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Threshold Events and Identification: A Study of Cash Shortfalls

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ABSTRACT

Threshold events are discrete events triggered when an observable continuous variable passes a known threshold. We demonstrate how to use threshold events as identification strategies by revisiting the evidence in Rauh (2006, Investment and financing constraints: Evidence from the funding of corporate pension plans, *Journal of Finance* 61, 33–71) that mandatory pension contributions cause investment declines. Rauh's result stems from heavily underfunded firms that constitute a small fraction of the sample and that differ sharply from the rest of the sample. To alleviate this issue, we use observations near funding thresholds and find causal effects of mandatory contributions on receivables, R&D, and hiring, but not on investment. We also provide useful suggestions and diagnostics for analyzing threshold events.

THE IDENTIFICATION OF CAUSAL EFFECTS is of central interest in empirical corporate finance. One particularly interesting class of identification strategies involves the use of threshold events, in which a discrete event or treatment occurs when an observable continuous variable passes a known threshold. These thresholds can occur because of accounting conventions, government regulations, or contractual agreements, among other things. Given the increasing use of identification strategies based on threshold events, the intent of this paper is threefold.

First, we provide general guidance on how one can and cannot use threshold events to identify causal relationships. We do so in the context of one of the first and most prominent examples of the use of threshold events to obtain identification: Rauh (2006). This paper is an ideal setting for understanding threshold events because the institutional framework provides more than one type of threshold to examine. The threshold events are the mandatory contributions that firms must make to their defined benefit (DB) pension plans. These contributions are a direct function of the level of pension plan funding (assets minus liabilities), and they jump up discretely at several different funding thresholds. The paper is also an ideal setting because it tackles the important question of whether the cash shortfalls accompanying mandatory contributions cause

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declines in investment. The challenge in identifying a causal relation is the possibility that mandatory contributions are endogenously determined with investment. Rauh finds that firms cut their capital expenditures 70 cents for every dollar of mandatory contributions to their pension plans. The paper then argues that this effect is causal because the jumps and kinks in the relation between contributions and funding provide a source of exogenous variation in cash flow and therefore identification of a causal effect of contributions on investment.

However, although the use of pension funding rules to obtain identification is clever, the result of a sharp cut in investment is puzzling because mandatory contributions are on average tiny relative to capital expenditures. It is hard to imagine that capital expenditures are sufficiently divisible to produce such a strong effect, especially in light of evidence of substantial capital stock adjustment costs (Cooper and Haltiwanger (2006)). Given this background, the second purpose of this paper is to understand the result of sharp investment declines. Finally, the third purpose of the paper is to exploit in detail the rich institutional setting of pension funding to determine exactly how firms respond to cash shortfalls.

In short, we find that the strong sensitivity of investment to mandatory contributions stems from heavily underfunded firms that constitute a small fraction of the sample and that are different from the rest of the sample in important ways, that is, the control group differs from the treated group. Thus, though we find some evidence that this sensitivity is causal, one cannot extrapolate or generalize the result beyond those severely underfunded firms, and it is difficult to rule out the possibility that firm characteristics cause both the pension underfunding and the investment declines. By closely examining those observations near the various discontinuities in the rule relating contributions to funding status, we find strong causal evidence that firms respond to these cash shortfalls by managing accounts receivable. This result makes sense given the existence of active secondary markets for receivables. We also find less robust evidence that these shortfalls induce changes in other types of working capital, employment growth, and R&D.

We also provide guidance on using threshold events to obtain identification. Teasing out the direct causal effect of a threshold event can be difficult because the forces that bring a firm to a threshold can also affect a firm's actions, that is, the variable (such as pension funding status) that defines the threshold is often endogenous. We therefore frame our discussion in terms of the econometric technique called regression discontinuity design (RDD), which sidesteps this difficulty in an intuitive way. The discontinuous event induced by the threshold rule induces a discontinuity in the actions taken by firms at the threshold. Because observations immediately to one side of the threshold are unlikely to differ in systematic ways from the observations immediately to the other side, the former group acts as a valid control for the latter. Therefore, as long as firms have imperfect control over their location vis-à-vis the threshold, a researcher can measure the causal effect of the event.

Pension funding is an ideal setting for understanding how to use threshold events for identification because it is not a strict application of RDD. Instead, it shares some important features of RDD, in particular, threshold events. Therefore, although examining the identification strategy in Rauh (2006) is only somewhat useful for understanding when RDD works and when its results can be misleading, such an examination is useful for exploring the broader question of when it is valid to extend the intuition behind RDD to more general regression settings.

We distill several important econometric takeaways from our analysis of pension funding. We demonstrate that including discontinuous variables in generic regressions usually does not provide identification, even if one controls for the threshold variable that induces the discontinuity. This conclusion follows from the important point that RDD has strong local validity but weak external validity. Put differently, RDD ameliorates the endogeneity problems that compromise clean inference only for those observations close to the threshold. In this regard, using threshold events to obtain identification is analogous to using any instrumental variables (IV) estimator. Just as IV estimators obtain identification from those observations that are affected by the instrument, threshold events usually only provide identification for those observations close to the cutoff. To address these identification issues, we offer some useful robustness checks for determining when threshold events are likely to be providing identification.

In addition, our analysis highlights the importance of understanding firm incentives and behavior when using threshold events as the basis for identification strategies. First, unobserved firm behaviors can change at thresholds, and these unobserved changes can have important effects on any outcome of interest. Here, we provide guidance for detecting such situations. Second, and more importantly, firms optimize subject to the constraints imposed by threshold rules and endogenously choose whether they want to be close to a threshold. That firms actively manage and anticipate future mandatory contributions is vividly illustrated in this quote from Ford Motor Company's 2005 10K:

> In 2005, we made $2.5 billion of cash contributions to our funded pension plans. During 2006, we expect to contribute $1.5 billion to our worldwide pension plans.... Based on current assumptions and regulations, we do not expect to have a legal requirement to fund our major U.S. pension plans in 2006.[1]

Clearly, Ford is planning its contributions in 2005 and 2006 to avoid having to face unanticipated financial shocks in 2006. We point out that, although RDD can remedy the endogeneity problems induced by such behavior, the identification afforded by RDD can be invalidated if this behavior leads firms to bunch on one side of a threshold or another. Fortunately, bunching does not pose a problem in the case of pension funding because the distance from the threshold is largely a function of stock market values, which are difficult to manipulate.

Third, threshold events are most likely to provide significant evidence when these events are themselves significant. This point is illustrated by our finding

[1] Available at http://www.sec.gov/Archives/edgar/data/37996/000003800906000031/fmc10K2005.htm (p. FS-40).

that the strongest evidence of changes in firm behavior occur around discontinuities that entail the largest cash shortfalls.

Our paper clearly fits into the rapidly growing set of studies in finance and economics that examine threshold events. Examples include changes in accounting standards when a firm's public float exceeds a certain level (Gao, Wu, and Zimmerman (2009)) or cash inflows that stem from option exercise (Babenko, Lemmon, and Tserlukevich (2011)). Further studies that explicitly use RDD include Chava and Roberts (2008), Beshears (2010), Cuñat, Gine, and Guadalupe (2011), and Keys, et al. (2010). These studies use RDD to measure the causal effects of bond covenant violations, strategic alliances, shareholder proposals, and loan securitization. Our paper also falls into a group of recent applied econometrics papers that provide guidance to empirical researchers by sorting through theoretical econometric results in concrete settings. For example, Bertrand, Duflo, and Mullainathan (2004) consider inference in difference-in-difference estimators, and Petersen (2009) examines the bias in standard errors calculated with panel data. In terms of economic content, our paper contributes to the extensive literature on financing constraints surveyed, for example, in Stein (2003) and Hennessy and Whited (2007). In particular, our paper helps clarify the common practice, dating back to the influential work of Fazzari, Hubbard, and Petersen (1988), of using the sensitivity of investment to cash flow as a metric for gauging the severity of finance constraints.

The rest of the paper proceeds as follows. In Section I, we provide a brief review of the identification conditions for RDD. Sections II and III describe the institutional setting and the data, respectively. Sections IV and V contain the results, and Section VI concludes.

I. Review of Regression Discontinuity Design

We now outline the basic features of RDD, which was first introduced by Thistlethwaite and Campbell (1960). More detailed presentations can be found in Van der Klaauw (2002) and Imbens and Lemieux (2008). Our description follows the presentation in Hahn, Todd, and Van der Klaauw (2001). We start with the intuitive description in the introduction that firms just above and below the threshold can be thought of as close-to-randomly assigned to a treatment, for example, a pension funding violation. Therefore, by calculating the average differences between the investment (or any other variable of interest) of these two groups of firms, one can estimate the causal effect of the treatment.

More formally, let y_i be a variable of interest, such as investment, employment, liquid assets, or external financing. We define y_{i1} as the outcome of firm i given treatment (pension funding violation) and y_{i0} as the outcome of firm i in the absence of treatment. The goal is to estimate $y_{i1} - y_{i0}$, which is the effect of treatment on firm i. It is important to note that y_{i1} and y_{i0} are the pair of potential outcomes for firm i and that we only observe one of these variables for each firm. We cannot roll back history and observe the other outcome, so this unobserved outcome is the counterfactual, which we have to estimate. This constraint forces us to focus on average effects of treatment over (sub)populations rather than on individual effects.

