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

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Abstract

We use the 2007 credit crisis to gauge the effect of financial contracting on real corporate behavior. We identify heterogeneity in financial contracting at the onset of the crisis by exploiting ex-ante variation in long-term debt maturity. Our empirical methodology accounts for observed and unobserved time-invariant firm characteristics by employing a difference-in-differences matching estimator. We find that firms whose long-term debt was largely maturing right after the third quarter of 2007 reduced investment by 2.5% more (on a quarterly basis) than otherwise similar firms whose debt was scheduled to mature well after 2008. This relative decline in investment is statistically and economically significant, representing one-third of pre-crisis investment levels. A number of falsification and placebo tests confirm our inferences about the effect of credit supply shocks on corporate policies. For example, in the absence of a credit shock ("normal times"), the maturity composition of long-term debt has no effect on investment. Likewise, maturity composition has no impact on investment in the crisis for firms for which long-term debt is not a major source of funding. Our study highlights the importance of debt maturity for corporate financial policy. It shows *how* financial contracting ties credit supply shocks and firm real decisions.

Key words: Financial crisis, debt maturity, matching estimators, investment spending, financing constraints

JEL classification: G31

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Abstract

We use the 2007 credit crisis to gauge the effect of financial contracting on real corporate behavior. We identify heterogeneity in financial contracting at the onset of the crisis by exploiting ex-ante variation in long-term debt maturity. Our empirical methodology accounts for observed and unobserved time-invariant firm characteristics by employing a difference-in-differences matching estimator. We find that firms whose long-term debt was largely maturing right after the third quarter of 2007 reduced investment by 2.5% more (on a quarterly basis) than otherwise similar firms whose debt was scheduled to mature well after 2008. This relative decline in investment is statistically and economically significant, representing one-third of pre-crisis investment levels. A number of falsification and placebo tests confirm our inferences about the effect of credit supply shocks on corporate policies. For example, in the absence of a credit shock ("normal times"), the maturity composition of long-term debt has no effect on investment. Likewise, maturity composition has no impact on investment in the crisis for firms for which long-term debt is not a major source of funding. Our study highlights the importance of debt maturity for corporate financial policy. It shows *how* financial contracting ties credit supply shocks and firm real decisions.

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1 Introduction

Does financial contracting have real implications? How do firms respond to shifts in the supply of credit? The endogeneity of financing and investment decisions makes it difficult to answer these questions. To complicate matters, credit supply shocks often confound financial and economic factors that affect firm behavior. One common-place approach to studying the effect of credit shocks on firm behavior is to look at financing activity (e.g., loans or equity issues) that takes place over the credit cycle. Unfortunately, this approach is compromised by the fact that observed transactions may reflect a shift in the supply of credit (e.g., lower supply of loans in a monetary contraction) as well as a shift in the demand for credit (firms demand less loans if economic conditions adversely affect their investment plans). Likewise, it is difficult to identify a causal link going from firm financing to firm investment during a credit contraction because unobserved economic factors (e.g., firm business fundamentals) may drive both ex-ante financial contracting and ex-post real outcomes.

We develop a novel strategy to gauge the effect of financial contracting on real corporate outcomes following a shift in the supply of credit. We do so using the credit crisis (or "panic") of 2007. This event is unique among other credit shortage episodes in that it originated from problems arising from non-corporate assets: housing mortgages. Gorton (2008) provides a detailed analysis of the various forces leading to a sharp reduction in liquidity that affected financial institutions dealing with subprime-based derivatives starting in mid-2007. The lack of transparency on long-term investments of financial institutions, and the possibility that losses on credit derivatives would be passed on to their balance sheets created a panic that effectively shut down short-term financing to banks and other institutions (Acharya et al. (2009)). As we document below, the crisis quickly spilled over into the market for long-term corporate debt, resulting in sharp increases in bond spreads in the Fall of 2007.

The 2007 episode arguably provides for a shock to the supply of external financing that is not caused by the weakening of firm business fundamentals. Naturally, businesses are eventually affected by credit shortages, ultimately changing their demand for credit. These confounding effects make it important that we identify a one-sided shock, when it emerges. We note, however, that simply exploring an event of this type is insufficient to identify a causal link between financial contracting and corporate outcomes. In particular, while general credit conditions may exacerbate the relation between variables such as financial leverage and investment, one cannot ascertain whether financial contracting *causes* firms to behave in a particular way. To establish that channel, one needs to identity a feature of financial contracts whose variation can be considered exogenous *at the time* of the credit shock.

We identify heterogeneity in financial contracting at the onset of the 2007 crisis by exploiting ex-ante variation in long-term debt maturity. In a nutshell, we examine whether firms with large fractions of their long-term debt maturing at the time of the crisis are forced to adjust their behavior (e.g., by cutting capital expenditures) in ways that are more pronounced than otherwise similar firms that need not refinance their long-term obligations during the crisis. To the extent that these effects are large, they constitute true costs of financial distress and provide evidence that the terms of financial contracting — in this case, contract maturity — can affect real corporate outcomes.

It is important that we discuss how our focus on long-term debt maturity works as an identification tool. The literature on debt structure has shown that the choice between *short- versus long-term* debt is correlated with firm characteristics such as size, profitability, and credit ratings (see, e.g., Barclay and Smith (1995) and Guedes and Opler (1996)). As such, in general, the determination of debt maturity creates difficulties for the identification of unconfounded causal effects of financial contracting on real outcomes. Rather than contrasting short- and long-term debt, we look at the *proportion of long-term debt* that matures right after August 2007 to assess how firms are affected by credit supply shifts. Note that long-term debt is typically publicly-held and difficult to renegotiate on short notice (see Bolton and Scharfstein (1996)). This makes it hard to argue that firms are at their "debt-maturity targets" at all times. Indeed, an extensive literature discusses how firms seem to deviate for years from their desired debt-to-asset ratios.[1] 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. Because these hard-to-reverse decisions affecting the maturity of a firm's long-term debt were made several years prior to the Fall of 2007, whether the firm was pre-scheduled to refinance a large fraction of its long-term debt *right at the onset of the crisis* is plausibly exogenous to the firm's performance following the crisis. We exploit this friction ("maturity-structure discontinuity") in our analysis, noting that, to our knowledge, none of the papers in the existing empirical literature has studied the implications of the maturity path of long-term debt contracts.

