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

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ABSTRACT

U.S. corporate leverage has decreased markedly since 1992. We find greater institutional ownership explains this deleveraging trend. Without institutions' influence, total leverage would be eight percentage points higher today. Detection of this relationship was elusive since researchers often combine all years and institutions. Yet legal barriers to institutional activism persisted until 1992. And while hedge funds may advocate for more debt, other institutional investors drive the deleveraging. We find active institutions with longer investment horizons drive the deleveraging. Their desire for change balances short-term gains with long-term financial distress. Supporting this conclusion, we find high-leverage and high-distress firms deleverage more, and they offset debt reductions with other agency-reducing incentives (i.e., covenants and dividends).

JEL classification: G3, G32, G34, G38, K22, E44

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1. Introduction

Economists have long called attention to the dangers of excessive corporate leverage. By reducing financial stability, corporate leverage amplifies the effects of adverse shocks to the economy (Bernanke and Gertler (1989)). The total leverage of non-financial, U.S. firms reached a peak of 47% in 1992 but has declined sharply since then. What changes in market conditions or business practices brought about this deleveraging trend? Leading explanations such as changes in tax rates, bankruptcy costs, and buyout rates cannot account for the trend (Graham, Leary, and Roberts (2014)). In this paper, we show the strengthening of power by active institutional investors explains part of the deleveraging.

Beginning in the late 1980s, legal reforms strengthened the power of institutional investors (Rock (2015)). Supplied with newfound rights, it is possible institutions could influence corporate leverage decisions. While the rise in ownership by institutions is well-documented (e.g., Aghion, Van Reenen, and Zingales (2013)), prior research has yet to detect a relationship between institutions and leverage. We argue the relationship remains unknown since researchers often combine all time periods and institutional investors. Of course, institutions are not all the same. These investors come in many different forms and with many different characteristics. They have a wide variety of distinct goals, strategies, and timeframes for their investments. As a result, their relationship with, and impact on, corporate leverage may manifest in different ways.

For example, passive institutions have limited resources to beat the benchmark. This deters them from engaging with executives over leverage (Appel, Gormley, and Keim (2016)). In contrast, hedge funds are known to engage over leverage (Brav et al. (2008)). Anecdotes also suggest active institutions advocate for leverage changes (e.g., New York Times (1990a)); New York Times (1993)). In theory, the three attributes of institutions that lead to varying degrees of impact on leverage are: (1) credible threat of exit; (2) resources available to beat the benchmark; and (3) investment horizon. While both hedge funds and active institutions may seek to influence leverage, active institutions have longer investment horizons than hedge funds (Cremers and Pareek (2016)). This creates a tension between recommending leverage levels that maximize short-run and long-run returns. Given that the probability of financial distress represents a long horizon and distressed stocks have abnormally low risk-adjusted returns (Campbell, Hilscher, and Szilagyi (2008)), we

hypothesize that active institutions will recommend lower debt levels that better optimize the long-term trade-off between high-powered incentives and financial distress than their hedge fund counterparts.

To test our thesis that active institutions lead firms to deleverage, we use a multi-pronged empirical approach. Such an approach helps address the challenge that neither leverage nor institutional ownership is randomly assigned.[1] To overcome this bias and other empirical challenges, we first test for a relationship between active institutional ownership and leverage using an instrumental variable. Our instrument is *implied* mutual fund trades, which we construct at the equity-year level. In construction, we use the idiosyncratic part of individuals' flows to and from mutual funds. We define the idiosyncratic part of individuals' flows to be orthogonal to market and fund characteristics. An idiosyncratic flow may arise when an individual withdraws cash from a fund for liquidity needs. It may also arise when an individual invests cash in a fund from a newfound inheritance. The key feature of idiosyncratic flows is they are not predictable yet correlate with active investors' trades (Coval and Stafford (2007); Edmans, Goldstein, and Jiang (2012)). Another key feature of our instrument is that it is an *implied* trade. This means we create a projection of how much each fund will trade based on the idiosyncratic individual flows. The projection eliminates indirect correlations between actual trades and other determinants of firm's leverage. Without any correlations to firm leverage, the instrument likely meets the exclusion restriction.

Second, we examine the relationship between active institutional ownership and leverage over time to understand how it relates to total corporate leverage. We continue to analyze within-firm variation rather than cross-sectional variation for identification purposes. To begin, we apply our instrumental variable approach to two different samples. Our first sample is the period before the legal reforms that strengthened the power of institutional investors. Our second sample is the period after the reforms. Then, we test for differences in these well-identified point estimates using Chow's test. We supplement this subsample approach by using the change in legal reforms as a quasi-natural experiment. We apply a difference-in-differences estimator. The first difference

[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 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.

is time, pre-reform vs. post, and the second difference is high active institutional ownership vs. low, which differences out the time trends that affected both treated and control firms. These complementary approaches reduce the extent to which identification challenges may influence our inferences.

Our analyses provide evidence for four new stylized facts. First, we find a negative relationship between leverage and active institutional investors. The instrumental variable strategy reveals a one standard deviation increase in active institutional ownership reduces leverage by 0.25 standard deviations. This means a 10 percentage point increase in active institutional ownership reduces leverage by 4.7 percentage points. This marginal effect is larger than that of profitability and tangibility but smaller than that of firm size.

Second, we find the legal reforms that strengthened the power of institutional investors allowed for a relationship between institutional ownership and corporate leverage to develop. Our instrumental variable approach reveals no relationship between active institutional ownership and leverage from 1980 to 1991. In contrast, a strong relationship is evident from 1992 to 2013. Our difference-in-differences estimator indicates a quick change in relationship. Using a three-year window around the reform, we find treated firms reduce leverage by 1.9 percentage points per year relative to control firms.

Third, we find the influence of active institutional investors dominate those of hedge funds. We find hedge fund activism leads to an increase in leverage but the economic size is small. While active institutional ownership reduces leverage by 0.25 standard deviations, a hedge fund activism campaign only increases leverage by 0.08 standard deviations. Moreover, hedge fund activism is much rarer than active institutional ownership. Evaluating the combined influence of institutional investors over time, we find they account for 11% of the total corporate deleveraging since 1992. By simulating each firm's leverage evolution had the relationship between institutional ownership and leverage not developed, we find total leverage would be eight percentage points higher.

Fourth, we find evidence consistent with the notion that active institutional investors consider the trade-off between agency-reducing incentives and financial distress when recommending deleveraging. The firms encouraged to deleverage do so by reducing long-term debt rather than through other types of debt or equity. Such an approach is consistent with executives making an intentional change to deleverage (Welch (2004)). We also find the firms influenced to deleverage are those

with higher probabilities of financial distress. Finally, our evidence suggests the firms influenced to deleverage adopt alternative agency-reducing mechanisms such as covenants and dividends.

To understand the robustness of our findings, we investigate several alternative explanations for our findings. We find the composition of firms does not affect our findings, in particular the arrival of tech firms or the departure of domestic-only firms. Our results are robust to variations in corporate and government debt supply, external financing costs, tax rates, leveraged buyouts, and corporate governance mandates. We study alternative mechanisms such as crowding out of activist investors by index funds and investment strategies related to valuation and beta anomalies. Such mechanisms do not explain our results. We explore variations in the definition of active institutional investors as well as alternative constructions of our instrumental variable. These do not change our findings. We run a placebo test that uses an alternative construction of our instrument. Specifically, we project implied mutual fund trades back in time rather than forward. If our instrument was spuriously correlated with some unobservable persistency, then, using a backward looking instrument should produce similar point estimates to our forward looking instrument. Yet we find no effect when we implement this placebo test. Finally, we examine if institutional sorting on leverage (i.e., reverse causality) is contributing to the deleveraging trend. Our evidence from this test suggests leverage clienteles exist, yet their size is so small they cannot rationalize deleveraging.

Our work relates to a number of strands in the literature. First, our findings add to research stressing how institutions shape firm policy (Grinstein and Michaely (2005); Aghion, Van Reenen, and Zingales (2013)). More broadly, this connects to research on optimal governance by showing institutions can provide advice and counsel rather than just discipline for executives (Adams and Ferreira (2007)). Second, our research complements studies emphasizing heterogeneity among institutions (Kahan and Rock (2007); Appel, Gormley, and Keim (2016)). Third, we add to research considering legal, financial, and institutional systems globally, and their effect on leverage (Welch (2004); Graham, Leary, and Roberts (2014)). Finally, our evidence advances the work examining when governance and activism have divergent effects on performance (Bebchuk, Brav, and Jiang (2015); Popadak (2016)).

2. Background, Theory, and Empirical Implications

2.1. Background: The Evolution of Institutional Investor Power

Shareholder passivity was the norm in the early 1980s. While institutions had already grown to a substantial size and owned significant percentages of individual companies, only 0.3% of the time between 1981 and 1985 did they not approve managements' choices for directors and control over the annual agenda (Schrager (1986)). As such, legal scholars began to argue that shareholder passivity was a function of legal rules more than traditional dispersed ownership arguments (e.g, Coffee (1991); Roe (1991); Black (1992)). As Black (1990) argues "institutional shareholders are hobbled by a complex web of legal rules that make it difficult, expensive, and legally risky to own large percentage stakes or undertake joint efforts. Legal obstacles are especially great for shareholder efforts to nominate and elect directors, even to a minority of board seats. Over the last 25 years, as institutional ownership has grown to levels that make shareholder activism feasible, legal obstacles have grown as well."

Policymakers agreed with the assessment that institutional investors had little appetite for activism and beginning in the late 1980s, they set out to change institutional investors' legal powers (Rock (2015)). The first key event in the new era of institutional investor power is the issuance of the Avon Letter in 1988 by the Department of Labor. 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 investor power occurred in 1992 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. This significant reduction in communication costs served to benefit shareholders through a disciplinary effect on management (Sharara and

Hoke-Witherspoon (1993)).

The argument that institutional investors' passivity is a phenomenon of the past has empirical support in the governance literature (Adams, Hermalin, Weisbach (2010) cover many papers in their review). For example, MacAvoy and Millstein (1999) find that activist pension funds such as CalPERS attempts to intervene in governance matters in the 1990s is positively correlated with accounting-based measures of firm performance. Similarly, Huson, Parrino, and Starks (2001) show activism amongst investors led to increased CEO turnover during this time period.

This strengthening of shareholder power from 1988 through 1992 also prepared the way for something else to happen. Namely, for a relationship between institutional ownership and corporate policies to evolve. As Romano (1993) put it, "with the lull in takeovers in the 1990s, commentators concerned about corporate performance have turned their attention to identifying alternative mechanisms for disciplining management. The principal solution has been to call for more active monitoring of management by institutional investors." In this paper, we focus on how the relationship between capital structure and institutional ownership changed after this evolution in shareholder power.

2.2. Theory: Capital Structure and Institutional Investors

To develop our hypotheses tests, we build off existing theoretical and empirical work related to capital structure and institutional investors. Capital structure theory shows each firm has a unique capital structure that optimizes trade-offs between agency costs, financial distress, information asymmetry, and taxes (Fama and French (2002)). Overall, the capital structure theory is ambiguous in its predictions of the dominant direction for the relationship between institutional holdings and leverage. 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 (Gilson and Kraakman (1991)) 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.

