time t. If the mutual fund receives an inflow from individual investors of \$1 million, then the implied trade at time $t+1$ is proportionate investment across the firms. Similarly, if the mutual fund must pay redemption requests equal to an outflow from individual investors of \$1 million, then the implied trade at time $t+1$ is proportionate sale of the mutual funds' equity stake across the firms. 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 second set of steps 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, 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 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/outflow from the top of the figure, if 50% of the variation is explained by performance and skills, we only project the trades attributable to the remaining \$500,000 in unexplained flows.

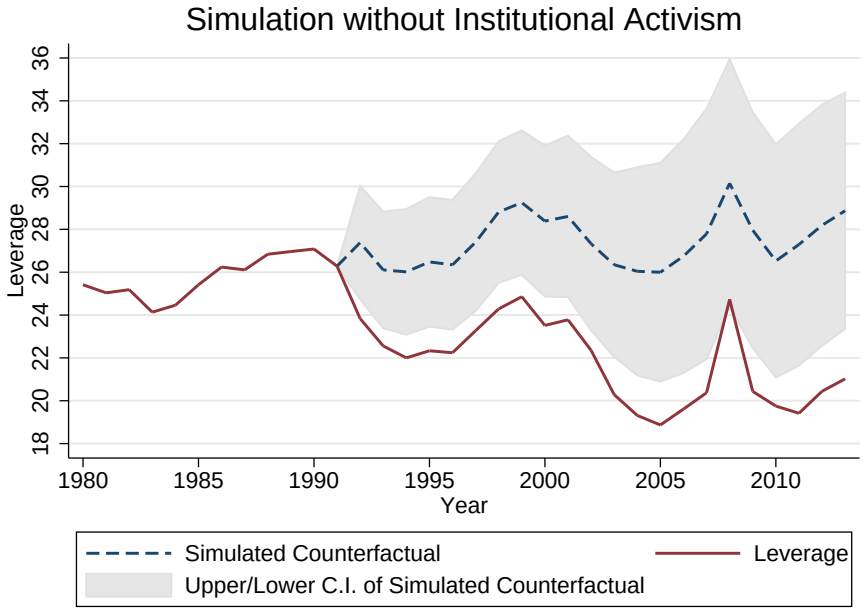


Figure 4. Simulated Counterfactual: The plot depicts actual leverage as a red, solid line for nonfinancial public firms and simulated leverage for nonfinancial public firms as a blue, dashed line. The simulation sets the influence of institutional investors on leverage to levels observed before the 1992 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.

	Full Sample		1980 to 1991		1992 to 2013	
Summary Statistics	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.

Panel A. Partial Correlations with Leverage					
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%
Panel B. Partial Correlations with Leverage including Leases					
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%
Panel C. Partial Correlations with Leverage in terms of Book Value of Capital					
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.

	IV	IV Robustness (Alt. Leverage Measures)	
	(1)	(2)	(3)
Dependent Variable =	Leverage	Leverage including Leases	Leverage in terms of Book Value of Capital
Institutional Holdings	-0.718***	-0.543***	-0.659***
	(0.144)	(0.123)	(0.145)
Firm Size	0.821***	0.507***	0.706***
	(0.090)	(0.077)	(0.090)
Tangibility	0.077***	0.028	0.004
	(0.021)	(0.020)	(0.021)
Intangibility	0.099***	0.084***	0.073***
	(0.009)	(0.008)	(0.009)
Collateral	0.146***	0.197***	0.175***
	(0.020)	(0.019)	(0.020)
Profitability	-0.021**	-0.059***	-0.006
	(0.010)	(0.010)	(0.012)
Losses	-0.014***	-0.015***	0.002
	(0.005)	(0.005)	(0.006)
Dividend Paying Firm	-0.091***	-0.080***	-0.111***
	(0.008)	(0.007)	(0.009)
Payout	0.017***	0.011***	0.022***
	(0.004)	(0.004)	(0.005)
Market-to-book	0.034***	0.031***	0.02
	(0.013)	(0.011)	(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 (1980-1989)	Post Onset to Pre SEC Reform (1989-1992)	Post SEC Reform (1992-2013)
Institutional Holdings	-0.718***	0.290	-0.411	-1.146***
	(0.144)	(0.402)	(0.374)	(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 (τ)	Coefficient at τ on Institutional Holdings	Leverage (τ)	Estimate of Leverage (τ) After 10% Increase in Institutional Holdings, All Else Equal
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

	Debt				Equity	
Dependent Variable =	Short-Term	Long-term	Issuance	Issuance (Net Retirements)	Buybacks	Issuance (Net Buybacks)
Institutional Holdings	-0.390	-1.554***	-0.616*	-0.405**	-0.266**	0.213
	(0.277)	(0.398)	(0.371)	(0.185)	(0.132)	(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

	Debt				
Dependent Variable =	Bonds and Notes	Commercial Paper	Terms Loans	Capital Leases	Other Borrowings
Institutional Holdings	-1.688***	-2.828***	0.378	-0.043	0.671
	(0.495)	(0.982)	(0.330)	(0.375)	(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.