While we argue that cross-firm variation in the proportion of long-term debt that comes due right after August of 2007 is likely to be exogenous to firm outcomes over the crisis, one might wonder if other sources of firm heterogeneity could underlie the relations we might observe. To tackle this concern, we use a difference-in-differences matching estimator approach that incorporates observable firm characteristics and accounts for unobservable, time-invariant firm effects. The goal of our empirical methodology is to replicate an experiment-like setting in which firm financial status can be seen as a "treatment." To minimize concerns about selection, we match firms that we expect to be more susceptible to the negative effects of financial distress (i.e., those firms that happened to have a non-trivial fraction of their long-term debt coming due when the crisis hit) with "control" firms that we do not expect to be susceptible to distress (firms that have most of their long-term debt coming due many years after the shock). We match these two groups of firms on the basis of their asset size, industry classification, credit ratings, Q, long-term leverage ratio, cash flows, and cash holdings. This matching is meant to assure that we are comparing otherwise similar firms, with the only salient difference between the

[1] Starting from Fischer, Heinkel, and Zechner (1989), researchers cite transactions costs arguments as a key reason why firms may not instantaneously adjust their debt ratios (see also Strebulaev (2007)). Alternative explanations include managerial "market timing" (Baker and Wurgler (2002)) and simple inertial (Welch (2004)).

two groups being the composition of their long-term debt maturity. The tests we perform further account for time-invariant firm 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 the shock.[2]

Importantly, we consider a number of alternatives to our basic empirical design. These alternative tests provide checks for the logic of our empirical approach and further minimize concerns about hard-wiring in our results. For example, we perform a battery of falsification tests that replicate our matching estimator 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 distressed-type behavior that can be linked to debt maturity. It is only the juxtaposition of firm debt maturity and a credit crisis 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 firm financing. According to the logic of our strategy, for those firms for which long-term debt is only a small fraction of total financing, we should not see a link between investment spending and the fact that some long-term debt is maturing in the crisis. To further ensure that the assignment of firms into treatment and control groups is exogenous to the post-2007 crisis outcomes, we also perform tests in which we measure maturity structure several years prior to the credit crisis. This allows us to rule out more subtle unobserved heterogeneity stories, such as "smart CEOs" anticipating the August 2007 panic and refinancing (prior to the crisis) the part of their firms' long-term debt that is scheduled to mature in 2008.

Our findings are as follows. We first document a pronounced cross-sectional variation in the maturity structure of long-term debt at the onset of the 2007 credit crisis. Variation in long-term debt maturity is persistent across time, and we find no evidence that it changed in the years leading up to the 2007 crisis. These results are interesting in their own right and suggest that future researchers may use long-term debt maturity structure to gauge a plausibly unconfounded source of heterogeneity in firm financial status. Importantly for our strategy, we are able to isolate firms with a large fraction of long-term debt maturing right after the crisis (the treated firms) that are virtually identical to other firms whose debt happens to mature in later years (the control group). These two groups of firms are similar across all characteristics we consider *except* debt maturity structure. For example, firms in the two groups display similar investment rates in the quarters leading up to the crisis (around 7.5% of capital on a quarterly basis).

We then show that a firm's debt maturity structure has consequences for post-crisis real outcomes.[3] For firms in the treatment group, quarterly investment rates decreased to 5.7% of capital on average

[2] We perform these tests using the Abadie and Imbens (2002) matching estimator. The same estimator has been used by Villalonga (2004), Malmendier and Tate (2009), and Campello, Graham, and Harvey (2009).

[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. The matching variables are averaged over the first three quarters of 2007. The treatment group contains firms for which the fraction of long-term debt maturing within one year (i.e.,during 2008) is greater than 20%, while the control group contains firms for which that fraction is lower than 20%. The baseline experiment focuses on firms whose long-term debt is greater than 5% of assets.

— a fall of 2.1% relative to their pre-crisis level. In contrast, firms in the control group hardly changed their investment. The Abadie-Imbens estimate of the difference-in-differences in investment behavior is −2.5% in our baseline experiment. This drop in investment is economically substantive as it represents a decline of approximately one-third of pre-crisis investment levels. Confirming the logic of our strategy, the relation between maturity structure 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 drop in investment is 3.4%). We also find that the effect of maturity structure on investment is robust to variations in the definitions of treatment and control groups. Moreover, it holds only for the 2007 crisis. In particular, we replicate our experiment over a number of years and find that maturity structure is unrelated with changes in investment for these non-crisis (placebo) periods.

A standard concern about 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 potential concern is whether alternative "macro effects" that differentially affect treatment and control groups might explain the differential behavior 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, which would suggest that these firms would respond very similarly to the recession in the absence of financing frictions. Still, one cannot completely rule out the possibility that there are latent differences between treatment and control groups and that these differences trigger contrasting behaviors in the post-treatment period because of events — other than the treatment — taking place in that period.

We consider both of these concerns in our analysis. First, we explicitly compare the pre-treatment trends in the outcomes (changes in investment) of our treatment and control groups. Going back several years prior to the 2007 shock, we find no evidence that the investment path of firms in those two groups followed different trends. Second, we consider the concern that the recession that followed the 2007 shock may drive a differential wedge in the post-crisis investment of treatment and control firms, irrespective of the observed credit shortage. To deal with this concern, 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 salient "credit component" of our treatment strategy, but allow for the same post-treatment macro effect (demand contraction) that could potentially drive our 2007 results. Although it is difficult to find a recession that is not preceded by a credit tightening, we argue that the 2001 recession was not preceded by a credit shortage that is comparable to the crisis that started in 2007. This falsification test shows no evidence of a differential recession-driven behavior for our treatment and controls firms. That is, one *does need* the pronounced credit component of the 2007 crisis to find that firms with more debt due around a credit shortage invest significantly less afterwards.

Naturally, the large effect of maturing debt on investment in 2008 raises the question of whether firms adjusted along other margins to accommodate the joint effects of the credit crisis and the need to repay a lot of debt in the short run. 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. To provide some evidence on this point, in the last part of the paper, we perform a "back-of-the-envelope" analysis of how the treated firms responded to the crisis. Our calculations suggest that the firms that were burdened with large amounts of maturing debt in 2008 tapped their "least costly" sources of funds. Notably, consistent with Almeida, Campello, and Weisbach (2004), we find that the brunt of the shock to external funding was absorbed by firms' cash balances. Reductions in inventory were also pronounced across treated firms (see Fazzari and Petersen (1993)).

There are only a handful of empirical papers looking at the dispersion of corporate debt maturity (e.g., Barclay and Smith (1995), Stohs and Mauer (1996), and Guedes and Opler (1996)). These papers consider issues other than the effect of supply shocks on real corporate policies. Barclay and Smith report that firms that are large and with fewer growth options have more long-term debt in their capital structures. In addition, Guedes and Opler show that large firms with high-quality credit ratings typically borrow on the short and long ends of the maturity spectrum, while firms with poor credit ratings borrow mid-term. Theory has also looked at the determinants of maturity structure, suggesting that both low- and high-credit quality firms are likely to borrow short-term, but for different reasons (Diamond (1991, 1993) and Flannery (1986)). High-quality credit firms borrow short-term to signal that they are not concerned with the possibility of liquidity shocks, while low-quality firms might have no alternatives to bank debt-financing with restrictive covenants and frequent renegotiations. The existing literature highlights the identification problem that we tackle in this paper. For instance, firms that use short-term bank financing are inherently more likely to be affected by a credit supply shock. As a result, one cannot measure the effect of maturity structure on real outcomes simply by relating the pre-crisis amounts of short- versus long-term debt and post-crisis outcomes.[4]

Our paper is related to recent studies on the effects of credit supply shocks and it is important that we differentiate our findings. Lemmon and Roberts (2009) examine the effects of a contraction in the supply of risky credit (junk bonds) in 1989 that was induced by regulatory changes and the collapse of Drexel Burnham Lambert. Their evidence suggests that junk bond issuers' investment declined as a result of changes in the bond market landscape. Our study differs from Lemmon and Roberts in a number of ways. Firstly, those authors' test strategy focuses on firms that are assigned to a different, high-risk category by the credit markets. It is fair to argue that junk bond issuers are of a inherently different quality, and it is ultimately difficult to find appropriate counterfactuals for them, even if one is able to find ordinary firms of similar size, age, profitability, and leverage ratios,