RDD comes in two flavors: sharp and fuzzy. We focus mainly on sharp in as much as it is the type of RDD that is most relevant to the pension funding threshold. Sharp RDD is distinguished by the key identifying assumption that firms are assigned to treatment *solely* on the basis of an *observed*, continuous measure s, called the selection (forcing, running, assignment) variable. The restriction that s must be observable rules out threshold events such as accounting disclosures in which the selection variable can only be observed on one side of the threshold. The selection variable can be either a single variable or a real-valued function of several variables. The observations that fall below some deterministic cutoff point $\bar{s}$ are placed in the control group, which we denote by setting an indicator variable, w_i, equal to zero. Those observations on or above that point are placed in the treatment group, that is, $w_i = 1$. To summarize

$$w_i = w(s_i) = 1\{s_i \geq \bar{s}\}. \tag{1}$$

A second identifying assumption is that the forcing variable, s, has a positive density in a neighborhood of the cutoff $\bar{s}$. This assumption rules out manipulation of the forcing variable, which might result in either no observations near the threshold or bunching of observations on one side of the threshold. McCrary (2008) notes that some manipulation of the assignment variable can be tolerated before identification is compromised, and he provides a test for detecting manipulation. Of course, visual inspection of the density of the forcing variable is the first way in which to address this issue because gross violations are usually easily visible.

Note that s_i is allowed to affect the outcome. In other words, assignment to treatment is not random. This condition is important because funding status (s_i) depends on firm financial health, which in turn affects investment. To understand how RDD identifies a causal effect in the face of this endogeneity problem, we write the treatment effect estimation problem as a regression,

$$y_i = \beta + \alpha w_i + u_i, \tag{2}$$

where β is a constant and α is the average treatment effect. For simplicity, we assume that the treatment effect is constant across observations.[2] The regression error, u_i, can be thought of as what would happen in the absence of treatment. Running ordinary least squares (OLS) on this regression does not produce causal inference because assignment to treated and control groups is by definition not random, so the error, u_i, is correlated with the regressor, w_i. In this setting, the key identifying assumption for sharp RDD—that s_i is the sole determinant of treatment—can be restated as saying that the probability of assignment jumps from zero to one at the cutoff. Formally,

$$\begin{aligned}\lim_{s \downarrow \bar{s}} \Pr(w_i = 1 \mid s_i) &= 1\\ \lim_{s \uparrow \bar{s}} \Pr(w_i = 1 \mid s_i) &= 0.\end{aligned} \tag{3}$$

[2] See Imbens and Lemieux (2008) for the more general case.

We now use this condition to show that the treatment effect, α, can be defined as:

$$\alpha = \lim_{s \downarrow \bar{s}} E(y_i \mid s_i) - \lim_{s \uparrow \bar{s}} E(y_i \mid s_i). \tag{4}$$

This quantity is simply the difference between expected outcomes for those observations just above and below the cutoff. To show that (4) holds, we first subtract the right limit of (2) from the left limit of (2). We obtain

$$\lim_{s \downarrow \bar{s}} E(y_i \mid s_i) - \lim_{s \uparrow \bar{s}} E(y_i \mid s_i) = \alpha \left(\lim_{s \downarrow \bar{s}} E(w_i \mid s_i) - \lim_{s \uparrow \bar{s}} E(w_i \mid s_i) \right) + \left(\lim_{s \downarrow \bar{s}} E(u_i \mid s_i) - \lim_{s \uparrow \bar{s}} E(u_i \mid s_i) \right). \tag{5}$$

If we assume that $E(u_i \mid s_i)$ is continuous in s_i, then the last term goes to zero and we have

$$\alpha = \left(\lim_{s \downarrow \bar{s}} E(y \mid s) - \lim_{s \uparrow \bar{s}} E(y \mid s) \right) \Big/ \left(\lim_{s \downarrow \bar{s}} E(w \mid s) - \lim_{s \uparrow \bar{s}} E(w \mid s) \right). \tag{6}$$

If the probability of treatment does jump from zero to one at $\bar{s}$, then the denominator of (6) equals one, and we have (4). Equation (6) implies that a simple way to estimate α is as the difference in means for the groups of observations just above and below the cutoff. Expression (6) also makes it clear that the assumption of a continuous density is crucial. Because limits of expectations define the average treatment effect, if the density is discontinuous at $\bar{s}$, these limits do not exist.

The assumption that $E(u_i \mid s_i)$ is continuous in s_i is also crucial, and it is therefore important to understand what this assumption means in economic terms. On one level, if one takes regression (2) seriously, it states that the only variable that should determine the outcome variable y_i (investment, employment, etc.) is the selection variable. This interpretation is, of course, extreme, but it points out that many determinants of y_i are omitted from (2) and are therefore implicitly contained in the error term, u_i. The continuity assumption then implies that none of these variables exhibits a discontinuity at the exact threshold point. On another level, the continuity assumption formalizes the notion that observations just above and below the cutoff are comparable. Therefore, the continuity assumption goes hand-in-hand with the observation that RDD provides sharp causal inference only near the cutoff.

Although RDD is typically a technique with strong internal (near the threshold) validity and weak external (far from the threshold) validity, it is useful to examine when one can extrapolate inferences that are valid near the threshold to points away from the threshold. We start by examining sharp RDD and applying the concept of a control function from Heckman and Robb (1985). In sharp RDD, the only determinant of treatment is the selection variable, s_i, so one can write the regression error, u_i, in (2) as

$$u_i = E(u_i \mid s_i) + e_i, \tag{7}$$

where e_i is, by definition, orthogonal to $w_i = w(s_i)$. Substituting (7) into (2) then gives

$$\begin{aligned} y_i &= \beta + \alpha w(s_i) + E(u_i \mid s_i) + e_i \\ &= \beta + \alpha w(s_i) + k(s_i) + e_i, \end{aligned} \tag{8}$$

where $k(s_i) \equiv E(u_i \mid s_i)$. In general, $k(s_i)$ is a smooth function of s_i. If one estimates (8) on points away from the cutoff, one can no longer rely on the local continuity of u_i to ensure that the treated and control groups are similar. One must then not only include $k(s_i)$ in the regression (and get the functional form correct), but one must also control for the fact that observations away from the cutoff may be systematically different from observations close to the cutoff. These requirements are important for understanding the specific identifying assumptions in Rauh (2006), which we discuss in detail in the next section.

II. Motivation and Institutional Setting

The identification strategy in this paper is not strictly based on RDD. However, it does share several features of RDD, in particular, the use of a threshold rule. The rule states that a firm must make mandatory contributions when its pension assets fall below its pension liabilities. Further, both the rate and level of mandatory contributions can change at additional institutionally determined levels of underfunding. The question is then whether the local identification given by RDD can be extended to provide identification in the more general case of using discontinuous variables, such as mandatory contributions in generic regressions. To answer this question, we first review the institutional details surrounding corporate defined benefit pension plans and then discuss the extent to which these details can deliver identification of causal effects.

Companies with DB pension plans promise employees a prespecified monthly benefit at retirement. Typically, this benefit is calculated as a function of employee salary and service, and government rules state that firms must contribute to their pension plans to be able to meet these future pension liabilities. If the market value of these contributions (pension assets) exceeds the expected future pension liabilities, the pension plan is considered overfunded. Conversely, if the value of the pension assets is less than the pension liabilities, the plan is considered underfunded. If the pension plan is overfunded, the firm is free to either contribute or not contribute to the plan, although contributions above a specified ceiling receive unfavorable tax treatment. If the firm withdraws funds from an overfunded pension plan, it can be subject to severe excise taxes. On the other hand, if the plan is underfunded, the firm is required to contribute more funds to its pension plan. These mandatory pension contributions (MPCs) are determined according to a government rule that is a discontinuous and kinked function of the funding gap (the difference between pension assets and liabilities).

Pension funding rules are interesting because they generate a clear threshold event. In fact, because the rule has both discontinuities and kinks, the rule provides more than one threshold event. Pension funding is also interesting because the selection variable, the funding gap, is not likely to be completely manipulable. Fluctuations in the funding gap are driven by the present value of the pension liabilities, by plan contributions, and by the performance of the invested pension assets. Firms have limited control over the actuarial assumptions used to calculate the value of liabilities because these assumptions are governed by restrictive Internal Revenue Service (IRS) rules. Therefore, although the firm can actively manipulate the pension funding gap by voluntary contributions, changes in employment, and changes in plan benefits, fluctuations in the market value of pension assets and in the present value of liabilities that accompany market interest rate changes are largely beyond the firm's control. Limitations on firms' ability to manipulate the funding gap are important for the use of threshold rules as identification strategies.

The example is interesting, finally, because of the transparency of the endogeneity problem. The question is whether internal resources affect investment, and this question is interesting because the answer sheds light on whether financial frictions affect real economic activity. However, it is difficult to determine whether internal cash flows have a causal effect on firm investment because unobservable changes in investment opportunities can drive both investment and cash flows. Further, the usual measure of investment opportunities, the market-to-book ratio, is unlikely to capture all of the variation in true investment opportunities (Erickson and Whited (2000)). However, if one can find a source of variation in cash flows that is uncorrelated with unobserved investment opportunities, then, even though a regression of investment on cash flow may omit these unobservables, one can still estimate the causal effect of cash flow on investment.