Each of the aforementioned theories uses the idea that institutional investors impact the severity of the trade-offs that determine a firm's optimal capital structure. Given that not all institutional investors have the same power and preferences, it is necessary to assess which attributes impact the severity of capital structure trade-offs. Figure 1 highlights the different attributes of institutional investors that lead to varying degrees of impact on capital structure trade-offs. The three attributes that Figure 1 highlights are: (1) credible threat of exit; (2) resources available to beat the benchmark; and (3) typical investment horizon. In contrast to passive institutional investors, hedge funds and active institutional investors have a credible threat of exit and resources available to beat the benchmark returns. Having resources available to beat the benchmark returns means they can target specific firms and communicate with management about corporate policies they believe will result in greater shareholder returns. Having a credible threat of exit means they can influence managerial action with regards to these policies. Empirical evidence using index assignment as variation in passive institutional ownership supports the view that passive institutional investors only seek to influence general best practices such as strengthening corporate governance rather than specific corporate policies (Appel, Gormley, and Keim (2016)).

While both hedge funds and active institutional investors seek to influence firm-specific policies such as leverage and payout, as Figure 1 illustrates hedge funds and active institutional investors vary in their investment horizon. This attribute is likely to lead hedge funds and active institutional investors to different policy recommendations. Hedge funds typically have shorter investment horizons (e.g., Brav et al. (2008) report a median holding period of 0.87 years by activist hedge funds). This implies hedge funds may recommend policies that maximize short-run shareholder return. In the context of capital structure policy, this tension between short-run and long-run returns may unfold in the way hedge fund's balance agency costs and financial distress trade-offs. Given that the probability of financial distress represents a long horizon and that distressed stocks have abnormally low risk-adjusted returns (Campbell, Hilscher, and Szilagyi (2008)), hedge fund recommendations are less likely to balance high-powered incentives to improve efficiency with their associated higher probabilities of financial distress. In contrast to hedge funds, active institutional investors are likely to balance the trade-off between agency-related incentives and longer-term prob-

abilities of financial distress. Indirect empirical evidence related to dividends suggests this is the case. Grinstein and Michaely (2005) find that institutional investors prefer dividend paying firms but not high dividend paying firms. Given that managers are reluctant to ever cut dividends, a high dividend payment can be viewed in the same vein as high interest payments resulting from high leverage. Because active institutional investors have long investment horizons, they want to balance the agency-reducing properties of dividends with the probability of completely draining the firm of its financial resources.

Anecdotal evidence supports the view that once institutional investors gained greater power over management in the early 1990s, they advocated for leverage policies that balanced the trade-off between agency-related incentives and longer-term probabilities of financial distress. New York Times (1993) reports institutional investors wanted firms to reduce their leverage because of concerns about whether they could survive a difficult economic environment. As an executive for Sante Fe Energy Resources put it, "Wall Street and the business community almost mandated that we get the debt load down," (New York Times (1990a)). This desire to reduce the probability of financial distress also extended to firms seeking to go public. Safeway had went through a leveraged buyout (LBO) in the 1980s and was seeking an initial public offering (IPO) in the early 1990s. The institutions investing in Safeway's IPO were concerned the company was too debt heavy, so forced Safeway to lower its offering from \$16 to \$11.25 a share (New York Times (1990b)).

Not only were institutional investors concerned about the levels of corporate debt in the early 1990s, they were also concerned about the types of financial leverage and the incentives they provided for management. While institutions recognized that debt may provide high-powered incentives to improve efficiency, they also saw that some managers were using it to entrench themselves. Many institutions believed managers overlevered themselves in the 1980s to protect themselves from buyout funds at shareholders expense (New York Times (1990a)). Another belief among institutional investors was that the growing use of off-balance-sheet financing and derivatives were not serving to discipline management but rather exacerbating uncertainty and potential distress. As one institutional investor summarized it, "[they] aren't as controllable as a bank loan," (Wall Street Journal (1992)). Closely-monitored bank debt provides stronger agency-reducing incentives than arm's-length public debt or more complex forms of debt both theoretically (Stiglitz and Weiss (1983); Diamond (1991)) and empirically (Roberts and Sufi (2009); Nini, Smith, and Sufi (2009)).

This view amongst institutional investors about corporate leverage policies was not limited to this period in the early 1990s. Documents uncovered through the legal discovery process clarify what happens when institutional investors meet to communicate with management. In particular, records from a court case involving HCA in the early 2000s reveal a situation where management wanted to increase the firms leverage. Long-term institutional investors expressed their aversion to the increased risk of financial distress inherent in such a situation. HCA hired a financial expert to evaluate their leverage. The financial advisor then reiterated to management that their stock price would decrease since institutions would exit the stock if they increased their leverage (Merrill Lynch (2006)). This example also serves to highlight the importance of institutions having a credible exit threat when trying to influence corporate policy.

In contrast to the anecdotes about institutional investors preferring lower levels of leverage and a more conservative composition of debt, the popular press detail how hedge funds try to influence firm executives to increase debt levels. Hedge funds engage in more vocal, public displays of activism because their small ownership stakes require them to coordinate with other investors to invoke change (Bebchuk et al. (2017)). The hedge funds argue that by increasing debt levels, executives will improve operational efficiency while simultaneously increasing the hedge funds return on their investment. As the Wall Street Journal (2005) reports, "hedge funds are not like mutual funds when it comes to leverage. The use of leverage need not be a bad thing because the sole purpose of a leveraged industrial corporate is not to use leverage. Rather, leverage is employed as a tool for implementing relative value." Such views about hedge funds and their desire for firms to use leverage is evident in large samples. Bebchuk, Brav, and Jiang (2015) report 19% of hedge fund activist interventions involve a strategy of increasing leverage and payout while decreasing investment.

2.3. Empirical Predictions

The background and theory lead us to several empirical predictions. First, an increase in power by institutional investors in the early 1990s allowed for a relationship between institutional ownership and corporate leverage to develop. Second, the relationship that developed was so meaningful economically that it can explain in part how aggregate corporate leverage evolves in the time series. Next, we posit the detection of this relationship has been difficult because researchers often lump

all institutional investors together. A key thesis of our paper is then that not all institutional investors have the same impact on leverage. We predict that hedge funds and active institutional investors will impact leverage policy but passive institutional investors will not. Further, we predict that hedge funds will want higher levels of leverage than active institutional investors because hedge funds have shorter investment horizons. The variation in investment horizon will lead each type of institutional investors to apply different weights to the trade-off between short-term agency-reducing incentives and longer-term probabilities of financial distress inherent with leverage increases.

3. 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.

To classify institutional investors as either active or passive, we use the classification proposed by Bushee (2001). This is the same procedure used by Appel, Gormley, and Keim (2016). In particular, it classifies "quasi-index" institutions as passive and "transient" or "dedicated" institutions as active. 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 106,537 firm-year observations, with non-missing data for all of the variables used in our analysis. All variables are formally defined in Appendix A.

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 2 plots corporate leverage and institutional holdings over time. First, we use aggregate leverage data compiled by Graham, Leary, and Roberts (2014) from 1920 through 2010. The upper plot shows aggregate leverage increased in the post-war period, peaked in 1992, and subsequently decreased. Second, in the lower plot, we use Compustat data matched to SEC 13F filings of institutional equity ownership to highlight the years around the 1992 peak. Comparing the leverage trend to the institutional holdings trends in that time period begins to suggest a pattern. There appears to be either positive correlation or little correlation between leverage and institutional holdings in the period prior to the onset of institutional activism in the late 1980s, but then, there is a sharp drop in leverage when institutional activism began, which is highlighted in the area with gray shading. Finally, the negative correlation between leverage and institutional ownership persists throughout the decades after the onset of increased shareholder power. While the figure provides suggestive evidence that the correlations shift over time, we rely on our regression analyses when making inferences.

Next, we move from plots of levels to first-differences, because our statistical analyses focus on within-firm variation over time. Figure 3 plots the annual within-firm variation in our two primary variables of interest – financial leverage and active institutional investors. The upper plot shows the 25th, 50th, and 75th percentile for annual within-firm changes in leverage over time. The lower plot shows 25th, 50th, and 75th percentile for annual within-firm changes in active institutional ownership over time. What is striking is how much first-difference variation there is in these variables over time. While the median firm persists with a negligible change in leverage and institutional holdings over time, examining the full distribution of first-differences reveals substantial variation in the upper and lower quartiles. Like the findings in Welch (2004), our figures suggest capital structure instability is prevalent. To see that such instability also extends to the percentage of shares held by active institutions is suggestive of a potentially important interrelation.

Table 1 complements the visual evidence from Figure 2 and Figure 3 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 financial leverage and institutional holdings measures also change substantially over time. Average holdings

nearly doubled from the first time period to the second time period (10.6% to 18.3%). The holdings of the five 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 doubling of the average number of active institutional investors from 16 to 34. 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 summarize other important determinants of leverage such as profitability, firm size, payout and sales growth.

Finally, to characterize the correlations between institutional holdings and leverage, Table 2 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, life-cycle 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 2 focuses on the partial correlations for financial leverage, Panel B focuses on the partial correlations for financial leverage including leases, Panel C for financial leverage where assets are quoted in market value, and Panel D for balance sheet leverage where again assets are quoted in market value. 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 -0.06 to -0.12. 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 -0.05 or -0.10 when institutional holdings is redefined as the percentage of shares held by the largest institutional positions in that equity, respectively.

3.1. The Effect of Active Institutional Investors on Leverage

3.1.1. Identification Strategy

We use an instrumental variable design to statistically test if the negative relationship between active institutional holdings and leverage documented in our summary statistics is more than a correlation. To estimate if changes in active institutional ownership affect corporate leverage, we use implied mutual fund trades induced by idiosyncratic individual-investor flows as an instrument for active 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 active 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 idiosyncratic, individual-investor flows. Figure 4 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 that are unpredictable and have no direct or indirect relationship with corporate leverage. In our second step, we use the actual stock holdings of the mutual fund in a given quarter to project proportionate changes in each of the fund's stock holdings in the next quarter based on the magnitude of the idiosyncratic individual-investor inflows/outflows in a given quarter. The final step aggregates all of our projected changes in stock holdings to an individual stock-year frequency.

By using implied mutual fund trades as an instrument, we eliminate the previous two arguments for indirect correlation that invalidated actual flows as an instrument (i.e., when mutual funds traded based on leverage and when managers timed mutual fund trades with equity issuances or repurchases). Another potential concern alleviated by using implied trades as an instrument is correlation with recent mutual fund performance. This concern is addressed by Step 2 in Figure 4 where we remove flows from chasing mutual fund returns. A final potential concern lessened with the construction of our instrument is correlation with unobservable market conditions. Given that we project trades one period in the future, if markets exhibit informational efficiency on an annual basis, then implied trades are not correlated with the future information that moves markets. This means even when an individual-investor's flow contains some information or sentiment (Frazzini and Lamont (2008)), the constructed instrument does not. The notion that an instrument based on individual-investor flows is uncorrelated with future market conditions is further supported by empirical evidence suggesting individual-investors' trades fail to systematically predict the market (Barber and Odean (2000)). Taken together, the logic presented thus far suggests implied mutual fund trades based on the idiosyncratic portion of individual-investor inflows/outflows plausibly should have no relationship to executives' capital structure choices.