[4] While a significant fraction of the total amount of debt in U.S. firms' balance sheets is provided by banks, the fraction of bank-funded debt is much lower when we consider only long-term debt. Rauh and Sufi (2009) show that for firms rated BBB, for example, the average maturity of bank debt is below 4 years, while public debt maturity exceeds 12 years.

as the authors do. Our treatment and control firms, in contrast, are only different because of a "local discontinuity" in their long-term maturity structure, a discontinuity that only becomes meaningful because of a sharp, well-identified credit shock. Secondly, those authors compare the behaviors of their treatment and control firms over a period encompassing the 1990-1991 recession. It is difficult to ascertain whether their treated (riskier) firms invested less after 1989 because of difficulties in the junk bond market or if they would invest less in the downturn independently of any developments in the market for junk bonds. Our paper also examines data from a recession, but we provide explicit tests that address this type of confounding "macro effects" problem. Indeed, we uniquely provide a number of falsification tests, challenging each of one of the elements of our strategy. Thirdly, it is a matter of argument whether regulation put in place to prevent S&Ls and insurance companies from investing in junk bonds are exogenous to the collapse of the junk bond market. In particular, it should not be surprising to see restrictions put in place to prevent financial institutions from investing in securities whose payoffs are particularly risky (junk bonds), an argument that implies a reverse causation between treatment and observed outcomes. Finally, Lemmon and Roberts's paper does not cover the current financial crisis, which is the sharpest credit shortage in nearly a century.[5]

Similarly to our paper, Duchin, Ozbas, and Sensoy (2009) focus on the impact of the current credit crisis on corporate investment. Their attempt at identifying firms that are more or less 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 will confound factors that may well explain those firms' post-crisis behavior. This makes it difficult to ascribe causality going from financial policy to real firm outcomes, which is the question of ultimate interest.

Our study contains important implications for corporate financial policy. Our results show, for example, that firms with similar debt-to-asset ratios may respond quite 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 sorting 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 corporate managers should pay to the maturity profile of their firms' debt. Debt maturity is an important aspect of financial flexibility, an aspect that — as we demonstrate — becomes particularly important during contractions. Finally, our study is one of only a handful of papers in corporate finance that uses well-identified elements of financial contracting to show *how* financial contracts affect firm behavior (see also Chava and Roberts (2008)).

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

[5] Other related papers that do not look at the current crisis are Chava and Purnanandam (2008), who examine the effects of the 1998 Brazil-Russia-LTCM crisis on the valuation of bank borrowers, and Leary (2009) who studies the consequences of the introduction of interest rate ceilings that took place in the 1960's.

check the robustness of our results. Section 5 concludes the paper.

2 Empirical Design

We start this section by describing our basic experimental design and the matching estimator methodology we employ in the paper. 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 episode as a way to identify the effect of credit supply shocks on corporate policies. Of course, the relevant question is how would the composition of long-term debt maturity 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 financing, such as commercial paper, bond placements, bank loans, and secondary 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 around the 2007 crisis may be expected to face tighter financing constraints than firms that do 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, Philippon, Richardson, and Roubini (2009), the current crisis probably started in 2006 by a reversal in housing prices, which in turn triggered a wave of default of subprime mortgages going into 2007. The increase in subprime defaults in the first half of 2007 initially affected financial institutions that had invested heavily in asset-backed securities (ABS). Acharya et al. identify the collapse of two Bear Sterns-managed hedge funds in June 2007 as a "salient" milepost of the systemic crisis. 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 rollover short-term financing to highly-levered institutions, as exemplified by the run on BNP Paribas' SIVs.[6] These 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 on to the balance sheet of banks that sponsored investment vehicles such as SIVs.

[6] See also Acharya, Gale, and Yorulmazer (2009) for a model of rollover risk that generates market freezes like the one observed in August 2007.

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

<ins>Figure 1 About Here</ins>

The repricing of credit instruments that followed by the 2007 panic quickly went beyond short-term bank financing, spilling over into longer-term instruments. Indeed, the crisis highlighted the interdependence of segments of the financial market that were once thought of as being 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 investment grade and high yield bonds (from Citigroup's *Yieldbook*).[7] 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 choose 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+. Thus, Figure 2 gives a fairly complete picture of the effect of the crisis on the spreads of bonds with different credit quality.

<ins>Figure 2 About Here</ins>

Notably, the spreads on long-term corporate bonds show a dramatic increase starting in August 2007, both for investment-grade and junk-rated firms.[8] 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 towards levels that approached 3% during early 2008. Junk bond spreads display a similar pattern, increasing from levels around 3% in early 2007 to 4.6% in August, and then to between 7% and 8% in early 2008.[9] Similar signs of a

[7] 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.

[8] The spreads we present are very similar to the high-yield bond spreads reported in Figure P.2 in Acharya, Philippon, Richardson, and Roubini (2009).

[9] 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.

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 were 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. Results from a recent study by Ivashina and Scharfstein (2009) are also consistent with a significant drop in the supply of new debt as a result of the financial crisis. The authors use Reuters' *LPC-DealScan* data to show that new loans to large borrowers fell by 79% from the peak of the credit boom (second quarter of 2007) to the end of 2008. Lending for real investment and restructuring (LBOs, M&A, share repurchases) show similarly large drops during the crisis period.

The available evidence substantiates our conjecture that the current financial crisis was associated with a substantial increase in the cost of both short-term and long-term financing for firms, starting with the events of August 2007. These increases appear to be at least partly due to a credit supply shock which initiated in the housing sector and eventually affected financial institutions and the overall credit markets. Such an 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 two conditions to be met. First, and most simply, there has to be enough variation in debt maturity to allow for comparisons across firms. In particular, there must exist a significant group of firms that have a spike ("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 first-order frictions making it difficult for firms to maintain their optimal capital structures — this, assuming firms do pursue such policies in the first place. 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 little guidance on this conjecture. Thus, it is interesting to discuss this possibility in more detail.

Figure 3 depicts the distribution of debt maturities for the sample of firms that we use in our analysis (the data are described in detail in Section 2.3), calculated at the onset of the 2007 crisis. For each firm, 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.[10] Figure 3 reports these amounts as a fraction of total long-term debt. If maturity structure was well diversified, we would expect this distribution to have a large mass around a specific value.[11] The figure makes it clear, however, that there is significant variation in maturity structures. Consider, for example, the fraction of long-term debt that is due in one year (i.e., in 2008). Figure 3 suggests that there exists a significant number of firms whose long-term debt maturity concentrates in the year of 2008. 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, there are many firms with maturity spikes occurring in 2012. 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 far beyond the 2007 crisis.

Figure 3 About Here

Two other features of the distribution of debt maturity measured at the end of 2007 are noteworthy (and useful for our test design). First, 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. Second, as depicted in Figure 4, the distributions of the long-term debt maturity of firms for years prior to 2007 are strikingly similar to that of 2007. In other words, there is no evidence of changes in long-term debt maturity structure in the years leading up to the 2007 crisis.