Rauh (2006) argues that the pension funding setting can identify independent variation in cash flow. Even though pension funding status is endogenously determined with investment opportunities, the rule that relates mandatory contributions to funding status contains discontinuities and kinks. Therefore, as long as one controls for funding status in a regression of investment on proxies for investment opportunities, and the relation between investment opportunities and funding status does not contain the same kinks and discontinuities as the relation between mandatory contributions and funding status, mandatory contributions are conditionally (on funding status) uncorrelated with investment opportunities.

This logic is analogous to the logic that provides identification in RDD, in particular, the part that requires that the error term in the regression not have any discontinuities at the threshold. However, this analogy does not extend to a generic regression setting, and it is primarily in this sense that Rauh (2006) is not an exact application of RDD.

The identification strategy in this paper is based on the idea that investment opportunities do not jump down at the point of underfunding or change in slope at the point at which the mandatory contribution function changes slope. Therefore, investment opportunities cannot be correlated with mandatory contributions, conditional on funding status. This assumption is plausible.

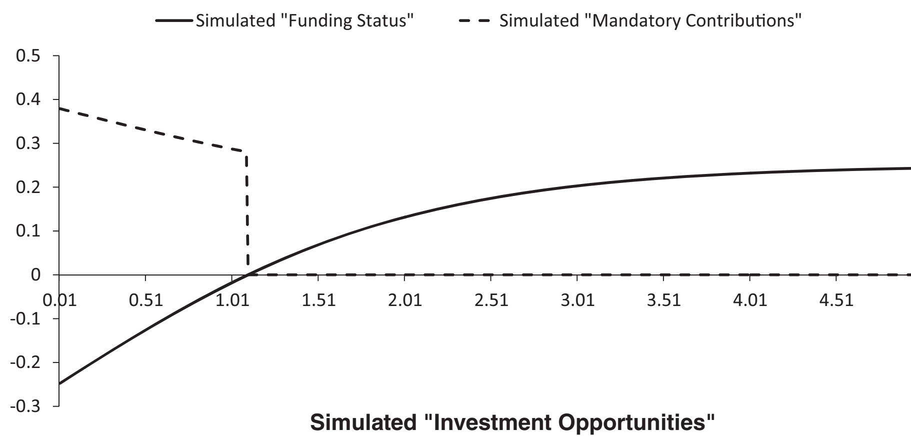


Figure 1. Simulated mandatory contributions. On the horizontal axis is simulated investment opportunities. Simulated funding status is a logistic function of investment opportunities, and simulated mandatory contributions are a kinked function of funding status.

However, although these kinks and jumps clearly diminish any correlation that might be present, they do not necessarily set it to zero. This point is easiest to see in a more general setting. Suppose that two random variables are correlated, and a third is a nonlinear function of the second. Then the claim is that the third is necessarily uncorrelated with the first, conditional on the second. This statement cannot be true if the conditional correlation is calculated via a linear regression because a linear regression cannot provide perfect conditioning in the presence of nonlinear relationships.

We also provide a simple counter example to illustrate this point. Suppose a random variable, y, has a standard lognormal distribution; a random variable, x, is a logistic function of y; and a third variable, z, is zero if $x > 0$ but equals $0.2 + 0.4\,(0.4 - x)$ if $x < 0$. This example captures the main features of the general relation between investment opportunities, funding status, and mandatory contributions; the relations between these variables are depicted in Figure 1. When we simulate a sample of 5,000 observations using these assumptions, we find that z (mandatory contributions) is correlated with y (investment opportunities), conditionally on x (funding status). The t-statistic is around 20. To summarize, it is entirely possible that mandatory contributions are conditionally correlated with investment opportunities and that the regressions in Rauh (2006) do not solve the endogeneity problem. This counterexample shows that identification strategies based on loose analogies to RDD need not be logically correct. However, they may be empirically relevant, so we now turn to examining this latter question.

III. Data

For our empirical analysis, we attempt to reconstruct the data set in Rauh (2006). We start with an unbalanced panel of Compustat firms from the 2007

Standard and Poor's Compustat industrial files. Definitions of the Compustat variables we use are in the Appendix. As in Rauh (2006), we restrict our attention to the subsample of Compustat firms that file an IRS 5500 form with the Department of Labor (DOL) and that sponsor DB pension plans. We need to impose the first restriction to calculate mandatory contributions accurately. All pension plans over a certain size must file an IRS form 5500 yearly. We impose the second restriction because only firms with DB pension plans must make mandatory contributions.

Compustat provides pension data based on firm Securities and Exchange Commission (SEC) filings; however, we do not use these data. As explained in more detail in Rauh (2006), Compustat pension data are aggregated to the firm level. They are therefore inadequate for our purposes because mandatory contributions are determined at the pension plan level—not the firm level. Approximately one-third of the firms in our sample have more than one pension plan, and relying on Compustat data would therefore lead to inaccurate computation of firms' mandatory contributions. Furthermore, firms have significantly more accounting discretion when submitting SEC filings than when filing form IRS 5500. SEC pension data also include both international and domestic pension plans, whereas only domestic plans are required to pay mandatory contributions. Finally, the methods for computing pension liabilities and costs for SEC filings are different from those that are required for computing mandatory contributions.

The sample period runs from 1990 to 1998. The sample starts in 1990 because IRS 5500 forms are first available in standardized form from the DOL in this year. The sample stops in 1998 because we match plans to firms primarily by using the CUSIP of the plan's sponsor, and in 1998 reporting requirements no longer forced pension plans to list the CUSIP pertaining to the plan. Although we match primarily by CUSIPs, missing data occasionally force us to match by employee identification numbers (EINs) or by hand using exact firm names. EINs and firm names only match plans that directly pertain to the Compustat firm. For instance, if a firm's subsidiary sponsors a plan, this plan cannot be matched without access to the CUSIP of the parent firm. Although we attempt to mitigate this problem by using subsidiary names from the Compustat Business Information File, without the CUSIP of the parent of the subsidiary sponsoring the plan, we face potential sample selection issues. After this matching process, we end up with 7,905 firm-year observations, a number somewhat smaller than that in Rauh (2006).

For each plan-year, we extract the following variables from the plan's IRS 5500 filing: pension assets, pension liabilities, funding credits, total contributions, and normal cost, which is the present value of the pension benefits that accrue during the current year. Pension assets are the present value of assets in the plan at the beginning of the year. Pension liabilities are the accumulated benefit obligation from Schedule B of the IRS 5500. From 1991 to 1994, the calculation of pension liabilities follows the regulations in the Omnibus Budget Reconciliation Act of 1987. After 1994, these calculations follow the regulations in the Retirement Protection Act of 1994.

How are mandatory contributions determined? Over the sample period from 1990 to 1998, mandatory contributions consist of two components: the minimum funding contribution (MFC) and the deficit reduction contribution (DRC). Firms must contribute the larger of the MFC or the DRC. The MFC is defined as the sum of the normal cost and an installment payment on unfunded liabilities. It is the normal cost component that provides the necessary discontinuity at the point of underfunding. Given that some discretion is allowed in calculating this installment, we follow Munnell and Soto (2004) and Rauh (2006) and estimate it as 10% of the funding gap. Therefore, the MFC is given by

$$MFC = (\text{Normal Cost}) + 0.1(\text{Funding Gap}).$$

The MFC can be offset by accumulated funding credits, which can be estimated from the 5500 filings. The DRC is given as a fraction of funding status:

$$\begin{aligned}
&\frac{DRC}{\text{Funding Gap}}\\
&\quad = \min\left[0.30, \left(0.30 - 0.4\left(\frac{\text{Pension Assets}}{\text{Pension Liabilities}} - 0.6\right)\right)\right], \qquad \text{before 1995}\\
&\quad = \min\left[0.30, \left(0.25 - 0.4\left(\frac{\text{Pension Assets}}{\text{Pension Liabilities}} - 0.35\right)\right)\right], \qquad \text{1995 and after.}
\end{aligned}$$

The mandatory contribution (MPC) is then given by the following function

$$MPC = \max\,[MFC, DRC],$$

if the funding gap is negative. The minimum and maximum in the above definitions create kinks in the mandatory contribution function. At these kinks, the proportion of the funding gap that must be filled changes. As long as one defines the threshold event as a change in this proportion, these kinks can also be used to define threshold events.

The change to the DRC in 1995 was also accompanied by two provisions that created further discontinuities in the mandatory contribution function. First, plans that were at least 90% funded were exempt from the DRC, and second, plans that were at least 80% funded and that had a recent history of being overfunded were also exempt. These thresholds imply actual discontinuities because at these thresholds the DRC can be much larger than the MFC.

In this setting, two natural definitions of the funding gap arise. In either case, for a firm with one plan, defining the funding gap is straightforward. However, defining a funding gap for a firm with multiple plans is more complicated. One alternative, from Rauh (2006), is to sum the gaps across all pension plans within a firm. This definition, which we refer to as the average funding status, correctly identifies the zero underfunding point only if firms can easily shift funds across plans. Another alternative is to define the funding gap as the smallest gap if all plans have positive funding status, and as the sum of the negative gaps if any plans have negative funding status. This definition captures the notion of the distance to the point of violation if firms cannot shift funds across plans. As we show below, the use of one definition or another is immaterial for our full-sample regressions.