We believe our construction of mutual funds' implied trades and the aforementioned economic

arguments suggest our instrument creates plausibly exogenous variation in active institutional holdings in the following regression specification:

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

where observations are at the firm-year level, $Leverage_{ijt}$ represents the book leverage ratio for firm i in industry j in year t, $ActiveHoldings_{ijt}$ 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. By including firm and industry-by-year fixed-effects, we are controlling for unobservables such as firm-specific production capabilities and industry-year-specific sentiment that otherwise may bias our estimates.

3.1.2. Empirical Results

Table 3 presents our test of the effect of active institutional investors on financial leverage. We find active institutional holdings are a significant determinant of firms' capital structures; Column (1) shows a one standard deviation change in active institutional holdings leads to a -0.25 standard deviation change in financial 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 0.47 percentage points for every percentage point increase in active institutional ownership, all else equal.

Additional tests displayed in Table 3 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 288.8. Both of these measures are well above recommended weak instrument thresholds and suggest the instrument is relevant. Column (2) through Column (4) of Table 3 show that the primary finding is robust to alternative definitions of leverage such as including off-balance-sheet items as well as adjusting for market definitions

of assets. Further, these findings are robust to a myriad of controls, which include firm size, tangibility, intangibility, collateral, profitability, losses, dividend paying firm, payout yield, 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. For all the details of the first-stage regression as well as weak instrument tests, please see Appendix Table B1.

The economic magnitude of the instrumented point estimate seems sensible. The point estimate indicates a 0.25 standard deviation decrease in leverage, for a one standard deviation increase in active institutional ownership. The magnitude of this change is consequential economically. For example, it suggests a 10 percentage point increase in active institutional ownership leads to a 4.7 percentage point reduction in leverage from an average leverage of 31.1%. The explanatory power of the first stage estimate is high as evidenced by the R^2 of 37% in Table 3 and the instrument is statistically significant in predicting active institutional investors.

Given that the instrument is broadly predictive, it appears reasonable to conclude the estimates are not stemming from a special subsample of firms. Furthermore, given that 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. Next, 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 Institutions on Total Corporate Leverage

3.2.1. Identification Strategy

We study if an increase in power by institutional investors from 1988 to 1992 allowed for the observed relationship between active institutional ownership and corporate leverage to develop. 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. We then assess the degree to which such a relationship can explain how

aggregate corporate leverage evolves in the time series. Recall from Figure 2 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.

To evaluate the hypothesis that the increased power of institutional shareholders explains part of the downtrend in aggregate leverage, we perform two tests. First, we perform a subsample test that extends our instrumental variable identification strategy. Second, we implement a difference-in-differences test surrounding the change in shareholder power. 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.

For the subsample test, we split the full sample period which spans from 1980 to 2013 into three subsamples, 1980 to 1989, 1989 to 1992, and 1992 to 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.

Our second test is a difference-in-differences analysis surrounding the 1992 reforms. Our first difference is time, pre vs. post reform. Our second difference is in active institutional ownership, high vs. low. We consider firms with above median institutional ownership to be treated firms while firms with below median institutional ownership to be control firms. Employing a difference-in-differences approach allows us to evaluate if the legal reforms really enabled the relationship by narrowing the analysis to a short period of time around the reform. Given that the increase in shareholder power was gradual rather than sudden, we consider two pre periods. First, we consider the years before the onset of institutional shareholder activism (pre 1989). Second, we consider the years before the SEC reform (pre 1992). In each case, the post period remains post 1992.

Our exact regression specification is as follows:

$$Leverage_{jt} = \alpha + \beta Post_t \times Treated_j + \Gamma X_{jt} + f_j + \delta_t + \epsilon_{jt} \tag{2}$$

where observations are at the firm-year level. $Leverage_{jt}$ represents the financial leverage ratio for firm j in year t, $Post_t$ is a dummy equal to one in the years following the reform and zero

in the years prior to the reform. $Treated_j$ is a dummy indicating whether firm j belongs to the treatment or control group. X_{it} is a vector of observable firm-specific covariates including firm size, tangibility, intangibility, collateral, profitability, losses, dividend paying firm, payout yield, 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. f_j is a firm fixed-effect, δ_t is a year fixed-effect, and ϵ_{jt} is the unobservable error component. By including firm and year fixed-effects, we need not include a $Post_t$ or $Treated_j$ dummy as they are not identified. The coefficient of interest is β which measures the change in $Leverage_{jt}$ following the reform for firms with high active institutional ownership relative to their controls.

3.2.2. Empirical Results

Table 4 provides evidence to suggest the strengthening of shareholder power allowed for a relationship to develop where active institutional investors influence management's leverage decisions. Table 4 shows that in the time period prior to the onset of shareholder activism, the effect of active institutional holdings on leverage is statistically indistinguishable from 0. After the second key event – the SEC reform in 1992 – a standard deviation increase in active institutional holdings leads to 0.27 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 it shifts from a positive to a negative sign. 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 grey area between 1989 and 1992 in terms of institutions' full impact, by 1992 the consolidation of institutional power appears to be fully felt.

Table 5 presents the difference-in-differences estimation of regulatory reforms to institutional power on financial leverage. Consistent with Table 4, Panel A shows that treated firms experience a significant decrease in leverage relative to control firms following the reform. The point estimate is significant at the 1% level. The point estimate of a 0.098 standard deviation decrease in leverage reported in column (3) of Table 5 corresponds to an economic reduction in leverage by 1.9 percentage points per year in the three years following the reform for treatment firms relative to control firms. Comparing Columns (1) and (2) with Columns (3) and (4), we find that both the onset of activism and the legal reforms contribute to the empowerment of active institutional investors to engage

with management over leverage. The fact that the effect is stronger after the second key event – the SEC reform in 1992 – indicates its importance for the interrelation between leverage and institutional ownership to develop. Finally, a comparison between the three-year windows reported in Columns (1) and (3) and the 5-year windows reported in Columns (2) and (4) reveals a quick change in relationship. Even in a small window, the effect is strong and pronounced. Panel B of Table 5 shows the parallel trend is validated in that there is no noticeable differences in leverage changes across the treated and control firms in the years prior to the reform.

Taken together, the within-firm evidence from the subsample and difference-in-differences tests provides strong support for a relationship between leverage and active institutional ownership developing in the early 1990s, just as the deleveraging began to occur. Next, we quantify the economic significance of these results in relation to the macroeconomic trend in corporate leverage. We do this by simulating aggregate leverage in a counterfactual world where there was no strengthening of institutional power. For each individual firm, we simulate leverage evolutions 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 active institutional investors is set to the coefficient on active institutional investors before the onset (i.e., Column (2) of Table 4). 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 (i.e., they are set to their coefficients for the post 1992 period). 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 in the period after 1992 that we actually observe in the data. This assumption means we include the actual firm-year specific residuals in our simulations.

Figure 5 displays the results of our simulation exercise. The figure suggests the aggregate leverage of unregulated U.S. firms would have been eight percentage points higher today without the influence of active institutional investors on managements' capital structure decisions. To put this in economic perspective, the actual total leverage today for unregulated U.S. firms is 25% less than what our counterfactual simulations suggest leverage would have been. Our simulations indicate 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 also contribute to the deleveraging.

3.3. Comparing Active Institutional Investors and Hedge Funds

3.3.1. Identification Strategy

To compare hedge funds' and active institutional investors' influence over corporate leverage decisions, we use a sample of 1,139 hedge fund 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. The 13D filings data are borrowed from Brav et al. (2008). We also examine the full duration 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 active institutional ownership is instrumented for using implied mutual fund trades. The main addition to the specification is to include controls for either hedge fund activism campaigns or hedge funds filing a 13D forms.

Our next three empirical tests seek to isolate the differences in motive between hedge funds' and active institutional investors' recommendations for corporate leverage. We begin by examining the firms that deleverage. We look at their method for deleveraging, what makes them a candidate for deleveraging, and what, if any, spillovers from the deleveraging to other corporate policies can be observed.

First, we first study the composition of the deleveraging. We use an instrumental variable framework similar to what we used for leverage, but we replaces leverage with the specific debt or equity component. We define equity issuance as the product of the split-adjusted change in shares outstanding and the split-adjusted average stock price, normalized by total assets in the previous year. Consequentially, a firm is an equity issuer if $\Delta Equity > 1\%$, and an equity repurchaser if $\Delta Equity < -1\%$. Debt issuance is defined similarly and includes long-term plus short-term debt, normalized by total assets in the previous year. This procedure adjusts for new issuances that

result from rolled-over debt. This procedure also distinguishes between equity issuance related to the exercise of employee stock options and actual seasoned equity offerings. We also examine the details of the debt type. Firm-level debt structure variables are gathered from Capital IQ. We aggregate debt types into five categories, including: 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.

Second, we study the firms targeted for deleveraging. The long-term investment horizon of active institutional investors suggests they place greater weight on long-term probabilities of financial distress. We evaluate this by considering if firms characterized by high leverage are more likely to deleverage. We test the hypothesis that active institutional investors are only important for leverage when leverage is in the high range of the distribution. We do this using an instrumental variables quantile regression framework.

The quantile regression specification is the same as in Equation (1) but varies across quantiles, τ. The exact specification is as follows:

$$Q(Leverage_{ijt}|\tau) = \alpha(\tau) + \beta(\tau)ActiveHoldings_{ijt} + \gamma(\tau)Z_{ijt} + f_i(\tau) + \delta_{jt}(\tau) + \epsilon_{ijt}(\tau) \qquad (3)$$

where observations are at the firm-year level. $Leverage_{ijt}$ represents the leverage ratio for firm i in industry j in year t, $ActiveHoldings_{ijt}$ 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.

Third, we study if active institutional investors make recommendations that balance the trade-off between short-term agency reduction and longer-term financial distress. We focus on other policies known to generate high-powered incentives for managerial efficiency. In particular, we examine debt covenants and dividend payments. Debt covenants incentivize operational efficiency (Nini, Smith, and Sufi (2009)). Dividend payments also incentivize operational efficiency, especially

when they are not high payments (Grinstein and Michaely (2005)). We use Thomson Reuter's Dealscan data to examine the financial covenants used in bank loans. We use Compustat to measure dividend payments. Our primary specification is the same instrumental variable approach used throughout the paper where active institutional ownership is instrumented for using implied mutual fund trades. The only differences are that covenant usage and dividend payments rather than leverage are the dependent variable in the specification. Also, when dividend payments is the dependent variable, leverage is included as a control variable.