Figure 4 About Here

One possible reason why some firms end up with spikes in their debt maturity distributions (such as those depicted in Figures 3 and 4) 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 total of 790 firms provide the information we need for this check. 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 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 COMPUSTAT sample is 0.28, suggesting that on average firms issue debt in only 3 of 10 years.

[10] 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.

[11] For example, if firms tend to regularly issue 10-year bonds we would expect to see a mass at the value of 10%.

The second condition that must be satisfied to validate a strategy based on debt maturity is that the variation in debt maturity at the onset of the crisis needs to be exogenous to observed post-crisis outcomes. In particular, one might worry that the same variables that determine the pre-crisis distribution of long-term debt maturity might also influence post-crisis corporate investment. Suppose, for example, that firms that have high growth opportunities tend to issue debt of longer maturities. Then, it might not be surprising to observe that these firms invest more during a crisis, relative to other firms that have shorter-maturity debt.

Our empirical strategy addresses this issue in several complementary ways. First, we focus on maturity variation in long-term debt (only) rather than in short- versus long-term debt to sort firms into treatment and control groups. This choice ensures that we are not simply comparing low-quality firms that must issue short-term debt to firms that can issue long-term debt. Second, we use matching estimator techniques (described in greater detail in Section 2.2) that minimize concerns about selection. This enables us to account for the effect of observables by matching firms with long-term debt maturing right after the credit crisis to "control firms" of similar size, industry, credit ratings, Q, long-term leverage ratio, cash flows, and cash holdings, with the key difference between the treatment and control firms being when their long-term debt, which was contracted long ago, happens to mature. Third, we consider a number of variations to our baseline empirical design that allow us to further distinguish among alternative explanations for the results we obtain.

Among other things, we perform a battery of falsification tests that replicate our matching estimator procedure in non-crisis periods. To see the logic behind these tests, consider Figure 4. The 2006 distribution of long-term debt maturity shows a significant fraction of firms with a lot of long-term debt maturing within one year's time (in this case, 2007). These firms are similar to firms at the end of 2007 that have a lot of long-term debt maturing within one year's time (in this case, 2008) in that both sets of firms have fast-approaching spikes in the maturity of their long-term debt. We expect, however, to find *no evidence* of a relation between debt maturity spikes and subsequent investment behavior for the test conducted in 2006 since, at that time, the need to refinance long-term debt does not coincide with a credit shortage. Our analysis will contrast the 2007- and 2006-based tests. Figure 4 also shows a group of firms that have, as of fiscal-year end 2005, a large fraction of their long-term debt maturing in 2008 (i.e., the third year after 2005). In one of our robustness checks, we will use this (long) *pre-determined* long-term debt maturity distribution to sort firms into treatment and control groups. This test allows us to verify whether some managers were better able to anticipate the effects of the credit crisis and refinance their long-term debt before the credit crisis hit; that is, we can check whether unobserved managerial quality could explain the post-crisis differences in investment behavior.

2.2 Matching Estimators

The use of matching estimators is a central feature of our test strategy and it is important to explain how we employ this technique. Recall, we want to test whether firms that need to refinance their long-term financial obligations at the time of a credit crisis alter decisions related to real-side variables. In particular, we want to determine whether re-financing constraints affect real firm outcomes. Our goal is to develop an identification strategy that is akin to an "experiment:" the firm's long-term debt maturity structure and developments in the financial markets coincide such that the firm is in need of refinancing a large fraction of its debt in the midst of a sharp credit contraction. If a firm's debt maturity is randomly assigned across firms, then it would suffice to compare the ex-post outcomes of firms that had significant debt maturing around the time of the crisis with those whose debt happened to mature at a later date. Our analysis, however, needs to allow for the fact that we are not using a true laboratory experiment, but instead relying on observational data.

Short of running a randomizing experiment with the firms' financing constraints, the econometric challenge is to gauge firms' likely outcomes *had they not* been caught between a credit crisis and the need to refinance their debt. Since we are interested in the impact of firm financing on real outcomes, we need to carefully identify a group of firms that also face the credit crisis and are virtually similar to those whose debt matures during the crisis *except* for the fact that their debt is not maturing in the crisis.

Traditionally, researchers have dealt with this problem in the context of ordinary least squares (OLS) estimators, where the group of interest is differentiated from other observations via an indicator variable. Under this standard parametric approach, the impact of the variable of interest on observed outcomes is measured by the coefficient returned for the indicator variable. The regression specification is determined according to a set of theoretical priors about the endogenous variable. These models are often simple, linear representations of a particular theory. In corporate finance research, controls such as firm size, profitability, and leverage are customarily added to the specification to capture additional sources of firm heterogeneity. If left unmodeled, that sort of variation could jeopardize the OLS estimator as it could explain both a firm's selection into the group of interest and its observed outcome.

A few concerns arise with the implementation of the standard OLS approach. First, the simple inclusion of control variables in the specification does little to address the fact that the groups being compared may have very different characteristics (for example, comparison groups may have markedly different size or profitability distributions). Unfortunately, OLS estimates will not alert the researcher that a poor distributional overlap might yield an ineffective control set. Second, and relatedly, the OLS approach will allow for extreme outliers in the estimation, outliers that can bias the estimates of interest — standard OLS is notoriously weak in dealing with outliers. Finally, the OLS approach may place undue importance on linear model parametrization in the estimation process. Depending on the application, one can improve the estimation of group differences by allowing for non-linear modeling of the outcomes of interest as well as by way of non-parametric methods.

The estimation strategy that we use in this paper is less parametric and more closely related to the notion of a randomized experiment. We use matching estimators in all of the tests performed in this paper. 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 a number of dimensions (*covariates*). In this estimation 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 as the control group actually did. The matches are carefully made so as to ensure that treated and control observations have identical distributions along the covariates chosen (dimensions such as firm size, profitability, leverage, risk, etc.). Inferences about the treatment of interest (re-financing constraints) are based on comparisons of the ex-post outcomes of treatment and control groups (outcomes such as investment spending).[12]

Although a number of matching estimators are available, we employ the Abadie and Imbens (2002) estimator.[13] Their non-parametric procedure most naturally fits our application. The Abadie-Imbens estimator allows one to match a treated firm with a control firm, with matching being made with respect to both categorical and continuous variables. The estimator aims at producing "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 matching estimations, the specification used is less centered around the idea of representing a model that fully explains the endogenous variable. Instead, the focus is in ensuring that variables that might *both* influence the selection into 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 estimations covariates for which one could make a reasonable case for simultaneity in the treatment–outcome relation. Among the list of categorical variables we include in our matching estimations are the firm's industrial classification and the rating of its public bonds (either speculative grade, investment grade, or unrated). Our non-categorical variables include the firm's market-to-book ratio (or "Q"), cash flow, size, and the ratio of long-term debt to total assets. Although our original approach already makes it hard to tell a story in which the covariates we consider would predict both treatment and outcomes, it is commonly accepted that those covariates capture a lot of otherwise unobserved firm heterogeneity.

[12] In the treatment evaluation literature this difference is referred to as the average treatment effect for the treated, or ATT (see Imbens (2004) for a review).