IV. Full-Sample Analysis

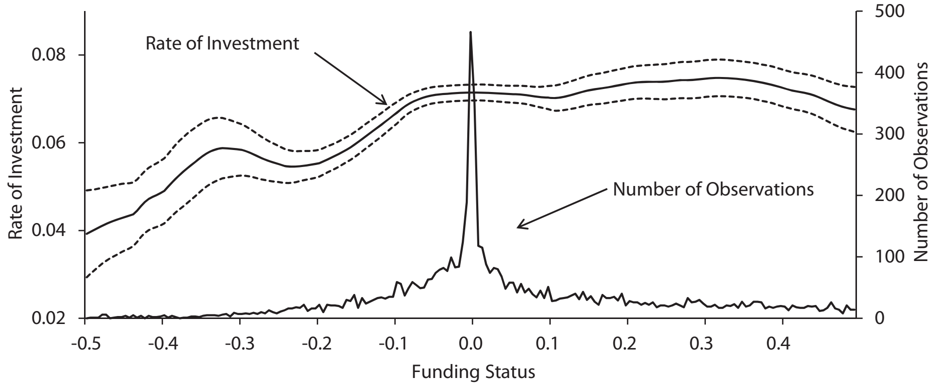
We first examine the entire spectrum of funding statuses to pinpoint exactly which kinks and jumps in the mandatory contribution function are likely to provide a causal effect on investment. Here, we use both informal visual and formal statistical analysis and pay close attention to the characteristics of the firms at different points along the function.

A. Visual Analysis

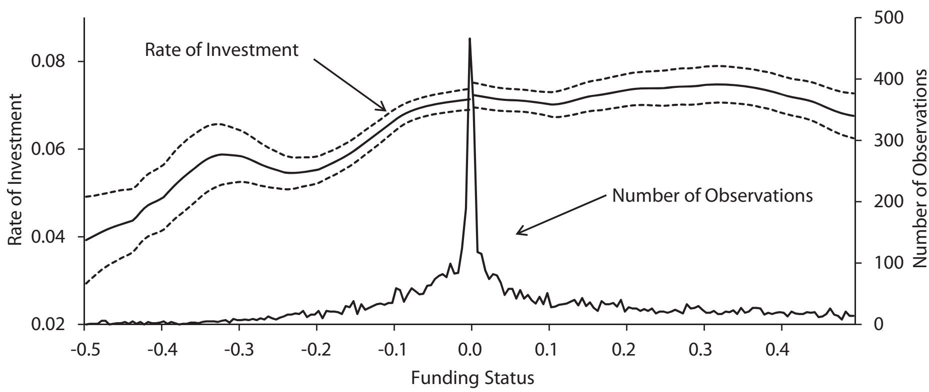
Panel A of Figure 2 plots a kernel regression of the ratio of investment to assets on average funding status, where the latter is scaled by pension liabilities for ease of interpretation. The relevant vertical axis is on the left. The solid line is the fitted regression line, and the dotted lines are 5% confidence bands. This regression closely replicates the result in Rauh (2006) that investment appears to drop off sharply after the point at which pension assets equal pension liabilities. However, the kernel regression does not tell the whole story. The solid line plots a histogram of the funding status, superimposed on the kernel regression, with the vertical axis on the right indicating the number of observations. The drop in investment does not occur until underfunding is between 10% and 15% of pension liabilities, but the histogram indicates that few observations in the sample can be found at or below this point. Indeed, the tenth percentile is at the 12% level of underfunding, and the fifth percentile is at the 22% level of underfunding.

This finding highlights one of the most important concerns facing studies that emphasize identification of causal effects: internal versus external validity. The figure suggests informally that although pension funding problems may indeed cause drops in investment, if this causal effect exists it is likely limited to a few firms. Providing more definitive evidence on this conjecture is our next task.

As a first step, we continue with our informal visual analysis to determine whether there are any causal effects around the point of zero underfunding, where a reasonably large part of the sample lies. Panel B of Figure 2 plots two kernel regressions of investment on the funding gap, one using only observations with a positive gap, and the other using only observations with a negative gap. This type of informal analysis is recommended in Imbens and Lemieux (2008), who comment that formal statistical analysis is essentially just a sophisticated version of this sort of basic plot. The intent is to find a discontinuity in the outcome of interest, which in our case is investment. If no discontinuity can be observed, there is little chance that formal analysis will lead to estimates with economically and statistically significant magnitudes. We find that the two regression lines almost meet at the discontinuity point



Panel A: Full sample regressions



Panel B: Regressions to the left and right of zero funding status

Figure 2. Kernel regressions and funding status histograms. Calculations are based on a sample of firms from Compustat from 1990 to 1998. Both panels plot the univariate relation between investment (vertical axis) and funding status (horizontal axis). Superimposed on these plots is a histogram of funding status. Funding status is defined as the sum of the plan surpluses minus the sum of the plan deficits. The kernel regression estimation is performed using an Epanechnikov kernel, with a bandwidth of 0.1. In Panel A, one regression is done for the full sample. In Panel B, separate regressions are performed for positive and negative funding statuses. Dotted lines indicate 5% confidence bounds. The results show a small discontinuity at the point of zero underfunding within the 5% confidence bounds.

corresponding to a zero funding gap. Further, their difference lies well inside the 5% confidence bands, that is, there is no evidence of a significant jump in investment at the threshold. The zero underfunding point is, hence, unlikely to be providing identification.[3]

[3] We obtain similar results when we include firm-fixed effects in the kernel regressions. Details are in the Internet Appendix for this article, which is available online in the "Supplements and Datasets" section at http://www.afajof.org/supplements.asp.

This result begs the question of whether one can find significant results by performing analogous tests using the other kinks and discontinuities in the pension funding function. The answer is no. Although we do observe small differences in investment for observations to the left and right of the 90% threshold, the 80% threshold, and the kink point at which the DRC equals the MFC,[4] none of these differences are significantly different from zero.[5] This last result needs to be interpreted with caution, however, because the lack of significance is likely due to the paucity of observations near these three thresholds.

In sum, Figure 2 illustrates a useful diagnostic to employ whenever one is trying to use a threshold event to obtain identification. The main intent is to find at the threshold an effect that is both statistically significant and visually large enough that economic significance is not an issue. The secondary intent is to determine what fraction of the sample lies at any discontinuity points.

B. Regression Analysis

Table I examines the effects of funding thresholds on investment in a regression framework. First, we replicate the results in Rauh (2006) by estimating his central regression:

$$\frac{I_{it}}{A_{i,t-1}} = a_i + a_t + b_1 Q_{i,t-1} + b_2 \frac{CF_{it}}{A_{i,t-1}} + b_3 \frac{FS_{it}}{A_{i,t-1}} + b_4 \frac{MC_{it}}{A_{i,t-1}} + u_{it}, \tag{9}$$

where I_{it} is capital expenditures for firm i at time t, $A_{i,t-1}$ is beginning-of-period book assets, $Q_{i,t-1}$ is the beginning-of-period market-to-book ratio, CF_{it} is nonpension cash flow, MC_{it} is the mandatory contribution, and FS_{it} is the average funding status of all plans in a firm. All of these regressions contain fixed firm and year effects, a_i and a_t, and the standard errors are robust to heteroskedasticity and clustering at the firm level. Before turning to the results, it is important to note that (9) is not an exact application of regression discontinuity because it does not contain a funding violation indicator or indicators for whether the firm is past the 80% or 90% funding thresholds or the kink point defined by MFC = DRC. Instead, it contains mandatory contributions because of the economic question involved, which is not whether funding violations themselves affect investment, but whether internal resources affect investment.[6]

The first two columns of Table I present estimates of (9) using two different measures of the distance to a funding violation: average funding status (in column 1) and the funding gap (in column 2). As in much of the investment

[4] For a firm with one pension plan, calculating this kink point is straightforward. For those with multiple plans, we calculate the kink using the pension plan for which the kink is closest to the zero funding point.

[5] These figures are contained in the Internet Appendix.

[6] Nonetheless, it is straightforward to gauge the magnitude of the coefficient on mandatory contributions in terms of the absolute drop in investment by multiplying the coefficient by the average mandatory contribution for payers.

Table I

Investment Regressions

Estimates are from a sample of unregulated and nonfinancial firms from the 2007 Compustat annual industrial files. The sample period is 1990 to 1998. Pension data are from IRS Form 5500. MPCs are mandatory pension contributions to underfunded plans, scaled by total assets. Funding Status is the average across plans of pension assets minus pension liabilities, as a fraction of total assets. Funding Gap is the smallest funding gap if all plans have positive gaps, and the sum of the negative gaps if any plans have negative gaps. Nonpension Cash Flow is income plus depreciation plus pension expense, all scaled by total assets. Violation Indicator is a dummy variable for whether the variable "Funding Gap" is positive or negative. Distances to 80% and 90% underfunding and the corresponding indicator variables are defined analogously. Kink is the point at which the MFC equals the DRC. All regressions contain fixed firm and year effects. Standard errors are in parentheses and are corrected for heteroskedasticity and clustering at the firm level.

