Critical Finance Review, 2011, 1: 3–58

Corporate Debt Maturity and the Real Effects of the 2007 Credit Crisis*

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ABSTRACT

We use the August 2007 crisis episode to gauge the *causal effect* of financial contracting on real firm behavior. We identify heterogeneity in financial contracting at the onset of the crisis by exploiting *ex-ante* variation in long-term debt maturity structure. Using a difference-in-differences matching estimator approach, we find that firms whose long-term debt was largely maturing right after the third quarter of 2007 cut their investment-to-capital ratio by 2.5 percentage points more (on a quarterly basis) than otherwise similar firms whose debt was scheduled to mature after 2008. This drop in investment is statistically and economically significant, representing a drop of one-third of pre-crisis investment levels. A number of falsification and placebo tests suggest that our inferences are not confounded with other factors. For example, in the absence of a credit contraction, the maturity composition of long-term debt has no effect on investment. Moreover, long-term debt maturity composition had no impact on investment during the crisis for firms for which long-term debt was not a major source of funding. Our analysis highlights the importance of debt maturity for corporate financial policy. More than reporting evidence of a general association between credit markets and real activity, our analysis shows how the credit channel operates through a specific feature of financial contracting.

ISSN ; DOI 10.1561/103.00000001
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1 Introduction

How do firms respond to disruptions in credit markets? Are those responses influenced by the characteristics of their financing? These are important but difficult questions to answer. In this paper, we design a strategy to pin down the effect of financial structure on real corporate outcomes following a credit shock. We do so using the crisis (or "panic") of August 2007. Gorton (2008) provides an in-depth analysis of the various forces leading to the sharp reduction in liquidity that affected financial institutions dealing with subprime-based derivatives starting in late 2007. The lack of transparency in long-term investments of financial institutions and the possibility that losses on credit derivatives would be passed onto their balance sheets led to a panic that shut down financing to banks and non-banking institutions. As we document below, the crisis spilled over into the market for long-term corporate debt in the fall of 2007, making it difficult for firms to roll over their long-term obligations.

The 2007 episode arguably provides for a shock to the supply of external financing that is not caused by the weakening of corporate business fundamentals. The advantage of using a well-identified *supply* shock, when it emerges, is that it ameliorates concerns that declines in the use of credit by firms might be driven by a drop in the *demand* for credit — credit shocks are usually followed by economic slowdowns. However, exploring a credit shock *per se* does not guarantee that one can establish a clear link between financial structure and real-side outcomes. In particular, while general credit conditions may exacerbate the correlation between variables such as financial leverage and corporate investment, one cannot pin down a *causal effect*. To establish that effect, one needs to identity a feature of financial contracting whose variation can be considered to be predetermined at the time of the credit shock. This feature of contracting must be relevant for overall firm financing, commonly observed, and relatively rigid (hard to recontract around on short notice).

We identify heterogeneity in financial contracting at the onset of the 2007 panic by exploiting *ex-ante* variation in firms' long-term debt maturity structures. We examine whether firms with large fractions of their long-term debt maturing at the time of the crisis had to adjust their behavior (e.g., cut capital expenditures) in ways that were more pronounced than otherwise

similar firms that did not have to refinance their long-term debt at that time. To the extent that these refinancing effects are large, they imply that the terms of financial contracting — contract maturity — affect real-side corporate outcomes.

Let us discuss how our focus on long-term debt maturity works as an identification tool. Prior corporate finance literature has shown that the choice between short- *versus* long-term debt is correlated with firm characteristics such as size, profitability, and credit ratings (see Barclay and Smith, 1995; Guedes and Opler, 1996). As such, in general, the use of debt maturity creates difficulties for the identification of unconfounded causal effects of financial contracting on real outcomes. At the same time, long-term debt is typically publicly held and difficult to renegotiate on short notice (Bolton and Scharfstein, 1996). Because cumulative, hard-to-reverse decisions made several years in the past affect current long-term debt maturity structures, it is hard to argue that firms are at their "optimal debt maturities" at all times.[1] Accordingly, whether a firm had to refinance a significant portion of its long-term debt right after August 2007 is plausibly exogenous to the firm's performance in the aftermath of the shock. We exploit this wrinkle — a maturity-structure discontinuity — in our analysis. Simply put, we use the *proportion of long-term debt* that was long prescheduled to mature *right after fall of 2007* to gauge how firms' real decisions were affected, in a *causal* sense, by financing constraints.

While our analysis treats variation in the fraction of long-term debt that comes due right after August 2007 as exogenous to firm outcomes, one might wonder if other sources of firm heterogeneity could underlie the relations we observe. To alleviate this concern, we use a difference-in-differences matching estimation approach that incorporates observable firm characteristics and accounts for unobservable, idiosyncratic firm effects. We design our tests so that firm refinancing status can be seen as a "treatment." The tests match firms that we expect were more susceptible to the negative effects of refinancing constraints (firms that had a large fraction

[1] A large literature discusses how firms may deviate for years from "optimal" debt-to-asset ratios (see, e.g., Baker and Wurgler (2002) and Welch (2004)). Arguably, the ability to secure an optimal debt-maturity composition would probably be a lower-order concern if firms are unable to secure the overall debt positions they might desire.

of their long-term debt coming due when the crisis hit) with "control" firms that did not have to renegotiate their debt. Specifically, we pair up firms in these two groups on the basis of their size, industry classification, credit ratings, *Q*, long-term leverage ratio, cash flows, and cash holdings. This approach allows us to compare otherwise similar firms, with the only difference being the profile of their long-term debt maturity. The tests account for time-invariant heterogeneity by comparing within-firm changes in the outcome variables of interest from the period that precedes the 2007 credit shock to the period that follows that shock.[2]

We consider a number of alternatives to our baseline experiment. These alternative experiments provide checks for the logic of our approach and further minimize concerns about hard-wiring in our results. For example, we perform a battery of falsification tests that replicate our matching estimation procedure in non-crisis periods. In principle, a firm whose debt matures at a time in which credit is easily available should not display a constrained-type behavior that can be linked to maturing debt. It is only the juxtaposition of a "local discontinuity" in the firm's debt maturity and a credit shortage that should affect investment. In addition, we redefine our treatment and control groups based on the degree to which long-term debt is an important component of overall firm financing. According to the logic of our strategy, for those firms whose long-term debt is only a small component of total financing, we should not see a link between investment spending and the fact that a large fraction of long-term debt might be maturing during the crisis.

Our base findings are as follows. First, we document pronounced cross-firm variation in long-term debt maturity structure at the onset of the 2007 crisis. Cross-sectional variation in long-term debt maturity is persistent over time, with similar dispersion patterns observed in the years preceding the crisis. Importantly for our strategy, we are able to isolate a sizable pool of firms with a large fraction of long-term debt maturing right after the crisis (treated firms) that are virtually identical to other firms whose debt happens to mature in later years (control firms). We show that these two groups of firms are similar across all characteristics we consider, except for a local discontinuity in their long-term debt maturity structure.

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[2] We perform these tests using the Abadie and Imbens (2006) matching estimator (discussed in detail below). We also perform similar tests using standard regression analysis later in the paper.

We then show that a firm's debt maturity structure has important consequences for post-crisis real outcomes.[3] For firms in the treatment group, quarterly average investment rates dropped from 7.8% of capital to 5.7% of capital — a fall of 2.1 percentage points relative to their pre-crisis level. Firms in the control group hardly changed their spending. The Abadie-Imbens estimate of the difference-in-differences in investment points to a drop of 2.5 percentage points. This drop in investment is economically substantive, representing a decline of approximately one-third of pre-crisis investment levels.[4] Confirming the logic of our strategy, the relation between maturing debt and investment disappears when we use firms with insignificant amounts of long-term debt in the experiment. On the flip side, that relation strengthens when we focus on firms for which long-term debt is a more important source of financing. In this case, the relative change in investment is a drop of 3.4 percentage points, implying that investment is cut almost in half. We also find that the effect of maturity structure on investment is robust to many variations in the definitions of treatment and control groups. Moreover, it *only holds* for the 2007 period. In particular, we replicate our experiment over a number of years and find that maturity structure is unrelated to changes in investment for these non-crisis (placebo) periods. In other words, while discontinuities in debt maturity structure are generally unrelated to investment spending, they bind firm behavior when credit is tight.

Standard falsification tests allow us to tackle unobserved heterogeneity that may help predict both a firm's debt maturity profile and its subsequent investment. However, they cannot rule out stories that could be specific to the current crisis. One such story is that "smarter CEOs" may have anticipated the August 2007 shock and refinanced (prior to the crisis) the part of their firms' long-term debt that was scheduled to mature in 2008. Another story is that "better firms" may have elongated their debt during the boom years preceding the crisis. While likely implausible in the context that we examine, we need to fend off these selection stories. We do so by performing

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[3] Anticipating the details of the experiment, the pre-crisis period is defined as the first three quarters of 2007 and the post-crisis period is defined as the first three quarters of 2008. In the baseline tests, the treatment group contains firms for which the fraction of long-term debt maturing within one year (i.e., in 2008) is greater than 20%; the control group contains firms for which that fraction is lower than 20%. Firms are matched on covariates measured in the pre-crisis period.

[4] Since the stock of fixed capital represents about 35% of the assets of the firms in our sample, the average decline in investment is close to 1% of total asset size.

experiments in which we measure maturity structure *several years prior* to the credit crisis (for example, we use 2005 data to predict which firms would have large portions of debt maturing in 2008). We find that these predetermined maturity profiles also predict changes in investment around the credit crisis.

A common concern with inferences from studies using the difference-in-differences estimator in a treatment-effects framework is whether treatment and control group outcomes followed "parallel trends" prior to the treatment — only in this case one can ascribe differences in the post-treatment period to the treatment itself. Another concern is whether alternative "macro effects" that differentially affect treatment and control groups might explain the behaviors that we observe in the post-treatment period. Our matching estimator ensures that we are comparing firms from the same industry with very similar characteristics such as credit quality, size, and profitability, suggesting that these firms would behave similarly in the absence of refinancing frictions. Still, one cannot rule out the possibility that there are latent group differences that trigger contrasting behaviors in the post-treatment period because of events — other than our proposed treatment — taking place in that period.

We tackle both of these concerns in our analysis. First, we compare pre-treatment trends in the outcomes (changes in investment) of our treatment and control groups. Going back several years prior to 2007, we find no evidence that the investment path of firms in those two groups followed different trends. Second, we examine the concern that the recession that followed the 2007 shock may have driven a differential wedge in the post-crisis investment of treatment and control firms, irrespective of the 2007 credit shortage. To deal with this issue, we look for a period that precedes a recession, but that *lacks* a sharp credit supply shock to identify a placebo treatment. In other words, we try to eliminate the "credit-supply component" of our treatment strategy, but allow for the same post-treatment macro effect (demand contraction) that could potentially drive our results. Although it is difficult to find a recession that is not preceded by a credit tightening, one can argue that the 2001 recession was not preceded by a credit shortage that is comparable to that of fall of 2007. This test shows no evidence of a differential recession-driven behavior for our treatment and control firms.

We also look at the value implications of maturing debt during the financial crisis. We do so by comparing stock returns and changes in Q of treated and control firms during the first three quarters of 2008 (the period in which

the treatment firms cut investment due to the financing shock). We find that firms facing large debt payment obligations in 2008 not only invested less but also lost relatively more value in the stock market. We further complement our analysis by examining whether firms adjusted along other margins to accommodate their refinancing gap. In particular, firms may have adjusted other real and financial policies, such as drawing down cash balances, reducing inventory stocks, repurchasing fewer shares, and cutting dividends. Our calculations suggest that the firms that were burdened with large amounts of maturing debt in 2008 tapped their "least costly" sources of funds the most. Consistent with Almeida *et al.* (2004), we find that the brunt of the shock to external funding was primarily absorbed by firms' cash balances. Reductions in inventory were also pronounced across treated firms (see Fazzari and Petersen, 1993). Perhaps surprisingly, however, those firms did not cut their cash dividends by much (see Brav *et al.*, 2005). We end our study with a post-crisis analysis of corporate welfare, looking at firm performance through June 2010.

As discussed above, the existing literature points to a link between debt maturity and underlying firm quality. It is thus important for our strategy that we compare firms that rely primarily on long-term debt financing (with the only difference being when that debt happens to come due). The downside of this approach is that, by focusing on a subset of firms, concerns may be raised about how our results would generalize to the full universe of firms. This concern reflects the common tradeoff between internal and external validity in experiment-type tests. We note, however, that our sample contains over one thousand firms that account for 46% of total market capitalization and 61% of corporate investment for the year 2007 in the United States. Firms in our sample represent an important part of the corporate sector in their own right. In some of our experiments, hundreds of these firms are selected into a treatment status, while in others only a few dozen. But these numbers are not crucial *per se*. The goal of the experimental design is to achieve a plausible near-randomness in the assignment of representative firms to a financial constraint status.

Similarly to our paper, Duchin *et al.* (2010) focus on the impact of the credit crisis on corporate investment. Their attempt at identifying firms that are affected by the crisis hinges on firms' cash and debt positions. While appealing, as discussed above, their proposed strategy is subject to the criticism that firms' cash and debt policies prior to the crisis may confound factors that explain those firms' post-crisis behavior. This makes it

difficult to ascribe causality going from financial policy to real firm outcomes. Related papers that do not look at the current crisis are Chava and Purnanandam (2011) and Lemmon and Roberts (2010). Chava and Purnanandam examine the effects of the 1998 Brazil-Russia-LTCM crisis on corporate valuation. The authors find a larger valuation impact upon bank-dependent firms whose main banks had greater exposure to Russia. Lemmon and Roberts examine the effects of a contraction in the supply of risky credit (junk bonds) caused by changes in regulation and the collapse of Drexel Burnham Lambert. Their evidence suggests that risky firms' investment declined following changes in the junk-bond market. Our study differs from these papers in that our strategy dispenses with the need to focus on bank-dependent or risky firms to assess the impact of credit supply shocks. In addition, we uniquely identify a feature of financial contracting that transmits the impact of credit shocks onto firm investment.