[13] In particular, we use the bias-corrected, heteroskedasticity-consistent estimator implemented in Abadie, Drukker, Herr, and Imbens (2004).

Lastly, we note that we model the outcomes in our experiments in a 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 follows that of Almeida, Campello, and Weisbach (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 log of 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 the maturity data in our quarterly tests. 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 the data on debt maturity appears inconsistent. By inconsistent we mean the following. Some firm show values of debt maturing in more than one year (*dltt*) that are lower than the sum of debt maturing in two, three, four, and five years (*dd2* + *dd3* + *dd4* + *dd5*), while others have debt maturing in one year (*dd1*) greater than the sum of *dd1* and *dltt*. These observations are deleted from the sample. For our baseline tests, we disregard firms for which liabilities such as notes payables, bank overdrafts, and loans payable to officers and stockholders (item *np* in the annual file) are greater than 1% of total assets. In our baseline tests, we require firms to have long-term debt maturing beyond one year (*dltt*) that represents at least 5% of assets (*at*). These debt-related restrictions help assure that the results in our paper do not come from comparisons between "low-quality" firms that need to rely on very short-term obligations with "high-quality" firms that can issue long-term debt.

We focus on firms that have 2007 fiscal year-end months in September, October, November, December, or January. The sample of firms with these 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 around Fall of 2007. For our benchmark specification, 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. 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 final 2007 sample consists of 1,067 individual firms.

In our basic experiment, the outcome variable is the change in the average quarterly investment over the first three quarters of 2008 relative to the first three quarters of 2007.[14] Investment is defined as the ratio of quarterly capital expenditures (COMPUSTAT's *capxy*) to the lag of quarterly property, plant and equipment (*ppentq*). 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 are defined as the ratio of cash and short-term investments (*cheq*) to total assets. Long-term 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 both 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'

[14] We use symmetric quarters around the fourth quarter of 2007 to avoid seasonality effects.

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.[15] Our 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. In the Fall of 2007, Dollar's proportion of total long-term debt maturing over the next year (i.e., 2008) was 34%. The proportion of long-term debt maturing between years 1 and 2 (2009), 2 and 3 (2010), 3 and 4 (2011), and 4 and 5 (2012), was, respectively, 0%, 19%, 19%, and 19%; the remainder 8% was due in five years or more. 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.

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

Other examples come from the communications industry (Dish Network is a treated firm and Equinix its control match) and the food industry (Coca-Cola versus Tyson Foods). Notably, a much-publicized case of crisis-related debt burden is also in our sample: Saks Inc. In late 2007, Saks had 56% of its entire long-term debt coming due in 2008. Our control match for Saks is Bon-Ton Inc. (who 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).[16]

[15] We later experiment with multiple alternative definitions of treatment and control groups.

[16] Interestingly, a large fraction of Bon-Ton's operations (a number of retail chains) was bought from Saks just a few years before the crisis. The two firms thus share a number of similarities, except the maturity structure of their long-term debt.

3 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 baseline empirical results.

3.1 Summary Statistics

Our matching approach is nonparametric, making it fairly robust to extreme observations. Treatment and control firm outcomes, however, are compared in terms of mean differences. To minimize the impact of gross outliers on these comparisons, we winsorize variables at the 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 continuity-corrected Pearson χ^2 statistic to test for differences in the medians of the variables of interest across those 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 median 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.

TABLE 1 ABOUT HERE

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 (thus, we have 86 firms in both the treated and control groups). Remarkably, there are *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. This evidence supports 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 happens to mature.

TABLE 2 ABOUT HERE

3.2 The Real Effects of the 2007 Credit Crisis

We examine the investment behavior of our treated and control firms around the 2007 credit crisis. Before doing so, however, we show a brief comparison between the 86 treated firms and the broader set of 981 firms that we classify as non-treated. These comparisons help better characterize our main findings below. 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. The difference is statistically significant, as indicated in the third row of the panel. The fact that both groups of firms have significantly different investment levels in the pre-crisis period suggests that comparisons between the two groups could be potentially confounded by other factors.

TABLE 3 ABOUT HERE

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%). In contrast, for non-treated firms, investment fell to 6.0% (a fall of 0.6%). These figures suggest that investment decreased by 1.6% *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 significantly different after the crisis. While the average quarterly investment of firms in the treatment group fell by 2.1%, control firms' investment remained virtually unchanged. The estimates imply that investment decreased by 2.2% *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 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, 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 (ATT). The ATT difference is equal to -2.5%.[17] This is the 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.[18] More generally, the estimates in Panel B imply that frictions that arose from firms' debt maturity structures generated financing constraints the 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 real 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 further 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 placebo 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 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% 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.

[17] That estimate would equal -2.2% (the simple average difference effect) if it were not for the "bias-correction" that is embedded in the estimator that helps dealing with the problem of matching on continuous variables (see Section 2.2).

[18] To ensure that our results are not explained by an extreme data point, we redo our experiment 85 times taking away one treated firm at a time. The lowest ATT result is –2.1% (significant at 5% test level) and the highest –2.9% (significant at 1%).

4 Extensions and Robustness Tests

In this section, we test additional implications of our basic argument, provide evidence that the benchmark results are robust to variations in the empirical specification we use, and show that the 2007 crisis results (reported in Table 3) do not obtain in non-crisis periods. We also show that our results cannot be ascribed to differential trends in the outcome of interest (investment rates), nor can they be attributed to differential responses across treated and control firms that could arise in recession periods (independently of the credit shortage). Finally, we provide a "back-of-the-envelope" calculation that shows how firms with balloon debt payments in 2008 responded to the credit crisis along other dimensions besides investment policy.

4.1 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 (e.g., long-term debt or bank financing). 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 then expect *not to find* the same 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.[19] 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 quarters 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 same sampling criteria, covariate matching approach, and definitions of treatment and control groups that we used for the credit crisis period.

The results are reported in Table 4, which also reports the results for 2006 and 2007 for quick 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 between 2001 and 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 the financial crisis.

TABLE 4 ABOUT HERE

[19] 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 (much like 2008), but not a credit crisis.

4.2 Parallel Trends and Macro Effects

4.2.1 Parallel Trends

A standard concern about inferences from studies using the treatment-effects framework is whether the data processes generating the treatment and control group outcomes had "common or parallel trends" prior to the treatment. Differences in the post-treatment period can be ascribed to the treatment only when this assumption holds. The outcome variable of our study is the within-firm change in investment spending. Recall, our matching procure rendered treatment and controls matches with very similar investment going back three quarters prior to the crisis (see Tables 1 and 2). The threat is that although quarterly investment levels might be similar for the two groups of firms for about a year before 2007, those firms' investments could be following different long-term trends in the period leading up to the crisis. While one could debate the likelihood of such patterns, 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 5 reports the mean and median quarterly change in investment for firms in the treatment and control groups going back 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 crisis (quarterly investment changes from 2005Q3 through 2007Q3). The mean (median) change in investment for the treatment group is –0.11% (0.07%), while for the control group the mean (median) change is –0.42% (0.05%). The table also reports p-values associated with test statistics for differences in means (standard t-test) and in medians (continuity-correct Pearson's χ^2) across groups. 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 (2004Q3 through 2007Q3). Subsequent rows go back farther in time.