3.3.2. Empirical Results

Table 6 provides empirical evidence that hedge funds follow an alternative tactic with regards to leverage than that of active institutional investors. We find a statistically significant positive interrelation between hedge fund activism and corporate leverage. Yet the inclusion of hedge fund activism events in our analyses does not meaningfully change our point estimate for active institutions. On average, a one standard deviation increase in active institutional ownership still leads to a 0.25 standard deviation decrease in leverage while the effect of a hedge fund activism event is much smaller in economic magnitude (0.08). Moreover, hedge fund activism is much rarer than active institutional ownership suggesting they contribute little to total changes in corporate leverage.

Table 7 Panel A provides estimates of the composition of the deleveraging. The first finding is that the deleveraging primarily occurs through debt changes rather than equity changes. The second finding is that deleveraging occurs through active reduction in long-term debt. Specifically, we find no meaningful change in equity issuance. In contrast, we find firms change their debt. We find significant reductions in long-term debt and new debt issuance. Table 7 Panel B provides estimates of the composition of the debt change. We find active institutional investors influence management to reduce bonds, notes, and commercial paper. Capital leases, term loans, and other borrowings, which include drawn credit lines, do not meaningfully change. The finding that firms deleverage primarily through reduction in long-term debt like bonds is consistent with Welch (2004). It suggests managers are taking decisive action to change their capital structure when active institutional investors encourage it.

Table 8 Panel A presents the quantile instrumental variable estimates for the effect of active

institutional holdings on leverage. They reveal highly levered firms deleverage more. 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. The estimates suggest the importance of active institutional investors varies over the leverage distribution. Consider the 33rd percentile, which represents leverage of 13%. For this quantile, a 10 percentage point increase in active institutions is associated with a 3 percentage point decrease in leverage. In contrast, for the 80th percentile, which represents a leverage of 54%, a standard deviation increase in active institutions is associated with a 6 percentage point decrease. Panel B confirms statistically the change in inferences across quantiles. By using point estimates from each of the 100 quantiles, we are able to reject the hypothesis that high and low leverage firms react the same to active institutional ownership.

Table 9 Panel A compares financially distressed firms with financially sound firms. Columns (1) and (3) show firms with higher probabilities of financial distress as measured by Altmans Z-score deleverage more. Whereas Columns (2) and (4) show much smaller changes in leverage for financially sound firms. These findings echo the findings in Table 8. Most importantly, they show highly levered, financially distressed firms react much more to increases in active institutional ownership. Much like the stories in the press, this suggests active institutional investors want firms to deleverage because they are worried about the probabilities of financial distress.

Table 9 Panel B reveals if deleveraging has consequences for other policies incentivizing managerial efficiency. We find meaningful spillover effects. Columns (1) through (3) show increases in active institutional ownership lead to increases in covenant usage. For example, a one standard deviation increase in institutional ownership leads to a 0.16 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. Further, Column (4) shows firms may also begin dividend payments in response to increases in active institutional ownership. This evidence is consistent with prior research examining dividends and institutional ownership (Grinstein and Michaely (2005)).

Taken together, Tables 6 through 9 show heterogeneity in institutional investor type is consequential for understanding the relationship between leverage and institutions. While vocal hedge funds are often glamorized in the business press for exerting their influence over management to increase corporate debt levels. Our evidence suggests hedge funds' modus operandi with respect to debt opposes that of active institutional investors. Rather active institutional investors often recommend deleveraging. When active institutions encourage deleveraging, it helps to balance the trade-off between short-term efficiency gains and long-term financial distress. Highly levered firms and financially distressed firms deleverage more and they do so by balancing reductions in long-term debt with the adoption of alternative agency-reducing incentives.

4. Robustness

Instrumental Variable. We run a placebo test, in which we project changes in mutual funds' stock holdings back in time rather than forward. If our instrument was spuriously correlated with some form of unobservable persistency, then, using the subsequent portfolios should produce similar point estimates. When examining changes in leverage that pre-date the placebo instrument, we find no effect. Appendix Table B4 shows the results from the placebo test analysis. Further, alternative constructions of the instrument such as using a parametric functional form for mutual fund returns or focusing on idiosyncratic individual-investor inflows rather than both inflows and outflows do not qualitatively change our conclusions. Panel C of Appendix Table B2 shows five such variations in the instrument.

Defining Institutional Ownership Changes. Variations in the definition of institutional ownership such as using only the holdings by the 5 institutions with the largest ownership stakes or using mutual fund active share (Cremers and Petajisto (2009)) do not change our conclusions. Panel B of Appendix Table B2 shows these results. Meaningful variation year-to-year in active institutional ownership drives our findings. While the average firm experiences little change in active institutions year-over-year, 40% of observations experience changes of more than 10 percentage points in a year. Appendix Table B3 shows the firms experiencing large changes in active institutional ownership explain the instrumental variable point estimate for the full sample.

Alternative Explanations. Alternative specifications that account for other known lever-

age determinants such as analyst coverage, corporate governance, 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. Accounting for cyclical variations such as changes in the supply and demand for corporate and government debt, the new IPOs during the dot-com boom, and waves of de-listing during LBO booms and stock market crashes does not change our findings. These findings are contained in Appendix Table B2.

Alternative Mechanism. An alternative hypothesis for the mechanism driving the negative interrelation between active institutional ownership and leverage is that the influx of passive institutional investors is crowding out activist hedge fund investors that prefer firms take on higher leverage. The evidence, however, does not support this mechanism. First, the timing is off. Our results suggest the deleveraging trend did not begin until 1992 when SEC regulatory reforms were enacted that incentivized greater institutional governance, yet Vanguard launched its first index mutual fund in 1976. And there were no noteworthy changes in assets pledged to index funds in the early 1990s. Moreover, our effect still holds when controlling for passive institutional investors. These results are reported in Panel A of Appendix Table B2. This suggests our findings are not a story of passive investors crowding out smart hedge funds.

Reverse Causality. Reverse causality refers to the direction of cause-and-effect. In this case, the reverse hypothesis is high or low leverage causes changes in institutional ownership. The instrumental variable approach used throughout our study produces unbiased inferences even in the presence of reverse causality. But reverse causality could contribute to the deleveraging trend, so we explore it further. To test the reverse hypothesis, we use a quasi-natural experiment. The experiment stems from the introduction of CDS contracts. Through the use of CDS contracts, institutions can financially engineer any leverage they want for a firm. This enables institutions to invest in firms that may not have their preferred level of leverage. Our experiment uses the staggered introduction of CDS contracts across stocks beginning in the early 2000s. Each new introduction of CDS produces a natural break in leverage clienteles. We apply a difference-in-differences estimator to this setting. We find the ability to hedge around a firm's leverage does produce changes in institutional ownership. But the size is so small (i.e., 1/10th of that stemming

from active institutions to leverage) that it cannot explain the deleveraging trend. Appendix C explains this test in more detail and presents the results from implementing it.

5. Conclusion

In this paper, we address several important and related questions. First, we examine the interrelation between institutional ownership and leverage decisions. Second, we use regulatory changes that strengthened institutions' power as an experiment to test if greater shareholder power was necessary to enable such a relationship to develop. Third, we assesses which institutions drive the interrelation. Given that 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.

We find increases in institutional ownership by actively managed funds explain the decline in leverage over the last 25 years. Our estimates suggest a 10 percentage point increase in active institutional ownership leads to a 4.7 percentage point decline in leverage. Supporting the hypothesis that institutional investors contributed to the deleveraging trend that began in 1992, we find the effect of active investors' on corporate leverage only became meaningful after regulatory changes. While hedge funds may advocate for more debt, active institutions drive deleveraging. When active institutions encourage deleveraging, it helps to balance the trade-off between short-term incentives and long-term distress. Distressed firms deleverage more and they do so by balancing reductions in long-term debt with the adoption of alternative agency-reducing incentives.

The economic importance of institutional investors' influence for total corporate leverage trends is large. Corporate leverage would have been eight percentage points higher today without their influence. While the evidence makes the relationship seem obvious now, prior detection was elusive. The need to disaggregate institutional investors by type and for well-identified point estimates prohibited its discovery.

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Figures and Tables

	Hedge Funds	Active Institutional Investors	Passive Institutional Investors
Credible exit threat?	Yes	Yes	No
Resources available to beat the benchmark?	Yes	Yes	No
Typical investment horizon	Shorter	Longer	Longer
Seeks influence over general best practices (e.g., strong governance and transparency)?	Yes	Yes	Yes
Seeks influence over firm-specific policies (e.g., leverage and payout)?	Yes	Yes	No
Firm-specific policy recommendations balance trade-off between agency-related incentives and longer-term probabilities of financial distress?	No	Yes	N.A.

Figure 1. Institutional Investor Attributes: The figure compares attributes of hedge funds, active institutional investors, and passive institutional investors.

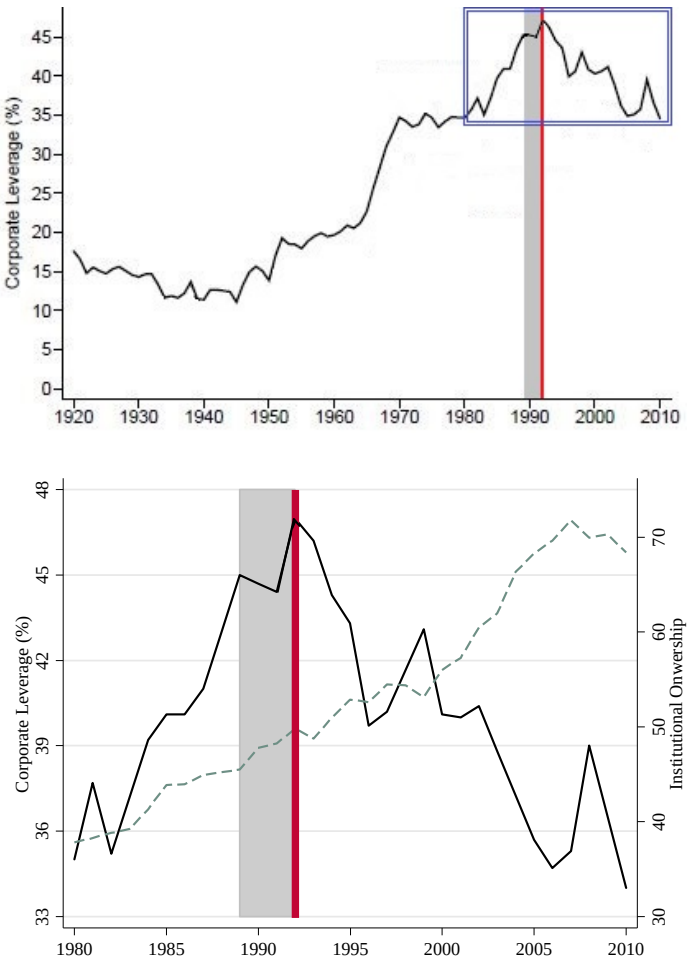

Figure 2. Capital Structure and Changes in Equity Ownership: The upper plot shows annual aggregate leverage as a black, solid line for all firms in the CRSP database that are also covered either in Compustat or Moody's Industrial Manuals from 1920 through 2010. The onset of institutional shareholder 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 black, solid line and institutional ownership as a dark gray, dashed line, the onset of institutional shareholder activism is shaded in light gray, and the formal adoption of SEC rules in red. Financial firms and utilities are excluded. 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.