Our study contains relevant implications for corporate financial policy. Our results imply, for example, that firms with similar debt-to-asset ratios may respond very differently to a credit supply shock. Indeed, firms with relatively low debt ratios can be more affected by such shocks, depending on the maturity composition of their debt. This suggests additional caution when classifying firms based on their observed leverage ratios as a way to gauge their response to macroeconomic events. Our study is new in highlighting the extra attention that corporate managers should pay to the maturity profile of their firms' debt. Debt maturity structure is a key aspect of financial flexibility. An aspect that, according to our evidence, becomes particularly important during credit contractions. Finally, our work adds to the understanding of contracting by using a well-identified element of financial contracts (contract maturity) to show *how* contracting affects firm behavior.

The remainder of our paper is organized as follows. We discuss our empirical strategy in Section 2. Our baseline result that financial structure affects real corporate outcomes is presented in Section 3. In Section 4, we conduct a number of additional tests designed to check the robustness of our results. Section 5 concludes the paper.

2 Empirical Design

We start this section by describing our basic experimental design as well as the matching estimator methodology that we employ. We then describe the data used in our tests.

2.1 The "Experiment"

Our basic insight is that of exploiting variation in long-term debt maturity at the onset of the 2007 crisis as a way to identify the effect of credit supply shocks on corporate policies. Of course, the relevant question is how the composition of long-term debt maturity would affect real corporate policies. In a frictionless capital markets, debt maturity is irrelevant because firms can always refinance and recontract their way around the potential effects of a balloon debt payment. What is special about credit crises is that financial markets are arguably less than frictionless during those times. The 2007 crisis, in particular, affected traditional modes of corporate funding, such as commercial paper, bond placements, bank loans, and equity issuance. In such an environment, soon-to-mature debt can effectively reduce corporate investment, as firms find it difficult to substitute across alternative funding sources while at the same time trying to avoid defaulting on their debt payments. As a result, firms that were "unfortunate" to have large chunks of debt maturing right around the 2007 crisis might be expected to face tighter financing constraints than firms that did not have to finance balloon debt payments during that same period.

2.1.1 The 2007 credit supply shock

As discussed by Gorton (2008) and Acharya *et al.* (2009), the current crisis started with a reversal in housing prices in 2006, which in turn triggered a wave of default of subprime mortgages going into 2007. The increase in subprime defaults during the first half of 2007 initially affected financial institutions that had invested heavily in asset-backed securities (ABS). These hedge funds and other special investment vehicles (e.g., bank SIVs) relied on short-term rollover debt to finance holdings of long-term assets. By early August 2007, it was clear that investors were no longer willing to roll over short-term financing to highly-levered institutions, as exemplified by the run on BNP Paribas' SIVs. Similar runs were observed across many countries and markets in subsequent weeks. They were largely attributed to the perceived lack of transparency of the investment portfolios of financial institutions, and the possibility that large losses would be passed onto the balance sheets of banks that sponsored investment vehicles such as SIVs.

As a result of these developments, the spreads on short-term financing instruments reached historically high levels. This is illustrated by the time series of the 3-month LIBOR and commercial paper spreads over

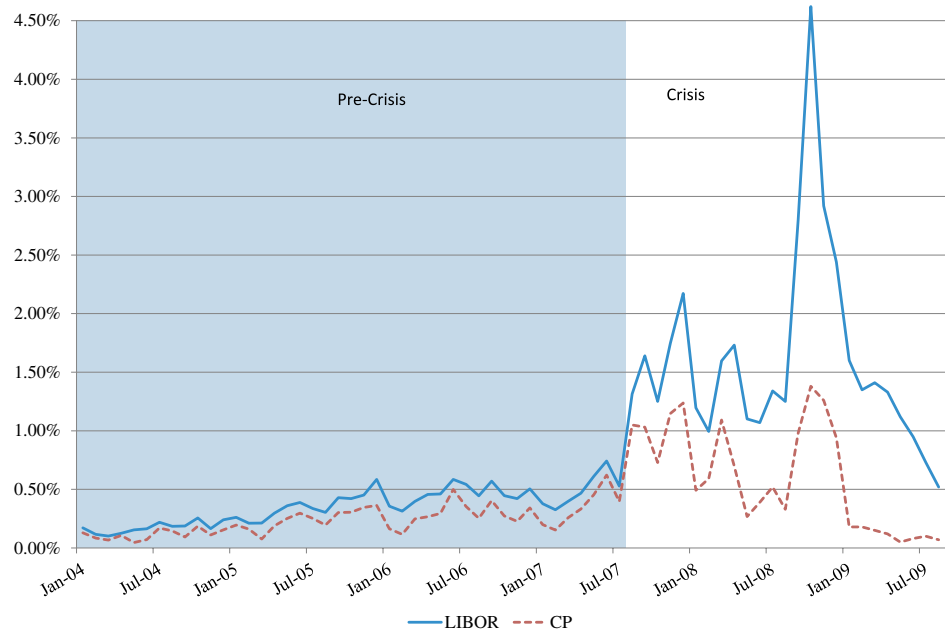

Figure 1. LIBOR and commercial paper (CP) spreads during the 2007–2009 credit crisis.

This figure displays the 3-month LIBOR and commercial paper (CP) spreads over comparable-maturity treasuries for the period of January 2004 to August 2009. The data is from http://www.federalreserve. gov/datadownload/.

Interpretation: This figure shows that LIBOR and commercial paper spreads started increasing significantly in the period around August 2007, and stayed higher than historical values for a long period after that.

comparable-maturity treasuries. These series are plotted in Figure 1. There is a sharp, large shock to both of these spreads around August 2007. Spreads go up from levels lower than 0.5% between 2001 and the summer of 2007, to levels between 1% and 2% following August 2007. In particular, in July 2007 the average 3-month LIBOR spread was 0.5%. The LIBOR spread jumped to 1.3% in the month of August, staying above 1% in the subsequent months.

The repricing of credit instruments that followed the 2007 panic quickly went beyond short-term bank financing, spilling over onto longer-term instruments. The episode highlighted the interdependence of segments of the financial markets that were once thought of as being fairly isolated from each other. The lack of availability of short-term financing is believed

to have softened the demand for long-term bonds by institutions such as hedge funds and insurance companies. The collapse of the "repo" market further affected the demand for highly-rated corporate bonds, which were used as collateral for borrowing agreements during "normal times." Current research on the crisis (and anecdotal evidence) suggests that these developments led spreads on long-term corporate bonds to increase sharply. In Figure 2, we report the time series of spreads for indices of

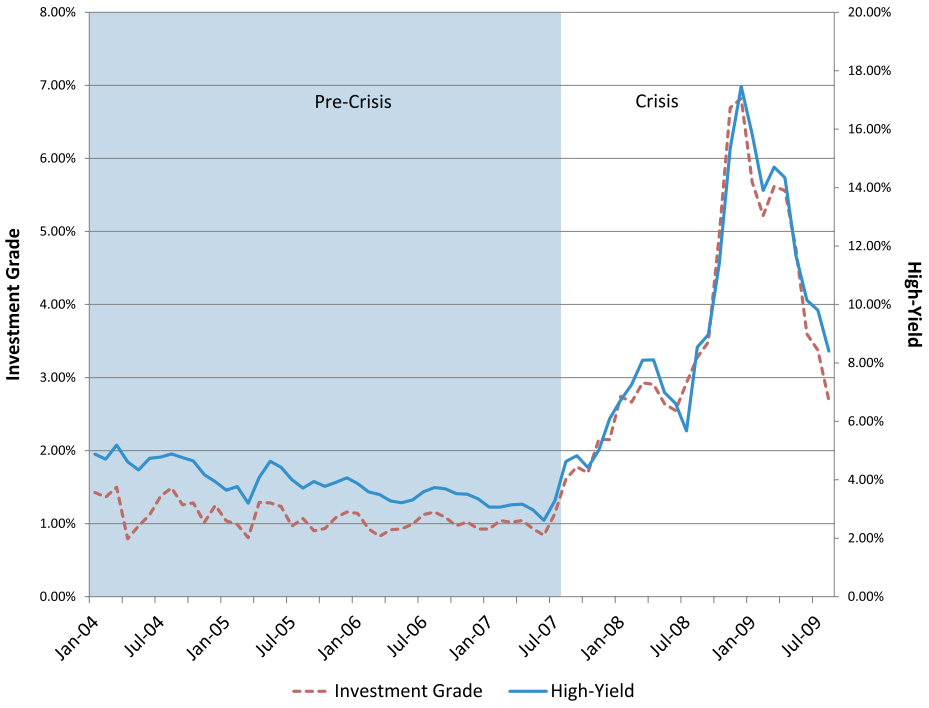

Figure 2. Corporate bond spreads during the 2007 credit crisis.

This figure displays the time series of spreads for indices of investment-grade (measured on the left axis) and high-yield bonds (measured on the right axis) from January 2004 to August 2009. The data are from Citigroup's Yieldbook. The investment-grade index is Citigroup's BIG_CORP index, which included only corporate bonds and has an average credit quality of A. The high-yield bond index is Citigroup's HY_MARKET index, which has an average credit quality equal to B+. The spreads are calculated with respect to the 5-year treasury rate (data from http://www.federalreserve.gov/datadownload/).

Interpretation: This figure shows that investment-grade and high-yield bond spreads started increasing significantly in the period around August 2007, and stayed higher than historical values for a long period after that.

investment-grade and high-yield bonds (from Citigroup's *Yieldbook*).[5] Citigroup reports average duration and maturity for the bond portfolios used in the construction of these indices. Given the reported durations, which hover between 4 and 7 years, we chose the 5-year treasury rate as a benchmark to calculate spreads. We note that the average credit quality of Citigroup's investment-grade and high-yield indices is, respectively, A and B+. Figure 2 gives a fairly complete picture of the effect of the crisis on the spreads of bonds of different credit quality.

The spreads on long-term corporate bonds show a dramatic increase starting in August 2007, both for investment-grade and junk-rated firms.[6] The figure shows that August 2007 represents a turning point for corporate bond spreads. Investment-grade spreads had been close to 1% since 2004. These spreads increased sharply to 1.6% in August of 2007, and toward levels that approached 3% during early 2008. Junk bond spreads display a similar pattern, increasing from levels of around 3% in early 2007 to 4.6% in August, and then to between 7% and 8% in early 2008.[7]

Similar signs of a credit squeeze in the U.S. bond markets can be gathered from quantity data. According to SDC's *New Issues Database*, the total debt issuance with maturity greater than one year for the third quarter of 2007 amounted to $63 billion. There was a total of 165 deals registered in that quarter. To put these numbers in perspective, the average quarterly amount of funds raised in the bond market in the two years preceding the crisis was $337 billion, while the average number of deals was 1,476. At the same time that firms found it difficult to raise funds in the bond markets, banks were also cutting the loan supply. New commercial and industrial loans extended by U.S. commercial banks dropped from $54 billion in February 2007 to about $44 billion in February 2008 (cf. Federal Reserve's *Survey of Terms of Business Lending*). Loans under commitment (lines of credit) dropped from $41 billion to $37 billion during the same period.

The existing evidence supports our conjecture that there was a substantial increase in the cost of short- and long-term financing as well as a fall

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[5] We use Citigroup's BIG_CORP (investment-grade) and HY_MARKET (high-yield) indices. Almeida and Philippon (2007) also use *Yieldbook* data to calculate corporate bond spreads by rating level.

[6] The spreads we present are very similar to the high-yield bond spreads reported in Figure P.2 in Acharya *et al.* (2009).

[7] Clearly, the Lehman crisis in the fall of 2008 had an additional negative impact on bond spreads, which shot up momentarily to levels close to 7% for investment-grade bonds, and above 15% for high-yield bonds.

in the quantity of credit available for firms starting in August 2007. These movements appear to be largely due to events that were initially associated with the housing sector, and eventually affected financial institutions and the overall credit markets. We believe this environment provides us with a unique opportunity to identify the effects of supply contractions on corporate policies.

2.1.2 The maturity structure of corporate long-term debt

Our identification strategy requires that there is enough variation in long-term debt maturity across firms. In particular, there must exist a significant group of firms that have a spike (or "discontinuity") in their long-term debt maturity structure appearing right after the crisis. Naturally, one could expect firms to have well-diversified maturity structures, so that they are never forced to repay or refinance significant amounts of debt in any particular year. If that was the case, it would be difficult for us to implement our proposed strategy. As discussed in the introduction, and elsewhere in the literature, there seems to exist a number of frictions making it difficult for firms to maintain their optimal capital structures (assuming firms do pursue such policies in the first place).[8] It would be hard to imagine that firms are generally unable to be at their optimal debt-to-asset ratios for many consecutive years, while at the same time maintaining an optimal debt maturity structure. The existing literature provides limited guidance on this conjecture. Hence, we find it interesting to investigate this in more detail.

Figure 3 depicts the distribution of debt maturities for the sample of firms used in our analysis (the data are described in detail in Section 2.3). For each firm in the third quarter of 2007, we have information on the amount of long-term debt that matures in each of the following five years: 2008, 2009, 2010, 2011, and 2012.[9] Figure 3 reports these amounts as a fraction of total long-term debt. Accordingly, for each vertical bar in the figure (representing a year), a firm can have anywhere between 0% and 100% of its long-term debt coming due. For ease of visualization, the figure pins down the debt maturity structures of two firms (described below). For example,

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[8] Starting from Fischer *et al.* (1989), researchers cite transactions costs arguments as a key reason why firms may not instantaneously adjust their debt ratios. Alternative explanations include managerial "market timing" (Baker and Wurgler, 2002) and simple inertia (Welch, 2004).

[9] We also know the amount of long-term debt that matures in more than five years (starting in 2013), though we do not have year-by-year information beyond five years.