TABLE 5 ABOUT HERE

It is apparent from the estimates reported in Table 5, in particular from the p-values for t- and Pearson-tests, 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. 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 crisis.

4.2.2 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 these differences trigger sharp treatment–control contrasts in the post-treatment period because of other changes in the environment.

Like 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. While the existing literature provides no evidence of such links, we design an additional test that speaks to this concern.

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 to identify a placebo treatment. In other words, we eliminate one of the key elements of our treatment strategy, but allow for the same macro effects (demand contraction) that could potentially 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 — may explain our baseline results.

Given the data requirements of our matching strategy, we focus on the 2001 recession.[20] It is easy to show that the credit conditions that accompanied the 2001 recession are 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).[21] 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

[20] Information on debt maturity from COMSPUSTAT for the 1980's and 1990's recessions is considerably more sparse.
[21] These data come from Citigroup's *Yieldbook* (described in Section 2.1.1).

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% (compared to equal to −2.2% in the 2007 baseline). Similarly, the Abadie-Imbens ATT estimate for this test is 1.4% (compared to −2.5% for 2007).

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

4.3 Changing Long-Term Leverage Cutoffs

Long-term debt maturity should matter only for firms that have significant amounts of long-term debt in their capital structures. Accordingly, in our benchmark specification we considered only those firms for which the ratio of long-term debt maturing in more than one year to total assets was higher than 5%.

In Table 6, we experiment with different inclusion rules that are designed to check the logic behind 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 our estimated effects.

Table 6 shows evidence that is consistent with these hypotheses. 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 our expectations, the estimated differences between treatment and control groups *disappears* 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 our benchmark result, which is reported in the second column of the table.

In the third column of Table 6, we perform an alternative experiment that 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 relative to control firms *increases* to 3.4% of capital (from 2.5% in the baseline experiment). This evidence helps further substantiate the hypothesis that treated firms found it difficult to refinance their maturing long-term debt in the post-crisis period, and thus were forced to substantially cut their investment spending.

TABLE 6 ABOUT HERE

4.4 Pre-Determined Maturity Tests

Our baseline experiment uses maturity variables measured at the end of 2007, just a few months following the August credit panic. As explained in Section 2.3, we made this choice to make sure that we capture the extent to which firms are constrained by debt maturity in the aftermath of the crisis. 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. A particularly problematic alternative explanation is the following. Suppose that higher quality managers were more likely to anticipate the credit crisis in early 2007, or even 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 in anticipation of the financial crisis by "smart CEOs" would leave only the "dumb CEOs" with long-term debt maturing in 2008, and these "dumb CEOs" may be forced to cut investment for non-maturity-related reasons after the credit crisis hits. The placebo tests of Section 4.1 do not address this self-selection concern because this is a crisis-specific story.

A simple way to ensure that the anticipation of the crisis by "smart CEOs" does not drive our results is to use maturity variables measured in 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 a group of 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 manager 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 pre-determined 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 pre-determined maturity profiles also help predict changes in investment around the credit crisis. As expected, the effects of maturity structure on investment (-1.4% when using the 2005 maturity and -0.6% when using the 2003 maturity) are somewhat smaller than those estimated in Table 3, nonetheless, they are still economically meaningful.[22] These results suggest that the managerial quality hypothesis cannot explain the relation between debt maturity and investment that we report in Table 3.

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

4.5 Different Specifications for the Matching Estimator

We have also experimented with several variations in our procedure to construct treatment and control groups, as well as in the set of matching covariates. To illustrate the robustness of our results, we report 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 maturities. However, they happen to have concentrated their maturity in a time period that lies far in the future.[23] The estimated difference in investment changes (the matching estimator ATT) remains negative, equal to -1.6%, 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; point estimate of -2.3%, with a standard error of 0.9.

4.6 How Did the Treated Firms Respond to the Credit Crisis?

The evidence so far suggests that firms with large amounts of debt maturing in 2008 were forced to decrease 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, and/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.[24] 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, Campello, and

[23] We choose five years because this is the farthest one-year information that is available in COMPUSTAT. As mentioned earlier, for debt maturing in more than five years, we have no information on the specific year of maturity.

[24] The survey evidence in Brav, Graham, Harvey, and Michaely (2005) suggests that share repurchases are the residual after the investment and dividend decisions have been made.

Weisbach (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 reductions) — inventories, share repurchases, dividends, and cash holdings. These variables were present for 77 of our 86 treated firms. We use these firms in the subsequent analysis.

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% for the treated firms reported in the first row of Panel B of Table 3, represents an annual decline of 8.4%. 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 5 provides a visual illustration of the treated firms' broader response to the credit crisis. In this figure, we report the average changes in various corporate policy variables as a fraction of the total amount of long-term debt maturing in 2008 for the treated firms we consider. 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, Graham, Harvey, and Michaely (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, with the remaining 29% to be explained by other factors (such as reductions in R&D, labor costs, and asset sales).[25]

While admittedly done solely for purposes of providing a crude approximation for how the treated firms responded to the financial crisis, the set of numbers depicted in Figure 5 fits our economic in-

[25] Campello, Graham, and Harvey (2009) survey 574 U.S. CFOs at the end of 2008. These managers 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.

tuition 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.

Figure 5 About Here

5 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 during the crisis. Our empirical methodology aims at replicating an experiment-like design in which we control for observed and time-invariant unobserved firm heterogeneity via a difference-in-differences matching estimator.

We find evidence that 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 reduced their quarterly investment rates by 2.5% 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 unique identification strategy reveals 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' debt levels. Undoubtedly, 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. Importantly, we are able to characterize one precise channel (a contracting feature) that shows *how* financing affects investment.

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Table 1: 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 percent and 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 percent. 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 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 continuity-correct Pearson's χ^2 statistic, with the p-values of this test reported at the bottom row of each panel.

	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 2: Distributional Tests of Treated, Non-Treated, and Control Firms at the end of 2007

This table compares the distributional properties 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 percent and 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 percent. 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 text for further variable definitions. The 25th percentile, median, and 75th percentile are reported for each firm characteristic. The test for a difference 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.

		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 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

Panel A and Panel B of this table present an estimate of the change in average quarterly investment rates from the first three quarters of 2007 to the first three quarters of 2008 (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 percent and 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 percent. 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.

	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**
	(0.72)	(0.55)	(0.65)
	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 (ATT)			–2.46**
			(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 (ATT)			0.01
			(1.09)

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

Table 4: Difference-in-Differences of Firm Investment from One Year to the Next: 2001 through 2007

This table presents an estimate of the change in investment 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 percent and 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 percent. 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.

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	–0.10	–0.04
Before Fall 2007 Credit Crisis	(0.30)	(0.31)

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

Table 5: Trends in Investment for Treated and Control Firms: Mean and Median Comparisons

Table 5 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). 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 (continuity-correct Pearson's χ^2) across groups.