Market-to-Book	0.019	0.019	0.019	0.019	0.019	0.019
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Nonpension	0.113	0.113	0.112	0.111	0.111	0.112
Cash Flow	(0.011)	(0.011)	(0.008)	(0.008)	(0.009)	(0.011)
MPCs	−0.638	−0.624				
	(0.267)	(0.268)				
Funding	0.024					
Status	(0.011)					
Funding		0.038	0.048			
Gap		(0.020)	(0.020)			
Violation			−0.020			
Indicator (×10)			(0.014)			
Distance from				0.053		
90% Underfunding				(0.021)		
90% Underfunding				−0.029		
Indicator (×10)				(0.014)		
Distance from					0.058	
80% Underfunding					(0.021)	
80% Underfunding					−0.022	
Indicator (×10)					(0.016)	
Distance from						0.030
the Kink						(0.030)
Kink Indicator						0.020
(×10)						(0.014)
R^2 (within)	0.101	0.101	0.100	0.100	0.100	0.100
R^2	0.684	0.684	0.684	0.684	0.684	0.691

literature, we find a small but statistically significant coefficient on $Q_{i,t-1}$ and a larger, significant coefficient on $CF_{it}/A_{i,t-1}$. For both measures of distance, the coefficient on mandatory contributions is negative and significant. The results using average funding status are almost identical to those in Rauh (2006), although our coefficient on mandatory contributions is somewhat smaller than in that study.

Figure 2 indicates that the relation between investment and funding status (and hence mandatory contributions) is nonlinear, and that the steepest portion of this relation may be confined to a small group of underfunded firms. As a first pass in examining this possibility, we include threshold dummies in the regression instead of mandatory contributions. We consider four thresholds:

zero underfunding, 90% funding, 80% funding, and the kink point at which DRC = MFC. In each of these regressions, we follow the RDD literature and estimate variants of (8) that include the distance to the relevant threshold point. Interestingly, only the coefficient on the 90% funding threshold is significantly negative. This finding is in accord with the evidence in Figure 2 that the large drop in investment happens for firms that are less than 90% funded.

Table II fleshes out this point in a different way by looking at subsamples defined by funding status. For brevity, we leave analysis of the kink point to the Internet Appendix because we find no economically or statistically significant results. This pattern makes sense in that defining an event as a change in slope is unlikely to provide much in the way of significant results inasmuch as the event itself is small.

The first column of Table II repeats the full-sample regression of investment on the market-to-book ratio, cash flow, the funding gap, and mandatory contributions, which is from column 2 in Table I. The next several columns examine the robustness of the coefficient on mandatory contributions to running this regression on different subsamples defined by funding status. This type of exercise is useful for understanding exactly where identification comes from when threshold events are involved. The second column shows that, when one drops the 12% of the sample that is less than 90% funded, the coefficient on mandatory contributions drops slightly and the significance disappears. In contrast, column 3 shows that, when one drops the 6% of the sample that is less than 80% funded, the coefficient on mandatory contributions increases slightly and becomes more significant.

This result suggests that it is the observations that are 80% to 90% funded that are driving most of the results. This conjecture is supported by the results in the fourth column, which correspond to a sample that omits the observations in the 80% to 90% funding interval. Here, the coefficient on mandatory contributions is insignificantly different from zero. The next four columns examine samples containing (i) all underfunded firms, (ii) firms less than 90% funded, (iii) firms less than 80% funded, and (iv) firms between 80% and 90% funded. In none of these regressions is the coefficient on mandatory contributions significantly different from zero. Especially interesting is the result for the firms that are between 80% and 90% funded. The contrast between columns 2 and 3 suggests that these firms are driving the result of a significant response of investment to mandatory contributions, yet an examination of these firms alone reveals no effect at all. One possible explanation is that it is the contrast between the treated (heavily underfunded) and not-treated (overfunded) firms that is driving the significant coefficient on mandatory contributions. We consider this possibility below when we examine sample summary statistics.

First, we use a complementary method to pinpoint the source of the identifying data variation. We run regression (9) using a kernel estimator in which the smoothing is done over the gap between pension assets and pension liabilities, scaled by pension liabilities. The results are depicted in Figure 3. The solid line is the fitted regression and the dotted lines are 5% confidence

Table II

Subsample Regressions

Estimates are from a sample of unregulated and nonfinancial firms from the 2007 Compustat annual industrial files. The sample period is 1990 to 1998. Pension data are from IRS Form 5500. MPCs are mandatory pension contributions to underfunded plans, scaled by total assets. Funding Status is the average across plans of pension assets minus pension liabilities, as a fraction of total assets. Funding Gap is the smallest funding gap if all plans have positive gaps, and the sum of the negative gaps if any plans have negative gaps. Nonpension Cash Flow is income plus depreciation plus pension expense, all scaled by total assets. All regressions contain fixed firm and year effects. Standard errors are in parentheses and are corrected for heteroskedasticity and clustering at the firm level.

	Full Sample	At Least 90% Funded	At Least 80% Funded	>90% or <80% Funded	Underfunded	Less than 90% Funded	Less than 80% Funded	Between 80% and 90% Funded
Market-to-Book	0.019	0.019	0.019	0.019	0.019	0.004	−0.011	0.009
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.004)	(0.007)	(0.005)
Nonpension	0.113	0.113	0.120	0.117	0.099	0.079	0.095	0.090
Cash Flow	(0.011)	(0.013)	(0.011)	(0.012)	(0.013)	(0.019)	(0.041)	(0.026)
MPCs	−0.624	−0.586	−0.746	−0.498	−0.566	−0.152	−0.242	0.071
	(0.268)	(0.434)	(0.317)	(0.317)	(0.511)	(0.451)	(1.025)	(0.551)
Funding	0.038	0.034	0.037	0.036	0.051	−0.087	−0.290	−0.170
Gap	(0.020)	(0.021)	(0.020)	(0.021)	(0.080)	(0.110)	(0.272)	(0.253)
R^2 (within)	0.101	0.106	0.105	0.100	0.100	0.061	0.069	0.145
R^2	0.684	0.693	0.690	0.686	0.712	0.759	0.717	0.862
Sample size	7889	6932	7461	7360	2769	975	428	529

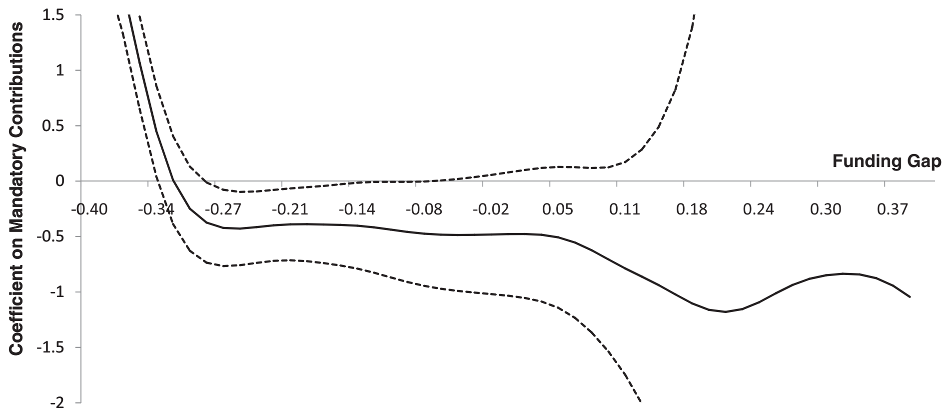


Figure 3. Kernel regression of investment on mandatory contributions. Calculations are based on a sample of firms from Compustat from 1990 to 1998. The regression estimated is the ratio of investment to assets on the market-to-book ratio, the ratio of cash flow to assets, the ratio of mandatory contributions to assets, and the distance to the point of zero underfunding. The smoothing is done over the gap between pension assets and liabilities, with this difference scaled by pension liabilities. A Gaussian kernel with a cross-validated bandwidth of 0.065 is used. The dotted lines indicate 5% confidence bands calculated with nonclustered standard errors. The results indicate a small, significant effect on investment for firms between 10% and 25% underfunded.

bands using nonclustered standard errors. Here, we see that most of the significant negative reaction of investment to mandatory contributions occurs for firms that are between 8% and 27% underfunded.[7] Because mandatory contributions are largest in this range, this piece of evidence underscores one of the main econometric messages of the paper: threshold events are likely to reveal causal effects when the events themselves are large.

This evidence is somewhat weak, however, in that the statistical significance of the results disappears with the use of clustered standard errors. Nonetheless, it does confirm the basic finding that the impact of mandatory contributions on investment is confined to about 5% of the observations in the sample.

C. Summary Statistics

Whether the association between mandatory contributions and investment is causal depends on whether the observations that are associated with the decline in investment are comparable to those in the rest of the sample. In general, it is important to examine whether treated firms (with large mandatory contributions) are different from the control group of untreated firms (with small or zero mandatory contributions), as it is possible that those characteristics that drive firms to be underfunded also drive investment. Table III

[7] The strong positive response for extremely underfunded firms is the product of a few outliers with very high investment.

Table III

Summary Statistics

The figures presented are means from a sample of unregulated and nonfinancial firms from the 2007 Compustat annual industrial files. The sample period is 1990 to 1998. Pension data are from IRS Form 5500. Mandatory Contributions are federally mandated contributions to a pension plan with assets less than liabilities. Total Contributions are mandatory plus voluntary contributions. Plan Violation Indicator equals one if the firm is making mandatory pension contributions, and zero otherwise. "In Violation" indicates that the Plan Violation Indicator is one. Average Funding Status is the average across plans of pension assets minus pension liabilities, as a fraction of total firm assets. Average Funding Status Indicator is one if average funding status is positive, and zero otherwise. Funding Gap is the smallest funding surplus if all plans are running surpluses, and the sum of the deficits if any plans are running deficits. Bond Rating is a dummy variable that takes a value of one if a firm has an S&P long-term bond rating, and zero otherwise. All variables except employment are deflated by total book assets.