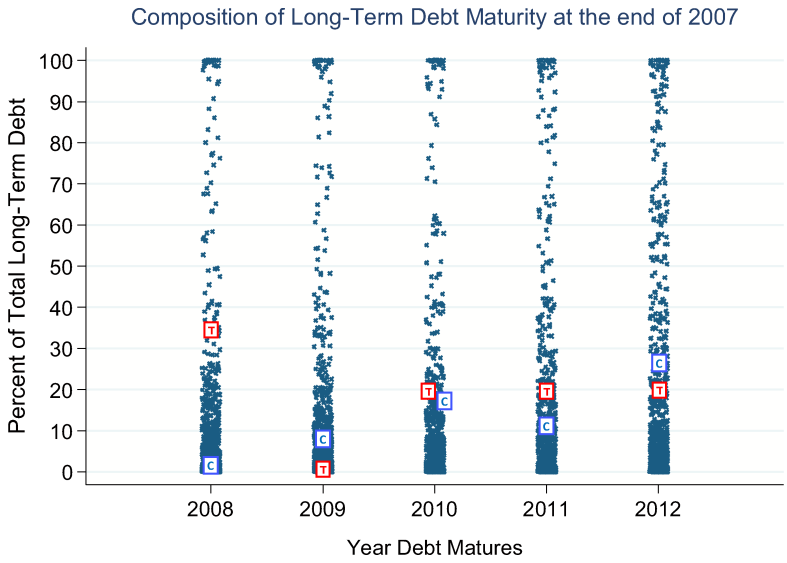

Figure 3. Composition of long-term debt maturity at the end of 2007.

This figure displays the proportion (%) of long-term debt maturing in the years of 2008, 2009, 2010, 2011, and 2012 for our sample firms. Maturity structure is measured at the end of the 2007 fiscal year. As an example, the squares with the letter "T" indicate the proportion of long-term debt that is maturing for Dollar-Thrifty (a treated firm) for each year between 2008 and 2012. The squares with the letter "C" represent the long-term debt maturity structure for Dollar-Thrifty's control match: Avis-Budget. At the end of 2007, Dollar's (Avis's) long-term debt maturity structure in the next five years is as follows: 34% (1%) due in 2008, 0% (7%) due in 2009, 19% (17%) due in 2010, 19% (11%) due in 2011, 19% (26%) due in 2012. The remainder of those companies' long-term debt was scheduled to mature in years beyond 2012.

Interpretation: This figure shows that there is substantial variation in long-term debt maturity across firms. In particular, there exists a significant group of firms that have a spike in their long-term debt maturity structure appearing right after the crisis, as well as other years.

at the end of 2007, the long-term debt maturity structure of Dollar-Thrifty (a treated firm, labeled [T] in Figure 3) is as follows: 34% of its long-term debt is due in 2008, 0% is due in 2009, 19% is due in 2010, 19% is due in 2011, and 19% is due in 2012 (the remainder matures after 2012). Dollar's control match is Avis-Budget (labeled [C] in the figure).

The figure makes it clear, however, that there is significant cross-firm variation in maturity structure. Consider, for example, the fraction of long-term debt that is due within the 1-year period following the 2007 panic

(i.e., due in 2008). Figure 3 suggests that there exists a significant number of firms whose long-term debt maturity concentrates in 2008 (some firms have nearly 100% of their long-term debt maturing that year). At the same time, many firms do not have any significant amount of long-term debt maturing in 2008. Similar variation in maturities obtains for the other individual years. For example, many firms have maturity spikes appearing in 2012, five years after the 2007 episode (some have 100% of their long-term debt maturing that year). These firms are similar to the ones with concentrated maturity in 2008 in that they, too, allow their debt maturity to concentrate in a particular year; however, their maturity is concentrated in a future year that lies well beyond the 2007 crisis.

Figure 3 shows that the distributions of long-term debt maturing in the individual years beyond 2008 (2009 through 2012) look fairly similar to the distribution of long-term debt maturing in 2008. This suggests that firms may not always try to renegotiate in advance and elongate maturities of debts that are soon to come due. We have also examined the distributions of debt maturities in years prior to 2007, and they look very similar to the distribution depicted in Figure 3.[10] In addition, we perform statistical tests to verify whether debt maturity distributions differ significantly across years. For example, we compare the cross-sectional distributions of the fraction of long-term debt maturing in one, two, three, four, and five years, across years 2006 and 2007. The lowest p-value is 0.2 for the fraction of debt maturing in 4 years, suggesting that the distributions are not statistically distinguishable across years 2006 and 2007. This result suggests that there is nothing special about the distribution of debt maturities on the verge of the crisis (in 2007), compared to the distribution measured one year earlier (in 2006).

One possible reason why some firms end up with spikes in their debt-maturity distributions is that they may concentrate debt issuance in particular years. To provide some descriptive evidence on these patterns, we use the Herfindahl index, a common measure of concentration. From the sample of 1,067 firms that we use in our main analysis, we select those whose long-term debt issuance variable (defined in detail below) is available for the last ten years, that is, from 1998 through 2007. A Herfindahl index is then calculated using the percentage of debt (normalized by assets) that the firm issued in a particular year with respect to the total issuance within the entire 10-year window. If firms perfectly diversify their debt issuance

[10] These distributions are depicted in a previous version of the paper.

over this 10-year window, we would see a Herfindahl index of 0.10. As it turns out, the average Herfindahl index calculated from our sample is 0.34, suggesting that on average firms issue debt in about 3 of 10 years.

2.2 Counterfactual Matching Approach

We want to test whether firms that need to refinance their long-term obligations at the time of a credit crisis alter decisions related to real-side variables. Our goal is to develop an identification strategy that resembles an "experiment:" the firm's long-term debt maturity structure and developments in the financial markets coincide such that the firm needs to refinance a large fraction of its debt in the midst of a credit contraction. If debt maturity was randomly assigned across firms, then it would suffice to compare the outcomes of firms that had significant debt maturing around the time of the crisis with firms whose debt happened to mature at a later date. Our analysis, however, needs to account for the fact that we are not doing a lab experiment, but are instead relying on non-experimental data.

The challenge is to gauge firms' outcomes *had they not* been caught between a credit crisis and the need to refinance their debt. Naturally, this is a difficult problem. One way to tackle this issue is to estimate differences between plausibly counterfactual outcomes and those that are observed in the data. Under this approach, a standard method is to use a parametric regression where the group of interest is differentiated from all other observations with a dummy variable. Outcome differences are then estimated by the coefficient on the group dummy. The regression model is specified according to a set of theoretical priors about the outcome variable — a simple, linear representation of a particular theory. Controls such as size, profitability, and leverage may be added to the specification to capture additional sources of firm heterogeneity. As shown by Heckman *et al.* (1998), however, the inclusion of controls in the regression *per se* does not address the fact that the groups being compared may have very different characteristics.[11] When control variables have poor distributional overlap, one can improve the estimation of group differences by allowing for nonlinear modeling as well as using nonparametric methods.

The strategy that we emphasize in our study is less parametric and more closely related to the notion of a "design-based" test (see Angrist and

[11] See also Dehejia and Wahba (2002).

Pischke, 2010). In particular, we conduct our analysis combining a natural experiment with the use of matching estimators.[12] The idea behind this family of estimators is that of isolating *treated* observations (in our application, firms with debt maturing during the crisis) and then, from the population of non-treated observations, look for *control* observations that best "match" the treated ones in multiple dimensions (covariates). In this framework, the set of counterfactuals are restricted to the matched controls. In other words, it is assumed that in the absence of the treatment, the treated group would have behaved similarly to the control group. The matches are made so as to ensure that treated and control observations have identical distributions along each and every one of the covariates chosen (dimensions such as firm size, profitability, leverage, credit risk, etc.). Inferences about the treatment of interest (refinancing constraints) are based on differences in the post-treatment outcomes of treated and control groups.[13]

Although a number of matching estimators are available, we employ the Abadie and Imbens (2006) estimator, as implemented by Abadie *et al.* (2004). In lieu of using a standard propensity score approach, the Abadie-Imbens matching estimator that we use minimizes the distance (i.e., the Mahalanobis distance) between a vector of observed covariates across treated and non-treated firms, finding controls based on matches for which the distance between vectors is smallest. The estimator allows controls to serve as matches more than once, which compared to matching without replacement, lowers the estimation bias (but can increase the variance). In our estimations we select one matched control for each treated firm (though more controls could be chosen). The Abadie-Imbens estimator produces "exact" matches on categorical variables. Naturally, the matches on continuous variables will not be exact (though they should be close). The procedure recognizes this difficulty and applies a bias-correction component to the estimates of interest. In addition, the estimator produces heteroskedastic-robust standard errors.

In matching estimations, the specification used is less centered around the idea of representing a model that explains the outcome variable (the approach is sometimes deemed "too atheoretical"). Instead, the focus is in ensuring that variables that might both influence the selection into

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[12] The matching estimator approach has been used by, among others, Villalonga (2004), Malmendier and Tate (2009), and Campello *et al.* (2010).

[13] Imbens (2004) provides an introductory review of the treatment evaluation literature.

treatment and observed outcomes are appropriately accounted for in the estimation. For example, the outcome that we are most interested in is investment spending. While there are numerous theories on the determinants of corporate investment, we only include in our test covariates for which one could make a reasonable case for simultaneity in the treatment–outcome relation. Among the list of categorical variables that we include in our estimations are firms' industrial classification codes and the rating of their public bonds. Our non-categorical variables include firms' market-to-book ratio (or "*Q*"), cash flow, cash holdings, size, and the ratio of long-term debt to total assets. It is commonly accepted that these covariates capture a lot of otherwise unobserved firm heterogeneity. The estimations implicitly account for all possible interactions between the included covariates.

We estimate Abadie-Imbens' average effect of the treatment on the treated (ATT). We model the outcomes in our experiments in differenced form — we perform difference-in-differences estimations. Specifically, rather than comparing the *levels* of investment of the treatment and control groups, we compare the *changes* in investment across the groups after the treatment. We do so because the investment levels of the treated and controls could be different prior to the event defining the experiment, and continue to be different after that event, in which case our inferences could be potentially biased by these uncontrolled firm-specific differences.

2.3 Data Collection and Variable Construction

We use data from COMPUSTAT's North America Fundamentals Annual, Fundamentals Quarterly, and Ratings files. We start from the quarterly file and disregard observations from financial institutions (SICs 6000–6999), not-for-profit organizations, and governmental enterprises (SICs greater than 8000), as well as ADRs. We drop firms with missing or negative values for total assets (*atq*), capital expenditures (*capxy*), property, plant and equipment (*ppentq*), cash holdings (*cheq*), or sales (*saleq*). We also drop firms for which cash holdings, capital expenditures or property, plant and equipment are larger than total assets.

Our data selection criteria and variable construction approach follow that of Almeida *et al.* (2004), who study the effect of financing constraints on the management of internal funds, and that of Frank and Goyal (2003), who look at external financing decisions. Similar to Almeida *et al.*, we discard from the raw data those observations for which the value of total assets is less than $10 million, and those displaying asset growth exceeding 100%

(including firm-quarters with missing values). We further require that firms' quarterly sales be positive and that the sales growth does not exceed 100%.

The data on debt maturity variables are only available in the COMPUSTAT annual file. We merge the annual and the quarterly files to make use of debt maturity information in our analysis. COMPUSTAT annual items *dd1, dd2, dd3, dd4*, and *dd5* represent, respectively, the dollar amount of long-term debt maturing during the first year after the annual report (long-term debt maturing in 2008 for firms with a December 2007 fiscal year-end), during the second year after the report (long-term debt maturing in 2009 for firms with a December 2007 fiscal year-end), during the third year after the report, and so on. COMPUSTAT annual item *dltt* represents the dollar amount of long-term debt that matures in more than one year. Accordingly, a firm's total long-term debt can be calculated as $dd1 + dltt$.

We apply the following filters to the debt variables. We delete firms with total long-term debt ($dd1 + dltt$) greater than assets (*at*, in the annual file) and firms for which debt maturing in more than one year (*dltt*) is lower than the sum of debt maturing in two, three, four, and five years ($dd2+dd3+dd4+dd5$).[14] In our baseline tests, we disregard firms for which liabilities such as notes payables, bank overdrafts, and loans payable to officers and stockholders are greater than 1% of total assets. For those tests, we require firms to have long-term debt maturing beyond one year (*dltt*) that represents at least 5% of assets (*at*).[15] These debt-related restrictions are meant to ensure that we are contrasting firms of seemingly comparable debt profile, with long-term debt representing an important source of funds, and with the demonstrated quality/ability to have substantial debt due beyond one year on the balance sheet.[16]

We focus on firms that have 2007 fiscal year-end months in September, October, November, December, or January. The sample of firms with these

[14] We investigated these cases using data from Capital IQ, and discovered that this problem is mostly due to rounding problems in Compustat. These are mostly firm-years in which all of their long-term debt is due in years 2 to 5 (that is $dltt = dd2+dd3+dd4+dd5$), but the sum is rounded in a way that it appears larger than *dltt*. In addition, we discovered a few cases that appeared to be true mistakes in the data. For example, we discovered two firms in our sample (Healthsports.Inc and Xata.Inc) for which COMPUSTAT reports interest payments as part of the principal for the variables *dd2* to *dd5*, but not for *dltt*. These problems create the discrepancy. The results are qualitatively similar if we include these firm-years in the sample.

[15] In subsequent tests, we vary this and other debt-related cutoffs to ensure that our inferences are robust.

[16] To operationalize our tests, we set the cutoff between short- and long-term debt at one year (the standard benchmark in the literature). As we report in Section 4.8, our treatment and control firms historically issued long-term debt at the same frequency, about once every three years.

fiscal year-end months corresponds to more than 80% of the universe of firms in fiscal year 2007. This restriction is due to the timing of the credit shock, which happened in the fall of 2007. For our benchmark tests, we want to avoid firms that filed their 2007 annual report before the crisis. These firms could have used the time period between filing the annual report and the credit crisis to rebalance their debt maturity, thus compromising our identification strategy. As noted above, the variables that detail the amount of long-term debt maturing within one, two, three, four, and five years from the date of the report are only available in the annual COMPUSTAT file. Accordingly, for a December fiscal-year-end firm, we cannot use the third quarter report to obtain a breakdown of timing of the debt maturity composition as of 9/30/2007. We instead use the firm's 2007 annual report to obtain the debt-maturity breakdown as of 12/31/2007. Finally, to make it into our final sample, a firm needs to have non-missing values for all variables that are used in our estimations, including all covariates and the outcome variable. Our 2007 sample consists of 1,067 individual firms. Appendix Table A1 gives additional detail on our sample selection screens.