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

Table 6: Difference-in-Differences of Firm Investment Before and After the Fall 2007 Credit Crisis: Different Leverage Cutoffs

This table presents an estimate of the change in investment from the first three quarters of 2007 to the first three quarters of 2008 (before and after the fall 2007 credit crisis) for various long-term leverage cutoffs: leverage ratio of more than 0%, leverage ratio of more than 5%, and leverage ratio of more than 10%. The leverage ratio cutoff of 5% presented in the middle column (i.e., long-term debt represents more than 5 percent of assets) reproduces the results presented in Panel B of Table 3 for ease of comparison. 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 percent of total long-term debt and 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 percent of total long-term debt. 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.

	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 percent levels, respectively.

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 treasuries, for the period of January 2004 to August 2009. The data is from http://www.federalreserve.gov/datadownload/.

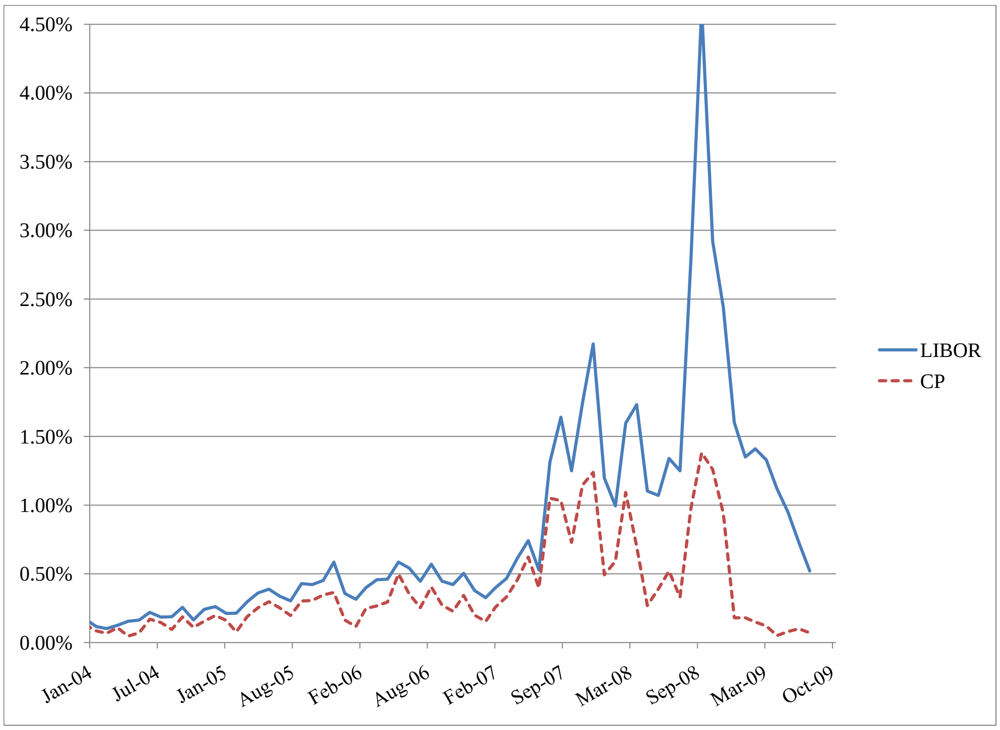
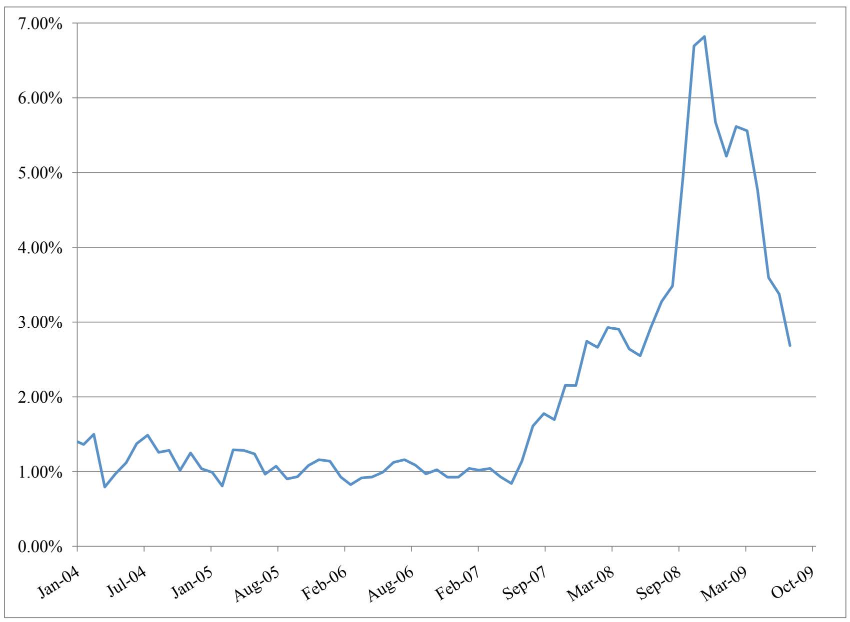


Figure 2: Corporate Bond Spreads During the 2007 Credit Crisis

This figures display the the time series of spreads for indices of investment grade and high yield bonds 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/.)

Panel A: Investment-grade spreads



Panel B: High-yield spreads

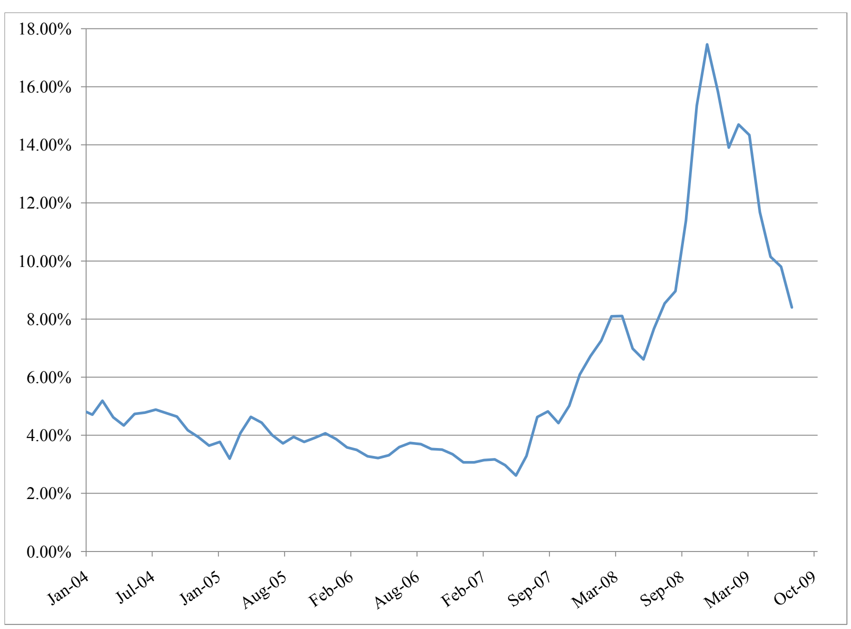


Figure 3: Composition of Long-Term Debt Maturity at the end of 2007

This figure displays the amount of long-term debt maturing in the years of 2008 to 2012, as a fraction of total long-term debt, for the sample of firms described in Section 2.3. Maturity structure is measured at the end of the 2007 fiscal year.