	Full Sample	In Violation	Not in Violation	<90% Funded	<80% Funded
Total assets	3,418	3,435	3,409	2,362	2,152
Average Funding Status Indicator	0.218	0.622	0.000	0.951	0.970
Plan Violation Indicator	0.351	1.000	0.000	1.000	1.000
Average Funding Status	0.036	0.005	0.052	−0.020	−0.025
Funding Gap	0.015	−0.012	0.030	−0.025	−0.031
Total Contributions	0.003	0.005	0.002	0.008	0.008
Pension Assets	0.143	0.143	0.143	0.115	0.090
Pension Liabilities	0.106	0.138	0.089	0.137	0.125
Mandatory Contributions	0.001	0.003	0.000	0.005	0.006
Investment	0.069	0.066	0.071	0.057	0.055
Cash Flow	0.096	0.089	0.100	0.068	0.057
Nonpension Cash Flow	0.100	0.095	0.102	0.077	0.067
Market-to-Book	1.481	1.460	1.493	1.363	1.346
R&D	0.017	0.018	0.016	0.016	0.016
Advertising	0.012	0.013	0.012	0.010	0.009
Debt-to-Assets	0.249	0.263	0.241	0.280	0.284
Bond Rating	0.411	0.400	0.417	0.271	0.193
Short Term Debt Issuance	0.005	0.004	0.006	0.005	−0.001
Long Term Debt Issuance	0.021	0.022	0.021	0.021	0.027
Saving	0.004	0.004	0.004	0.003	0.003
Cash	0.070	0.065	0.073	0.072	0.076
Dividends	0.019	0.015	0.021	0.010	0.011
Common Dividends per Share	0.615	0.482	0.688	0.296	0.292
Equity Issuance	0.012	0.013	0.012	0.015	0.016
Equity Repurchases	0.012	0.012	0.012	0.007	0.007
Employment % Change	0.808	0.520	0.965	−1.691	−2.853
Earnings	0.042	0.034	0.046	0.014	0.006
Z-Score	2.780	2.280	3.052	1.667	1.599

presents several summary statistics for our sample of firms, with each column containing figures for a particular subsample. All of the variables are scaled by firm assets, except employment, which is measured in logs.

Before examining different subsamples near the different discontinuity points, to provide context we first examine the full sample. Average firm size is quite large. For example, the mean value of assets for all firms in Compustat between 1990 and 1998 is 1,670 million dollars, whereas the mean for this sample is 3,447 million dollars. This result is not surprising in that older, more mature firms tend to be those with DB pension plans. The large average size of the firms in this sample is also evident in the high incidence of firms that have bond ratings relative to the rest of the Compustat universe. A final important pattern to notice is the small size of mandatory contributions relative to investment: the latter is on average over 40 times larger than the former.

The next two columns of the table investigate the differences between the firms with positive and negative pension gaps. These two groups of firms are surprisingly similar, except along a few dimensions. The firms that are not in violation have slightly higher levels of Tobin's q, invest slightly more, and have slightly higher cash flow. One figure that immediately stands out in the third line of the table is that only 62% of the firms classified as having all plans in the black also have positive average funding status. To the extent that different plans are not fungible, this finding means that *average* funding status is not a good indicator of the point of zero underfunding status.

The fourth column examines firms that are less than 90% funded, where the level of funding is measured by the pension gap. (As demonstrated in the Internet Appendix, nearly identical results are produced by average funding status.) This group is systematically different from the rest of the sample. Not only do these firms invest less, but they have lower values of Tobin's q, and they have cash flow that is about three-fourths that of the sample average. Further, they are two-thirds as large as the average firm, they pay half the dividends that the average firm pays, and they are much less likely to have a bond rating. More strikingly, they have less than one-third the earnings of their overfunded counterparts, their Altman's Z-scores are just over half the average Z-score in the rest of the sample, and they are shrinking their workforces at a rate of over 1.7% per year. Although these firms do not appear to be in extreme financial distress, they are markedly worse off along a number of dimensions than the firms in the rest of the sample. The fifth column shows summary statistics for the firms that are less than 80% funded. This group closely resembles those that are less than 90% funded but are in more serious distress.

Why are these differences in observable characteristics important? They hint at potentially large differences in unobservables, and they lend credence to the hypothesis that over the whole sample these differences have as much a role to play in the determination of investment as do mandatory contributions. The two characteristics that are particularly interesting along this line are size and the incidence of bond ratings. Both characteristics have long been used as indicators of access to external capital markets. Therefore, it is hard to rule out the possibility that financial constraints cause both the underfunding of the pension plans and the investment declines. So even though financial constraints may be affecting the investment of these firms, it is hard to argue that mandatory contributions themselves *cause* investment declines. It is also hard to argue that financial constraints manifest themselves as a sensitivity of investment to cash flow shortfall.

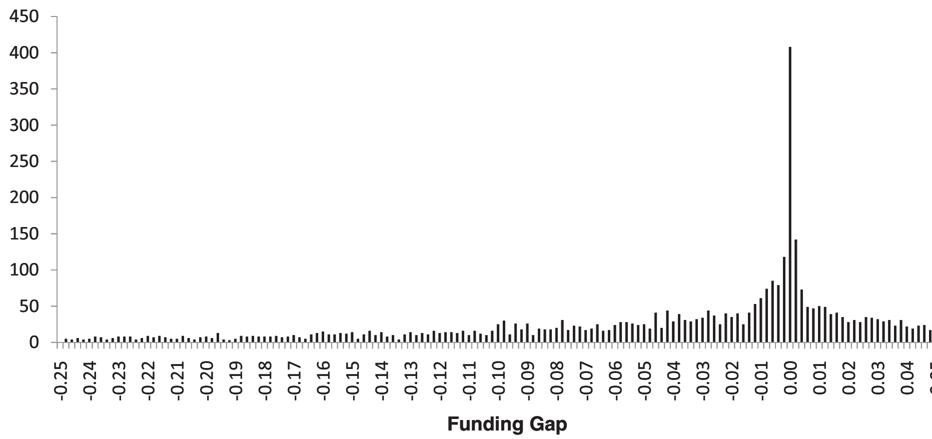


Figure 4. Funding gap density. Calculations are based on a sample of firms from Compustat from 1990 to 1998. The pension gap is defined as the minimum plan surplus if all of the plans in a firm have a surplus and as the sum of the deficits if at least one of a firm's plans has a deficit. This figure depicts a histogram of funding gaps as a fraction of total pension liabilities.

Of course, it is rarely possible to test exact identifying assumptions that lead to causal inference. What is possible is a variety of robustness checks that can either lend credence to or question an identification strategy. For example, when using threshold events to obtain identification, it is important to understand exactly what parts of the sample are providing identification and to understand any systematic difference between these observations and the rest of the sample. The reason is the classic trade-off between internal and external validity. If the parts of the sample that provide identification are systematically different from the rest of the sample, then it is difficult to argue for causality or extrapolate any results obtained from one part of the sample to the rest of the sample.[8] Although these robustness checks can never be definitive in a strict statistical sense, they can often be informative and useful.

V. Local-Sample Analysis

At this point we have several pieces of evidence that make it hard to infer that the correlation between mandatory contributions and investment is causal. We can go further, however, given that funding violations are threshold events. In particular, we can use RDD to examine what happens at these points.

[8] A useful counter example is in Babenko, Lemmon, and Tserlukevich (2011), who use the exercise of executive options as a threshold event to examine the sensitivity of investment to exogenous sources of cash flow. Because exercise thresholds differ widely across firms, they are less likely to be highly correlated with either financing constraints or investment opportunities.

First, we examine the density of the selection variable (the funding gap) around the points of zero underfunding, 90% funding, and 80% funding.[9] The intent is to determine whether there is a discontinuity in its density. If there is such a discontinuity, firms might be manipulating the funding gap, thus invalidating the RDD design. It is important to note here that a continuous density does not imply that funding status is exogenous. Instead, it only implies that RDD can be used to obtain causal inference in the face of an endogenous funding status.

Figure 4 plots a histogram of the ratio of the funding gap to pension liabilities, with a bin width of 0.002. We observe a great deal of clustering around the zero underfunding point, but no bunching of observations on one side or the other. Two explanations are possible for the clustering. First, firms lose tax benefits from contributing to their plans if the plans are too overfunded. This provision gives them an incentive not to build too much of a cushion against a possible funding shortfall. Second, the Pension Benefit Guarantee Corporation insures the pension plans, which gives firms less of an incentive to overfund. One does nonetheless wonder why firms do not crowd just above the cutoff point if they can indeed manipulate the pension gap. Two explanations are possible. First, they might not care about making a mandatory contribution if they make regular contributions in the first place or if the consequences for being barely underfunded are small. Second, even if the firms do care about being above the gap, random fluctuations in the value of pension assets can undo their attempts to avoid a funding violation. Both cases support the idea that the location of firms close to one side or the other is approximately random. In contrast to the case of the zero underfunding point, we observe neither clustering nor bunching around the 80% and 90% funding points. In sum, all of this evidence is exactly what is needed to use threshold events as identification strategies.