In our base experiment, the outcome variable is the change in firm investment. Investment is defined as the ratio of quarterly capital expenditures (COMPUSTAT's *capxy*) to the lag of quarterly property, plant and equipment (*ppentq*).[17] We measure the change in a firm's investment around the fourth quarter of 2007 by taking the difference between the average quarterly investment of the first three quarters of 2008 and the first three quarters of 2007. We use symmetric quarters around the fourth quarter of 2007 to avert seasonality effects. We avoid using data from the fourth quarter of 2008 to sidestep the effects of the Lehman debacle and the deep recession that ensues soon after that event.

As discussed earlier, we match firms based on *Q*, cash flow, size, cash holdings, and long-term leverage. *Q* is defined as the ratio of total assets plus market capitalization minus common equity minus deferred taxes and investment tax credit ($atq + prccq \times cshoq - ceqq - txditcq$) to total assets ($atq$). Cash flow is defined as the ratio of net income plus depreciation and amortization ($ibq + dpq$) to the lag of quarterly property, plant and equipment. Size is defined as the log of total assets. Cash holdings is the ratio of cash and short-term investments ($cheq$) to total assets. Long-term

[17] Note that *capxy* represents "year-to-date" capital expenditures. We transform this variable so that it reflects quarterly values.

leverage is the ratio of total long-term debt ($dd1 + dltt$) to total assets. Our matching estimator uses the averages of the first three quarters of 2007 of each of these variables as covariates.

We also match firms on industry and credit ratings categories. Industry categories are given by firms' two-digit SIC codes. Our credit ratings categories follow the index system used by S&P and are defined as: investment grade rating (COMPUSTAT's *splticrm* from AAA to BBB–), speculative rating (*splticrm* from SD to BB+), and unrated (*splticrm* is missing). Matching treatment and control firms within the same industry *and* within the same debt ratings categories ensures that differences in firms' underlying business conditions (e.g., product demand) and credit quality may not explain our results.

We construct treatment and control groups based on firms' long-term debt maturity schedule. In our benchmark specification, the treatment variable is defined by the ratio of long-term debt maturing within one year ($dd1$) to total long-term debt ($dd1 + dltt$). Firms for which this ratio is greater than 20% are assigned to the treatment group, while firms for which this ratio is less than 20% are assigned to the non-treated group. We stress that these criteria are used for convenience as a way to initialize our test and that they will be altered later as way to check the test's internal consistency and generalize our findings. This base procedure assigns 86 firms to the treatment group. While we provide a full characterization of the treatment and control firms in Section 3.1, it might be useful to describe a few concrete examples of firms in our sample. We do this in turn.

2.4 Examples of Treatment and Control Firms

One of the firms in our treatment group comes from the car rental business: Dollar-Thrifty. As depicted in Figure 3, in the fall of 2007, Dollar's fraction of total long-term debt maturing in 2008 was 34%. The fraction of long-term debt maturing in 2009, 2010, 2011, and 2012, was, respectively, 0%, 19%, 19%, and 19%; the remaining 9% was due in more than five years. It is apparent that Dollar's long-term maturity schedule happened to have a "discontinuity" right at the time of the crisis.

Our sample match for Dollar is Avis-Budget. The two firms are in the same industry, have about the same size, and are both high-yield bond issuers. However, Avis's long-term debt maturity structure was different from Dollar's at the end of 2007. In particular, Avis had to refinance less than

1% of its debt in 2008. In the subsequent four one-year windows (starting from 2009), it would have to repay 7%, 17%, 11%, and 26% of its long-term debt, with 39% due in later years. The wedge between Dollar's and Avis's long-term debt maturity structures is depicted in Figure 3.

Another example of a treated firm in our sample comes from the trucking industry. In the fall of 2007, JB Hunt's long-term maturity profile was such that 26% of its debt was due in 2008. By comparison, Con-way was scheduled to refinance only 2% of its long-term debt in 2008 (but over 20% in 2010). JB Hunt and Con-way are investment-grade bond issuers, and both these firms enter our sample: Con-way appears as JB Hunt's control match.

A much-publicized case of crisis-related debt burden is also in our sample: Saks Inc. In late 2007, Saks had 56% of its long-term debt coming due in 2008. Our control match for Saks is Bon-Ton Inc. (which operates, among others, Bergner's and Belk stores). Bon-Ton's long-term debt due in one year was less that 1% of the total (but 28% of its debt was scheduled to come due in 2011).[18] Another example comes from the communications industry, where Dish Network is a treated firm and Equinix its control match.

3 Baseline Results

We start by providing summary statistics for our samples of treated, non-treated, and control firms. Our initial goal is to show that our procedure does a good job of matching treatment to control firms along observable dimensions. We then present our base empirical results. These results should be seen as a benchmark only, in that the magnitude of the firm responses to our refinancing constraint treatment will depend on how we define the treatment. For our benchmark case, we define treatment in a manner that strike us as reasonable, but we later alter the parameters that define the baseline treatment status to check its internal consistency.

3.1 Summary Statistics

Our matching approach is nonparametric, making it fairly robust to extreme observations. Treatment and control firm outcomes, however, are compared

[18] A portion of Bon-Ton's operations (several retail chains) was bought from Saks just a few years before the crisis. These two firms thus shared a number of similarities in the fall of 2007, except the maturity structure of their long-term debt.

in terms of mean differences. To minimize the impact of gross outliers on these comparisons, we winsorize variables at the top and bottom 0.5 percentile. Table 1 reports the (pre-crisis) median values of the variables used in our matching procedure across various data groups. We use the Pearson χ^2 statistic to test for differences in the medians of the variables of interest across the groups.

Panel A compares the 86 treated firms in our sample with the remaining 981 firms that are not assigned into the treated group. The treated firms have higher median *Q*, cash flows, and cash holdings. Treated firms are also smaller and have a lower leverage ratio. As discussed above, these sample differences are expected, given that we are relying on observational data rather than running a true experiment. The goal of matching estimator techniques is to control for these distributional differences, which could affect both the selection into the treatment and the post-crisis outcomes.

Panel B compares median values for treated and matched control firms. The Abadie-Imbens estimator identifies a match for each firm in the treatment group. We have 86 firms in both groups, but since matching is done with replacement, we have only 79 unique firms in the control group.[19] Notably, after the matching procedure, one finds *no* statistical differences in the median values of the covariates we consider across treated and control firms.

Table 2 compares the entire distributions — rather than just the medians — of the various matching covariates across the three groups. The results mirror those reported in Table 1. Panel A shows that treated firms differ significantly from non-treated firms. In particular, a Kolmogorov-Smirnov test of distributional differences returns highly significant statistics for virtually all of the matching covariates. As in Table 1, these differences disappear when we compare the treated firms to the group of closely-matched control firms. In particular, Panel B of Table 2 shows that there are no statistical differences in the distributions of the various matching covariates across the treated and control firms. These statistics support the assertion that the matching estimator moves our experiment closer to a test in which treatment and control groups differ only with respect to when their long-term debt matures.

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[19] A list with the names of all control and treatment firms is available from the authors.

	Q	Cash Flow	Size	Cash	LT Leverage	Investment
Panel A: Medians for Treated and Non-Treated Firms in 2007						
Treated	1.728	0.076	5.870	0.080	0.244	0.047
Non-Treated	1.499	0.056	6.784	0.045	0.294	0.047
Difference	0.229	0.020	−0.914	0.035	−0.050	0.000
Median Test *p*-value	0.005	0.009	0.005	0.005	0.093	0.918
Panel B: Medians for Treated and Control Firms in 2007						
Treated	1.728	0.076	5.870	0.080	0.244	0.047
Control	1.599	0.070	6.266	0.063	0.233	0.051
Difference	0.129	0.006	−0.396	0.017	0.011	−0.003
Median Test *p*-value	0.286	0.446	0.286	0.879	0.647	0.879

Table 1. Pre-crisis characteristics of treated, non-treated, and control firms at the end of 2007.

This table compares the properties of treated, non-treated, and control firms (median comparisons). The 1,067 sample firms are split into treated and non-treated groups. The treated firms are defined as those for which the percentage of long-term debt maturing within one year (i.e., 2008) is greater than 20%. The non-treated firms are defined as those for which the percentage of long-term debt maturing within one year is less than or equal to 20%. Control firms are a subset of the non-treated firms selected as the closest match to the treated firms based on a set of firm characteristics: Q, cash flow, size, cash holdings, long-term debt normalized by assets, 2-digit SIC industry, and credit ratings. There are 86 treated firms and 86 control firms. The medians of Q, cash flow, size, cash holdings, and long-term leverage are displayed for the three samples of firms (treated, non-treated, and controls). The average quarterly investment-to-capital ratio over the first three quarters of 2007 is also displayed. See the text for further variable definitions. The test for a difference in the medians of a firm characteristic across two groups is conducted by calculating the Pearson's χ^2 statistic, with the *p*-values of this test reported at the bottom row of each panel.

Interpretation: This table shows that the samples of treated and non-treated firms differ in several important dimensions, but that these differences disappear once we compare treated firms to the sample of control firms that is generated by the matching estimator.

3.2 The Real Effects of the 2007 Panic

We examine the investment behavior of our treated and matched control firms around the 2007 credit crisis. Before doing so, however, we perform a group-mean comparison between the 86 treated firms and the broader set of 981 firms that we classify as non-treated. Note that these comparisons are

		25th %	Median	75th %	Kolmogorov-Smirnov Test *p*-value
Panel A: Characteristics of Treated vs. Non-Treated Firms in 2007					
Q	Treated	1.341	1.728	2.305	0.006
	Non-Treated	1.185	1.499	2.081	
Cash Flow	Treated	0.033	0.076	0.150	0.013
	Non-Treated	0.026	0.056	0.116	
Size	Treated	4.320	5.870	7.640	0.000
	Non-Treated	5.730	6.784	7.883	
Cash	Treated	0.021	0.080	0.184	0.005
	Non-Treated	0.017	0.045	0.126	
LT Leverage	Treated	0.159	0.244	0.356	0.096
	Non-Treated	0.186	0.294	0.427	
Investment	Treated	0.027	0.047	0.095	0.365
	Non-Treated	0.027	0.047	0.082	
Panel B: Characteristics of Treated vs. Control Firms in 2007					
Q	Treated	1.341	1.728	2.305	0.160
	Control	1.263	1.599	2.063	
Cash Flow	Treated	0.033	0.076	0.150	0.676
	Control	0.043	0.070	0.124	
Size	Treated	4.320	5.870	7.640	0.676
	Control	4.549	6.266	7.237	
Cash	Treated	0.021	0.080	0.184	0.416
	Control	0.019	0.063	0.161	
LT Leverage	Treated	0.159	0.244	0.356	0.977
	Control	0.154	0.233	0.341	
Investment	Treated	0.027	0.047	0.095	0.915
	Control	0.028	0.051	0.091	

Table 2. Pre-Crisis Distributional Tests of Treated, Non-Treated, and Control Firms at the End of 2007.

This table compares distributional properties of the various matching covariates of treated, non-treated, and control firms. The 1,067 sample firms are split into treated and non-treated groups. The treated firms are defined as those for which the percentage of long-term debt maturing within one year (i.e., 2008) is greater than 20%. The non-treated firms are defined as those for which the percentage of long-term debt maturing within one year is less than or equal to 20%. Control firms are a subset of the non-treated firms selected as the closest match to the treated firms based on a set of firm characteristics: Q, cash flow, size, cash holdings, long-term debt normalized by assets, 2-digit SIC industry, and credit ratings. There are 86 treated firms and 86 control firms. The medians of Q, cash flow, size, cash holdings, and long-term leverage are displayed for the three samples of firms (treated, non-treated, and controls). The average quarterly investment-to-capital ratio over the first three quarters of 2007 is also displayed. See the text for further variable definitions. The 25th percentile, median, and 75th percentile are reported for each firm characteristic. The test for differences in the distribution of a firm characteristic across two groups is conducted by calculating the corrected Kolmogorov-Smirnov's D-statistic, with the *p*-values of this test reported in the rightmost column.

Interpretation: This table shows that the distributions of covariates for the samples of treated and non-treated firms are significantly different statistically. These differences disappear once we compare treated firms to the sample of control firms that is generated by the matching estimator.

equivalent to a standard OLS regression in which the outcome of interest (investment changes) is regressed on a dummy for treated firms. Panel A of Table 3 shows that prior to the crisis, both the treated and non-treated firms were investing at different rates. The average investment-to-capital ratio in the three first quarters of 2007 (the pre-crisis period) is 7.8% for the treated firms and 6.5% for the non-treated firms, though the difference is not statistically significant, as indicated in the third row of the panel. Nonetheless, the fact that both groups of firms had different investment levels in the pre-crisis period suggests that comparisons between the two groups could be potentially confounded by other factors.

Panel A of Table 3 also shows the investment levels in the first three quarters of 2008 (the post-crisis period). Notice that the investment of the treated and non-treated firms fell in 2008. For firms in the treatment group, the average investment dropped to 5.7% of capital, a fall of 2.1 percentage points (or nearly 27% lower investment rates). In contrast, for non-treated firms, investment fell to 6.0% of capital, a fall of 0.6 percentage points. These figures suggest that investment decreased by 1.6 percentage points *more* for firms that happened to have a lot of long-term debt maturing right after the credit crisis hit, relative to the general population of firms whose long-term debt did not come due so soon.

Panel B of Table 3 presents a full-fledged implementation of our difference-in-differences matching estimator. Firms in the treatment groups are now compared with closer counterfactuals (matched controls). Not surprisingly, we see that the 2007 (pre-crisis) investment levels of treatment and control firms are economically similar and statistically indistinguishable. Results in Panel B show that the investment policies of the treated and control firms became significantly different after the crisis. While the average quarterly investment of firms in the treatment group fell by 2.1 percentage points of total capital, control firms' investment remained largely unchanged. The estimates imply that investment decreased by 2.2 percentage points *more* for firms that had a lot of long-term debt maturing right after the crisis, relative to otherwise similar firms whose long-term debt did not come due as soon.

One interesting observation about the figures in Panel B is that the investment of the control firms did not fall in 2008. The characteristics of the *treated firms* may explain why the investment of the control firms does not decline following the crisis. Notice that firms in the treatment group have greater cash holdings, higher cash flows, and lower leverage ratios

than those in the general, non-treated sample population (see Table 1). By construction, firms in the control group will then also have greater cash holdings, higher cash flows, and lower leverage than the average sample firm. Given that they did not have to refinance significant amounts of debt following the crisis, control firms could use their more liquid positions to support investment going into 2008. In other words, corporate investment falls *only* for the group of high-cash holdings, high-cash flows, low-leverage firms that happen to have long-term debt repayment spikes appearing in 2008 (treated firms).