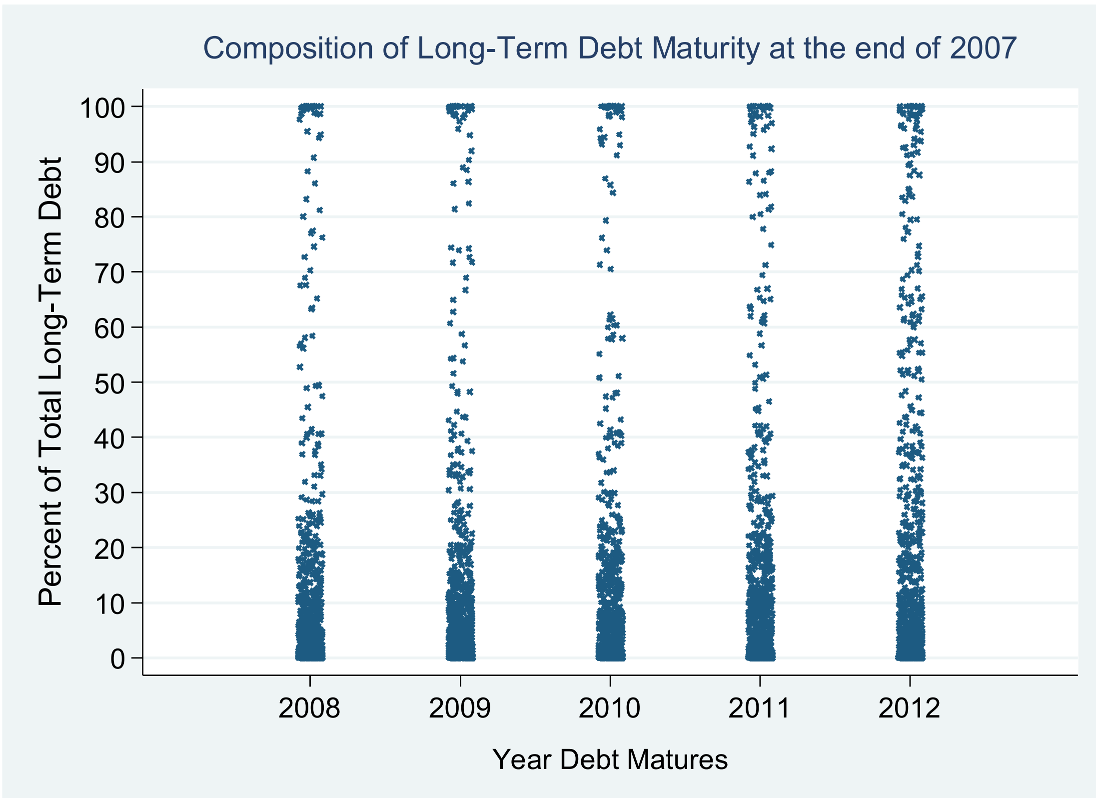


Figure 4: Composition of Long-Term Debt Maturity: 1999 to 2006

This figure displays the amount of long-term debt maturing in one to five years away from an initial year t, as a fraction of total long-term debt, for the sample of firms described in Section 2.3. Maturity structure is measured at the end of fiscal year t, with t varying from 1999 to 2006.

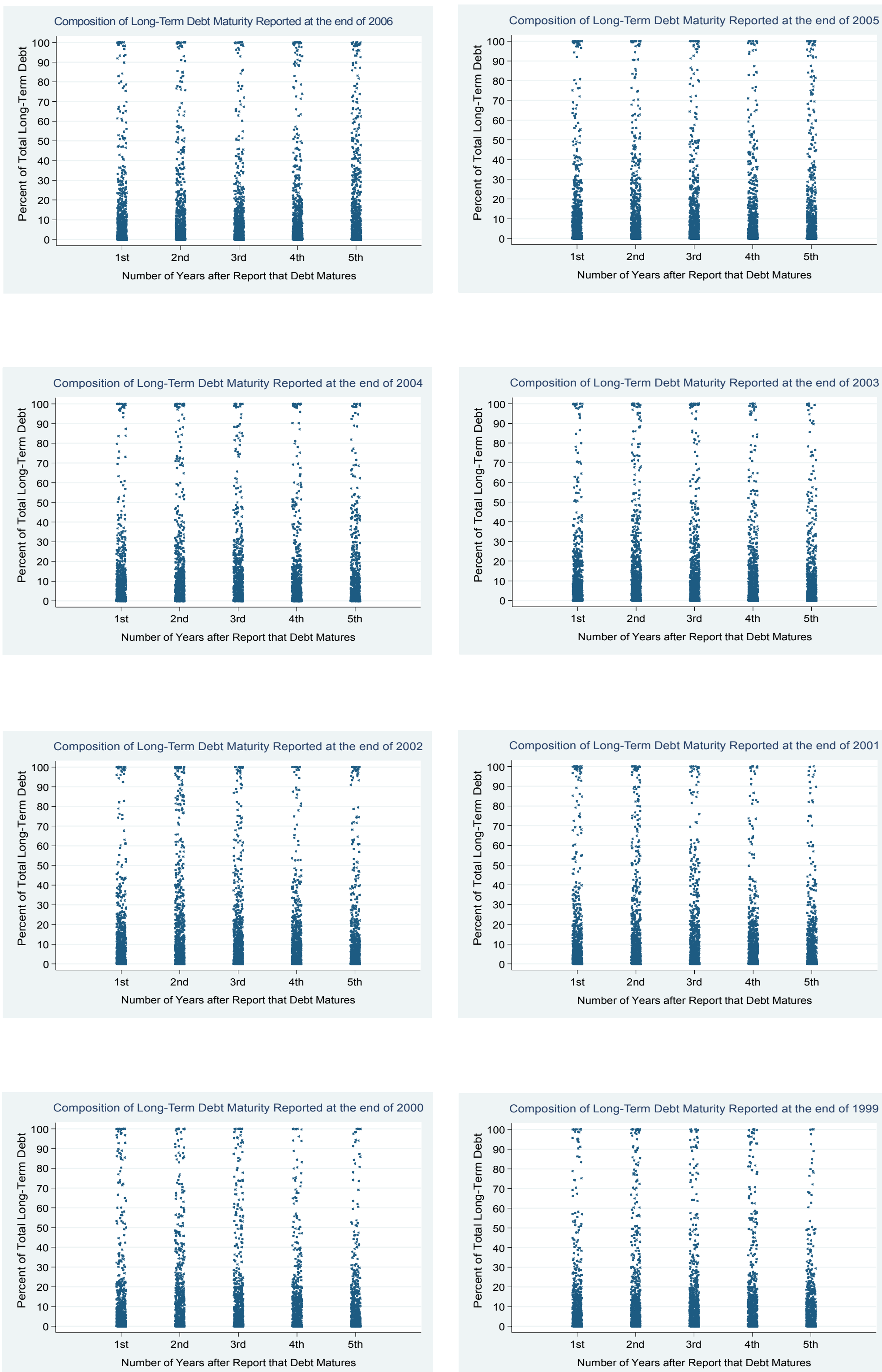


Figure 5: How did Treated Firms Pay Off Their Debt?

This figure displays 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 which have more than 20% of their long-term debt maturing in 2008.