Our next step is to estimate an average treatment effect of these thresholds on a variety of real and financial variables. The definition in (4) provides no guidance for picking a sample that is "close" to the funding violation threshold. We therefore follow Hoxby (2000), who starts with a tiny sample size and then examines the results when the sample size is increased. This strategy allows us to demonstrate the extent to which our results are sensitive to the window width that defines a small sample. We estimate the treatment effect by regressing a variable of interest on a dummy for negative funding status and fixed firm and year effects.[10] We consider 17 variables: mandatory contributions, sales of capital goods, investment, R&D, advertising, leverage, short-term debt issuance, total debt issuance, the change in cash, the level of cash, dividends,

[9] We omit the kink point for brevity, given no evidence of significant causal effects.

[10] As shown in Hahn, Todd, and Van der Klaauw (2001), a simple difference in means between firms on either side of the threshold can have more asymptotic bias than a local linear regression of the variable of interest on a violation indicator and the distance to the point of violation. However, in a local sample this second method can introduce collinearity between the threshold dummy and the distance to the threshold. We have also estimated a treatment effect using a local linear regression, with the main difference being less evidence of an effect on inventories. See the Internet Appendix for details.

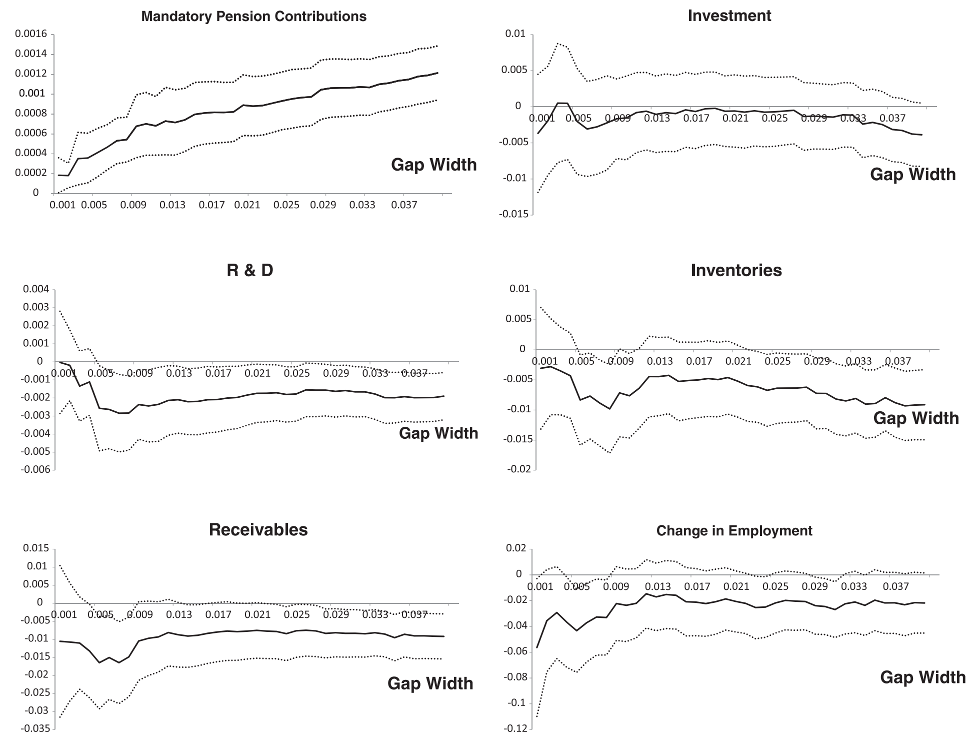


Figure 5. Local responses to funding violations. Calculations are based on a sample of firms from Compustat from 1990 to 1998. Each panel in this figure plots regression coefficients from 40 regressions as a function of the sample used for the estimation. Each panel is labeled by the dependent variable in the regression. The regressors are a pension violation indicator and firm and time fixed effects, and the coefficient of interest is that on the violation indicator. On the horizontal axis is a measure of the sample used for estimation, which is the absolute value of the pension gap as a fraction of plan assets. The pension gap is defined as the minimum plan surplus if all of the plans in a firm have a surplus and as the sum of the deficits if at least one of a firm's plans has a deficit. The sample used for the calculation is all observations for which the absolute value of the pension gaps is less than the specified value on the horizontal axis. All dependent variables except employment are expressed as a fraction of total assets. Employment is measured as the natural log of millions of employees.

receivables, equity issuance, stock repurchases, the change in working capital, the change in employment, and the level of employment. All variables except for employment are measured as a fraction of total book assets. Employment is measured as the log of total employees. We only report results for mandatory contributions, investment, and the significant results for any of the rest of these variables. All results for the other variables are contained in the Internet Appendix.

The results for the point of zero underfunding are presented in Figure 5. The coefficient on the violation dummy is on the vertical axis. This coefficient

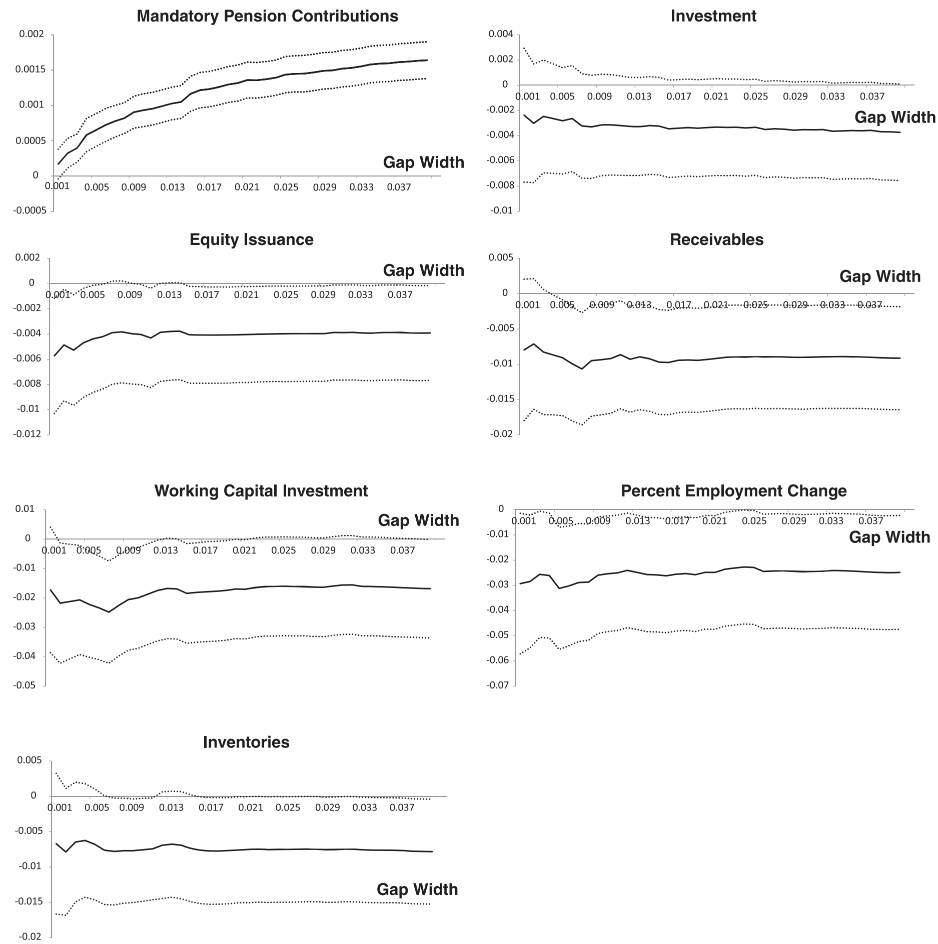


Figure 6. Local responses to 10% underfunding. Calculations are based on a sample of firms from Compustat from 1990 to 1998. Each panel in this figure plots regression coefficients from 40 regressions as a function of the sample used for the estimation. Each panel is labeled by the dependent variable in the regression. The regressors are an indicator for whether any firm pension plan is at least 90% underfunded and firm and time fixed effects, and the coefficient of interest is that on the underfunding indicator. On the horizontal axis is a measure of the sample used for estimation, which is the absolute value of the distance from the point of 90% underfunding, measured as a fraction of pension liabilities. All dependent variables except employment are expressed as a fraction of total assets. Employment is measured as the natural log of millions of employees.

measures the within-firm difference in the dependent variable between observations just to the left and just to the right of the cutoff. The distance from a zero funding gap is on the horizontal axis, with the distance measured as a fraction of pension liabilities. Larger values for the gap correspond to larger sample sizes. For example, a gap of 0.002 corresponds to a sample size of 406, and a gap of 0.04 corresponds to a sample size of 2,180. The solid line represents the coefficient estimate as a function of the distance to the threshold. The dashed lines are 5% confidence bands calculated using clustered standard errors.

The first panel shows the response of mandatory contributions. We calculate this response for two reasons. First, we know, a priori, that there is a causal response of mandatory contributions to funding thresholds. If local regressions cannot pick up this effect, then there is little hope that they have sufficient power to pick up other effects. Fortunately, this is not the case. We find a statistically significant difference between the near violators and near escapees even for the smallest gaps. The second reason for calculating the response of mandatory contributions is to gauge the magnitude of the responses of the other variables. It is worth noting that the response of mandatory contributions is quite small—between 0.02% and 0.1% of total firm assets.