Panel B also reports the differential change in investment that is produced by the Abadie-Imbens matching estimator of the average effect of the treatment on the treated (ATT). The ATT difference is equal to minus 2.5 percentage points. This is a central result of our paper. It indicates that investment for the treated firms during the first three quarters of 2008 fell by about one-third of their pre-crisis investment levels.[20] More generally, the estimates in Panel B imply that frictions that arose from firms' debt maturity structures generated financing constraints that led to lower corporate investment rates following the 2007 credit crisis. These findings highlight the importance of debt maturity structure for corporate managers. They are also interesting for economic policymakers when designing policies aimed at softening the impact of credit contractions on the economy.

Given the similarity between firms in the treatment and control groups, the evidence presented is indicative of a causal effect of debt maturity on investment. In order to strengthen the interpretation of the results, we replicate exactly the same "experiment" that we run for the crisis period around a *placebo period* dated one year earlier. That is, we use 2006 maturity information to sort firms into treatment and non-treated groups and 2006 covariates to produce a matched group of firms. We then examine firms' investment behavior during the first three quarters of 2007. This

[20] To ensure that our ATT results are not explained by extreme data points, we redo our experiment 85 times taking away one treated firm at a time. In absolute terms, the lowest ATT estimate is minus 2.1 percentage points (significant at 5% test level) and the highest minus 2.9 percentage points (significant at 1%). In addition, we also examine the effect of different degrees of winsorization on our results. In our benchmark results, we use a winsorization of 1% of the data (0.5% on the top and bottom of the distribution). If we completely remove winsorization, the results are similar but less significant (matching estimator = minus 3.64 percentage points with a p-value of 7.5%), suggesting an increased degree of noise in the data without any winsorization. A similar result obtains if we instead winsorize 5% of the data (matching estimator = minus 1.23 percentage points with a p-value of 10%), suggesting a less precise matching of firms.

Average Quarterly Investment/Capital Stock (in percentage points)

Panel A: Investment Before and After the Fall 2007 Credit Crisis

Investment in 2008 (Q1 to Q3) vs. Investment in 2007 (Q1 to Q3)

	2007	2008	2008–2007
Treated Firms	7.83***	5.70***	−2.13**
	(0.89)	(0.50)	(0.84)
Non-Treated Firms	6.54***	5.98***	−0.56***
	(0.20)	(0.16)	(0.18)
Difference	1.29	−0.28	−1.57*
	(0.91)	(0.53)	(0.85)

Panel B: Investment Before and After the Fall 2007 Credit Crisis

Investment in 2008 (Q1 to Q3) vs. Investment in 2007 (Q1 to Q3)

	2007	2008	2008–2007
Treated Firms	7.83***	5.70***	−2.13**
	(0.89)	(0.50)	(0.84)
Control Firms	7.26***	7.35***	0.09
	(0.70)	(0.64)	(0.71)
Difference	0.57	−1.65***	−2.21**
	(0.96)	(0.62)	(1.01)
Matching Estimator			−2.46**
(ATT)			(1.07)

Panel C: The Placebo Test

Investment in 2007 (Q1 to Q3) vs. Investment in 2006 (Q1 to Q3)

	2006	2007	2007–2006
Treated Firms	7.27***	6.86***	−0.41
	(0.63)	(0.65)	(0.72)
Control Firms	7.17***	6.89***	−0.28
	(0.76)	(0.66)	(0.84)
Difference	0.10	−0.03	−0.13
	(0.84)	(0.79)	(1.02)
Matching Estimator			0.01
(ATT)			(1.09)

***, **, * indicate significance at the 1, 5, and 10% levels, respectively.

Table 3. Difference-in-differences of firm investment before and after the fall 2007 credit crisis with a placebo test conducted a year before the credit crisis.

This table presents estimates of the change in average quarterly investment rates from the first three quarters of 2007 to the first three quarters of 2008 in Panels A and B (before and after the fall 2007 credit crisis). Panel C presents an estimate of the change in investment from the first three quarters of 2006 to the first three quarters of 2007 (a placebo test conducted before the credit crisis). In Panel A, the average of quarterly investment during the first three quarters of 2008 and the first three quarters of 2007 is calculated for the treated firms and non-treated firms, as well as the difference in the difference between the two groups of firms over the two years. The average quarterly investment is normalized by the capital stock at the preceding quarter; that is, by lagged property, plant, and equipment. The treated firms are defined as those for which the percentage of long-term debt maturing within one year (i.e., 2008) is greater than 20%. The non-treated firms are defined as those for which the percentage of long-term debt maturing within one year is less than or equal to 20%. There are 86 treated firms and 981 non-treated firms in Panel A. In Panel B, the average of quarterly investment during the first three quarters of 2008 and the first three quarters of 2007 is calculated for the treated firms and control firms, as well as the difference in the difference between the two groups of firms over the two years. Control firms are a subset of the non-treated firms selected as the closest match to the treated firms based on a set of firm characteristics: Q, cash flow, size, cash holdings, long-term debt normalized by assets, 2-digit SIC industry, and credit ratings. There are 86 treated firms and 86 control firms in Panel B. Panel C is constructed analogously, but the tests are conducted one year earlier (before the credit crisis). There are 113 treated firms and 113 control firms in Panel B. ATT is the Abadie-Imbens bias corrected average treated effect matching estimator (Matching Estimator). Heteroskedasticity-consistent standard errors are in parentheses.

Interpretation: This table reports the central results of the paper. Firms whose long-term debt was largely maturing right after the third quarter of 2007 (treated firms) cut their investment-to-capital ratio by 2.5 percentage points more (on a quarterly basis) than otherwise similar firms whose debt was scheduled to mature after 2008. This drop in investment is statistically significant. In contrast, there is no difference in investment behavior across treated and control firms in a non-crisis period (2006–2007).

falsification test can help us rule out alternative explanations for the results reported in Panel B. For example, there could be unobservable characteristics that generally predict both a short-maturity profile for long-term debt and a drop in investment (characteristics that are not captured by the matching estimator procedure described in Section 2.2). If this is the case, then maturity structure and investment should be correlated in 2006 as well, and not just in the 2007 crisis period.

The results from this base placebo test are reported in Panel C of Table 3. As in Panel B, treated and control firms have virtually identical investment behavior in 2006. Firms with more than 20% of their long-term debt maturing in 2007 (the "treatment" group) display an investment rate of 7.3% of capital in the first three quarters of 2006, while their control counterparts'

investment rate is 7.2%. Notably, there is *no difference* in investment behavior across these two groups of firms in the post-"treatment" period (first three quarters of 2007), despite the different maturity profiles of long-term debt: both groups invest 6.9% on average in the first three quarters of 2007. The average treatment effect (ATT) in this case is virtually zero, and statistically insignificant. Simply put, our treatment–control contrasts do not appear in 2006, when there was no credit shortage.

3.3 Valuation Effects

While the tests in Table 3 show that firms with debt maturing right after the 2007 panic invested less, they are silent on the value implications of that effect. One would like to know, for example, if the treated firms were overinvesting in the pre-crisis period, in which case the decline in investment could be value-enhancing. Our empirical strategy is designed to ensure that the investment policies of treated and control firms were as similar as possible in the pre-August 2007 period.[21] Yet a valuation-based test could give additional context to the real-side implications of debt maturity and credit shortages.

To gauge the value implications of our investment-based tests, we compute the cumulative stock returns and the percentage change in Q for our treated, control, and non-treated firms over the outcome window (i.e., the first three quarters of 2008 during which the treatment firms cut investment due to the financing shock). We then perform mean-comparison tests to assess group differences. The results are reported in Table 4. Focusing first on returns, the table shows that non-treated firms' stock values declined, on average, 21% over the first three quarters of 2008 (the S&P return for that same period is -22%). Control firms' stocks fared slightly better, declining 18%. Treated firms, in contrast, saw their stocks decline by 29%. Thus, when we compare the stock performance of treated and control firms, we see a 11% differential in returns in favor of control firms. This difference is not only economically meaningful, but also statistically significant (p-value of 2%). Comparisons based on changes in Q lead to similar inferences about

[21] Recall, we match firms on a number of dimensions that are associated with their pre-crisis policies and valuation (such as cash holdings, leverage, cash flow, and Q). In addition, we show below that the investment spending of the treated and control firms followed "parallel trends" prior to August 2007.

	Return	Q
Treated	−28.97***	−19.20***
	(3.62)	(2.27)
Control	−18.16***	−14.11***
	(3.23)	(2.08)
Non-Treated	−20.55***	−12.86***
	(1.01)	(0.63)
Difference	−10.81**	−5.09*
Treated–Control	(4.85)	(3.06)

***, **, * indicate significance at the 1, 5, and 10% levels, respectively.

Table 4. Value analysis during the outcome period for treated, control, and non-treated firms.

This table presents value analysis for the first three quarters of 2008, which our tests define as the outcome period. The treated firms are defined as those for which the percentage of long-term debt maturing within one year (i.e., 2008) is greater than 20%. The non-treated firms are defined as those for which the percentage of long-term debt maturing within one year is less than or equal to 20%. Control firms are the closest matches to the treated firms based on a set of firm characteristics (see the description in Table 3 for details). We examine two variables: the total stock market return, and the percentage change in Q with respect to the end of 2007. Heteroskedasticity-consistent standard errors are in parentheses.

Interpretation: This table shows that firms that faced greater refinancing constraints in the aftermath of the credit episode lost more value, relative to similar firms whose debt was scheduled to mature after 2008.

the more pronounced value losses observed by treated firms in the first three quarters of 2008.

It is very difficult to ascertain whether firms were overinvesting or underinvesting in the pre-crisis period. We can say, however, that firms that were faced with large debt repayment obligations in 2008 invested less and lost more value at the same time that they were slashing capital expenditures. While we do not push a causal link between these two results, our findings are consistent with the argument that firms that faced greater refinancing constraints lost more value, possibly due to the real-side adjustments they were forced to make.[22]

[22] As suggested by the referee, another effect could play a role in the results of Table 4. It is possible that treatment firms that refinanced during the crisis had to pay an interest rate that embedded large

4 Robustness Tests

In this section we show that our benchmark results are robust and internally consistent. We first show that our results cannot be explained by firms selectively modifying their debt maturity structure in the years preceding the crisis. We also show that the 2007 crisis results do not obtain in non-crisis periods. In addition, we show that our results cannot be ascribed to differential trends in the outcome of interest (investment), nor can they be attributed to differential responses across treated and control firms that could arise in recession periods (independently of the credit shortage). We demonstrate that the treatment–outcome relations that we uncover in our baseline tests respond sensibly to changes in treatment intensity (changes in leverage cut-offs and amount of debt due in 2008). The results are also robust to changes in the design of the matching estimator. Finally, we report the results obtained when we use standard regression techniques.

4.1 Predetermined Maturity Tests

Our baseline experiment uses maturity variables measured near the end of 2007. As explained in Section 2.3, we made this choice to ensure that we capture the extent to which firms are constrained by debt maturity during the shock. This requirement should increase the power of our tests. However, it may raise the concern that measured variation in maturity reflects the anticipated effects of the crisis. For example, suppose that higher quality managers were more likely to anticipate the credit crisis in early 2007, or even in 2006. Then, it is possible that unobservable managerial quality could explain both longer maturity profile and superior firm performance in the aftermath of the crisis. Such refinancing activity by "smart CEOs" would leave only the "dumb CEOs" with long-term debt maturing in 2008. In this way, some firms (those with "dumb CEOs") may cut investment for non-maturity-related reasons after the credit crisis hits. Another alternative explanation speaks to the credit easing that took place before the crisis. One could argue that firms elongated their debt in the years prior to the crisis and that "better firms" elongated their debt maturity by more, reducing the odds that they would be caught with large amounts of debt coming due

premiums, and hence bore part of the deadweight loss from the disruption in credit. In contrast, the market understood that control firms had debt that was likely to mature after the crisis, at which point interest rates would not reflect these increased premiums.

in 2008. Both stories relate to dynamics that are differentially pronounced across our two firm groups, that potentially confound quality, and that are timed so as to potentially affect the interpretation of our 2007 tests.

A simple way to ensure that the above self-selection stories do not drive our results is to use maturity variables measured *several years prior* to the end of 2007. For example, we can examine firms' maturity profiles at the end of 2005 — about two years before the crisis — and identify firms that had a large fraction of their long-term debt maturing in three years (i.e., in 2008). Since it is unlikely that even the best managers could have anticipated the 2007 credit crisis back in 2005, such modification of our basic specification can address the unobservable managerial quality story. For robustness, we also experiment with using a maturity profile measured an additional two years earlier — fiscal-year end 2003 — which is the earliest we can go back, given COMPUSTAT's information on long-term debt maturity. Naturally, as we go back to earlier years to measure maturity, the effect of maturity structure on 2008 investment should decrease in magnitude (since the maturity information becomes stale with time). For both earlier snapshots (2003 and 2005), the treatment group again includes firms that have more than 20% of their long-term debt at the time maturing in 2008. Other than using alternative predetermined maturity profiles to assign treatment and non-treatment groups, all other components of the experiment remain unchanged. Accordingly, the outcome variables are defined identically to those in Table 3, that is, changes in investment between the first three quarters of 2008 and the first three quarters of 2007.

The results (untabulated) suggest that the predetermined maturity profiles also help predict changes in investment around the credit crisis. As should be expected, the effects of maturity structure on investment spending to total capital are smaller than those in Table 3 (minus 1.4 percentage points when using the 2005 maturity and minus 0.6 points when using the 2003 maturity). Despite the lower estimates, cross-group difference in treatment effects are still economically meaningful.[23] While better quality firm/managers may have elongated their debt maturity in the years preceding the crisis, it was not the case that they did so differentially across treatment and control groups that are determined on the basis of long-term

[23] The difference in investment using the end-of-year 2005 debt maturity is significant at the 5% level. The difference in investment using the end-of-year 2003 debt maturity is statistically insignificant (*t*-statistic equal to 1.0).

debt structure in 2003 or 2005. Simply put, the results we obtain suggest that unobserved quality stories that influence debt maturity structure (refinancing) in the years leading up to the crisis are unlikely to explain the relation between debt maturity and investment that we report in Table 3.

