Three Essays in Macroeconomics and Finance

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Abstract

This thesis consists of three chapters on topics in macroeconomics and finance. In the first chapter, I use texts from corporate filings of US companies to investigate if liquidity shortages that occurred during the late-2000 financial crisis were different from cases that occur during more normal times. In the second chapter, I quantify narrative evidence from corporate filings to construct a novel dataset on the price-setting behavior of companies. I then use this dataset to investigate what factors cause firms to change the prices of their products or prevent them from doing so. In the third chapter, I use a number of high-frequency financial market estimates to identify the monetary policy shock in a non-recursive Factor Augmented Vector Autoregression of monthly frequency.

Resumen

Esta tesis consta de tres capítulos sobre temas de macroeconomía y finanzas. En el primer capítulo, utilizo textos de presentaciones corporativas estadounidenses para investigar si las faltas de liquidez que se produjeron durante la crisis financiera de finales del 2000 fueron diferentes de los casos que ocurren durante tiempos más normales. En el segundo capítulo, cuantifico evidencia narrativa de presentaciones corporativas para construir un nuevo conjunto de datos sobre el comportamiento de fijación de precios de las empresas. Luego utilizo este conjunto de datos para investigar qué factores hacen que las empresas cambien o dejen sin cambios los precios de sus productos. En el tercer capítulo, utilizo un número de estimaciones de datos financieros de alta frecuencia para identificar el shock de política monetaria en un modelo FAVAR de frecuencia mensual.

Preface

This thesis consists of three chapters on topics in macroeconomics and finance. In the first chapter, I investigate to what extent firm-level liquidity shortages related to the late-2000 financial crisis were different from cases that tend to occur during more normal times. To do so, I first search the texts of more than 900,000 corporate filings from the past twenty years and extract a large number of verbal discussions of firm-level liquidity shortages. Then, I quantify the contents of these discussions and link them back to closely related observable variables to verify their informational content. My results suggest that the crisis was unique in that a significant fraction of the firms affected by the related liquidity shortages were relatively large and profitable. I also find that, even though the types of firms that experienced shortages during the crisis and normal times were very different, the actions they took in response were not. Finally, I document that, while the number of firms that experienced liquidity shortages during the crisis was very high, these shortages were not unusually severe at the level of individual firms. I discuss what these findings reveal about the nature of the financial crisis and what they imply for related theoretical and empirical research.

In the second chapter, I quantify narrative evidence from 20 years of archived corporate filings to construct a unique dataset on the price-setting behavior of publicly traded companies. Based on verbal discussions of 1,949 pricing decisions taken by 983 different firms, I obtain four main findings. First, the causes of price changes are highly asymmetric, with raw material costs mainly driving prices up, and considerations about the behavior of competitors mainly driving prices down. Second, considerations about the behavior of competitors are also the main factor preventing firms from raising their prices, followed by weak aggregate demand conditions and existing contracts. Third, the presence of real rigidities is also borne out by a large number of cases in which firms adjust prices after changes in costs but explicitly describe these adjustments as incomplete. Fourth, the number or reported price changes is positively related to the number of reported price rigidities. I discuss these findings and their implications in the context of widely-used price-setting models.

Finally, in the third chapter, I use high-frequency financial market estimates to identify the monetary policy shock in a non-recursive 133 variable FAVAR. All identifying restrictions are imposed exclusively on impact, and only on financial market variables. Using the economy's underlying factor structure as the link between its real and financial sides, I find that high-frequency responses contain valuable information about the behavior of lower-frequency macro variables. Even though the proposed identification scheme does not fall back on any of the standard (FA)VAR identifying assumptions, it confirms the classical finding that monetary policy has strong and significant delayed effects on real activity. I also obtain stock market responses that are compatible with the efficient market hypothesis and find that consumer prices react very little to monetary policy.

Contents

[1] A slightly earlier version of this chapter was circulated as Sveriges Riksbank Working Paper No. 267.

Chapter One: Real Implications of Shortages in Corporate Liquidity: Was the Financial Crisis Unique?

1.1 Introduction

During the 2008/2009 financial crisis, a large negative shock to the aggregate supply of credit caused many firms to experience acute shortages in external finance and liquidity. In response, these firms adjusted important real policies such as their employment (Chodorow-Reich, 2014), innovation efforts (Brown and Petersen, 2014; Paunov, 2012) and capital spending (Campello et al, 2010; Duchin et al, 2010). However, apart from a sudden decrease in aggregate credit supply, firms may also experience liquidity shortages for very different reasons such as weak firm-level fundamentals or deteriorating aggregate demand. This raises the question to what extent the real effects of liquidity shortages observed during the crisis extend to cases that occur during more normal times and thus are not related to large financial shocks.

Unfortunately, for at least three reasons, this question is difficult to answer using standard econometric techniques. First, the concept of liquidity is inherently difficult to measure.[2] For example, it is not clear if items such as undrawn credit lines, cash held abroad or even certain types of inventories should be included in the definition. Second, because firms' desired levels of liquidity are not observable, it is difficult to establish which firms are actually experiencing shortages at a given point in time. Finally, credibly exogenous variation in corporate liquidity is particularly scarce outside of the financial crisis. Therefore, it is not clear how standard econometric techniques such as difference-in-difference specifications could be used to investigate how liquidity shortages affect real firm decisions during more normal times.

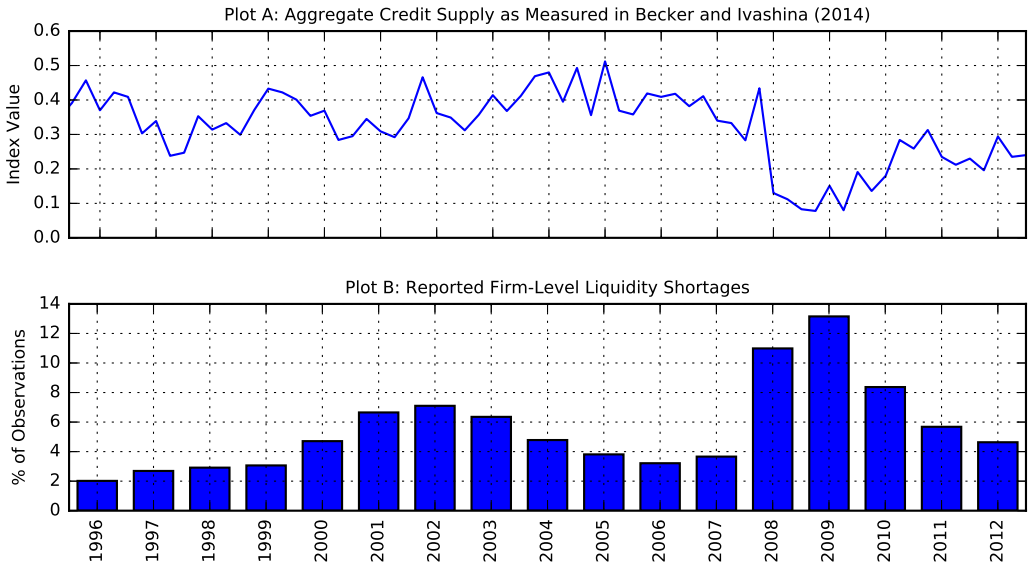
We tackle these empirical challenges using a text-based approach that does not rely on exogenous variation in liquidity and instead exploits narrative discussions contained in official corporate filings of publicly traded US companies. More precisely, searching the texts of more than 900,000 archived documents from the past 20 years, we first extract a large number of verbal discussions of firm-level liquidity shortages. These discussions explicitly state that the reporting firms were engaging in cash preservation efforts, presumably because they were unable to obtain additional cash inflows at reasonable costs. Then, exploiting the fact that the discussions also distinguish between cause and effect, we manually translate their contents into statistical variables that allow us to quantify how the reporting companies reacted to the liquidity shortages they faced.

Given that this approach does not require exogenous variation to capture the firm-level reactions to liquidity shortages, it allows us to compare cases that occurred during the crisis to those that occurred during more normal times. In fact, as Figure 1 shows, while our dataset contains discussions of more than 400 shortages related to the late-2000 drop in aggregate credit supply as measured by Becker and Ivashina (2014), we also observe

[2] See Almeida et al (2014) for a discussion of this issue.

approximately 900 cases that are not driven by a large financial shock. The three main findings that we obtain from this comparison are that many of the firms that experienced liquidity shortages during the crisis were relatively large and profitable, that the real reactions induced by crisis-related liquidity shortages were not unique, and that these shortages were not unusually severe at the level of individual firms.

Figure 1: Credit Supply and Reported Liquidity Shortages Over Time



Notes: The Figure illustrates the co-movement over time between aggregate credit supply and the number of firms reporting liquidity shortages in the corporate filings. Plot A shows aggregate credit supply as measured in Becker and Ivashina (2014). We thank Bo Becker for providing us with this updated time series. Plot B shows the fractions of all extracted liquidity shortages reported in a given year. The reported liquidity shortages shown in Plot B are obtained as described in Section 1.2. Liquidity shortages reported for fiscal years ending between January and May are assigned to the preceding calendar year.

While we are mainly interested in differences between the crisis and more normal times, we first use the full sample of all reported liquidity shortages in order to investigate the general validity of our narrative approach. Here, we show that our results confirm the important existing findings that liquidity shortages and limited access to external finance in a more general sense cause firms to employ fewer people, to reduce investment in fixed capital and R&D, and to change their payout policies.[3] Thus, even though we rely exclusively on the grammatical structures of narrative statements, our findings are consistent with those the existing literature has obtained using standard econometric techniques. Furthermore,

[3]See Chodorow-Reich (2014) for evidence on the employment effects, Campello et al (2010) and Duchin et al (2010) for the effects on investment in fixed capital, Paunov (2012) and Brown and Petersen (2014) for the effects on R&D efforts, and Bliss et al (2015) for the effects on payout policies.

by correlating the narrative evidence with closely related firm-level observables, we show that the extracted discussions are consistent with the reporting firms' true behavior.

Next, having provided this evidence for the general validity of our approach, we turn to the main question of the paper and investigate the extent to which liquidity shortages reported during the crisis differ from those reported during normal times. Consistent with the hypothesis that the fundamental causes of these two groups of shortages are different, we find that the types of firms that report them also have very different characteristics. More precisely, while firms reporting shortages during normal times tend to be unprofitable, small and subject to large cash outflows, many of the firms that report shortages during the crisis are relatively large, profitable and even paid dividends in prior periods. This makes them difficult to distinguish from their unconstrained peers based on their observable characteristics alone.