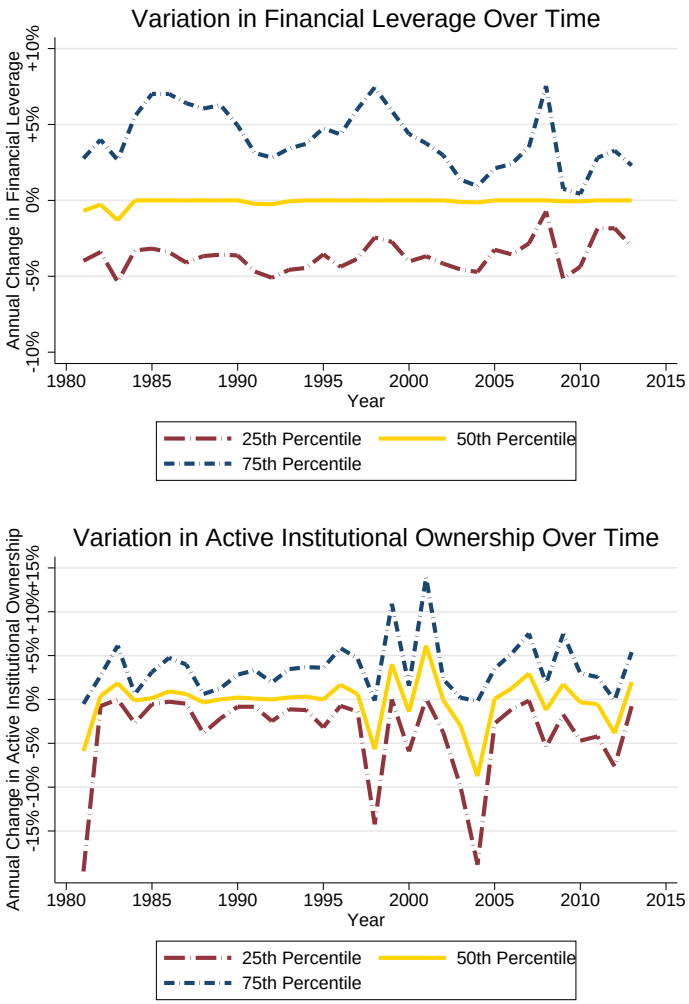

Figure 3. Interquartile Range of Annual Changes in Leverage and Ownership: The upper plot shows the 25th, 50th, and 75th percentile for annual within-firm changes in financial leverage over time. The bottom plot shows 25th, 50th, and 75th percentile for annual within-firm changes in active institutional 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
	Receives \$1M inflow from individual investors at *t*			
Step 1:	**Isolate idiosyncratic part of individual-investor flows**			
	Investors chasing returns	Remove flows from fund returns as in Chevalier and Ellison (1997)		
	Investors chasing reputation/skill	Remove flows from fund fixed effects as in Berk and Green (2004)		
	Focused-funds correlated with leverage	Drop funds that hold firms in one industry		
Result:	**Estimate \$500,000 of \$1M inflow is idiosyncratic**			
Step 2:	**Project "implied" *t + 1* trades from idiosyncratic flow at *t***			
	\$1M inflow (\$500,000 idiosyncratic)	\$0.1M	\$0.15M	\$0.25M
Then:	**Aggregate implied *t + 1* trades across all funds to firm-level**			

Figure 4. Implied flow-induced Trading Example: Assume a mutual fund holds equity in Firm A, Firm B, and Firm C at time t equal to \$2 million, \$3 million, and \$5 million, respectively, and has \$10 million total in assets under management at time t. Next, assume the mutual fund receives an inflow from individual investors of \$1 million. For our instrument to be plausibly exogenous, we need the implied $t+1$ trades to be correlated with actual trades but uncorrelated with Firm A, Firm B, or Firm C's next period leverage. By assuming equal proportion mutual fund trades, we bypass the potential confounding effect of mutual fund managers allocating more (or less) of the investors inflows to a Firm that is likely to have higher (or lower) leverage in the future. Because large positive or negative fund flows are not allocated randomly to mutual funds, a first step is needed to bypass the potentially confounding effects of smart money. To isolate the portion of the mutual funds' trades driven by idiosyncratic reasons such as the individual investors' liquidity needs or lumpy inflows from bonuses and tax returns, we remove the proportion of the variation in flows that can be explained by mutual fund returns as well as the proportion of the variation in flows that can be explained by mutual portfolio management skills (Berk and Green (2004)). To accomplish this, we estimate a non-linear relationship between actual flows and fund returns as in Chevalier and Ellison (1997) and include mutual fund fixed effects to control for management skills. Using the fitted values from these regressions, we calculate the residual unexplained variance in actual flows. We use this residual flow (i.e., the idiosyncratic individual-investor flows) to project the implied trade at time $t+1$. The final rows of the figure shows that instead of the\$1 million inflow from the top of the figure, if 50% of the individual-investor flow variation is explained by performance and skills, we only project the mutual fund trades attributable to the remaining \$500,000 in unexplained, idiosyncratic individual-investor flows. Finally, we aggregate all the implied mutual funds trades from all mutual funds to the firm-year level.

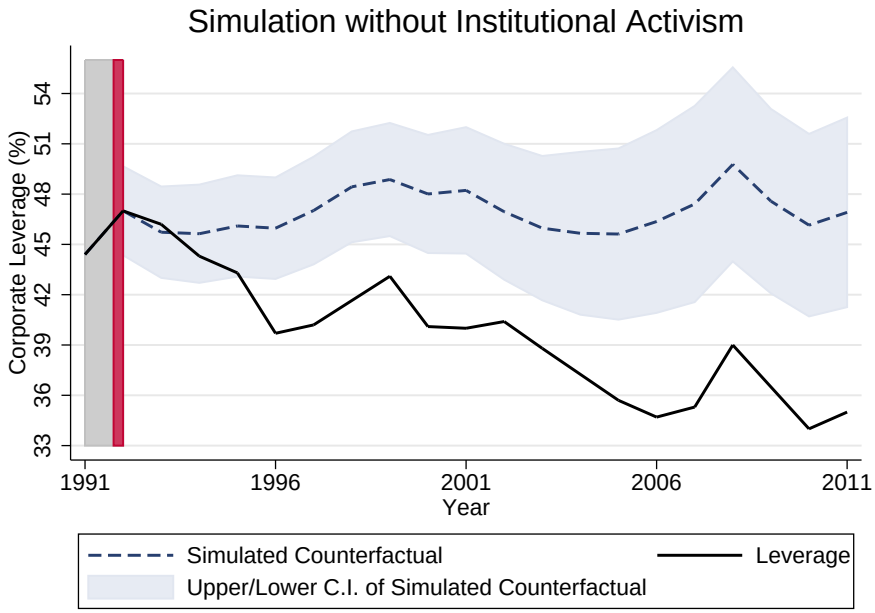

Figure 5. Simulated Counterfactual: The plot depicts annual total leverage as a black, solid line for nonfinancial public firms and simulated leverage for nonfinancial public firms as a blue, dashed line. The simulation sets the influence of active institutional investors on leverage to levels observed before the 1992 SEC proxy reform. The bluish-gray shaded area represents the 90% confidence interval for the simulated counterfactual. The onset of institutional shareholder activism is shaded in light gray, and the formal adoption of SEC rules in red.

Table 1 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 variable definitions.

	Full Sample		1980 to 1991		1992 to 2013	
	Mean	SD	Mean	SD	Mean	SD
Summary Statistics	(1)	(2)	(3)	(4)	(5)	(6)
Institutional Investors	36.5%	29.3%	21.2%	19.9%	43.9%	30.2%
Active Institutional Investors	15.8%	14.2%	10.6%	11.3%	18.3%	14.8%
Block Holdings	11.8%	12.9%	5.8%	8.7%	14.6%	13.6%
Number of Institutional Investors	73	112	36	69	90	124
Number of Active Institutional Investors	28	41	16	30	34	44
Number of Blockholders	1.4	1.4	0.7	0.9	1.7	1.5
Holdings, Top 1 Institution	7.4%	5.6%	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%
Institutional Concentration (HHI)	0.234	0.258	0.312	0.311	0.196	0.219
Financial Leverage	31.1%	27.0%	34.9%	25.6%	29.3%	27.5%
Financial Leverage incl. Leases	43.5%	29.9%	46.1%	28.7%	42.2%	30.4%
Financial Leverage (Market)	23.7%	23.8%	29.3%	24.4%	21.0%	23.0%
Balance Sheet Leverage (Market)	37.3%	24.1%	43.0%	24.0%	34.6%	23.6%
Firm Size	5.41	2.01	5.06	2.01	5.58	1.99
Market-to-book	1.58	1.51	1.31	1.29	1.71	1.59
Lifecycle Stage	-0.32	1.63	-0.01	1.02	-0.47	1.83
Profitability	6.4%	21.4%	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%	46.0%	49.8%	29.1%	45.4%
Payout Yield	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	31.2%	38.4%	28.8%	37.0%	32.3%	39.0%
Sales Growth	15.2%	47.2%	14.4%	45.8%	15.6%	47.9%
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	-5.9%	10.7%	-12.0%	6.5%	-3.0%	11.1%
Altman's Z-Score	1.34	2.43	1.86	2.00	1.09	2.57
Amihud Liquidity	10.82	45.32	24.50	71.27	4.28	21.78
Insider Ownership	2.2%	5.1%	2.0%	5.4%	2.3%	5.0%
Observations	106,537		34,469		72,068	

Table 2 Partial Correlations

Panel A reports partial correlations from multivariate regressions relating financial leverage and active institutional investors, Panel B reports for financial leverage with leases, Panel C for financial leverage where assets are quoted in market value, and Panel D for balance sheet leverage where assets are quoted in market value. Additional 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. Firm fixed effects and industry-by-year fixed effects are included in each regression. Estimated coefficients are scaled by the corresponding variable's standard deviation. ***, **, and * represent statistical significance at the 1%, 5%, and 10% level, respectively. Robust standard errors, clustered at the firm level, are reported in parentheses. Appendix A provides variable definitions.