	High Potential for Agency Conflicts		
Dependent Variable = Leverage	Defn. 1.	Defn. 2.	Defn. 3.
Institutional Holdings	-0.698***	-0.698***	-0.706***
	(0.146)	(0.144)	(0.147)
Institutional Holdings x Agency Conflicts	-0.151**	-0.014	-0.114
	(0.060)	(0.071)	(0.228)
Agency Conflicts	0.275***	0.061*	0.093
	(0.085)	(0.036)	(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***	0.640***	0.342*	0.448**
	(0.144)	(0.235)	(0.184)	(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***	1.220**	1.354***	1.455***
	(0.234)	(0.527)	(0.399)	(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***	-0.715***	-0.720***	-0.716***
	(0.144)	(0.144)	(0.145)	(0.144)
Hedge Fund Campaign		0.084***		
		(0.025)		
Hedge Fund Activist Crosses 5% Threshold			0.058**	0.065***
			(0.023)	(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**	-0.681**	-0.691**	-0.692**
	(0.319)	(0.317)	(0.320)	(0.322)
Hedge Fund Activism Campaign		0.042*		
		(0.022)		
Hedge Fund Activist Crosses 5% Threshold			0.023	0.034
			(0.020)	(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.

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.

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

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

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

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

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

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.

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

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

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

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

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

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

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.

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 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.

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 builds on recent research. The use of implied rather than actual trades differs from Coval and Stafford (2007). The use of idiosyncratic inflows and outflows rather than exact outflows 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, but unlike Edmans, Goldstein, and Jiang (2012) we do not assume the implied trades are only correlated with market prices and friction-driven price pressure. In contrast, we allow for greater flexibility because we assume friction-driven price pressure is just one potential source

of systematic correlation between implied mutual fund trades and actual institutional investors' holdings.

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.

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

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

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

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

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

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.

BN = ($capitalstructuresubtypeid = 4$) = bonds and notes,

CP = ($capitalstructuresubtypeid = 1$) = commercial paper,

TL = ($capitalstructuresubtypeid = 3$) = 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.

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.

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
Baseline Effect of Institutional Holdings on Leverage	-0.718***	106,176	231.9	9.76
	(0.144)			
Panel A. Robustness Checks with Additional Covariates				
Analyst Coverage	-0.706***	106,176	231.9	9.76
	(0.150)			
Variation in External Financing Costs	-0.677***	87,903	69.8	8.36
	(0.166)			
Credit Ratings	-0.733***	106,176	231.9	9.76
	(0.144)			
Tax Rates	-0.645***	101,835	59.2	9.46
	(0.147)			
Research & Development Activity	-0.713***	106,176	231.9	9.76
	(0.145)			
Domestic Only Operations	-0.719***	106,176	231.9	9.76
	(0.145)			
Limiting Sample to Firms with 10	-0.798***	76,225	224.8	8.18
Year History of Financial Data	(0.176)			
Limiting Sample to Firms with 20	-0.758***	37,677	28.0	5.29
Year History of Financial Data	(0.263)			
Panel B. Robustness Checks with Alternative Definitions of Institutional Holdings				
Percentage Held by Top 5 Institutions	-0.479***	106,176	92.87	10.18
	(0.100)			
Percentage Held by Top 10 Institutions	-0.508***	106,176	58.09	10.67
	(0.104)			
Percentage Held by Blockholders	-0.516***	106,176	65.47	7.61
	(0.118)			
Panel C. Robustness Checks with Alternative Instrument Construction				
Without Fund Fixed Effects	-0.710***	106,176	230.96	10.03
	(0.140)			
Using Linear Functional Form for Fund Returns	-0.693***	106,176	233.54	11.06
	(0.125)			
With Funds Experiencing >200% Change in AUM	-0.852***	106,176	339.83	10.03
	(0.151)			

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.

	Original IV	Placebo IV	Placebo IV Robustness (Alt. Defn.)	
	(1)	(2)	(3)	(4)
Dependent Variable =	Leverage	Leverage	Leverage including Leases	Leverage in terms of Book Value of
Institutional Holdings	-0.718***	-0.160	-0.177	-0.093
	(0.144)	(0.699)	(0.614)	(0.731)
Firm Size	0.821***	0.487	0.288	0.368
	(0.090)	(0.417)	(0.366)	(0.436)
Tangibility	0.077***	0.070***	0.023	-0.004
	(0.021)	(0.023)	(0.021)	(0.023)
Intangibility	0.099***	0.105***	0.088***	0.079***
	(0.009)	(0.011)	(0.010)	(0.012)
Collateral	0.146***	0.173***	0.215***	0.203***
	(0.020)	(0.039)	(0.035)	(0.041)
Profitability	-0.021**	-0.022**	-0.059***	-0.007
	(0.010)	(0.010)	(0.010)	(0.011)
Losses	-0.014***	-0.01	-0.012*	0.006
	(0.005)	(0.008)	(0.007)	(0.008)
Dividend Paying Firm	-0.091***	-0.098***	-0.085***	-0.118***
	(0.008)	(0.011)	(0.010)	(0.012)
Payout	0.017***	0.011	0.007	0.016*
	(0.004)	(0.008)	(0.007)	(0.009)
Lifecycle Stage	-0.073***	-0.080***	-0.148***	-0.076***
	(0.013)	(0.015)	(0.015)	(0.016)
Market-to-book	0.034***	-0.01	0.002	-0.025
	(0.013)	(0.056)	(0.049)	(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:

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

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 δ_{tc} is a time-cohort fixed effect. Standard errors are clustered at the firm-cohort level. The coefficient of interest is β, 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:

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

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 δ_{tc} is a time-cohort fixed effect. Standard errors are clustered at the firm-cohort level. The coefficient of interest is β_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 β_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 β_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.