Then, considering how firms responded to the reported shortages, we find that the importance of real reactions relative to financial measures such as changes in dividend payments or share repurchases is virtually identical between the two time periods. Furthermore, even within these two reaction categories, the observed differences are remarkably small. In fact, the only reaction for which we find a statistically significant difference is the number of firms that implement dividend reductions. However, this difference, too, disappears once we control for prior dividend policies and prior cash holdings. Thus, in spite of the fact that shortages across the two periods arise within very different types of firms and for arguably different reasons, they have almost identical effects on the affected firms' real and financial policies.

Finally, we also explore potential differences in the severity of the shortages reported during the two time periods. For this, we first introduce the number of implemented reactions as a measure of severity and provide some empirical evidence for its informational content. In particular, we illustrate that it identifies those financial reactions as particularly costly for which prior research has shown that they indeed are. Then, having introduced this proxy for the severity of liquidity shortages, we apply it to the crisis and show that the average number of reactions to liquidity shortages during this time was neither unusually high nor unusually low. In other words, we find that, even though the number of firms that reported shortages exhibits a large spike in the years 2008-2010, at the level of individual firms these shortages do not seem to have been unusually severe.

Overall, the findings presented in this paper suggest that observable firm characteristics may not always be informative about firm-level financial constraints, especially during empirically important crisis episodes. Furthermore, they highlight that the role of financial constraints during a crisis may not be well captured by structural models in which a single firm characteristic such as size consistently distinguishes constrained from unconstrained firms.[4] Finally, our results also highlight important ways in which liquidity shortages associated with financial crises do not appear to differ from those that occur during more normal times, even though the latter are not caused by large financial shocks and arise in

[4] An example of this is Khan and Thomas (2013).

very different types of firms than the former.

This paper aims to contribute to a growing empirical literature that studies how the financial health of firms affects their real and financial policies. In particular, it is closely related to a number of recent studies that have exploited the large shock to credit supply associated with the late-2000 financial crisis. For example, Chodorow-Reich (2014) investigates how the availability of credit affects employment, Campello et al (2010, 2011) and Duchin et al (2010) consider implications for corporate investment, and Bliss et al (2015) estimate the effects on corporate payout policies. Moreover, Paunov (2012) and Brown and Petersen (2014) investigate the effects on research and development efforts.

We extend this existing work in three main ways. First, by exploiting narrative evidence on liquidity shortages instead of exogenous variation, we are not limited to specific historical episodes and can instead analyze firm-level reactions to liquidity shortages over the past twenty years. This allows us to investigate to what extent firm-level responses to liquidity shortages differ between cases related to aggregate financial market conditions and cases driven by other factors such as weak demand or firm-level fundamentals. Second, instead of focusing on one specific type of reaction, we provide a joint assessment of all reactions that firms report. Third, because we do not need to make ex-ante assumptions about what types of firms face liquidity shortages, our analysis actually yields their characteristics as a result.

In terms of its methodology and data, our paper is also closely related to a number of existing studies that use textual information contained in corporate filings to investigate the reporting firms' financial situations and constraints. For example, Kaplan and Zingales (1997) study texts of corporate filings to investigate the result of Fazzari et al (1988) that firms' cashflow sensitivities of investment tend to reflect their financial constraints. Using a similar approach, Hadlock and Pierce (2010) also score corporate reports to capture firms' financial constraints and then emphasize the specific role of age and size. Furthermore, Buehlmaier and Whited (2014), Bodnaruk et al (2015) and Hoberg and Maksimovic (2015) apply automated text search methods to large numbers of corporate filings in order to construct financial constraint measures that are independent of standard observables.[5]

Like us, these papers recognize and document that narrative evidence from corporate

[5]The texts of corporate filings have also recently been used to investigate a number of other economic issues. For example, Hoberg and Phillips (2010) and Li et al (2013) analyze different aspects of competition and chapter two of this thesis investigates firm price-setting behavior. Within finance, the narrative evidence contained in corporate filings has also been exploited by Ivashina and Scharfstein (2010) to show that credit-line drawdowns during the crisis were particularly large for banks that had co-syndicated a larger fraction of them with Lehman Brothers. Furthermore, Ippolito et al (2015) search the texts of filings in order to capture whether or not firms hedge floating rate loans. The texts of 10-k filings have also been used in the accounting literature, for example to assess the informational content of mandatory risk-factor disclosures (e.g. Kravet and Muslu, 2013; Bao and Datta, 2014; Campbell et al, 2014), to investigate the informational content of forward-looking statements (Li, 2010), and to assess the role of the complexity and tone of the texts firms provide (e.g. Li, 2008; You and Zhang, 2009; Feldman et al, 2010). To the best of our knowledge, however, we are the first to systematically exploit the precise grammatical structures of narrative corporate-filing discussions to identify firms that are experiencing acute liquidity shortages, to establish how they reacted to these shortages, and to investigate differences between the crisis and more normal times in this respect.

filings contains information about the reporting firms' financial health. However, the work that we present here differs from the existing studies in that it uses the narrative information from corporate filings not only to *measure* firms financial constraints, but also to establish a link to the subsequent reactions. More precisely, instead of aiming to assess how likely firms are to experience a liquidity shortage or financial constraints in a more general sense, we condition on such shortages and then exploit the narrative evidence to assess how firms actually respond to them. This allows us to make statements about the reactions to liquidity shortages without relying on exogenous variation.

Finally, in spirit, this paper is also very closely related to a specific survey question discussed in Campello et al (2010). In an electronic survey sent out in the aftermath of the 2008/2009 financial crisis, they asked corporate CFOs if they were planning to invest less because of financial difficulties they were facing. Like our narrative evidence, the answers to this question allow them to establish a direct link between firms' financial situations and their subsequent behavior without relying on standard identifying assumptions. However, because our sample covers not only the financial crisis but also more normal times, we are able to investigate differences between the two time periods. Furthermore, our sample of constrained firms is several times larger and, instead of asking firms about the effects on one specific policy variable, we record information about 16 different types of real and financial reactions.

1.2 The Dataset

In this section, we present our dataset. We first illustrate how we extract the narrative evidence about liquidity shortages from the corporate filings and then provide some descriptive statistics. Furthermore, we also discuss the general reliability of the narrative information that we extract from the corporate filings.

1.2.1 Construction of the Dataset

In order to identify firms that face liquidity shortages and to quantify what actions they take in response, we use narrative information contained in official corporate filings of publicly traded US companies. To do so, we search these filings for verbal discussions in which the reporting firms explicitly disclose that they are engaging in cash preservation efforts or imply that they have recognized an acute need to do so. That is, we define liquidity shortages as cases in which firms believe that they need to limit their outflows of cash and other liquid assets, presumably because they find themselves unable to generate additional cash inflows at reasonable costs.

Importantly, this definition avoids the need to measure actual and desired levels of liquidity and instead emphasizes the reporting firms' own views and their resulting decisions. Furthermore, it does not require a distinction between cases that are driven by changes in the desired amounts of liquidity and cases that result from changes in the amount actually held. Finally, it also allows us to remain agnostic about what fundamental factors may

actually cause the relevant changes in these two variables. This feature is particularly desirable given that our general aim is to capture both shortages that were driven by the large shock of the financial crisis and shortages that occurred in the absence of such a shock.

For the actual construction of the dataset, we first download all annual and quarterly corporate reports available in the EDGAR database maintained by the SEC.[6] In this way, we obtain approximately 920,000 documents filed by almost 19,000 different firms over the past 20 years. Second, we extract the texts contained in these reports and break them down into sentences using a disambiguation algorithm from the computational linguistics literature.[7] Finally, as discussed above, we identify firms facing liquidity shortages as those whose reports contain sentences in which they explicitly state that they are engaging in cash preservation efforts or that they are actively exploring ways to do so.

The main reason why we run our searches on both annual and quarterly filings is that firms may sometimes discuss actions only in the context of a specific quarter and then not mention them again in the corresponding annual report. However, since this approach may in principle also lead to double counting, we eventually aggregate our data to annual frequency and record each type of information only once. For example, if a firm reports a liquidity shortage both in the first quarter and the corresponding annual filing of the same fiscal year, we do not count this shortage twice.

Given the large number of reports and the amount of data each one of them contains, identifying liquidity-constrained firms in this way is a computationally intensive task. At least conceptually, however, two of the three steps outlined above involve relatively little discretion. In particular, the electronic infrastructure maintained by the SEC allows us to download all of the relevant files, and the sentence disambiguation technique established by the computational linguistics literature mainly requires the choice of an appropriate training dataset.[8] However, the step that is significantly more elaborate than the other two is the one that considers the texts of all reports and separates out only those sentences in which firms actually disclose that they are taking measures to preserve their cash resources. The main reason why this step of the data construction process is particularly complicated is that the millions of sentences contained in the 920,000 reports are are too numerous to be read manually and too unstructured for a fully automatic classification. We approach this challenge by combining manual readings with an automatic pre-selection step. More precisely, we first search for a number of flexibly written verbal patterns that typically

[6] We consider all documents filed as 10-K, 10-Q, 10-K405, as well as the corresponding small business form types.

[7] Before the sentence boundary detection, we also automatically remove tables and other non-text contents such as HTML formatting instructions. The sentence disambiguation algorithm that we use is that of Kiss and Strunk (2006) as implemented in the Python Natural Language Toolkit (NLTK). An alternative method would have been to simply assume that sentences end whenever a full stop occurs. However, this can lead to mistakes given that full stops also have other functions such as indicating abbreviations and separating decimals from integers.

[8] The disambiguation algorithm that we use is trained on the Wall Street Journal data of the Penn Treebank. The language of this training dataset is relatively similar to that used by firms in their corporate reports.

indicate efforts of cash preservation. Then, reading all sentences that contain at least one of these patterns, we manually remove false positives.
The exact verbal patterns that we use for the automatic pre-selection step are summarized in Table 1, with round brackets indicating that an element is optional and square brackets denoting that only one of the expressions they contain must occur. Generally speaking, the patterns aim to capture cases in which firms explicitly disclose both their goal to preserve cash and the specific measures they are taking in order to do achieve that goal. Furthermore, to avoid biases with respect to the disclosed reactions, the patterns only reflect very general grammatical structures that do not favor any specific one.

Table 1: Verbal Patterns Used to Pre-Select Candidate Sentences About Liquidity Shortages

1. to [preserve/conserve] *(up to 5 additional words)* [cash/liquid/liquidity] *(up to 3 additional words)* [we/ the company/the corporation]

1. [we/the company/the corporation] *(up to 20 additional words)* to [preserve/conserve] *(up to 5 additional words)* [cash/liquid/liquidity]

Notes: The Table shows verbal patterns used to pre-select candidate sentences that are likely to contain information about firm-level liquidity shortages and the corresponding reactions. Round brackets indicate optional elements. Square brackets denote that only one of the elements they contain is required to occur. The element *word* is a placeholder satisfied by any single word.