4.2 Evidence from Non-Crisis Periods

Our identification strategy relies on the assumption that firms with maturing long-term debt find it difficult to refinance their obligations by tapping other external financing sources. The 2007 credit crisis provides us with an ideal setting in which this assumption is likely to hold. By the same token, the assumption is unlikely to hold in periods of easier credit. If our identification strategy is correct, we would expect *not to find* similar effects of maturity structure on investment during non-crisis periods. Panel C of Table 3 verifies whether this is true for the year of 2006 (one year before the August 2007 credit event). Here, we generalize these placebo tests across years prior to 2006, reporting results on a year-by-year basis as well as pooled over the pre-crisis 2002–2006 period.[24] To replicate our testing strategy for years prior to 2006, we sort firms into treatment and non-treatment groups considering maturity structures measured in 2001 through 2005, as if there were credit crises in the fourth quarter of each of those years. We then examine the differential change in investment for treated and control firms. We perform this test for each individual non-crisis year, using the exact same sampling criteria, covariate matching approach, and definitions of treatment and control groups that we used for the 2007 crisis.

The results are reported in Table 5, which also reports the results for 2006 and 2007 for reference. The estimated difference in investment changes across treatment and control groups is economically small and statistically insignificant for placebo crises in all years between 2001 and 2006. The pooled ATT estimate from 2001 through 2006 is 0.0%. These findings are internally consistent and support our assertion that debt maturity affects investment through a (re-)financing constraint channel in the aftermath of a credit supply contraction.

It is important to highlight how the falsification tests of Table 5 help us establish our economic inferences. For example, one potential concern with

[24] We start in the early 2000's because it is difficult to classify the late 1990's as a non-crisis period in light of episodes such as the LTCM debacle and the Asian crisis. In addition, we later focus separately on the year 2001 because it contains a recession, but not a credit crisis.

Investment Change	Difference in the change in investment between treated and control firms (in percentage points)	Matching Estimator (ATT) (in percentage points)
2008–2007	−2.21**	−2.46**
	(1.01)	(1.07)
2007–2006	−0.13	0.01
	(1.02)	(1.09)
2006–2005	0.17	0.15
	(1.00)	(0.96)
2005–2004	−0.70	−0.54
	(0.50)	(0.50)
2004–2003	0.28	0.20
	(0.49)	(0.52)
2003–2002	0.21	0.30
	(0.54)	(0.54)
2002–2001	0.22	0.57
	(0.87)	(0.90)
Pooled Analysis: All Years Before Fall 2007 Credit Crisis	−0.10	−0.04
	(0.30)	(0.31)

***, **, * indicate significance at the 1, 5, and 10% levels, respectively.

Table 5. Difference-in-differences of firm investment from one year to the next: 2001 through 2007.

This table presents the change in the average quarterly investment-to-capital ratio from the first three quarters of a given year to the first three quarters of the next year. The first row replicates the Difference-in-Differences and Matching Estimator (ATT) from Panel B of Table 3, and the second row replicates the Difference-in-Differences and Matching Estimator (ATT) from Panel C of Table 3. Analogous results are then presented for the other years. The treated firms are defined as those for which the percentage of long-term debt maturing within one year is greater than 20%. Control firms are defined as those for which the percentage of long-term debt maturing within one year is less than or equal to 20%. Control firms are the closest matches to the treated firms based on a set of firm characteristics (see the description in Table 3 for details). ATT is the Abadie-Imbens bias-corrected average treated effect matching estimator (Matching Estimator). Heteroskedasticity-consistent standard errors are in parentheses.

Interpretation: This table shows that there is no significant difference in investment behavior across treated and control firms for periods other than the credit episode (2007–2008).

our baseline results is that firms may match the maturity of their investment opportunities with the maturity of their long-term debt. In that case, the spending decline that we observe in 2008 may be due to a natural exhaustion of firms' investment opportunities, which (by design) were financed with debt that matured at the same time. If it was natural to observe a decline in investment together with large proportions of debt coming due, we should observe an association between maturing debt and investment declines in other periods as well, not just in 2008. The tests in Table 5, however, show no indication that this is the case. The large chunks of debt that matured in those years were not followed by investment declines. One could perhaps argue the point further and suggest that this coincidence only happens before recessions (such as the 2008–2009 recession), when investment opportunities are truly depressed and firms let debt mature without reinvesting. We address this issue shortly.

4.3 Parallel Trends and Macro Effects

4.3.1 Parallel Trends

One concern about inferences from studies using the treatment-effects framework is whether the data processes generating the treatment and control group outcomes followed "common or parallel trends" prior to the treatment. Differences in the post-treatment period can only be ascribed to the treatment when this assumption holds. The outcome variable of our study is the within-firm change in investment spending. Recall that our matching procedure rendered treatment and control matches with very similar investment going back three quarters prior to the crisis (see Tables 1 and 2). Nonetheless, even though quarterly investment levels might be similar for the two groups of firms for about a year prior to 2008, those firms' investments could be following different long-term trends in the period leading up to the crisis. The best way to address this concern is to look at data associated with the outcome variable (changes in investment) going farther back in time.

Table 6 reports the mean and median quarterly change in investment for firms in the treatment and control groups going back up to ten years prior to the fourth quarter of 2007. The first row in the table reports statistics for changes in investment going back two years prior to the 2007 crisis quarter (quarterly investment changes from 2005Q3 through 2007Q3). Similar

Time Horizon	Treatment Mean [Median]	Control Mean [Median]	*P*-Value of Difference *t*-test [Pearson χ^2]
	(in percentage points)		
2 years prior to 2007Q4	−0.11 [0.07]	−0.42 [0.05]	0.60 [0.99]
3 years prior to 2007Q4	−0.20 [0.03]	−0.16 [0.10]	0.93 [0.47]
4 years prior to 2007Q4	−0.07 [0.05]	−0.10 [0.11]	0.94 [0.55]
5 years prior to 2007Q4	−0.19 [0.04]	−0.06 [0.11]	0.70 [0.45]
10 years prior to 2007Q4	−0.21 [0.03]	−0.18 [0.03]	0.89 [0.92]

Table 6. Trends in investment for treated and control firms: Mean and median comparisons.

This table reports the mean and median quarterly change in investment for firms in the treatment and control groups going back many years prior to the fourth quarter of 2007. The first row in the table reports statistics for changes in investment going back two years prior to the crisis (quarterly investment changes from 2005Q3 through 2007Q3 normalized by the firm's capital stock). A similar calculation is reported in the second row of the table, but the data goes back three years prior to the 2007 crisis quarter (starting in 2004Q3). Subsequent rows go back farther in time at larger increments. The table also reports *p*-values associated with test statistics for differences in means (standard *t*-test) and in medians (Pearson's χ^2) across groups.

Interpretation: This table shows that our outcome variable (within-firm change in investment) follows common trends in years prior to the treatment period for treated and control firms.

statistics are reported in the second row of the table, where the data go back three years (2004Q3 through 2007Q3). Subsequent rows go back farther in time. The table also reports *p*-values associated with test statistics for differences in means (standard *t*-test) and in medians (Pearson's χ^2) across groups.

It is apparent from the estimates reported in Table 6 that our experiment's outcome variable was indistinguishable across treatment and control

firms going back as far as ten years prior to the fourth quarter of 2007 (see *p*-values for *t*-tests and Pearson-tests). It is difficult to make the case that the investment processes of firms in those two groups were following very different trends before the credit shock.

4.3.2 Macro effects

Another potential concern regarding our difference-in-differences approach is whether other "macro effects" affecting both treatment and control firms might explain the differential behavior we observe in the post-treatment period (irrespective of any effects arising from differences in debt-maturity composition). This concern is valid when one has reasons to believe that there are important, latent differences between treatment and control firms and that these differences trigger sharp treatment–control contrasts in the post-treatment period because of other changes in the environment.

As in previous papers examining the consequences of a credit crisis, our post-treatment period encompasses a recession, a time when corporate demand for investment generally declines. The advantage of our strategy over other comparable studies is that it does not rely on firm policies (e.g., leverage, size, or cash holdings) that are inherently linked to factors that can drive differential behavior over the business cycle. For instance, it would not be surprising to see high-leverage/low-cash firms performing particularly poorly during the recession that followed the 2007 crisis if confounding heterogeneity in firm quality (related to profitability, risk, access to capital, etc.) was not properly accounted for. Regarding our strategy, in contrast, it is difficult to articulate an argument for a systematic association between the maturity structure of long-term obligations and firm quality; especially an association that would be more pronounced during the 2007 period. While the existing literature provides no evidence of such links, we design an additional test that speaks to this concern.

To recap, we argue that the combination of a credit supply shock with maturing debt may have pronounced effects on corporate spending. The concern, however, is that the ensuing recession may somehow drive a differential wedge in the post-crisis investment behaviors of treatment and control firms, a difference that could explain our findings. To examine this argument, we look for a period that precedes a recession, but that *lacks* a credit supply shock in order to identify a placebo treatment. In other words,

we eliminate one of the key elements of our treatment strategy (credit shortage), but allow for the same potentially confounding macro effects (demand contraction) that could drive our 2007 findings to see if similar treatment–control contrasts emerge. If they do emerge, then there is reason to believe that developments in the general environment that followed our proposed treatment — and not the treatment itself — may explain our results.

Given the data requirements of our matching strategy, we focus on the 2001 recession.[25] It is easy to show that the credit conditions that accompanied the 2001 recession were very different from the credit crisis that started in 2007. Consider, for example, the figures that we analyzed in Section 2.1.1 . At the onset of the crisis (February 2001), 3-month LIBOR and commercial paper spreads were at 0.4% and 0.3%, respectively. These spreads *declined* during 2001, to levels close to 0.1% (LIBOR) and 0.1% (commercial paper) in December 2001. There is also no evidence of increases in credit spreads during 2001. Investment-grade and junk bond spreads were 1.9% and 8.2%, respectively, at the onset of the recession (February 2001). They remained close to these levels during 2001, ending the year at 1.8% (investment-grade) and 8.0% (junk). The evidence we gather suggests that the 2001 recession was not accompanied by a credit supply shock of significant magnitude.

We replicate our baseline experiment for the 2001 recession *as if* there was a pronounced credit supply shock at the beginning of that recession. To be precise, we take that the treatment period is the first quarter of 2001 (as opposed to the fourth quarter of 2007). Analogously, the pre-treatment and post-treatment periods are, respectively, the last three quarters of 2000 and the last three quarters of 2001. If our prior results simply reflected the differential response of treatment and control groups to a recession (regardless of the credit contraction), we should see similarly strong treatment–control contrasts in these new tests. However, this is not what we find. The simple difference-in-differences estimator for investment outcomes in the 2001 recession yields a *positive*, statistically insignificant value of 1.2 percentage points (compared to minus 2.2 points in the 2007 baseline). Similarly, the Abadie-Imbens ATT estimate for this test is a positive 1.4 percentage points (compared to minus 2.5 points for 2007).

[25] Information on debt maturity from COMSPUSTAT for the 1980's and 1990's recessions is very sparse.

Our post-treatment–recession check makes it difficult to argue that effects that are associated with recessions — and not a credit supply shortage — might explain the results of our tests.

4.4 Robustness of Treatment Assignments

To test whether refinancing frictions have real implications, our benchmark estimation assigns to the treatment group those firms whose long-term debt due in 2008 is greater than 20% of total long-term debt. The benchmark case also focuses on firms for which the ratio of long-term debt maturing in more than one year to total assets was higher than 5%. These are arbitrary choices that we make for the purpose of operationalizing our test. It is important that we verify what happens when we alter these criteria.

4.4.1 Changing the due-to-total long-term debt cutoff

We first experiment with changes in the ratio of due-to-total long-term debt. The test we design is such that, all else the same, this cutoff captures the importance of the financing shortfall caused by the maturing debt. One would expect the impact of the maturing debt to be smaller if firms had smaller proportions of their debt coming due in 2008, and larger if firms had larger proportions of their debt maturing at that time. In the logic of the treatment-effect framework, this is akin to expecting smaller (larger) effects to be associated with smaller (larger) doses of the treatment. Accordingly, we examine what happens to our central results as we experiment with alternative cutoffs of the due-to-total long-term debt ratio. We do so by focusing on cutoffs that are located in the neighborhood of the benchmark case.

The results of this experiment are presented in Table 7. In the first column, we report the changes in investment that obtain when we experiment with a 15% cutoff for the ratio of long-term debt due in 2008 to total long-term debt. As should be expected, the differences between treatment and control groups become smaller after we allow into the treatment group firms with lower proportion of debt maturing in 2008 (the treatment group size increases to 129). The simple difference-in-differences estimate is minus 1.5 percentage points of total capital, while the ATT is minus 1.3 points (both only marginally statistically significant). This contrasts with our benchmark

	Long-Term Debt Due in 2008 > 15%	Long-Term Debt Due in 2008 > 20%	Long-Term Debt Due in 2008 > 25%
Change in Investment for Treated Firms	−1.72***	−2.13**	−2.81**
	(0.62)	(0.84)	(1.14)
Change in Investment for Control Firms	−0.27	0.09	0.56
	(0.56)	(0.71)	(0.91)
Difference	−1.45*	−2.21**	−3.37**
	(0.76)	(1.01)	(1.34)
Matching Estimator (ATT)	−1.34*	−2.46**	−3.71***
	(0.76)	(1.07)	(1.40)
Firms in Treatment Group	129	86	62

***, **, * indicate significance at the 1, 5, and 10% levels, respectively.

Table 7. Difference-in-differences of firm investment before and after the fall 2007 credit crisis: Different cutoffs for the ratio of long-term debt due in 2008 to total long-term debt.

This table presents the change in the average quarterly investment-to-capital ratio from the first three quarters of 2007 to the first three quarters of 2008 for alternative treatment-assignment cutoffs for the proportion of long-term debt due in 2008 to total long-term debt: (1) more than 15%, (2) more than 20%, and (3) more than 25%. The benchmark case result (from Panel B of Table 3) is presented in the middle column for ease of comparison. Control firms are the closest matches to the treated firms based on a set of firm characteristics (see the description in Table 3 for details). ATT is the Abadie-Imbens bias-corrected average treated effect matching estimator (Matching Estimator). Heteroskedasticity-consistent standard errors are in parentheses.