Panel A. Partial Correlations with Financial Leverage			
	(1)	(2)	(3)
Active Institutional Investors	-0.068***		
	(0.006)		
Holdings, Top 10 Institutions		-0.074***	
		(0.009)	
Institutional Concentration HHI			0.086***
			(0.006)
Adjusted R-squared	66%	66%	66%
Panel B. Partial Correlations with Financial Leverage incl. Leases			
Active Institutional Investors	-0.058***		
	(0.006)		
Holdings, Top 10 Institutions		-0.063***	
		(0.008)	
Ownership Concentration HHI			0.073***
			(0.005)
Adjusted R-squared	69%	69%	69%
Panel C. Partial Correlations with Financial Leverage (Market)			
Active Institutional Investors	-0.113***		
	(0.005)		
Holdings, Top 10 Institutions		-0.099***	
		(0.008)	
Ownership Concentration HHI			0.098***
			(0.005)
Adjusted R-squared	72%	71%	71%
Panel D. Partial Correlations with Balance Sheet Leverage (Market)			
Active Institutional Investors	-0.117***		
	(0.005)		
Holdings, Top 10 Institutions		-0.099***	
		(0.007)	
Ownership Concentration HHI			0.097***
			(0.005)
Adjusted R-squared	77%	77%	77%
Firm-level Controls	Yes	Yes	Yes
Firm Fixed Effect	Yes	Yes	Yes
Industry-by-Year Fixed Effects	Yes	Yes	Yes
Observations	106,537	106,537	106,537
Unique Firms	10,396	10,396	10,396

Table 3 The Effect of Active Institutional Investors on Leverage

This table presents instrumental variable estimates exploring if active institutional investors shape leverage policy. It uses the specification outlined in Eq. (1). The instrument for active institutional investors is mutual funds' implied trades. In Column (1) financial leverage is the dependent variable. In Columns (2) through (4) alternative measures of leverage are considered. They include financial leverage with leases, financial leverage where assets are quoted in market value, and balance sheet leverage where assets are quoted in market value, respectively. Additional 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. Firm fixed effects and industry-by-year fixed effects are included in each regression. Estimated coefficients are scaled by the corresponding variable's standard deviation. ***, **, and * represent statistical significance at the 1%, 5%, and 10% level, respectively. Robust standard errors, clustered at the firm level, are reported in parentheses. Please see Appendix Table B1 for additional details on the first stage of the regression. Appendix A provides variable definitions.

	IV	IV Robustness (Alt. Leverage Measures)		
	Financial Leverage	Financial Leverage incl. Leases	Financial Leverage (Market)	Balance Sheet Leverage (Market)
	(1)	(2)	(3)	(4)
Active Institutional Investors	-0.245***	-0.180***	-0.487***	-0.445***
	(0.048)	(0.045)	(0.050)	(0.046)
Firm Size	0.456***	0.245***	0.668***	0.523***
	(0.032)	(0.030)	(0.032)	(0.029)
Tangibility	0.002	-0.059***	0.029	-0.055***
	(0.020)	(0.020)	(0.019)	(0.017)
Intangibility	0.084***	0.075***	0.051***	0.021***
	(0.009)	(0.008)	(0.008)	(0.007)
Collateral	0.203***	0.271***	0.143***	0.180***
	(0.019)	(0.019)	(0.018)	(0.016)
Profitability	-0.007	-0.045***	-0.032***	-0.076***
	(0.010)	(0.010)	(0.007)	(0.007)
Dividend Paying Firm	-0.121***	-0.103***	-0.130***	-0.121***
	(0.008)	(0.007)	(0.008)	(0.007)
Lifecycle Stage	-0.061***	-0.120***	-0.022**	-0.063***
	(0.012)	(0.012)	(0.009)	(0.009)
Market-to-book	-0.020***	-0.031***	-0.114***	-0.267***
	(0.007)	(0.007)	(0.006)	(0.007)
Investment	-0.021***	-0.018***	-0.024***	-0.028***
	(0.003)	(0.003)	(0.003)	(0.003)
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	288.8	288.8	288.8	288.8
T-stat on Instrument in First Stage	16.99	16.99	16.99	16.99
First-Stage R-squared	37%	37%	37%	37%
Adjusted R-squared	66%	69%	72%	77%
Observations	106,537	106,537	106,537	106,537
Unique Firms	10,396	10,396	10,396	10,396

Table 4 Active Institutions' Role in Deleveraging: Subsample Tests

This table presents instrumental variable estimates exploring if active institutional investors shape leverage policy across different time periods. It uses the specification outlined in Eq. (1). The instrument for active institutional investors is mutual funds' implied trades. In all specifications financial leverage is the dependent variable. Column (1) through (4) reports estimates for the full sample, the sample before the onset of institutional activism, for the sample post onset but pre SEC reform, and for the sample post SEC reform, respectively. A Chow Test F-stat comparing the pre and post-SEC reform periods is reported in the grey box. Additional 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. Firm fixed effects and industry-by-year fixed effects are included in each regression. Estimated coefficients are scaled by the corresponding variable's standard deviation. ***, **, and * represent statistical significance at the 1%, 5%, and 10% level, respectively. Robust standard errors, clustered at the firm level, are reported in parentheses. Appendix A provides variable definitions.

	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)
Dependent Variable = Financial Leverage	(1)	(2)	(3)	(4)
Active Institutional Investors	-0.245***	0.089	-0.185	-0.268***
	(0.048)	(0.089)	(0.131)	(0.057)
Chow Test F-stat		Pre vs. Post SEC Reform		67.74***
Sum of Squared Residuals	33,844.3	5,596.3		23,476.3
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	288.8	55.2	52.5	210.8
T-stat on Instrument in First Stage	16.99	7.43	7.23	14.52
First-Stage R-squared	37%	22%	15%	22%
Adjusted R-squared	66%	76%	84%	69%
Observations	106,537	24,623	9,846	72,068
Unique Firms	10,396	4,383	3,290	7,825

Table 5 Active Institutions' Role in Deleveraging: Difference-in-Differences

This table presents the difference-in-differences estimates of the effect of the changes in institutional power via SEC reforms on financial leverage in Panel A. Panel B reports pre-treatment statistics for control and treated firms from 1986 to 1988. The regression specification is outlined in Eq. (2). *Post* is a dummy equal to one in the years following the SEC reform (1992 to 1996) and zero in the years prior to the reform. Column (1) uses a pre period of 1986 to 1988. Column (2) uses a pre period of 1984 to 1988. Column (3) uses a pre period of 1989 to 1991. Column (4) uses a pre period of 1987 to 1991. Firms with above median active institutional ownership are treated firms and firms with below median active institutional ownership are control firms. Additional 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. Firm fixed effects and year fixed effects are included in each regression. Estimated coefficients are scaled by the corresponding variable's standard deviation. ***, **, and * represent statistical significance at the 1%, 5%, and 10% level, respectively. Robust standard errors, clustered at the firm level, are reported in parentheses. Appendix A provides variable definitions.

	Activism Onset (1989)		SEC Reform (1992)	
Panel A. Difference-in-differences	3-yr. window	5-yr. window	3-yr. window	5-yr. window
Dependent Variable = Financial Leverage	(1)	(2)	(3)	(4)
Post x Treated	-0.085**	-0.059***	-0.098***	-0.064***
	(0.039)	(0.020)	(0.031)	(0.018)
Firm Controls	Yes	Yes	Yes	Yes
Firm Fixed Effect	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Adjusted R-squared	23%	22%	23%	22%
Observations	18,877	33,581	19,170	34,088
Unique Firms	5,395	6,820	4,845	6,121

Panel B. Pre-Treatment Statistics	Year (1)	Control Mean (2)	Treated Mean (3)	Difference in Levels (4)
Financial Leverage	1986	33.7%	37.1%	3.4%
	1987	34.0%	38.1%	4.1%
	1988	34.6%	38.3%	3.7%

Table 6 Comparison of Active Institutional Investors and Hedge Funds

This table presents instrumental variable estimates exploring if active institutional investors shape leverage policy even when hedge funds are intervening at the firm. It uses the specification outlined in Eq. (1). The instrument for active institutional investors is mutual funds' implied trades. In all specifications financial leverage is the dependent variable. Column (1) includes an additional control for hedge fund campaigns while Column (2) includes an additional control for when a hedge fund activist obtains a 5% ownership stake. Additional 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. Firm fixed effects and industry-by-year fixed effects are included in each regression. Estimated coefficients are scaled by the corresponding variable's standard deviation. ***, **, and * represent statistical significance at the 1%, 5%, and 10% level, respectively. Robust standard errors, clustered at the firm level, are reported in parentheses. Appendix A provides variable definitions.

Dependent Variable = Financial Leverage	(1)	(2)
Active Institutional Investors	-0.244***	-0.245***
	(0.048)	(0.048)
Hedge Fund Campaign	0.087***	
	(0.024)	
Hedge Fund Activist Crosses 5% Threshold		0.032
		(0.021)
Additional Firm Controls	Yes	Yes
Firm Fixed Effect	Yes	Yes
Industry-by-Year Fixed Effects	Yes	Yes
First-stage F-stat	288.1	290.0
T-stat on Instrument in First Stage	16.97	17.03
First-Stage R-squared	24%	24%
Adjusted R-squared	66%	66%
Observations	106,537	106,537
Unique Firms	10,396	10,396

Table 7 The Structure of the Deleveraging

This table presents instrumental variable estimates exploring the association between active institutional investors and the structure of firms' deleveraging. Panel A reports standardized coefficient estimates from instrumental variable regressions of debt and equity changes. 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 active institutional investors is mutual funds' implied trades as is outlined in Eq. (1). Additional 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. Firm fixed effects and industry-by-year fixed effects are included in each regression. Estimated coefficients are scaled by the corresponding variable's standard deviation. ***, **, and * represent statistical significance at the 1%, 5%, and 10% level, respectively. Robust standard errors, clustered at the firm level, are reported in parentheses. Appendix A provides variable definitions.

Panel A. Deleveraging through Debt and Equity Changes

	Short-Term Debt	Long-term Debt	Net Debt Issuance	Equity Buybacks	Net Equity Issuance
Dependent Variable = Security Type	(1)	(2)	(3)	(4)	(5)
Active Institutional Investors	-0.082	-0.327***	-0.169*	-0.207**	0.051
	(0.058)	(0.065)	(0.089)	(0.098)	(0.062)
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	210.8	210.8	210.8	210.8	210.8
T-stat on Instrument in First Stage	14.52	14.52	14.52	14.52	14.52
First-Stage R-squared	22%	22%	22%	22%	22%
Adjusted R-squared	47%	69%	8%	23%	37%
Observations	71,897	71,897	71,897	71,897	71,897
Unique Firms	7,825	7,825	7,825	7,825	7,825

Panel B. Deleveraging through Debt Structure Changes in Detail

	Bonds and Notes	Commercial Paper	Terms Loans	Capital Leases	Other Borrowings
Dependent Variable = Debt Type	(1)	(2)	(3)	(4)	(5)
Active Institutional Investors	-0.360***	-0.609***	0.082	-0.009	0.137
	(0.078)	(0.175)	(0.070)	(0.081)	(0.106)
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	138.2	138.2	138.2	138.2	138.2
T-stat on Instrument in First Stage	11.76	11.76	11.76	11.76	11.76
First-Stage R-squared	28%	28%	28%	28%	28%
Adjusted R-squared	61%	22%	51%	49%	33%
Observations	40,552	40,552	40,552	40,552	40,552
Unique Firms	5,435	5,435	5,435	5,435	5,435

Table 8 Deleveraging Occurs in Highly Levered Firms

This table presents estimates of the effect of active institutional investors at different levels of financial leverage. Panel A presents estimates across various quantiles from instrumental variable quantile regressions as is outlined in Eq. (3). Panel B reports test statistics that use point estimates from each of the 100 quantiles to test the hypothesis that high and low leverage firms react the same to active institutional ownership. In all specifications, the instrument for institutional holdings is mutual funds' implied trades. Additional 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. Firm fixed effects and industry-by-year fixed effects are included in each regression. Firm fixed effects and year fixed effects are included in each regression. Estimated coefficients are scaled by the corresponding variable's standard deviation. ***, **, and * represent statistical significance at the 1%, 5%, and 10% level, respectively. Appendix A provides variable definitions.