To illustrate the types of sentences that we obtain in this way, Table 2 displays a number of examples that survived both the automatic pre-selection step and the manual removal of false positives. In line with the above definition of liquidity shortages, all of these sentences reflect cases in which the reporting companies are taking specific actions to preserve their cash resources or have at least recognized a need to do so. Furthermore, the sentences establish causality in the sense that they identify the liquidity shortage as the cause and the reactions as the corresponding effects. That is, they state that the companies experienced a need to preserve their existing liquid assets and, in order to achieve that goal, they took a specific action. Thus, the sentences imply that the reporting firms would not have taken the specific actions in a counter-factual world without the reported liquidity shortage.

Table 2: Sample Sentences About Liquidity Shortages

Sample Sentence	Company Name	Fiscal Year
"During 1993, the company restricted capital expenditures in order to conserve cash."	Calmat Co.	1993
"We reduced our selling and marketing personnel significantly in an attempt to reduce operating expenses and to conserve cash."	Network Engines Inc.	2001
"On July 25, 2012, we suspended our dividend payments to preserve cash as a result of our operating performance."	Radioshack	2012

Notes: The Table shows extracted corporate report sentences about firm-level liquidity shortages and the corresponding reported reactions. The sentences were extracted from the corporate filings using the pre-selection patterns shown in Table 1 and a subsequent manual reading step.

In addition to identifying the firms that face liquidity shortages, we also use the extracted sentences to quantify exactly what measures these firms take in response to them. In particular, we consider all sentences in which firms explicitly disclose at least one such measure and group them into a number of different categories. Given the clear grammatical structures of the extracted sentences, we generally refer to the measures as the reported reactions to liquidity shortages. For ease of reading, we introduce and discuss the classification categories that we use for this below, in the context of the actual analysis.
Finally, we augment our narrative data with some additional information about the reporting companies. In particular, we extract industry membership in the form of SIC codes directly from the filings and also link the data to Compustat. For this, we collect the header information contained in each of the reports and link the two data sources at the level of fiscal years and the Compustat gvkey identifier.

1.2.2 Dataset Properties and Firm Characteristics

Our final dataset contains a total of 1,338 reported firm-level liquidity shortages, 1,289 of which we are able to link to Compustat. These shortages are reported by 687 different US companies as identified by their Compustat gvkeys. Out of all 1,338 reported firm-level shortages, 435 occurred in the context of the financial crisis, whereas 903 occurred during the remaining sample years. Thus, our sample contains sufficient numbers of observations for a direct comparison between the two time periods.
Table 3 presents some descriptive statistics of those liquidity constrained firms that we are able to link to Compustat, as well as corresponding values for all firms that do not

report a liquidity shortage in a given year. It shows that the firms we identify as liquidity constrained tend to be smaller than their unconstrained peers, both in terms of total assets and employees. Furthermore, they tend to be less profitable, have lower cashflows and typically do not pay dividends. They also have higher *levels* of cash than the unconstrained firms, consistent with a precautionary motive of cash holding and existing evidence such as that of Opler et al (1999) and Denis and Sibilkov (2010).

Table 3: Observable Characteristics of Liquidity Constrained and Unconstrained Firms

	Total Assets	Number of Employees	Firm Age	Firm Profitability	Cashflow Ratio	Cash Ratio	Dividend Payer	KZ Index	Hadlock-Pierce Index	Whited-Wu Index
					Unconstrained Firms					
Mean	6,055.62	7.51	13.0	-0.71	-0.96	1.27	0.34	-93.86	-2.84	-0.04
Std. dev	62,287.89	34.26	12.76	32.58	36.38	80.96	0.48	4162.11	1.26	7.17
Min	0.0	0.0	0.0	-9859.67	-9935.33	-0.23	0.0	-1121176.0	-5.67	-525.38
25%	23.71	0.09	4.0	-0.02	-0.05	0.02	0.0	-7.85	-3.51	-0.24
50%	158.08	0.52	8.0	0.08	0.04	0.05	0.0	-0.62	-3.02	-0.1
75%	973.87	3.2	18.0	0.15	0.09	0.19	1.0	1.26	-2.32	0.09
Max	3,771,200.0	2,545.21	63.0	215.73	20.75	18,941.0	1.0	62,800.86	7.22	2,355.99
N	204,486	171,233	204,520	195,814	171,573	198,406	204,520	140,014.0	207,782.0	162,758.0
					Liquidity-Constrained Firms					
Mean	1,466.92	2.93	12.61	-3.19	-3.9	1.19	0.15	-85.84	-2.44	0.06
Std. dev	5,592.69	9.85	12.2	71.89	77.72	5.53	0.36	445.77	1.54	0.84
Min	0.0	0.0	0.0	-2,507.0	-2,558.0	0.0	0.0	-9,716.12	-5.52	-14.95
25%	8.0	0.03	5.0	-0.68	-0.7	0.03	0.0	-23.34	-3.35	-0.17
50%	50.81	0.13	8.0	-0.11	-0.14	0.12	0.0	-3.64	-2.67	0.01
75%	604.9	1.14	16.0	0.06	0.02	0.44	0.0	0.74	-1.84	0.26
Max	8,0448.9	107.0	61.0	10.52	2.0	102.48	1.0	826.2	5.26	9.55
N	1,289	1,233	1,289.	1,221	1,095	1,277	1,289	1,041.0	1,292.0	1,038.0

Notes: The sample includes all firms matched to Compustat at the gvkey/fiscal-year level. Constrained firms are those that report a liquidity shortage in a given fiscal year. All variable definitions are shown in Table 10 in the appendix.

1.2.3 Reporting Incentives and Reliability of the Narrative Information

Given the role narrative discussions from corporate filings play in our analysis, one important question is how reliable they are. We are able to investigate this question in a very direct way by testing whether the actions firms report in the extracted sentences are consistent with the true behavior of closely related observable variables. As we show below, we find strong evidence suggesting that this is the case. Moreover, in addition to this direct type of evidence, there are a number of theoretical arguments as well as existing empirical findings suggesting that the texts of corporate filings do contain relevant information

One of the arguably most important theoretical arguments in favor of the informational content of the narrative discussions we use in this paper is that regulation enforced by the Securities and Exchange Commission legally requires the reporting firms to provide accurate and complete information in their filings.[9] Accordingly, firms that misrepresent such information risk being investigated and reprimanded by the SEC. Furthermore, as shown by Karpoff and Lou (2010) based on a sample of 454 specific cases, such firms are also severely punished by financial markets and typically lose around 20% of their value when an SEC investigation is announced. In addition, the Sarbanes-Oxley Act establishes personal liability at the senior management level, requiring that both CFOs and CEOs of the reporting firms vouch in writing for the truthfulness and completeness of the filings.[10]

Another factor that imposes discipline on firms' narrative reporting is the fact that their verbal discussions are only credible if they are consistent with related observable information and externally audited accounting measures. In the context of our analysis, this mainly applies to the specific reactions firms take in response to the reported shortages. In addition, specialized financial analysts that typically follow only small numbers of firms often know even more intricate facts about the reporting firms that allow them to better assess the credibility of the narrative information.

With the texts of corporate filings becoming a more popular source of data in applied research, empirical evidence for their informational content is also growing. For example, Balakrishnan et al (2010) document that the texts can be used to form stock portfolios that outperform the market, Li et al (2013) show that a text-based measure of competition correlates positively with more standard measures, and chapter two of this thesis shows that narrative discussions of price changes extracted from corporate filings can be used to construct an index that closely co-moves with aggregate inflation.

Finally, another important aspect is that we are mainly interested in comparing those liquidity shortages that occurred during the financial crisis to cases that tend to arise in more normal times. Therefore, even if the firms' reporting behavior were to some extent biased with respect to specific types of reactions, our main conclusions would remain valid as long as such potential biases are approximately constant over time.

[9] See the discussion in Griffin (2003).

[10] See the discussions in Geiger and Taylor (2003) and Marden et al (2003).

1.3 Full Sample Analysis of the Reported Liquidity Shortages and Reactions

Having discussed the construction of the dataset, we now investigate the reported reactions to firm-level liquidity shortages using the full sample of all extracted sentences. The reason for this is that it allows us to provide a general overview of the reported firm behavior and to gauge the validity of our narrative approach. As a first test, we investigate if our results are qualitatively consistent with the ones prior studies have obtained using more standard techniques. Furthermore, we perform statistical tests of the informational content of the narrative discussions by linking them to closely related firm-level observables.

1.3.1 Classification of the Reported Reactions

In order to quantify the reactions that firms report in the extracted narrative discussions, we group them into a number of distinct categories that reflect various real and financial policies. Generally speaking, we define these categories such that they both reflect the wording that firms generally use and correspond to relatively well-defined economic concepts. Furthermore, we take into account how well the resulting categories map into variables that can be constructed from observable accounting measures and thus help validate the informational content of the narrative discussions.

The resulting categories are displayed in Table 4, together with some sample expressions firms regularly use to describe them. Importantly, while we find that these and other sample expressions are often very informative, we do not rely on them in the actual classification. Instead, to ensure a high level of accuracy, we code each sentence manually based on all information it contains. The exact rules that we follow when assigning reported reactions to specific categories are shown in the appendix.

Furthermore, we also define three rules that determine whether or not we actually code reported behavior as a *reaction* to a liquidity shortage. First, to exclude hypothetical considerations, we do not code actions that firms were merely contemplating or planning to implement in the future. Second, we code only cases for which the sentences clearly indicate that the liquidity shortage was the cause and the reaction was the corresponding effect. Third, as long as this requirement is satisfied, we code both absolute changes and deviations from prior plans.[11]

[11] For example, a company may either report that it reduced capital expenditures in response to a liquidity shortage, or that the liquidity shortage caused it to raise capital expenditures by less than initially planned . In our analysis, both of these cases appear as reductions in capital expenditures due to a liquidity shortage.