In contrast, the response of investment is statistically insignificant from zero. The next four panels show that we find significant coefficients on the violation dummy variable in the regressions for R&D, inventories, receivables, and the change in employment. What is striking about these results, however, is not that they are significant but rather that the magnitude of the response is much larger than the magnitude of the rise in mandatory contributions. This finding is interesting because it brings up the important point of concurrent unobservable treatments. The only way that tiny mandatory contributions can "cause" large changes in firm policies is via expectations about the future. If managers perceive current violations as signals of many years of funding difficulties, they may cut back in large ways today to avoid these problems. However, absent a dynamic model of the firm, it is only possible to conjecture about such an effect. One other possibility that can explain the relatively large responses of these variables is a *discontinuous* concurrent change in some other variable that affects managerial decisions. To explore this possibility, we calculate similar response functions for cash flow and the market-to-book ratio. As documented in the Internet Appendix, we find an insignificant response of market-to-book but a strong and significant response of cash flow. Of course, the natural thing to do is to rerun the local regressions conditioning on cash flow. Even in this case, however, we find similar magnitudes and levels of significance. See the Internet Appendix for details. In sum, although it is possible that mandatory contributions cause significant changes in both real and financial variables, it is also likely that something else is going on at this point, given the relative magnitudes of firm choice variables and mandatory contributions.

Figure 6 contains analogous results corresponding to the 90% funding point. In this case, it is possible to exploit the 1995 change in the laws to exempt from the DRCs plans that were at least 90% funded. Figure 6 is therefore different from Figure 5 in that the dummy variable used in the local regressions is interacted with a dummy that equals one if the year is greater than 1994. Once again, we find an insignificant response of investment, but significant responses of mandatory contributions, receivables, inventories, and changes

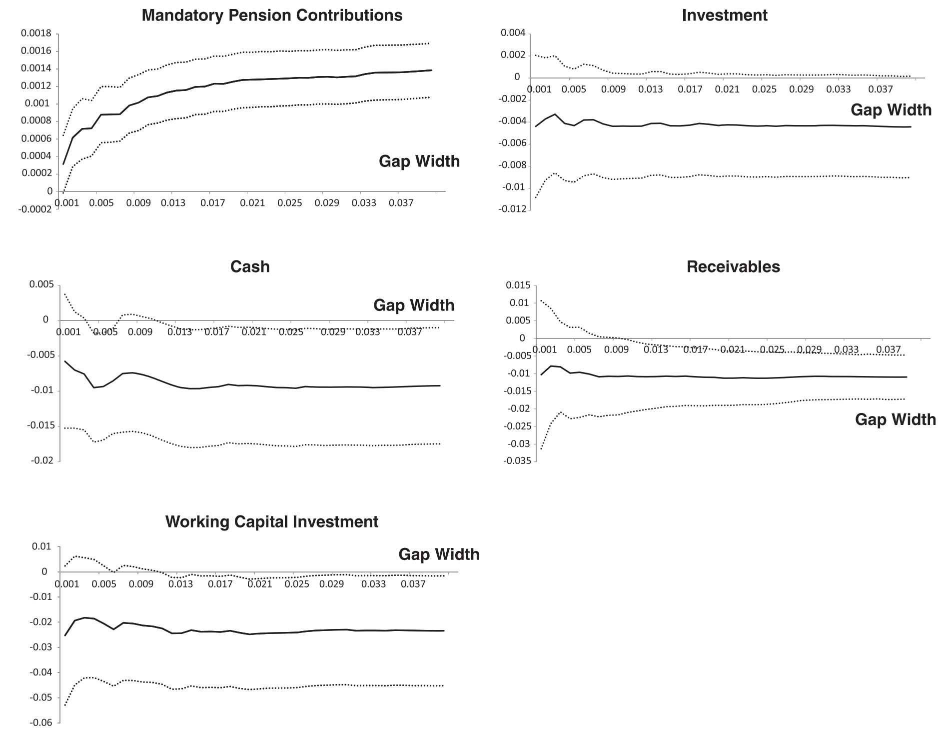


Figure 7. Local responses to 20% underfunding. Calculations are based on a sample of firms from Compustat from 1990 to 1998. Each panel in this figure plots regression coefficients from 40 regressions as a function of the sample used for the estimation. Each panel is labeled by the dependent variable in the regression. The regressors are an indicator for whether any firm pension plan is at least 80% underfunded and firm and time fixed effects, and the coefficient of interest is that on the underfunding indicator. On the horizontal axis is a measure of the sample used for estimation, which is the absolute value of the distance from the point of 80% underfunding, measured as a fraction of pension liabilities. All dependent variables except employment are expressed as a fraction of total assets. Employment is measured as the natural log of millions of employees.

in employment. For the 90% threshold, we also find significant responses of equity issuance and working capital investment. As in the case of the point of zero underfunding, the responses of firm policies are larger in magnitude than the responses of mandatory contributions. Interestingly, we find no responses of any variables except mandatory contributions when we interact the 90% funding dummy with a dummy that equals one if the year is before 1995. This type of placebo test is an important part of any study that uses threshold events to identify causal effects.

Finally, Figure 7 contains analogous results corresponding to the 80% funding point. As in the case of the 90% threshold, we interact the threshold dummy with a post-1995 dummy. In this case, we find no responses of real variables. Only cash, receivables, and working capital investment produce significant effects. Our pre-1995 placebo tests yield no significant responses. We take these results with a grain of salt, however, given that there are very few observations near the 80% funding point.

VI. Conclusion

This paper seeks to understand how firms react to quasi-exogenous threshold events in which something happens when an observable continuous variable passes a threshold. It also seeks to clarify how to use threshold events to obtain identification of causal effects in corporate finance applications. We frame our discussion around the evidence in Rauh (2006) that firms cut investment sharply in response to mandatory contributions to their pension funds. This example is particularly instructive because of the rich institutional detail characterizing pension funding.

From examining our entire sample of firms with DB pension plans, we find that, although we can replicate the result of a strong negative correlation between mandatory contributions and investment, we also find that this correlation stems from firm-year observations in which pension plans are seriously underfunded. Further, these observations constitute a small fraction of the total sample, and they are systematically different from the rest of the sample in terms of several observable characteristics.

We also use local linear regression techniques from the RDD literature to ascertain whether discontinuities in the function relating mandatory contributions to pension funding can be used to isolate causal effects. Here, we find robust evidence of causal effects on both accounts receivable management and hiring. We also find less robust effects on other components of working capital and on R&D. The interesting economic takeaway from all of these results is that the link between real economic activity and finance likely extends beyond the heavily studied link between finance and investment. Other real decisions, especially employment decisions, are likely to be just as important. This evidence is supported by the result in Campello, Graham, and Harvey (2010) that firms planned to cut employment and other variables during the financial crisis.

What econometric takeaways have we gleaned from examining pension funding? This setting is ideal for understanding how to use threshold events to identify causal effects because it is not an exact application of RDD. It is therefore possible to understand the extent to which the intuition from RDD can be extended to broader circumstances. We conclude that using the reasoning behind RDD loosely in a general regression setting can lead to misleading inferences. Although a threshold rule such as a pension violation can provide strong local identification of a causal effect, this identification is likely to disappear as one moves away from the threshold. The lesson we take away from this example is that researchers who use these types of analogies to RDD need to examine their data carefully to determine exactly where their identification comes from. If this identifying data variation is isolated, as is the case here, it is important to determine whether this part of the sample is different from the rest of the sample. Otherwise, the strong internal validity one can obtain from examining threshold events has little if any broader external validity. A greater concern is that the results obtained from any discontinuities may actually be the direct product of any differences between those observations affected by the discontinuity and those not affected. In the current case, this concern is real inasmuch as the differences are strongly related to indicators of financial constraints.

Finally, we illustrate how to use RDD around points of discontinuity. We emphasize understanding firm incentives to locate either near or far from any discontinuities. We also emphasize checking relative magnitudes of responses to causal events and using placebo tests to validate any findings. These checks are essential to help rule out the possibility of concurrent unobserved events around the discontinuity points.

In conclusion, threshold events occur frequently in corporate finance because of the rich accounting and regulatory framework in the field. We therefore suspect that their use in formulating identification strategies is likely to grow, and we hope that this study provides a useful template for studying threshold events.

Appendix

Our data for our pension application come from the 2007 Compustat industrial files. We define book assets as item 6, cash as item 1, inventories as item 3, working capital as item 4 minus item 5, capital expenditures as item 128, dividends as the sum of items 19 and 21, long-term debt as item 9, short-term debt as item 34 plus item 44, the number of common shares as item 25, the share price as item 199, balance sheet deferred taxes as item 74, equity issuance as item 108, employment as item 29, the book value of common equity as item 60, and share repurchases as item 115. As in Rauh (2006), we define nonpension cash flow as the sum of items 14, 18, and 43, and cash flow as nonpension cash flow minus total pension contributions from IRS form 5500. The numerator of the market-to-book ratio is book assets minus the book value of common equity minus deferred taxes plus the product of the share price and the number of shares outstanding. The denominator is total book assets. A bond rating is the S&P long-term domestic issuer credit rating (SPDRC). Altman's Z-score is defined as $1.2 \;\; ((\text{item } 4 - \text{item } 5)\,/\text{item } 6) + 1.4\left(\text{item } 36/\text{item } 6\right) + 3.3\left(\text{item } 13/\text{item } 6\right) + 0.6\,((\text{item } 25 \times \; \text{item } 199)\,/\,(\text{item } 9 + \text{item } 34)) + 0.999\left(\text{item } 12/\,\text{item } 6\right)$.

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