Panel A. Effect of Active Institutional Investors Across Leverage Quantiles

Quantile (τ)	Coefficient at τ on Active Institutional Investors	Financial Leverage (τ)	Estimate of Financial Leverage (τ) after 10% Increase in Active Institutional Investors
(1)	(2)	(3)	(4)
0.95	-0.388***	84.1%	76.7%
0.90	-0.348***	69.3%	62.7%
0.85	-0.329***	60.5%	54.3%
0.80	-0.327***	54.0%	47.8%
0.75	-0.272***	48.7%	43.5%
0.66	-0.194***	40.6%	36.9%
0.50	-0.197***	27.7%	23.9%
0.33	-0.177***	12.9%	9.5%

Panel B. Tests that Active Institutional Investors Exert Greater Influence on Highly Levered Firms

	95% Test Statistic	Kolmogorov-Smirnov Test for Equality
	(1)	(2)
Constant Effect ($\tau < 0.33$ equals $\tau > 0.66$)	(13.11)***	D = 0.966, Reject Equality
Constant Effect ($\tau < 0.5$ equals $\tau > 0.5$)	(15.76)***	D = 0.952, Reject Equality

Table 9 Trade-off Between Financial Distress and Agency Reduction

This table presents instrumental variable estimates exploring the trade-off between financial distress and agency reduction that firms face when deleveraging. Panel A reports standardized coefficient estimates from instrumental variable regressions for firms with high financial distress in Columns (1) and (3) and low financial distress in Columns (2) and (4). It uses the specification outlined in Eq. (1). The instrument for active institutional investors is mutual funds' implied trades. In all specifications financial leverage is the dependent variable. Additional 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. Firm fixed effects and industry-by-year fixed effects are included in each regression. Estimated coefficients are scaled by the corresponding variable's standard deviation. ***, **, 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 B reports standardized coefficient estimates from instrumental variable regressions when the dependent variable is an alternative agency-reducing mechanism. In columns (1) through (3) the dependent variables are debt covenants. In Column (4) the dependent variable is dividend payments. Additional firm controls are exactly the same as above for Columns (1) through (3). In Column (4) dividend payments and payout are excluded from the control list while leverage is included. Appendix A provides variable definitions.

Panel A. Influence Financially Distressed Firms to Deleverage Dependent Variable = Financial Leverage	Financial Grey Zone Firms (1)	Financial Safety Zone Firms (2)	Above Median Financial Distress (3)	Below Median Financial Distress (4)
Active Institutional Investors	-0.266***	-0.162**	-0.281***	-0.159***
	(0.057)	(0.076)	(0.075)	(0.060)
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	208.8	71.6	115.4	135.1
T-stat on Instrument in First Stage	14.45	8.46	10.74	11.62
First-Stage R-squared	23%	27%	22%	27%
Adjusted R-squared	66%	75%	67%	74%
Observations	76,899	28,483	52,446	52,567
Unique Firms	9,133	3,607	7,414	5,622

Panel B. Support Alternative Agency-reducing Incentives Dependent Variable = Alternative Agency-reducing Mechanisms	Net Worth Covenants (1)	Financial Covenants (2)	Total Covenants (3)	Dividends (4)
Active Institutional Investors	0.220***	0.124*	0.160**	0.158***
	(0.080)	(0.063)	(0.064)	(0.048)
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	288.8	288.8	288.8	279.9
T-stat on Instrument in First Stage	16.99	16.99	16.99	16.73
First-Stage R-squared	37%	37%	37%	24%
Adjusted R-squared	40%	57%	56%	76%
Observations	106,537	106,537	106,537	106,537
Unique Firms	10,396	10,396	10,396	10,396

Appendix A. Variable Definitions

Institutional Investors 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.

Active Institutional Investors combines Thomson-Reuters 13F SEC filings with the classification of 13F filing institutions from Bushee (2001). Bushee classifies institutions as either "dedicated," "transient," or "quasi-index." We designate "quasi-index" investors as passive and all others as active. This is consistent with Appel, Gormley, and Keim (2016)).

Financial Leverage $= \frac{DLTT+DLC}{DLTT+DLC+SEQ+MIB}$ (see Welch (2011)).

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

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

Financial Leverage (Market) $= \frac{DLTT+DLC}{(AT-SEQ-MIB+MEQ)}$ (see Welch (2011)).

Balance Sheet Leverage (Market) $= \frac{LT}{(AT-SEQ-MIB+MEQ)}$ (see Welch (2011)).

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.

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.

Implied Mutual Fund Trades is the firm-specific, calendar-year annual dollar change in mutual fund holdings implied by idiosyncratic individual-investor inflows and outflows. To construct this variable, we combine quarterly data on mutual fund holdings from Thomson Reuters and quarterly data on stock returns, assets under management, and fund flows from CRSP. First, we apply definitions from Sirri and Tufano (1998) to isolate individual investor inflows to and outflows from each mutual fund in a given quarter. Next, we cleanse the individual-investor flows of variation attributable to individual-investors' chasing managerial skill and reputation (Berk and Green (2004)) as well as chasing fund performance (Chevalier and Ellison (1997)). Specifically, we regress quarterly individual-investor flows on a flexible form of mutual fund returns and mutual fund fixed effects. We label the residual from these regressions as the idiosyncratic individual-investor flow because they are meant to represent variation in individual-investors' flows attributable to phenomenon such as liquidity needs that have no direct or indirect relationship with corporate leverage. Then, we use the actual holdings of the mutual fund in a given quarter to project changes in individual stock holdings in the next quarter based on the magnitude of the idiosyncratic, individual-investor flows and trading volume for that stock in the given quarter. We limit the set of mutual funds projected to change their individual stock holdings to those that do not experience a greater than 200% change in assets under management, and we limit the set of mutual funds to those that do not specialize in specific industries. The industry exclusion, which is defined at the two-digit SIC level, mitigates concerns that investors' flows are correlated with industry-specific business cycles. Finally, we sum the projected changes in stock holdings from the mutual funds that remain in the set to an individual stock-year frequency. Figure 3 illustrates this procedure.

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

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

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 (see Brav et al. (2008)).

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 (see Brav et al. (2008)).

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

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

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

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

Net Equity Issuance = $\frac{SSTK-PRSTKC_{t-1}}{AT}$

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.

Financial Grey Zone Firms = $Altman's\ Z - Score < 2.6$

Financial Safety Zone Firms = $Altman's\ Z - Score > 2.6$

Above Median Financial Distress = $Altman's\ Z - Score < 1.75$

Below Median Financial Distress = $Altman's\ Z - Score > 1.75$

Covenants data comes from DealScan. Financial and net worth covenant data at the packageid level are matched to loan facilities. Based on the start date of the loan facility, a count of covenants by covenant type are matched to a calendar year and firm.

Analyst Coverage uses IBES data to obtain the total number of analysts covering a stock in a calendar year.

Variation in External Financing Costs = $\frac{(SSTK-PRSTKC)+(DLTIS-DLTR+(DLC-DLC_{t-1}))}{\sum(SSTK-PRSTKC)+\sum(DLTIS-DLTR+(DLC-DLC_{t-1}))}$ (see Baker and Wurgler (2002)).

Credit Ratings come from Mergent's Fixed Income Securities Database (FISD), which provides rating at issuance from S&P, Moody's, Fitch, and Duff & Phelps. All ratings are converted into a numeric variable that preserves the ranking (i.e., Aaa = 1 for Moody's and AAA = 1 for S&P, Aa1 = 2 for Moody's and AA+ = 2 for S&P, etc ...). If FISD is missing ratings data, S&P Ratings Xpress from Compustat is used as a supplement.

Tax Rates are simulated marginal tax rates (see Graham (2000)).

Research & Development Activity = Log(XRD)

Domestic Only are firms for which $\frac{PI}{PIFO} < 0.05$ (see Graham (2000)).

Entrenchment-Index (E-index) measures the strength of corporate governance using the most relevant governance matters for firm value as shown by Bebchuk, Cohen, and Ferrel (2009).

Cremers & Petajisto Active Share is the average percentage of mutual fund owners in a stock that are active as defined in Cremers and Petajisto (2009). The measure captures the percentage of the mutual fund's portfolio that differs from the passive benchmark index. Any fund with an active share greater than 60% is considered active. The data are available at http://www.petajisto.net/data.html.

Non-recessionary Year is a dummy variable that takes a value of 1 if the U.S. economy was not officially in a recession for 2 or more quarters in that year according to the NBER's Business Cycle data. The data are available at http://www.nber.org/cycles/recessions.html.

Low Government Debt Year is a dummy variable that takes a value of 1 if the U.S. government debt issuance decreased from the previous year. The data are available at http:

//www.treasury.gov/Pages/default.aspx.

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.

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Appendix B. Additional Tables

Table B1 The Effect of Active Institutional Investors on Leverage (Details)

This table details the first stage of the regression used to estimate the effect of active institutional investors on financial leverage as outlined in Eq. (1). Estimated coefficients are scaled by the corresponding variable's standard deviation. ***, **, and * represent statistical significance at the 1%, 5%, and 10% level, respectively. Robust standard errors, clustered at the firm level, are reported in parentheses.