Table 4: Categories of Real and Financial Reactions to Liquidity Shortages

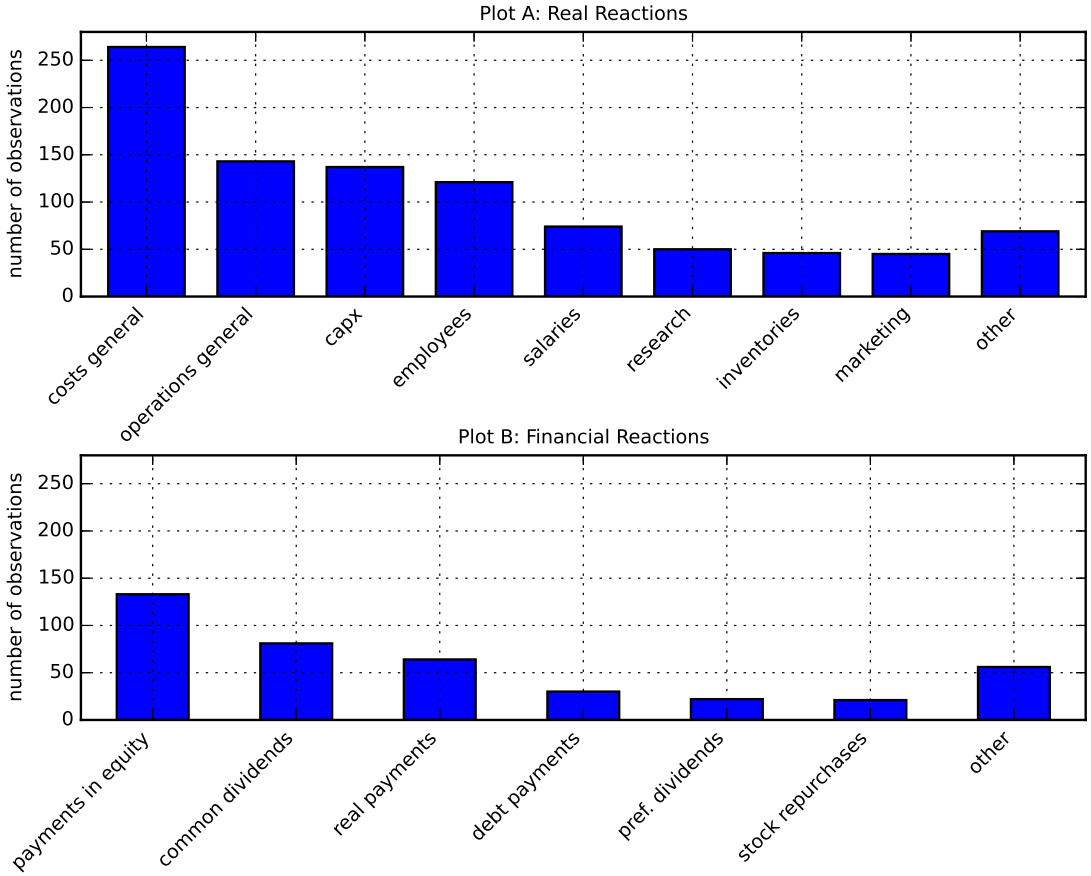
Category	Short Name	Sample Expressions
Real Reactions		
General Cost Reduction Efforts	Costs General	'cost cutting measures' , 'expense reductions' , 'cost containment plan'
General Extent of Operations	Operations General	'reduction of operations' , 'temporary shutdown ' , 'scope of its operations'
Capital Expenditures	Capital Expenditures	'capital expenditures' , 'capital spending' , 'capital investment'
Number of Employees	Employees	'workforce' , 'headcount' , 'number of employees'
Salaries and Wages per Employee	Salaries and Wages	'salary reductions' , 'salary cuts', 'freeze on wage rates'
Research and Development Efforts	Research	'research and development' , 'innovation efforts' , 'product development'
Inventory Holdings	Inventories	inventory reductions', 'tighter inventory levels' , 'reduce excess inventory'
Marketing and Sales Efforts	Marketing	'promotional expenditures' , 'sales and marketing expenses' , 'advertising expenses'
Financial Reactions		
Payments in Equity and Related Securities	Equity Pay	'stock in lieu of cash' , 'common stock to pay' , 'options as a means of compensation'
Dividends on Common Stock	Common Dividends	'dividends on its common stock' , 'cash dividends' , 'dividend payments'
Payments Related to Real Transactions	Real Payments	'payments to some of our suppliers' , 'deferred payment to consultants' , 'salary deferment program'
Payments Related to Debt	Debt Payments	'debt service' , 'quarterly interest' , 'payments of interest'
Dividends on Preferred Stock	Preferred Dividends	'dividends on [...] preferred stock' , 'preferred dividends' , 'preferred dividend payments'
Stock Repurchases	Stock Repurchases	'market purchases of treasury stock' , 'share repurchases' , 'stock repurchase program'

Notes: The Table shows the categories into which the reported reactions to liquidity shortages were grouped. For each category, the column 'Sample Expressions' displays a selected number of corresponding phrases that regularly occur in the assigned respective sentences. The sample expressions are not exhaustive. The sentences were assigned manually to the categories based on all information they contain. The sentences were assigned to multiple categories whenever that best reflected their contents. The exact rules applied when coding the sentences are shown in the appendix.

1.3.2 Distribution of the Reported Reactions

Having coded and thus quantified the reported reactions in this way, we begin our analysis by exploring their full sample distributions. As Plot A of Figure 2 shows, the most commonly reported real response to liquidity shortages are general cost and expense reduction programs, followed by permanent or temporary downsizing efforts, reductions in capital expenditures, and reductions in the number of employees. In addition, firms sometimes also react by reducing salaries or wages per employee, r&d expenses, inventories and marketing efforts.

Figure 2: Reported Reactions to Liquidity Shortages: Manual Grouping



Notes: The Figure illustrates the distribution of reported real and financial reactions to liquidity shortages. Sample expressions and category definitions are shown in Table 4. The sentences were assigned to multiple categories whenever that best reflected their contents. The exact rules applied when coding the sentences are shown in the appendix. All sentences were manually assigned to the categories based on all information they contain.

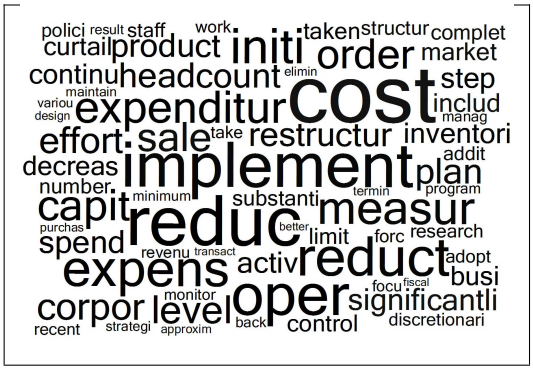
Next, turning to the reported financial reactions, we find that the measures used most frequently to overcome cash shortages are equity-based payments and reductions in the dividends paid on common stock. Moreover, firms also react by delaying various types of payments and, in a limited number of cases, by reducing stock repurchases. In addition,

we find that financial reactions as a whole appear to be much less common than reactions in terms of real variables: While we count 936 unique cases of real reactions to liquidity shortages, the corresponding number for financial reactions is significantly lower at only 400.

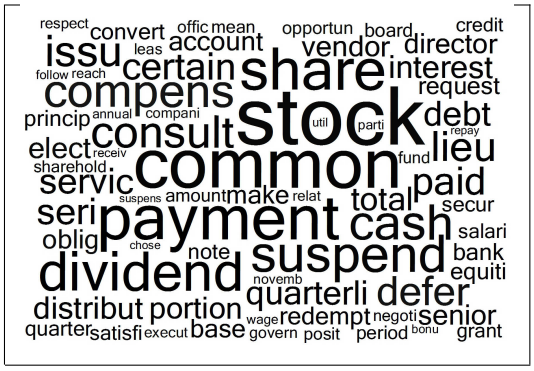
To complement these findings, we also construct word-clouds that reflect differences in word-frequencies between sentences describing real and financial reactions, respectively (Figure 3) . While these graphical representations are by construction less precise than the groupings shown above, they do not require manual classification and can therefore serve as robustness checks. We find that the word clouds are generally consistent with the above results.

Figure 3: Reported Reactions to Liquidity Shortages: Word Clouds

Plot A: Reported Real Reactions



Plot B: Reported Financial Reactions



Notes: The word-clouds illustrate how often firms use specific words when describing their real and financial reactions to liquidity shortages. Plot A reflects only sentences in which firms describe at least one real reaction. Plot B reflects only sentences in which firms describe at least one financial reaction. The size of each displayed word reflects the number of times it occurs in this category, relative to its average frequency. All numbers as well as a list of very common English language words not related to the information of interest (stop words) are not displayed.

Overall, the findings presented here suggest that liquidity shortages affect firm behavior along many dimensions, with adjustments occurring both in terms of real and financial policies. Furthermore, while the number of different real reactions firms is relatively large, the main financial tools firms apply to overcome liquidity shortages seem to be changes driven by adjustments in treasury stock holdings and common dividends.

1.3.3 Testing the Informational Content and Validity of the Reported Information

Given that much of the analysis presented in this paper is based on verbal information firms provide in their annual and quarterly corporate filings, we next run a number of probit regressions that link the reported narrative information to closely related accounting variables. This allows us to assess the extent to which the extracted narrative information

is consistent with true behavior. The observables that we use for this are shown in Table 5, together with the corresponding reaction categories.
In terms of the real reactions, we are able to define related observable measures for all eight categories and can thus provide relatively detailed cross-checks. For the reported financial reactions, matters are a bit more complicated as not all of them have observable counterparts.[12] However, both common and preferred dividends are directly measurable, and treasury stock holdings can be used to capture both payments made in equity and share repurchases. Furthermore, we construct two measures that are related to the fractions of total expenses and interest expenses paid within the year, respectively.

Using these correspondences, we run probit regressions that relate the probability of a decease in the respective observable variables to narrative statements that such a decrease has taken place. Thus, if the narrative information is generally informative about true firm behavior, we would expect the coefficient on the relevant narrative statements to be statistically significant and positive. For the real reactions, Table 6 shows that this is indeed the case for all categories except for wages and salaries per employee. However, even in that case, the coefficient has the expected sign.

[12]For example, payments related to real transactions are difficult to disentangle from other types of payments.