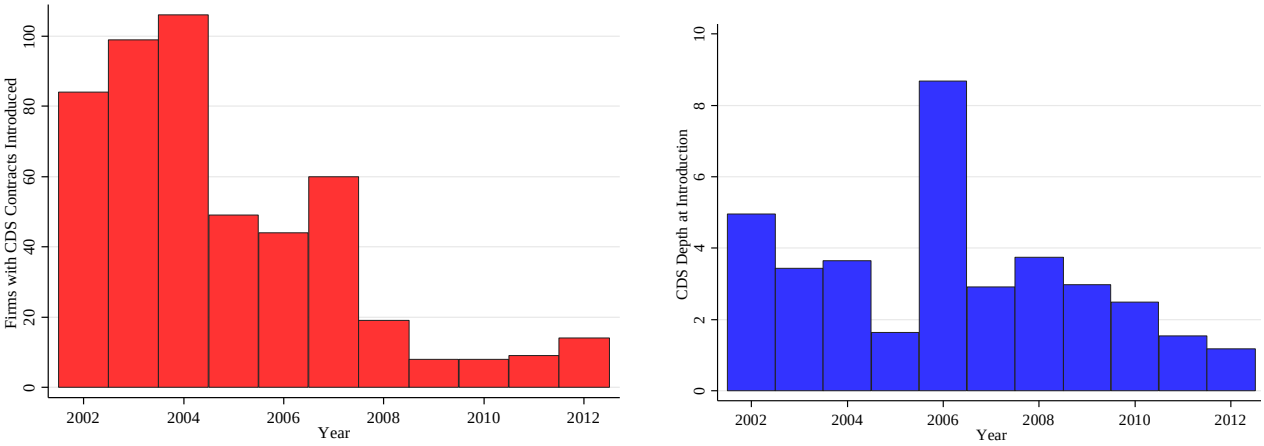


Figure C1. Introduction of CDS: The left-hand side plot shows the frequency by year that firms first had CDS contracts trade on their outstanding debt. The right-hand side plot shows the CDS depth, which we define as the typical number of contributors providing daily CDS price quotes for the firms that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

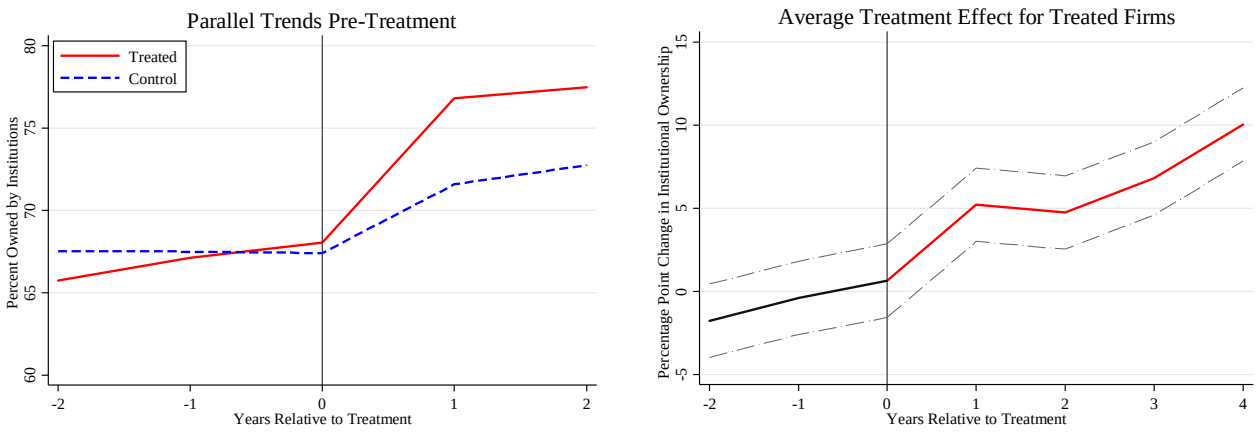


Figure C2. Evidence for Leverage Clienteles: The left-hand side plot shows the treated firms (i.e., those with CDS contracts) as a red, solid line and the control firms as a blue, dashed line. Both sets of firms follow parallel paths prior to the introduction of CDS contracts but diverge post-introduction. 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***	0.043**				
	(0.017)	(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

	Controls		Treated		Difference T-
Panel B. Falsification Test, Pre-Treatment Statistics	Mean	SD	Mean	SD	statistic
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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