Interpretation: This table shows that the fall in investment for treated firms relative to control firms becomes more pronounced as we increase the fraction of long-term debt that is scheduled to mature in 2008.

result (20% cutoff), which is reported in the second column of the table. In the third column of the table, the test only includes firms whose proportion of long-term debt due in 2008 is higher than 25% of their long-term debt. The test now focuses on 62 firms with very large portions of debt coming due in the crisis. Consistent with our priors, the fall in investment for treated firms relative to control firms becomes more pronounced, equal to minus 3.7% of the capital base (significant at the 1% test level).

4.4.2 Changing the long-term leverage cutoff

Long-term debt maturity should matter only for firms that have significant amounts of long-term debt in their capital structures. According to the logic of our strategy, increasing the cutoff for the fraction of long-term debt in firms' capital structures should result in larger post-crisis effects of maturity on investment. By the same token, including firms that do not have significant long-term debt should weaken the estimated effects.

Table 8 shows evidence that is consistent with this hypothesis. In the first column, we report the changes in investment that obtain when we allow into the sample those firms whose long-term debt maturing in more than one year is less than 5% of assets (i.e., we eliminate the 5% debt-to-asset cut-off). Consistent with expectations, the estimated differences between treatment and control groups disappear after this change. The simple difference-in-differences estimate is 0.0%, while the ATT is now positive at 0.2% (both are statistically insignificant). This contrasts with the 5% benchmark case, which is displayed in the second column of the table. In the third column, the test only includes firms whose long-term debt maturing in more than one year is greater than 10% of assets. Now, the fall in investment for treated firms is much deeper, equal to −3.4% of capital (significant at 5% level).

In all, the evidence in Tables 7 and 8 helps substantiate the hypothesis that treated firms found it difficult to refinance their maturing long-term debt during the crisis period, cutting their investment as a result. As we increase (relax) the severity of their circumstances ("treatment dosage"), the effects we measure increase (diminish).[26]

4.5 Alternative Specifications for the Matching Estimator

In addition to the checks described above, we have experimented with several variations in our procedure to construct treatment and control groups,

[26] Another sample selection screen we apply that is related to the firm's debt structure concerns a firm's use of short-term debt. In our benchmark tests, we eliminate firms for which short-term liabilities such as notes payables, bank overdrafts, and loans payable to officers and stockholders are greater than 1% of total assets. This debt-related restriction is meant to help assure that we are contrasting firms of seemingly comparable debt profile and with the demonstrated quality/ability to have substantial debt due beyond one year on the balance sheet. We find that this filter is important to our empirical design in that our benchmark result goes away once we include firms with substantial short-term debt (matching estimator = −0.97 with a p-value of 0.28). This is not surprising, as we discuss in the paper, given that firms with significant short-term debt could be of significantly different quality from the other firms in our sample, thus biasing the empirical design.

	Long-Term Leverage > 0%	Long-Term Leverage > 5%	Long-Term Leverage > 10%
Change in Investment for Treated Firms	−1.09*	−2.13**	−2.72**
	(0.62)	(0.84)	(1.18)
Change in Investment for Control Firms	−1.09*	0.09	−0.54
	(0.49)	(0.71)	(1.02)
Difference	−0.01	−2.21**	−2.19
	(0.73)	(1.01)	(1.49)
Matching Estimator (ATT)	0.23	−2.46**	−3.38**
	(0.78)	(1.07)	(1.33)
Firms in Treatment Group	236	86	64

***, **, * indicate significance at the 1, 5, and 10% levels, respectively.

Interpretation: This table shows that the fall in investment for treated firms relative to control firms becomes more pronounced as we increase firms' long-term leverage ratios.

Table 8. Difference-in-differences of firm investment before and after the fall 2007 credit crisis: different cutoffs for the long-term leverage ratio.

This table presents the change in the average quarterly investment-to-capital ratio from the first three quarters of 2007 to the first three quarters of 2008 for alternative cutoffs for the ratio of debt due in more than one year to total assets: (1) more than 0%, (2) more than 5%, and (3) more than 10%. The benchmark case result (from Panel B of Table 3) is presented in the middle column for ease of comparison. Control firms are the closest matches to the treated firms based on a set of firm characteristics (see the description in Table 3 for details). ATT is the Abadie-Imbens bias-corrected average treated effect matching estimator (Matching Estimator). Heteroskedasticity-consistent standard errors are in parentheses.

as well as in the set of matching covariates. To illustrate the robustness of our results, we discuss two of these exercises in this section.

Our benchmark specification defines the treatment group as all firms for which the ratio of long-term debt maturing within one year to total long-term debt is greater than 20%. The non-treated group contains all the other firms that satisfy the sampling restrictions (in particular, a minimum level of long-term debt over assets). As an alternative approach, we considered a control group that includes *only* firms that have more than 20% of their long-term debt maturing in *exactly* five years (that is, in 2012). These firms are similar to those in the treatment group in that they also allow their maturity structures to be poorly diversified across years ("poor-debt-maturity management"). However, they happen to have concentrated their

maturity in a time period that lies far in the future.[27] The estimated difference in investment changes (the matching estimator ATT) remains negative, equal to minus 1.6 percentage points, and statistically significant (standard error of 0.9) after this change in definition.

We have also experimented with including the 2007 investment level among the set of matching covariates to ensure that we are comparing firms that were at the same starting point of investment before the crisis. The matching estimator's average treatment effect is virtually unchanged after this modification in the set of covariates; an estimate of −2.3 percentage points, with a standard error of 0.9.

4.6 Standard Regression Tests

While the nonparametric matching approach is well-suited for our test strategy, it is useful to show that our results also hold when we use a standard regression approach. To do this, we regress investment on a dummy variable that takes the value of one if the ratio of long-term debt maturing in 2008 to total long-term debt is greater than 20%, and zero otherwise. For all specifications, we also perform placebo regressions that focus on changes in investment during non-crisis periods (the years between 2001 and 2007). We also run a pooled 2001–2008 OLS regression to estimate differences between the crisis and non-crisis periods.

As shown in Panel A of Table 3, without including controls in the OLS regression, firms with over 20% of their long-term debt maturing in 2008 cut their investment by 1.6% of capital more than other firms. That group-mean difference estimate is significant at the 10% test level. Over the pre-crisis period (2001–2007), when we would expect a firm's debt maturity to have no effect on investment decisions, the difference across the two groups is essentially zero (point estimate of −0.1%). The difference in the two estimates (financial crisis effect less pre-financial crisis effect) is a fall in investment of 1.5% of capital (significant at the 10% test level). We also estimate investment regressions including all of the controls used in Panel B of Table 3 (size, industry, credit ratings, *Q*, long-term leverage ratio, cash flows, and cash holdings). While these firm controls predict changes in investment in their own right, their inclusion does not materially alter

[27] We choose five years because this is the farthest one-year information that is available in COMPUSTAT.

the coefficient on debt maturity.[28] The estimated group-mean difference changes slightly to 1.7 percentage points (significant at the 5% level) after we add those controls. Over the pre-crisis period, this difference is again essentially zero (point estimate of 0.0%), yielding a difference in the two estimates (financial crisis less pre-financial crisis) of a fall in investment of 1.7 percentage points (significant at the 5% level). These results are fully consistent with those reported under the matching estimator approach.[29]

4.7 Further Analysis of Treated Firms' Finances

One potential concern is that treated firms' long-term debt maturity could be comparatively more concentrated, and that could be correlated with characteristics that explain their performance during the crisis (for example, poor financial management). To examine this issue, we consider the Herfindahl index (HHI) measure of debt concentration that we discuss in Section 2.1.2 . We find that the HHI of long-term debt for treated, control, and non-treated firms are, respectively, 0.38, 0.37, and 0.34. These numbers are economically and statistically identical, suggesting that these groups of firms historically issued debt at the same frequency (roughly once every three years), but the treated firms were just unfortunate enough to have a good share of their debt coming due right after August 2007.

Finally, we make use of *ex-post* data to check our hypothesis about a freeze in the market for long-term debt during the crisis (refinancing constraints). To do so, we look at the debt issuance activity of our treated firms, calculating the ratio of long-term debt issuance in 2008 to the long-term debt that was due in 2008. Corroborating our hypothesis, we find that the mean (median) issuance-to-maturing debt ratio is only 12.6% (0.0%) in 2008. Our study traces the impact of this abrupt external financing shortfall on firms' outcomes.

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[28] We discuss the results for regression models that use the same controls of the matching estimations for consistency. However, a number of other OLS specifications (with added controls, such as including changes in the right-hand side variables and lagged changes in investment) lead to very similar results.

[29] Greenwood (2003) also runs regressions of investment on debt maturity variables and shows that investment is more sensitive to interest rates for firms with high levels of short-term debt. His results are consistent with ours, though they differ in the focus on interest rates (rather than the financial crisis) and on short-term debt (rather than the maturity structure of long-term debt).

5 Extensions

In this section, we extend our base analysis to other dimensions of firm policy-making and also consider the broader and longer-term consequences of the refinancing constraint status we used in our tests. We start with a "back-of-the-envelope" calculation that shows how firms with ballooning debt payments in 2008 responded to the credit crisis along other dimensions besides investment policy.

5.1 What Else Did Firms Do?

The evidence thus far suggests that firms with large amounts of debt maturing in 2008 were forced to cut investment in order to be able to repay their maturing debt. However, investment is not the only policy variable that these firms could have adjusted in the aftermath of the crisis. Here, we examine post-crisis changes in other policies that the treated firms could have used to absorb the effect of the credit squeeze. Even if it was difficult or impossible for firms to respond to the crisis by issuing additional external finance, they could potentially make up for the debt payment by adjusting other variables, such as drawing down cash reserves, reducing stocks of inventory, repurchasing fewer shares, or cutting dividends. If the treated firms found it necessary to cut investment (which is a costly measure), one would also expect them to adjust, for example, the amount of share repurchase activities that they undertake in the aftermath of the crisis.[30] In addition, one could expect firms to draw down on their cash balances and reduce inventories. The literature suggests that cash balances are held in part to hedge against negative shocks such as the 2007 crisis (see Almeida *et al.*, 2004). Moreover, there is evidence that firms use inventories to smooth out the effects of fluctuations in the availability of internal funds (Fazzari and Petersen, 1993).

To provide some evidence on these additional policies, we perform a simple "back-of-the-envelope" analysis of how the firms in our experiment responded to the credit crisis. Across our treated firms, we calculated the average amount of long-term debt due in 2008, as well as the amount of "cuts" conducted elsewhere to help pay off this debt (besides investment

[30] The survey evidence in Brav *et al.* (2005) suggests that share repurchases are the residual after the investment and dividend decisions have been made.

reductions) — inventories, share repurchases, dividends, and cash holdings. These variables were present for 77 of our 86 treated firms.

For this sample of 77 firms, we compute the average changes in all of the policy variables above, between the first three quarters of 2007 and the first three quarters of 2008. For our two stock variables (cash holdings and inventories), we just take the differences in the average value of their levels in the first three quarters of 2008 relative to the first three quarters of 2007. For the quarterly flow variables (investment, share repurchases, and dividends), we convert the differences in the average quarterly flow to an annual flow basis for ease of comparison with the stock variables. For example, the quarterly reduction of investment (normalized by capital) of 2.1 percentage points for the treated firms reported in the first row of Panel B of Table 3 represents an annual decline of 8.4% percentage points. To facilitate comparisons with our estimate of the fall in investment, we normalize all other variables by the value of the capital stock as well. We then take averages across all 77 of our treated firms to see how much they drew down their cash reserves, cut dividends, etc. We finally compare these figures with the average amount of debt they had coming due in 2008.

Figure 4 provides a visual illustration of the treated firms' responses to the credit crisis. The figure displays the average changes in various corporate policy variables as a fraction of the total amount of long-term debt maturing in 2008. The decline in investment spending in 2008 represents about one-eighth of the amount of long-term debt these firms had coming due in 2008. By comparison, the treated firms drew down from their cash reserves amounts that represent about two-fifths of the amount of debt due in 2008. These firms reduced share repurchases (relative to 2007 levels) by an amount representing about one-tenth of the debt due. And reductions in their inventories accounted for another 7% of the 2008-maturing debt. Given executives' strong aversion to cutting dividends (see Brav *et al.*, 2005), it is perhaps not surprising that dividend cuts during 2008 accounted for only 1% of the amount of debt due for the treated firms. The remaining 29% is explained by other factors (such as reductions in R&D, labor costs, asset sales, and perhaps limited issuance of securities).[31]

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[31] Campello *et al.* (2010) survey 574 U.S. CFOs at the end of 2008, asking managers about the measures they adopt to cope with the credit crisis. The managers in their survey report cuts of 11% in their firms' R&D expenditures and another 4% in their work force. Moreover, nearly 50% of the CFOs surveyed say that they sold assets in 2008 to cope with the credit squeeze.

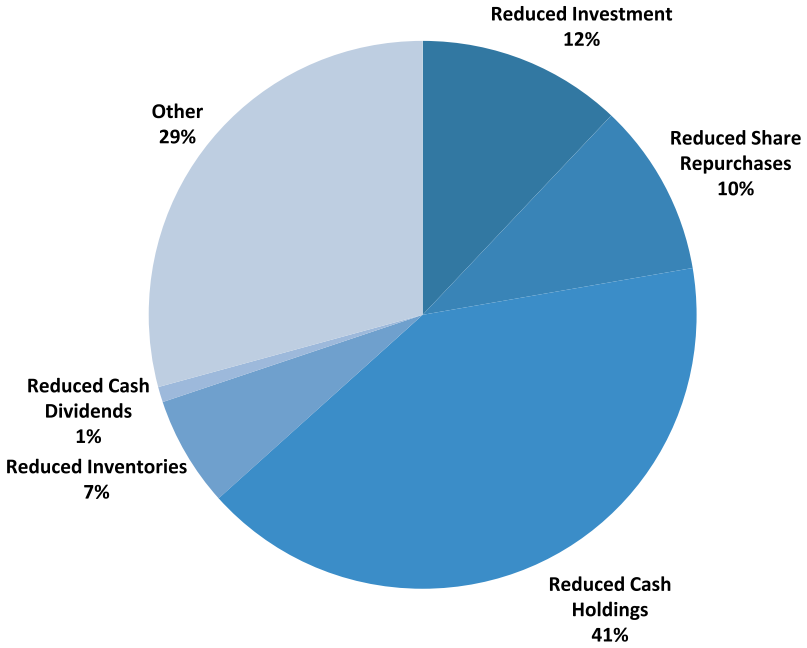

Figure 4. How did treated firms pay off their debt?