Table 5: Reported Reactions and Corresponding Observables

Reaction Category	Reaction Group	Compustat Definition of Assigned Observable
Costs General	Real	*Total Operating Expenses* (*xopr*)
Operations General	Real	*Total Assets* (*at*)
Capital Expenditures	Real	*Capital Expenditures* (*capx*)
Employees	Real	*Employees* (*emp*)
Salaries and Wages	Real	*Staff Expense* (*Income Account*) (*xlr*) , if missing *Total Staff Expense* (*xstf*)
Research and Development	Real	*Research and Development Expense* (*xrd*)
Inventories	Real	*Total Inventories* (*invt*)
Marketing Efforts	Real	*Advertising Expense* (*xad*)
Equity Pay / Share Repurchases	Financial	*Treasury Stock* (*tstk*)
Common Dividends	Financial	$\frac{\textit{Common Cash Dividends (dvc, if missing dv, if missing 0)}}{\textit{Common Shares Outstanding (csho)}}$
Preferred Dividends	Financial	*Preferred Dividends* (*dvp, if missing* : 0)
Percentage of Expenses Paid	Financial	$\frac{\textit{Accrued Expenses (xacc)} - \textit{Lagged Accrued Expenses (xacc)}}{\textit{Operating Expenses (xopr)}}$
Percentage of Interest Paid	Financial	$\frac{\textit{Interest Paid Net (intpn)}}{\textit{Total Interest and Related Expenses (xint)}}$

Notes: The Table shows a subset of the reaction categories defined in Table 4, together with formal definitions of closely related variables constructed from observable accounting measures obtained from Compustat. Brackets contain the Compustat item codes used to construct the respective variables. All variables are winsorized at the 5% level to reduce the impact of outliers.

Table 6: Observable Decreases and Reported Decreases in Selected Real Variables: Probit Regressions

VARIABLES	Decrease in Operating Expenses	Decrease in Total Assets	Decrease in Capital Expenditures	Decrease in Number of Employees	Decrease in Wages & Salaries	Decrease in R&D Expenses	Decrease in Total Inventories	Decrease in Advertising Expenses
Narratively Reported Decrease	0.97***	0.92***	0.82***	0.98***	0.32	1.63***	1.02***	0.90***
in the Related Reaction Variable	(12.68)	(8.80)	(7.47)	(8.47)	(1.42)	(9.04)	(5.41)	(4.66)
Constant	-0.73***	-0.62***	-0.52***	-0.70***	-1.81***	-1.25***	-0.61***	-1.43***
	(-276.53)	(-241.47)	(-200.97)	(-258.78)	(-399.12)	(-374.49)	(-210.85)	(-401.65)
Observations	198,839	204,805	170,583	164,232	29,537	69,765	133,895	49,853

The Table shows probit regressions that investigate to what extent narrative corporate filing information about specific variables is consistent with the true behavior of these variables. Definitions of the dependent variables and the corresponding reaction categories are shown in Table 5. The sample includes all firms matched to Compustat at the gvkey/fiscal-year level. T-statistics based on robust standard errors reported in parentheses. *** , ** and * denote statistical significance at the 1% , 5% and 10% levels, respectively.

This general finding is also true for the financial variables (Table 7), albeit at somewhat lower significance levels and with the exception of the preferred dividend category. However, there are two main reasons why the lower levels of statistical significance do not necessarily imply that the informational content of statements about these variables is not high. First, the financial reactions are generally less frequent. Thus the regressions are based on a much smaller number of narrative statements about these variables. Second, as argued above, the conceptual similarity between the reported reactions and the respective observables is generally somewhat weaker for the financial variables and thus more prone to measurement error. In fact, in the case of common dividends - for which have both a large number of observations and a clean link between observable and narrative category - the coefficient is highly significant. Thus, the reported information about both real and financial variables is generally consistent with their true behavior.

Table 7: Observable Decreases and Reported Decreases in Selected Financial Variables: Probit Regressions

VARIABLES	Decrease in Treasury Stock	Decrease in Common Dividends	Decrease in Preferred Dividends	Decrease in Percentage of Expenses Paid	Decrease in Percentage of Interest Paid
Narratively Reported Decrease	0.49**	1.66***	0.24	0.45***	0.79***
	(2.10)	(8.64)	(0.73)	(2.82)	(3.45)
Constant	-1.07***	-0.82***	-1.01***	-0.81***	-0.71***
	(-236.72)	(-187.34)	(-205.53)	(-297.40)	(-264.17)
Observations	117,501	104,617	95,670	268,737	263,837

The Table shows probit regressions that investigate to what extent narrative corporate filing information about specific variables is consistent with the true behavior of these variables. Definitions of the dependent variables and the corresponding reaction categories are shown in Table 5. The sample includes all firms matched to Compustat at the gvkey/fiscal-year level. T-statistics based on robust standard errors reported in parentheses. *** , ** and * denote statistical significance at the 1% , 5% and 10% levels, respectively.

1.4 Differences Between the Financial Crisis and Normal Times

In the previous section we presented a full-sample overview of the firm-level reactions to liquidity shortages and provided empirical evidence for the general validity of our narrative approach. Building on these findings, we now turn to the main question of the paper and investigate potential differences between those shortages that resulted from the financial crisis and those that occurred during more normal times.[13] For this, we exploit the fact

[13]To assess the robustness of our findings, we also repeat the main analysis using two different subsamples. First, we exclude the dotcom episode, which we define as fiscal years 2001-2003. Second, we exclude all financial firms. The results of these two robustness tests are shown in the appendix. We find that all of

that our approach infers causality directly from the narrative corporate filing discussions and therefore is not limited to the arguably exogenous shock associated with the financial crisis. The three main questions that we address here are:

1. Was the composition of firms that experienced liquidity shortages during the crisis unique?

2. Did the firms that experienced liquidity shortages during the crisis react differently than those firms that experienced shortages during more normal times?

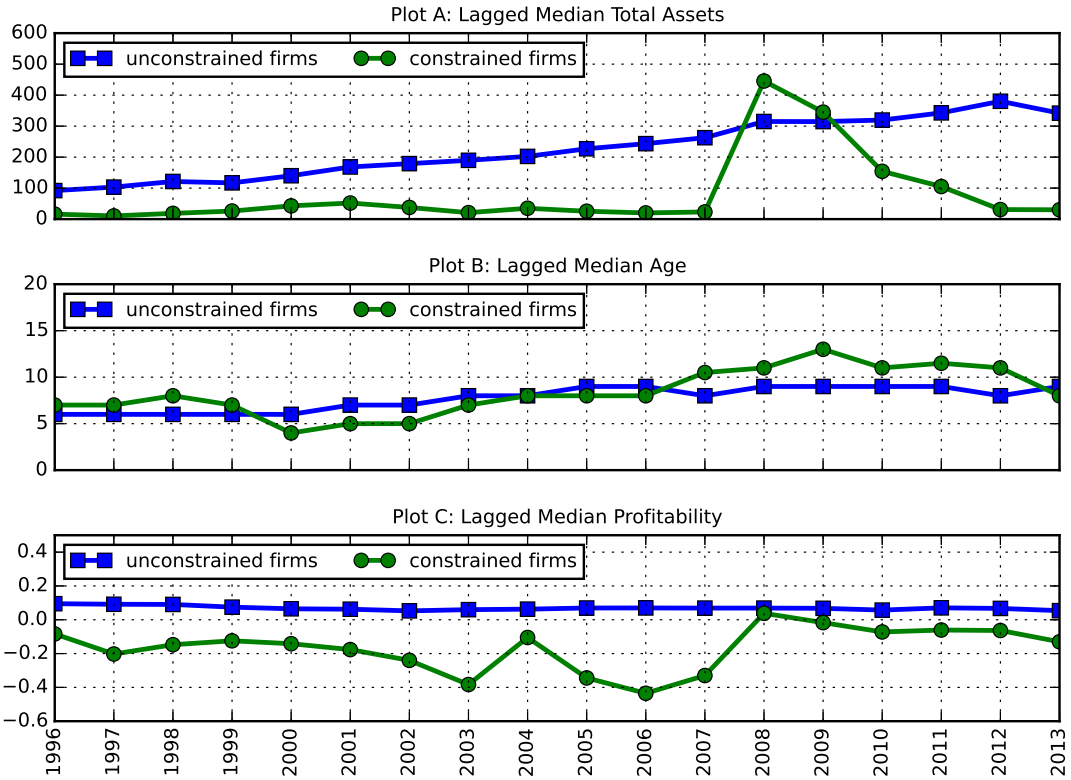
3. Were the shortages firms faced during the crisis unusually severe?

1.4.1 Characteristics of Constrained and Unconstrained Firms Over Time

The first aspect that we analyze here is the extent to which the characteristics of the liquidity constrained firms vary across the two economic regimes that we are interested in. In other words, we investigate whether the firms that reported shortages in the context of the financial crisis have different observable characteristics than those that did so in other times. This is important for our analysis because we would generally expect to see such differences if the shortages occurring across the two regimes do indeed have fundamentally different causes. Furthermore, any variation in firm characteristics that we do observe here could potentially affect the types of reactions firms choose and may thus be important control variables in our extended analysis.
We begin by plotting lagged median values of selected real characteristics for each fiscal year, distinguishing between those firms that report liquidity shortages in that year and those that do not (Figure 4). For the largest parts of the sample and in line with the general descriptive statistics shown above, we find that liquidity constrained firms are relatively small and less profitable than their unconstrained peers. However, these differences are not constant over the entire sample period and decrease drastically during the years of the financial crisis.

our main results are qualitatively robust to these changes.

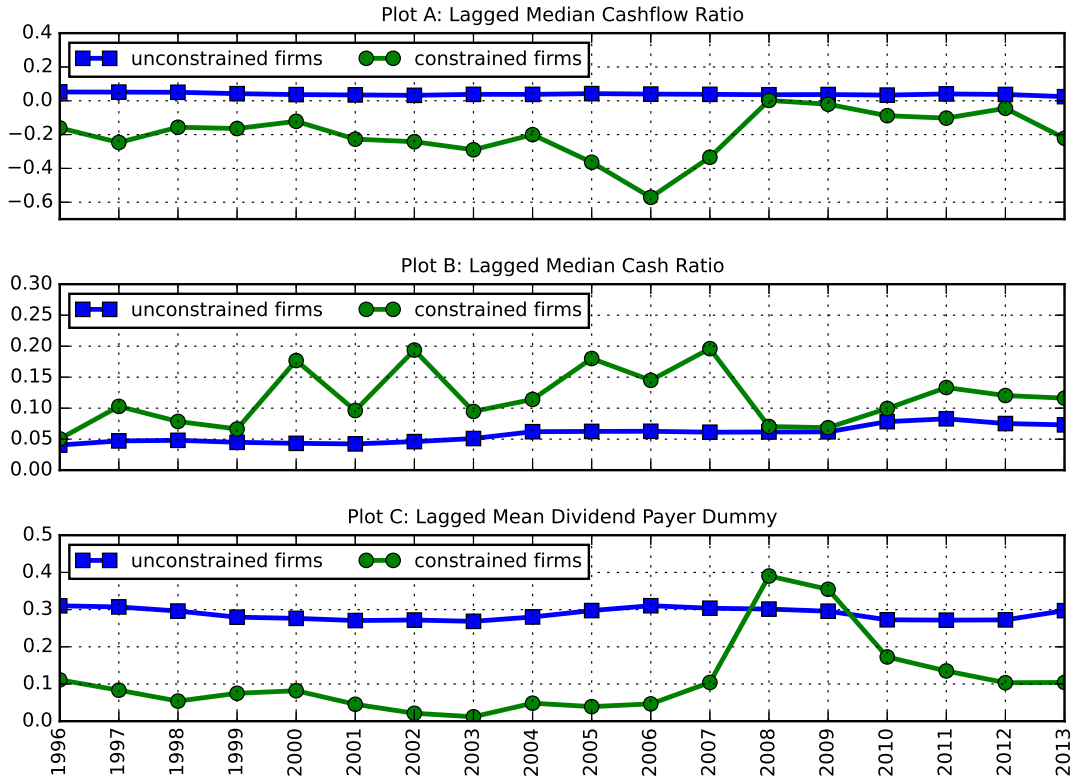
Figure 4: Time-Variation in Selected Real Firm Characteristics



Notes: The Figure shows lagged median real characteristics of constrained and unconstrained firms. Constrained firms are those that report a liquidity shortage in a given fiscal year. All variable definitions are shown in Table 10 in the appendix.