Dependent Variable =	First Stage Active Institutional Investors (1)	Second Stage Financial Leverage (2)
Mutual Funds' Implied Trades	0.049***	
	(0.003)	
Active Institutional Investors		-0.245***
		(0.048)
Firm Size	0.440***	0.456***
	(0.019)	(0.032)
Tangibility	-0.007	0.002
	(0.015)	(0.020)
Intangibility	-0.040***	0.084***
	(0.007)	(0.009)
Collateral	-0.059***	0.203***
	(0.014)	(0.019)
Profitability	0.051***	-0.007
	(0.007)	(0.010)
Losses	0.001	0.006
	(0.004)	(0.005)
Dividend Paying Firm	-0.023***	-0.121***
	(0.007)	(0.008)
Payout Yield	-0.026***	0.007
	(0.004)	(0.004)
Lifecycle Stage	-0.028***	-0.061***
	(0.008)	(0.012)
Market-to-book	0.083***	-0.020***
	(0.005)	(0.007)
Investment	0.029***	-0.021***
	(0.003)	(0.003)
Sales Growth	0.002	0.015***
	(0.002)	(0.003)
Asset Growth	0.010***	0.024***
	(0.003)	(0.003)
Firm Risk	0.044***	0.012***
	(0.003)	(0.004)
Market Risk	-0.038***	-0.015***
	(0.005)	(0.006)
Adjusted Returns	-0.008**	-0.001
	(0.004)	(0.004)
Amihud's Illiquidity	0.006***	0.039***
	(0.002)	(0.005)
Altman's Z-Score	0.027***	-0.284***
	(0.008)	(0.013)
Insider Holdings	-0.021***	0.004
	(0.003)	(0.003)
Firm FE & Industry-by-Year FE	Yes	Yes
F-stat	288.8	17.1
Kleibergen-Paap F-stat	278.2	
Adjusted R-squared	37%	66%
Observations	106,537	106,537

Table B2 The Effect of Active Institutional Investors on Leverage

This table provides evidence from several robustness checks of the estimated effect of active institutional investors on financial 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 3. Estimated coefficients are scaled by the corresponding variable's standard deviation. ***, **, 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. Robustness Checks with More Controls	IV Estimate (1)	Observations (2)	First-stage F-stat (3)	T-stat on Instrument (4)
Analyst Coverage	-0.229***	106,537	288.1	16.97
	(0.048)			
Entrenchment-Index (E-Index)	-0.474***	20,126	35.4	5.95
	(0.161)			
Variation in External Financing Costs	-0.215***	87,917	225.3	15.01
	(0.055)			
Credit Ratings	-0.257***	106,537	292.4	17.10
	(0.048)			
Tax Rates	-0.201***	102,186	294.2	17.15
	(0.047)			
Research & Development Activity	-0.242***	106,537	288.9	17.00
	(0.048)			
Domestic Only Operations	-0.245***	106,537	290.4	17.04
	(0.048)			
Passive Institutional Investors	-0.279***	106,537	273.6	16.54
	(0.051)			
Panel B. Robustness Checks with Alternative Definitions of Active Institutional Investors				
Cremers & Petajisto Active Share	-0.471***	75,229	109.91	10.48
	(0.098)			
Percentage Held by Top 5 Institutions	-0.480***	106,537	102.74	10.14
	(0.104)			
Percentage Held by Top 10 Institutions	-0.510***	106,537	112.65	10.61
	(0.108)			
Percentage Held by Blockholders	-0.518***	106,537	57.33	7.57
	(0.123)			
Panel C. Robustness Checks with Alternative Instrument Construction				
Without Fund Fixed Effects	-0.238***	106,537	310.23	17.61
	(0.046)			
Using Linear Functional Form for Fund Returns	-0.237***	106,537	358.24	18.93
	(0.043)			
With Funds Experiencing >200% Change in AUM	-0.332***	106,537	231.69	15.22
	(0.057)			
Without Individual-investor Redemptions	-1.159***	106,537	36.04	6.00
	(0.231)			
Without Individual-investor Inflows	-0.471***	106,537	210.1	14.49
	(0.066)			
Panel D. Robustness Checks with Alternative Timing				
Lagged Active Institutional Investors	-0.195***	94,211	82.07	9.06
	(0.048)			
Panel E. Robustness Checks with Alternative Samples				
Limiting Sample to Firms with 10 Years of Data	-0.269***	76,608	217.76	14.76
	(0.057)			
Limiting Sample to Firms with 20 Years of Data	-0.257***	37,910	98.10	9.90
	(0.086)			
Limiting Sample to Constant Number of Firms	-0.207***	52,972	108.49	10.42
	(0.080)			
Limiting Sample to Non-recessionary Years	-0.231***	87,734	397.45	19.94
	(0.041)			
Limiting Sample to Low Government Debt Years	-0.103**	50,170	97.91	9.89
	(0.040)			

Table B3 Institutional Characteristics in Relation to Estimated Effects

This table explores which characteristics of the active institutional investors drive the estimated effect on financial 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 active institutional holdings is mutual funds' implied trades. The vector of firm controls is the same as in the baseline specification in Table 3. 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. Estimated coefficients are scaled by the corresponding variable's standard deviation. ***, **, and * represent statistical significance at the 1%, 5%, and 10% level, respectively. Robust standard errors, clustered at the firm level, are reported in parentheses.

Quantile	(1)	(2)	(3)	(4)	(5)
Mean Change in Active Institutional Investors in Quantile	-11.6%	-2.0%	0.1%	2.6%	11.1%
Median Change in Active Institutional Investors in Quantile	-9.0%	-1.9%	0.0%	2.5%	9.1%
Dependent Variable = Financial Leverage					
Active Institutional Investors	-0.331**	-0.257**	-0.334*	-0.302**	-0.358**
	(0.149)	(0.128)	(0.177)	(0.148)	(0.166)
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	45.1	49.1	25.2	39.5	27.9
T-stat on Instrument in First Stage	6.71	7.01	5.02	6.29	5.28
First-Stage R-squared	32%	25%	19%	28%	30%
Adjusted R-squared	68%	67%	68%	68%	68%
Observations	19,091	19,090	19,091	19,090	19,090
Unique Firms	6,542	6,935	6,416	6,660	6,381

Table B4 Placebo Test of Active Institutional Investors on Leverage

This table explores active 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 3. Estimated coefficients are scaled by the corresponding variable's standard deviation. ***, **, 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	Pacebo IV
Dependent Variable =	Financial Leverage	
	(1)	(2)
Active Institutional Investors	-0.245***	0.021
	(0.048)	(0.136)
Firm Size	0.456***	0.339***
	(0.032)	(0.066)
Tangibility	0.002	0.003
	(0.020)	(0.020)
Intangibility	0.084***	0.094***
	(0.009)	(0.010)
Collateral	0.203***	0.220***
	(0.019)	(0.020)
Profitability	-0.007	-0.021*
	(0.010)	(0.012)
Losses	0.006	0.006
	(0.005)	(0.005)
Dividend Paying Firm	-0.121***	-0.115***
	(0.008)	(0.008)
Payout	0.007	0.014***
	(0.004)	(0.005)
Lifecycle Stage	-0.061***	-0.053***
	(0.012)	(0.012)
Market-to-book	-0.020***	-0.043***
	(0.007)	(0.013)
Insider Holdings	0.004	0.009**
	(0.003)	(0.004)
Additional Firm Controls	Yes	Yes
Firm Fixed Effect	Yes	Yes
Industry-by-Year Fixed Effects	Yes	Yes
First-stage F-stat	288.8	39.1
T-stat on Instrument in First Stage	16.99	6.25
First-Stage R-squared	37%	23%
Adjusted R-squared	66%	66%
Observations	106,537	106,537
Unique Firms	10,396	10,396

Appendix 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. 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.

Given that 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. 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 C1; 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.

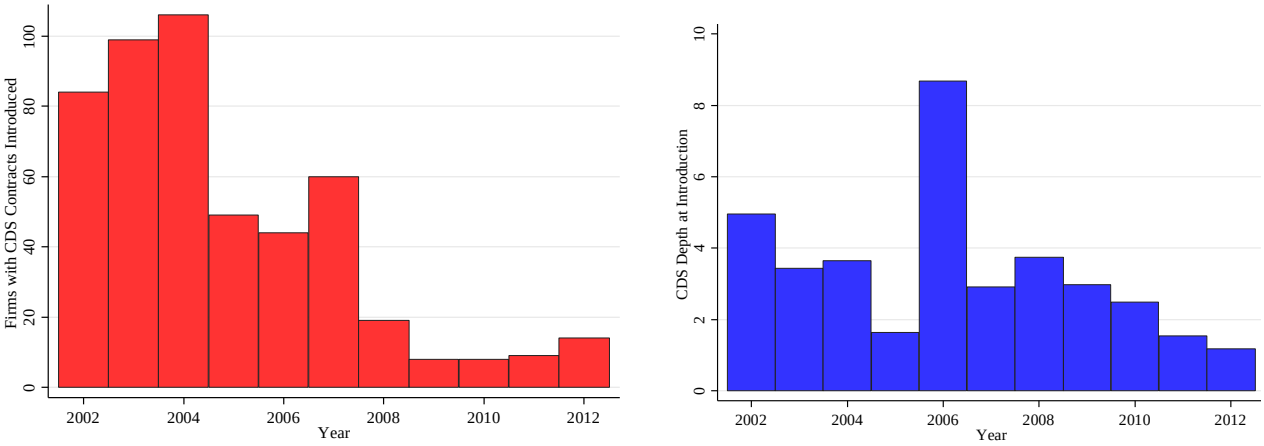

Figure C1. Introduction of CDS: The left-hand side plot shows the frequency by year that firms first had CDS contracts trade on their outstanding debt. The right-hand side plot shows the CDS depth, which we define as the typical number of contributors providing daily CDS price quotes for the firms that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

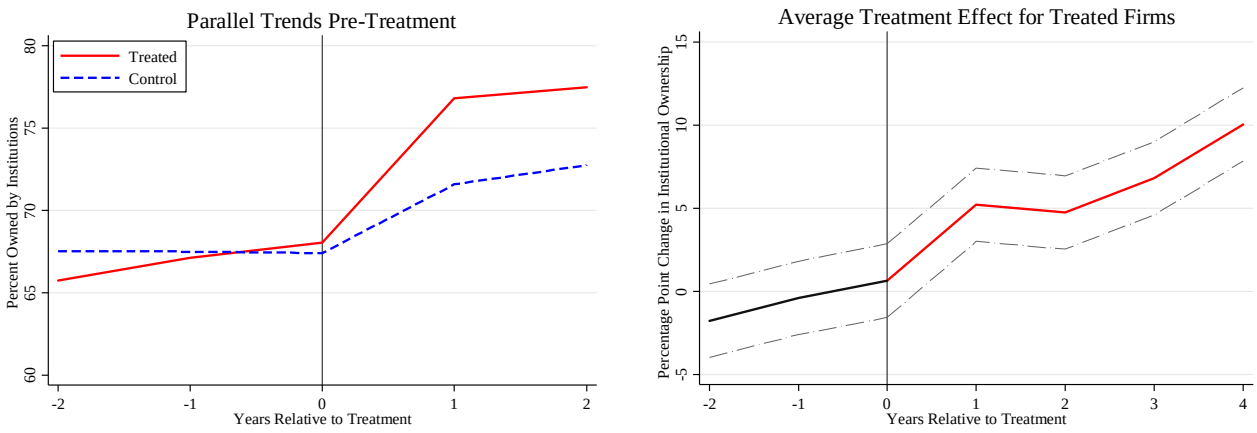

Figure C2. Evidence for Leverage Clienteles: The left-hand side plot shows the treated firms (i.e., those with CDS contracts) as a red, solid line and the control firms as a blue, dashed line. Both sets of firms follow parallel paths prior to the introduction of CDS contracts but diverge post-introduction. 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 C1 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)

References

Saretto, Alessio, and Heather Tookes. 2013. Corporate Leverage, Debt Maturity, and Credit Supply: The Role of Credit Default Swaps. *Review of Financial Studies* 26:1190–1247.