This figure displays the average changes in policy variables from the first three quarters of 2007 to the first three quarters of 2008, as a fraction of the amount of long-term debt maturing in 2008, for the sample of 77 treated firms for which we have complete data on investment, cash holdings, cash dividends, inventories, and share repurchases. Treated firms are those that have more than 20% of their long-term debt maturing in 2008.

Interpretation: This figure shows that besides cutting investments, treated firms also reduced cash holdings, inventories, and share repurchases (among other variables) to absorb the joint effect of the credit squeeze and maturing debt.

While admittedly done solely for purposes of providing a crude approximation for how the treated firms responded to the financial crisis, the numbers depicted in Figure 4 fit our economic intuition very well. In particular, the figure suggests that firms that were burdened with large amounts of maturing debt in 2008 drew heavily on their least costly sources of funds (such as cash holdings) in order to mitigate the effects of maturing debt, but had to ultimately cut back on real activities such as investment spending.

5.2 Longer-Term Consequences of Refinancing Constraints

Our empirical analysis evaluates the consequences of the August 2007 panic over the period that immediately follows that event (the first three quarters

of 2008). We do so according to the design of our identification strategy. However, it is natural to wonder if the financing effects we identified had long-term impacts on firm welfare. While a complete analysis of the long-term consequences of debt maturity structure is beyond the scope of our paper, we investigate the longer-term implications of our experiment's "treatment" on firm welfare through June 2010 (the last available data point). For simplicity, we do so using graphical analysis.

Our baseline investigation revolves around investment spending. While Table 3 describes the investment of treated and control firms over the first three quarters of 2008, we now examine how those firms invested beyond that time. Panel A of Figure 6 depicts the longer-term investment effects of our 2008-maturing debt treatment over the period from the first quarter of 2008 ("Mar-08") to the second quarter of 2010 ("Jun-10"). Recall that before the financial crisis, the treated firms and their matched counterparts had very similar (and statistically indistinguishable) cash flow, cash holdings, size, leverage and *Q*. They also were investing at the same rate in 2007, as shown in Panel B of Table 2. Figure 6 shows that by the end of 2008 (following Lehman's failure) investment-to-capital ratios dropped across *all* firms, but more so for the treated firms that had a lot of debt maturing in 2008.[32] By the summer of 2009, both firm-types were investing at similar levels. After hitting a bottom (at roughly 50% of pre-crisis levels), investment starts to grow again in 2010. The patterns in the graph suggest that debt maturity structure created an investment wedge between firms during the initial phases of the crisis. As the crisis deepened, however, that wedge softened, as all firms in the economy implemented drastic spending cuts. As conditions improved, the investment rate of the treated firms recovered more rapidly (perhaps to make up for the fall in investment relative to their less-constrained peers in 2008).

We also look at firm operating performance during the 2008–2010 period. The time-series of the ratio of operating income to total assets is depicted in Panel B of Figure 5. Following Lehman's failure, all firms observe drastic declines in operating income. Since then, both treatment and control firms show improvements in their performance. However, the firms affected by a refinancing constraint in 2008 (the treated firms) report lower operating performance in all but one of the ten quarters following the 2007 credit

[32] As discussed above, our baseline tests avoided data from the fourth quarter of 2008 to sidestep the effects of the Lehman debacle and the deepening of the aggregate recession.

event. Before this shock, their cash flow was the same. This deterioration in the operating performance of the treated firms following the August 2007 panic is consistent with the fall in stock prices documented in Table 4. As shown by Campello *et al.* (2011), declines in operating performance also made it difficult for firms to get access to bank financing (in the form of credit lines) in 2008 and 2009. Firms that faced difficulties in initiating or renewing credit line facilities, in turn, invested less.

To sum up, the debt refinancing constraints that we use to identify our tests seem to have significant effects on firm investment and operating performance in the first three quarters of 2008 (our treatment window). As

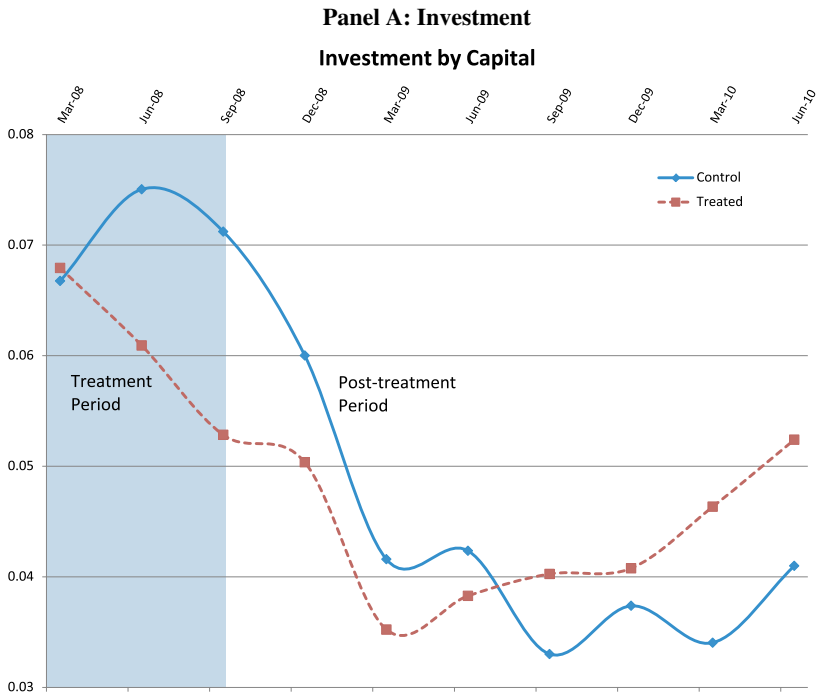

Figure 5. Longer-term consequences of refinancing constraints.

These figures display the time-series evolution of investment spending and operating income for treated and control firms over the 2008:Q1 to 2010:Q2 window (i.e., "Mar-08" to "Jun-10"). Investment is the quarterly ratio of capital expenditures to the capital stock. Operating income is the quarterly ratio of operating income to assets.

Interpretation: These figures show that debt refinancing constraints had a significant effect on investment and operating performance during the first three quarters of 2008. As the financial crisis deepened, firms behave similarly irrespective of debt refinancing constraints.

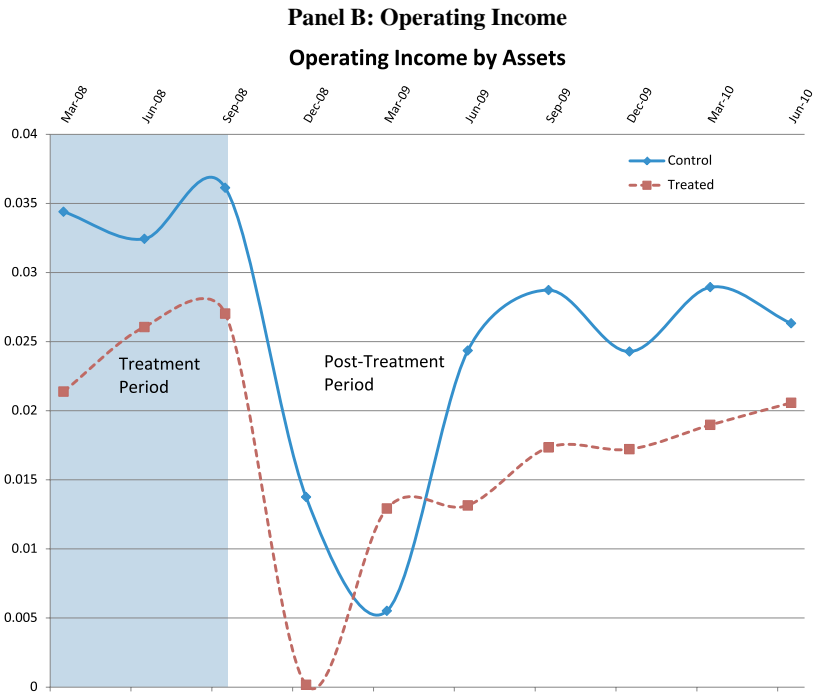

Figure 5. (*Continued*)

the financial crisis deepened, however, all firms in our sample seem to be engulfed by the adverse circumstances of the aggregate economy. By early 2009, treated and control firms seem to have similar behaviors (just as they had in August 2007, when we defined their "pairings") and appear to respond roughly similarly to developments in the economy (but with some identifiable differences). The extension of this section provides additional economic context to the connections between financial contracting and real outcomes that our tests identify.

5.3 Economy-Wide Consequences of Refinancing Constraints

While it is difficult to extrapolate our findings, the evidence we present allows us to compute an estimate of the fraction of investment cuts in 2008 that was due to the 2007 freeze-up. In particular, it allows us to gauge the fraction of corporate investment contraction that is due to debt maturity constraints for firms that rely on long-term debt financing. Using the

estimates in Panel A of Table 3, the average decline in quarterly investment in 2008 is equal to 0.69% of capital (this figure corresponds to the sample-weighted average percentage decline across treated and non-treated firms). The estimates in Panel B then tell us the decline in investment that is due to maturity-related refinancing constraints. For example, the matching estimator's ATT is −2.46%. Using this figure, and noting that there are 86 treated firms in our sample out of a total of 1,067 firms, we can calculate the fraction of the average decline in investment that is due to refinancing constraints. To wit, this fraction is equal to $(86/1,067) \times (-2.46\%)/(0.69\%)$, or 29% of the observed investment cuts. Thus, one can argue that the panic of 2007 had a measurable impact on the real economy.

6 Concluding Remarks

We use the August 2007 credit panic to assess the effect of financial contracting on real corporate policies. In particular, we test whether firms with large fractions of their long-term debt maturing at the time of the crisis observe more pronounced negative outcomes than otherwise similar firms whose debt structure is such that they did not need to refinance a lot of debt during the crisis. Our empirical methodology aims at replicating an experiment-like test in which we control for observed and time-invariant unobserved firm heterogeneity via a difference-in-differences matching estimator.

We find evidence that the terms of long-term financial contracting can have significant implications for firms' real and financial policies when they face a credit shock. Firms whose long-term debt was largely maturing right after the third quarter of 2007 cut their quarterly investment rates by 2.5 percentage points more than otherwise similar firms whose debt was due well after the crisis. This relative decrease in investment for firms with maturity "spikes" during the crisis is statistically significant and economically large (approximately one-third of the pre-crisis level of investment for these firms). A number of falsification and placebo tests confirm our inferences about the effect of credit supply shocks on corporate policies.

Our results contribute to the literature in a number of ways. First, our identification strategy shows a novel link between debt maturity and corporate investment. In particular, our results point to the importance of

maturity structure for corporate financial flexibility. As a matter of corporate policy, our study highlights the extra attention firm managers should pay to the maturity profile of their firms' debt. Second, our results provide evidence that the 2007 credit crisis had significant real effects on corporate behavior in 2008. Third, our evidence suggests that debt maturity structure is an important variable in understanding how credit supply shocks spread through the corporate sector — beyond what one can learn by looking at firms' leverage ratios. Understanding the effects of credit cycles (and credit crises in particular) is not only of interest for corporate finance researchers, but also important for economic policymakers. More broadly, our findings provide new evidence that financial contracting has causal effects on real corporate outcomes. Our study characterizes one precise channel (a contracting feature) that shows *how* financing affects investment.

Acknowledgements

Our paper benefited from comments from an anonymous referee, Joseph Fan, Daniel Ferreira, Miguel Ferreira, Mark Flannery, Fritz Foley, Laurent Fresnard, Robin Greenwood, Andrew Karolyi, Josh Pollett, Michael Roberts, Philipp Schnabl, Wei-Ling Song, and Ivo Welch (the editor). We also thank seminar participants at the AFA Meetings (2010), China International Conference in Finance (2010), Cornell University, Dartmouth College, ESMT-Berlin, FIRS Conference (2010), Georgia Tech, Georgetown University, Goethe University, HEC-Paris, International Monetary Fund, LUBRAFIN Conference (2010), Napa Conference on Financial Markets Research (2010), City University of Hong Kong Conference on Corporate Finance (2009), Harvard Business School, Hong Kong University of Science and Technology, Korea University, Korea America Finance Association, Nanyang Technological University, Singapore Management University, Rice University, Rising Stars Conference (2010), Simon Fraser University, Southern Methodist University, Temple University, UCLA, University of Alberta, University of British Columbia, University of Illinois, University of Miami, University of Michigan, University of Virginia-Darden, World Bank, and the Yale/RFS Financial Crisis Conference (2009). Igor Cunha, Jaehoon Lee, and Quoc Nguyen provided excellent research assistance.

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Appendix

Number of firms with fiscal year 2007	10178
Drop firms with:	
SIC 8000s & 9000s	−437
SIC 6000s	−3147
ADR	−416
Double Data Y, Q, M	−2
ATQ <10	−734
Growth Assets > 100% in a quarter	−87
Missing sales number	−336
Negative sales	−3
Growth of Sales > 100% in a quarter	−20
Cash is greater than Assets	−1
PPE is greater than Assets	−1
Missing DD1 or Missing DLTT	−60
Notes Payable over Assets >= 10%	−1080
DD1 + DLTT > Assets	−63
DLTT < DD2 + DD3 + DD4 + DD5	−382
Fiscal year end months 2, 3, 4, 5, 6, 7, 8	−598
DLTT over Assets <= 5%	−1197
Missing Outcome and Control variables	−547
Number of firms after all screens	1067

Table A1. Sample Selection Screens.

Appendix Table A.1 summarizes the sample selection screens that we applied to COMPUSTAT data to arrive at our final sample of 1,067 firms in 2007, starting from a sample of 10,178 firms with data available for fiscal year 2007.