Next, to assess the affected firms' financial characteristics, we also plot median cash flow ratios, median normalized cash holdings, and the fractions of firms that paid dividends in the periods immediately prior to the reported liquidity shortages. (Figure 5). Here, too, we observe that the differences between the two groups of firms decrease sharply during the years of the financial crisis. More precisely, while constrained firms typically have strongly negative cash flows, higher cash holdings and are less likely to pay dividends than the unconstrained firms, all of these differences vanish almost entirely during fiscal years 2008 and 2009.

Figure 5: Time-Variation in Selected Financial Firm Characteristics



Notes: The Figure shows lagged median financial characteristics of constrained and unconstrained firms. Constrained firms are those that report a liquidity shortage in a given fiscal year. All variable definitions are shown in Table 10 in the appendix.

Overall, these results suggests that firms reporting liquidity shortages during the crisis were very different from those that did so in more normal times. This is consistent with the view that observations occurring in these two periods were driven by fundamentally different causes. Moreover, the observable characteristics suggest that, while the crisis did not affect a specific and well-identified subset of firms, shortages occurring in more normal times are closely associated with small firms, low profitability and large cash outflows.

This behavior lends support to our initial motivation that liquidity shortages occurring during the crisis were unique and thus their properties may or may not extend to other periods. In this respect, they highlight the need for additional evidence from non-crisis periods that can help assess external validity and provide a more general analysis. Furthermore, the findings suggest that this is especially true if we believe that the actions firms take to preserve liquidity may depend on their own fundamentals or the underlying causes of the liquidity shortages they face.

Another implication is that observable firm characteristics by themselves may not always be sufficient for identifying those types of firms that are most likely to be financially constrained. In fact, the link between observable characteristics and liquidity shortages appears to be weakest during the financial crisis, i.e. the one event on which much of the

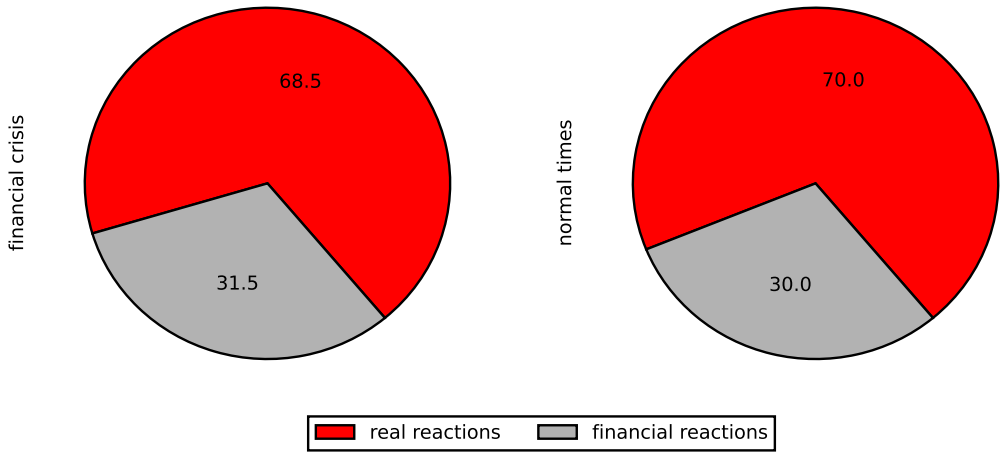
recent empirical research on the real implications of financial constraints has focused.

1.4.2 Reported Reactions During the Crisis and Normal Times

Having investigated what types of firms reported liquidity shortages during the crisis and more normal times, we next turn to the actions that these firms took in response. For this purpose, we define the financial crisis as the years 2008-2010 and normal times as all remaining years in the sample. As illustrated by Figure 1 above, this grouping reflects both the pronounced drop in credit supply that occurred at the beginning of the crisis (e.g. Becker and Ivashina, 2014) as well as the sharp increase in firms that reported liquidity shortages in the shock's immediate aftermath.

Using this grouping, we first investigate potential differences at a relatively high level, distinguishing only between the relative importance of real and financial reactions, respectively. As Figure 6 illustrates, we find that the two periods are remarkably similar in this regard. Both during the financial crisis and the more normal times do financial and real reactions account for about 30% and 70% of all implemented measures, respectively. In other words, while both the shock associated with the crisis and the groups of firms affected by the resulting liquidity shortages are arguably unique, the relative frequencies at which these firms use financial and real measures to overcome the shortages they face are not.

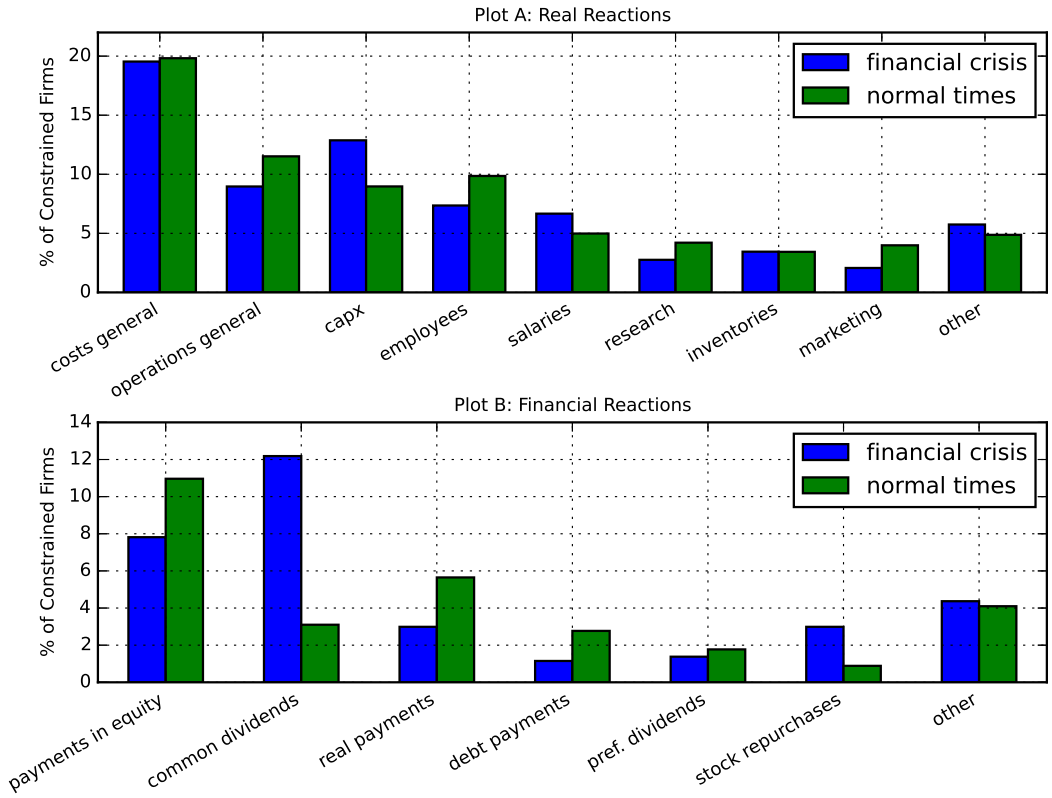
Figure 6: Time-Variation in the Relative Importance of the Real and Financial Reaction Categories



Notes: The Figure illustrates the relative importance of real and financial reactions during the financial crisis and normal times. The financial crisis is defined as fiscal years 2008-2010. Normal times are defined as all other fiscal years in the sample. Sample expressions and category definitions are shown in Table 4. The sentences were assigned to multiple categories whenever that best reflected their contents. The exact rules applied when coding the sentences are shown in the appendix. All sentences were manually assigned to the categories based on all information they contain.

These initial findings suggest that neither the underlying causes of the liquidity shortages that firms experience nor their own fundamentals are systematically related to the reactions they implement. However, even though the relative frequencies of the real and financial reaction categories are very similar across shortages that occur during the crisis episode and normal times, there still may exist significant variation *within* these two groups. In Figure 7 we investigate this graphically.

Figure 7: Time-Variation in Reported Reactions within the Real and Financial Categories



Notes: The Figure illustrates the distribution of reported real and financial reactions to liquidity shortages, distinguishing between the period of the financial crisis and normal times. The financial crisis is defined as fiscal years 2008-2010. Normal times are defined as all other fiscal years in the sample. Sample expressions and category definitions are shown in Table 4. The sentences were assigned to multiple categories whenever that best reflected their contents. The exact rules applied when coding the sentences are shown in the appendix. All sentences were manually assigned to the categories based on all information they contain.

For the real reactions (Plot A), we discover only limited variation across the two time periods. For example, while reductions in capital expenditures seem to be a slightly more common means of cash preservation during the crisis, this difference does not seem to be of a qualitative nature. However, within the group of financial reactions (Plot B), one very pronounced difference emerges: Reductions in common dividends were much more frequently used to overcome liquidity shortages during the the financial crisis than they

were during normal times.

Overall, the analysis shown so far suggests that the reactions used by firms to overcome liquidity shortages during the crisis and normal times are very similar. This is somewhat surprising given that, as we have argued above, the large shock to financial market conditions associated with the crisis is unique in our sample. Furthermore, the types of firms that experienced shortages during normal times and the crisis, respectively, had very different observable characteristics. We now turn to a more formal regression analysis to investigate if those relatively small differences in reactions that do exist are statistically significant, and - to the extent that they are - if observable firm characteristics can account for them.

For this purpose, we use the full sample of liquidity shortages and consider all reaction types that the firms report at least 50 times. We then run probit regressions of dummy variables indicating the occurrence of a particular reaction on a crisis indicator as well as a number of optional firm-level controls.[14] The controls that we use here are total assets, the number of employees, age, cashflows, cash holdings, profitability, Tobin's Q and a measure of asset tangibility.[15] Furthermore, we include a set of dummies that captures whether or not firms paid dividends in the prior period, had positive R&D expenditures, and invested in fixed capital.

Table 8 shows the estimation results for the real reaction variables. More precisely, for each variable it presents one specification that only includes the financial-crisis indicator as well as a second specification that controls for the firm-characteristics discussed above. We generally interpret the former type of specification as a test of whether or not a given reaction was more common in the financial crisis. The second type of specification, on the other hand, allows us to investigate the role of the firm characteristics in explaining the potential differences in reactions between the two time-periods.

[14] By running separate regressions for each action we implicitly assume independence across these actions. This is an assumption that we could in principle have relaxed by estimating multinomial models using categories for specific actions and their combinations. However, the number of observations in many of the resulting categories would have been very low and thus resulted in imprecise estimates.

[15] All variable definitions are shown in Table 10 in the appendix.

Table 8: Firm Characteristics and Probabilities of Specific Real Reactions: Multivariate Probit Regressions

VARIABLES	costs general		operations general		capital expenditures		employees		salaries		research	
Financial crisis	-0.01	-0.23*	-0.14	-0.02	0.21*	-0.14	-0.16	0.09	0.15	0.20	-0.19	-0.13
	(-0.10)	(-1.80)	(-1.13)	(-0.16)	(1.79)	(-0.90)	(-1.17)	(0.55)	(1.10)	(1.10)	(-1.08)	(-0.54)
Age		-0.01		-0.00		0.01**		-0.01**		0.01		-0.00
		(-1.51)		(-0.76)		(2.09)		(-2.49)		(0.88)		(-0.36)
Total Assets		0.00		0.00		0.00		0.00		0.00***		-0.00
		(1.54)		(1.06)		(0.07)		(0.95)		(2.87)		(-0.95)
Employees		-0.02***		0.00		0.01		0.00		-0.00		0.00
		(-2.70)		(0.52)		(1.06)		(0.25)		(-0.48)		(0.29)
CAPX ratio		-1.28		1.66**		-0.46		0.46		-3.43		-1.03
		(-1.62)		(2.54)		(-0.51)		(0.44)		(-1.61)		(-0.49)
R&D ratio		-0.02**		0.00		-0.00		-0.00		0.00***		0.00
		(-2.45)		(0.02)		(-0.98)		(-0.51)		(3.71)		(1.25)
Cashflow ratio		-0.02***		0.19		1.94		0.02		0.02***		-0.01
		(-2.95)		(0.65)		(1.56)		(0.71)		(2.66)		(-0.94)
Cash ratio		0.73**		0.20		-0.17		0.31		-0.60		0.96**
		(2.43)		(0.57)		(-0.47)		(1.03)		(-1.19)		(2.56)
Profitability		0.05***		-0.18		-1.61		-0.00		0.02		0.01
		(2.70)		(-0.62)		(-1.27)		(-0.03)		(0.74)		(1.16)
Tobin's q		-0.08		-0.00		-0.07		0.01		0.12**		0.02
		(-1.64)		(-0.09)		(-1.33)		(0.31)		(2.37)		(0.33)
Tangibility		0.58*		0.03		0.80**		-0.65		-0.38		-0.24
		(1.75)		(0.06)		(2.19)		(-1.36)		(-0.71)		(-0.33)
Positive Dividends		0.06		-0.57**		0.21		-0.57**		-1.07**		-0.18
		(0.29)		(-2.31)		(0.96)		(-1.97)		(-1.97)		(-0.64)
Observations	1,338	863	1,338	863	1,338	863	1,338	863	1,338	863	1,338	863

The Table shows the results of multivariate probit regressions that investigate which time-periods and firm characteristics are associated with specific real reactions to liquidity shortages. The sample includes all firms matched to Compustat at the gvkey/fiscal-year level. T-statistics based on robust standard errors are reported in parentheses. *** , ** and * denote statistical significance at the 1% , 5% and 10% levels, respectively. Constants are included but not reported. All variable definitions are shown in Table 10 in the appendix. All firm characteristics are lagged by one period and winsorized at the 5% level to reduce the impact of outliers.

As Table 8 shows, the regression results for the real reaction variables confirm our general interpretation of the graphical analysis presented above. In particular, the baseline regressions that do not include the lagged firm characteristics show that the financial crisis dummy does typically not carry a coefficient that is significantly different from zero. One exception are the reactions in terms of capital expenditures, but even here we reject the null-hypothesis only at the 10% level. Thus, overall, there appears to be no significant difference in terms of which real reactions firms use to overcome liquidity shortages during the crisis and more normal times, respectively.

The regression specifications that do contain the additional firm-level controls generally suggest a similar conclusion, but instead of capital expenditures, we now find that the importance of adjustments in salaries is significantly different at the 10% level between the two time periods. In terms of the significance of the firm characteristics, we find that a number of coefficients are statistically different from zero at higher levels, but we do not interpret these coefficients further given that they do not reflect random variation.

For the financial reactions, we report the corresponding regressions in Table 9. Here, too we confirm the findings implied by the graphical analysis. In particular, we find that the only highly significant difference is that reductions in common dividends were far more commonly used as a reaction to liquidity shortages during the crisis than they were during normal times. However, we also find that this significance disappears once the firm-level characteristics are included. The particular characteristics that appear to explain this difference are the dummy that indicates that a firm actually paid dividends in the prior period, and the amount of cash firms held.

Again, we note that the coefficients shown in these regressions do not warrant a causal interpretation, but the role of prior dividend policy is consistent with the above finding that only firms that experienced shortages during the crisis actually had positive dividend payments that they could reduce. Furthermore, another interesting observation is that firms with a higher Tobin's Q (which approximately corresponds to the market-to-book ratio) more frequently use payments made in equity instead of cash. This is consistent with the existing finding that firms are more willing to issue equity when the market value of their shares is high.[16]

[16]For discussions see Graham and Harvey (2001) and Baker and Wurgler (2002).

Table 9: Firm Characteristics and Probabilities of Specific Financial Reactions: Multivariate Probit Regressions

VARIABLES	Payments in Equity		Common Dividends		Real Payments	
Financial crisis	-0.19	0.16	0.70***	0.23	-0.30*	-0.08
	(-1.34)	(0.81)	(5.37)	(1.18)	(-1.73)	(-0.39)
Age		-0.02**		0.01		0.00
		(-2.29)		(1.18)		(0.31)
Total Assets		-0.00**		-0.00		-0.00
		(-2.22)		(-1.55)		(-0.91)
Employees		0.01		-0.01		-0.00
		(0.39)		(-0.90)		(-0.01)
CAPX ratio		-0.93		1.08		-0.23
		(-0.88)		(1.03)		(-0.20)
R&D ratio		-0.00		-8.22		-0.18**
		(-1.26)		(-1.26)		(-2.27)
Cashflow ratio		0.06		2.25		0.22
		(1.01)		(1.56)		(0.83)
Cash ratio		-0.85**		-4.07***		-0.45
		(-2.42)		(-3.00)		(-0.91)
Profitability		-0.05		-0.96		-0.12
		(-0.90)		(-0.73)		(-0.44)
Tobin's q		0.18***		0.02		0.16***
		(3.57)		(0.13)		(2.99)
Tangibility		0.16		-0.55		0.09
		(0.36)		(-1.04)		(0.21)
Positive Dividends		0.27		1.11***		-0.56
		(0.90)		(4.90)		(-1.29)
Observations	1,338	863	1,338	863	1,338	863

The Table shows the results of multivariate probit regressions that investigate which time-periods and firm characteristics are associated with specific financial reactions to liquidity shortages. The sample includes all firms matched to Compustat at the gvkey/fiscal-year level. T-statistics based on robust standard errors are reported in parentheses. *** , ** and * denote statistical significance at the 1% , 5% and 10% levels, respectively. Constants are included but not reported. All variable definitions are shown in Table 10 in the appendix. All firm characteristics are lagged by one period and winsorized at the 5% level to reduce the impact of outliers.

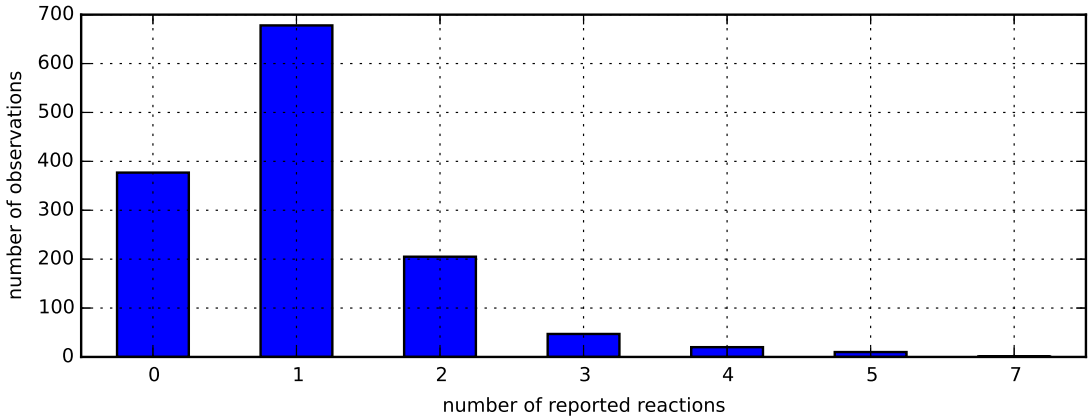
1.4.3 Severity of the Reported Liquidity Shortages

Another important dimension along which liquidity shortages reported during the crisis and normal times may differ is their intensity or severity. Unfortunately, given that neither desired nor actual liquidity holdings of firms are easily measurable, the concept of severity is difficult to capture using standard types of data. However, one potential proxy that we can construct directly from the extracted narrative discussions is the number of reactions firms take in response to a given shortage. For instance, we might argue that shortages

to which firms respond with exactly one reaction are on average less severe than those to which they simultaneously respond in two or more ways.

To illustrate the extent to which our dataset contains variation in this measure, Figure 8 plots its distribution based on all extracted narrative discussions. It shows that, while the majority of observations reflect shortages for which firms report exactly one reaction, it also contains approximately 250 cases in which they report two or more. Thus, assuming that the numbers of reactions firms implement do indeed proxy the intensity of the reported shortages, we can in principle use them to investigate whether shortages that occurred during the financial crisis were less or more severe than those that occurred during the more normal economic times.

Figure 8: Distribution of the Number of Reported Reactions



Notes: The Figure illustrates how frequently firms respond to the reported liquidity shortages with a given number of reactions. Cases with zero reactions are those in which firms disclosed that they were facing a liquidity shortage but had not yet reacted to it. Both real and financial reactions are included.

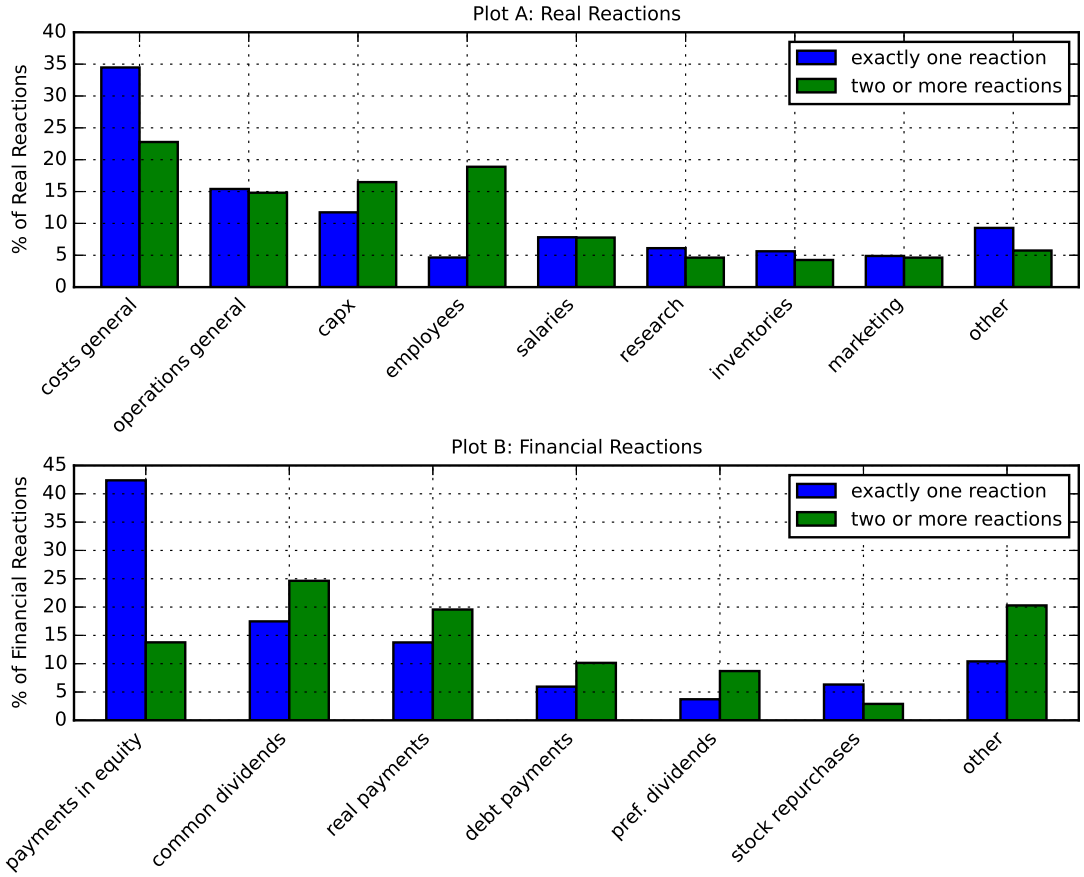
However, before we actually apply the measure in this way, we first assess its validity by exploiting the fact that existing research makes relatively clear predictions about which types of *financial* reactions are particularly costly. In particular, we first group the reported liquidity shortages according to whether firms responded to them with one specific reaction or more. Then, we investigate whether those financial reactions that prior work identifies as particularly costly are indeed concentrated in the latter group. One of the existing results that we consider for this informal test of validity is that firms tend to avoid dividend adjustments whereas they perceive share repurchases as a measure that can be used in a very flexible manner.[17] Furthermore, we investigate if a higher number of reported reactions is also more commonly associated with both changes in preferred dividends and delayed or decreased payments related to debt transactions.

For this purpose, Plot (B) of Figure 9 shows the financial reactions firms use in response to liquidity shortages, distinguishing between cases in which firms respond with one or more specific reactions, respectively. It illustrates that our hypothesized measure of severity

[17]Brav, Graham and Harvey (2005), Leary and Michaely (2011), Bliss et al (2015).

is consistent with prior results on the relative costliness of different types of financial reactions. In particular, while we find that share repurchases are more commonly used as reactions to those shortages we classify as less severe, reductions in debt payments and adjustments to common and preferred dividends are concentrated in the group that we argued is likely to contain the more severe cases.[18] This suggests that our proxy does indeed capture a property of the reported liquidity shortages that we can reasonably think of as severity.

Figure 9: Real and Financial Reactions to Liquidity Shortages by Severity



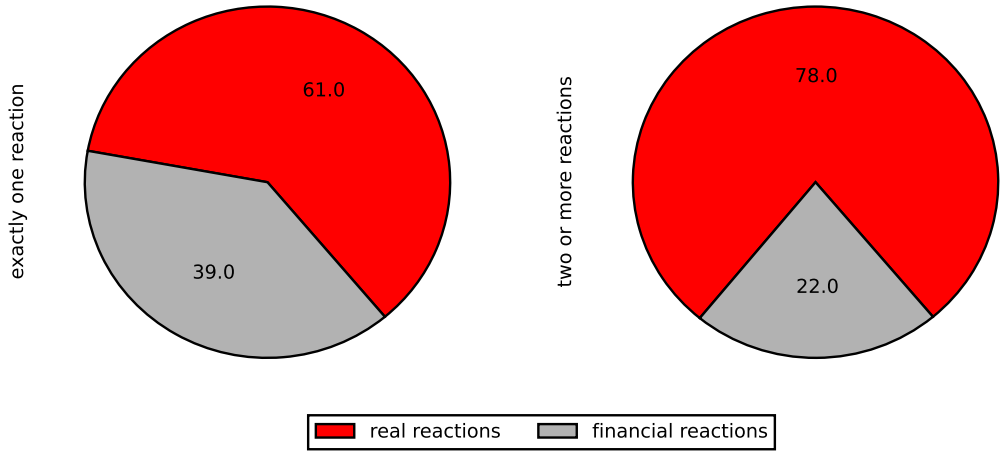
Notes: The Figure illustrates the distribution of reported real and financial reactions, distinguishing between shortages to which firms responded with exactly one reaction or more reactions, respectively. Sample expressions and category definitions are shown in Table 4. The sentences were assigned to multiple categories whenever that best reflected their contents. The exact rules applied when coding the sentences are shown in the appendix. All sentences were manually assigned to the categories based on all information they contain.

For completeness, we also investigate differences in the distributions of the reported *real* reactions between the more and less severe shortages, respectively. As documented in plot (A) of Figure 9, we find that reductions in both capital expenditures and the size of the

[18]Given the small number of observations for each one of these categories, we do not run separate regressions to test if they are statistically significant. However, in a joint regression four all four measures we find that the difference is statistically significant.

workforce are concentrated in the arguable more severe shortages. This suggests that they are more costly than the other real reactions. Furthermore, when comparing the relative frequencies of all real and financial reactions, respectively, we find that real reactions are particularly frequent in the more severe cases (Figure 10). One potential explanation for this finding is that firms may only use real reactions to the extent that they are not able to offset the shortages in terms of financial measures alone.

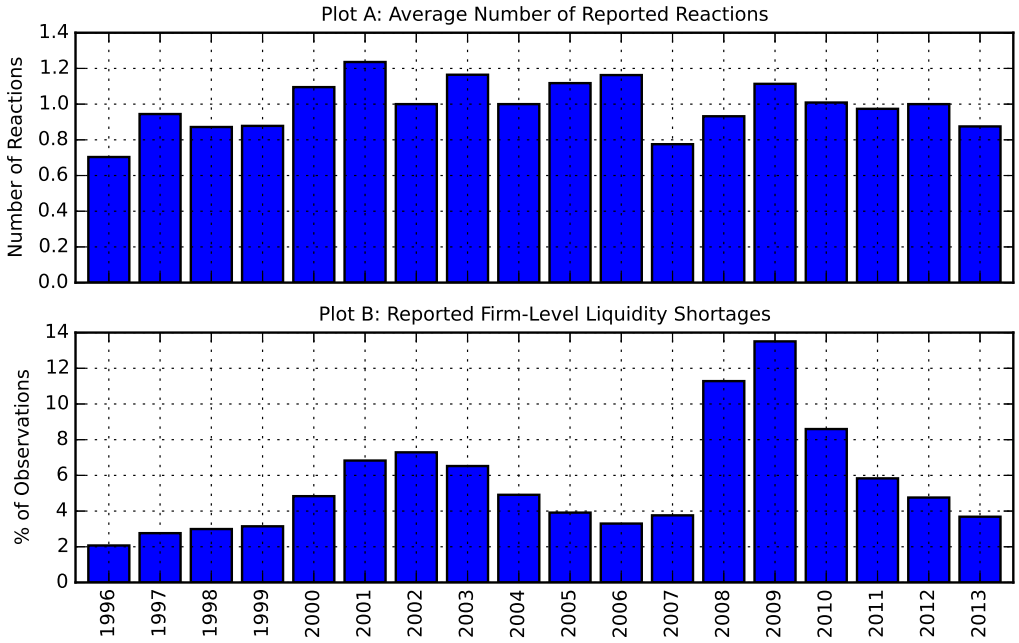
Figure 10: Relative Importance of Real and Financial Reactions by Severity



Notes: The Figure illustrates the relative importance of real and financial reactions, distinguishing between shortages to which firms responded with exactly one reaction or more reactions, respectively. Sample expressions and category definitions are shown in Table 4. The sentences were assigned to multiple categories whenever that best reflected their contents. The exact rules applied when coding the sentences are shown in the appendix. All sentences were manually assigned to the categories based on all information they contain.

Finally, having argued that the number of associated reactions is a valid proxy for the severity of the reported shortages, we now actually use it to investigate if those shortages that arose during the crisis were unusually severe. For this, we graphically examine the distributions over time of both the number of firms that reported liquidity shortages, and how many reactions these constrained firms implemented on average. As Figure 11 reveals, we find that the sharp increase in firms experiencing shortages during the crisis is not associated with a corresponding increase in the average number of reported reactions per firm. This suggests that the crisis-related shortages were not unusually severe at the level of individual firms.

Figure 11: Reported Liquidity Shortages and Numbers of Reported Reactions Over Time



Notes: The Figure illustrates the number of firms reporting liquidity shortages in their corporate filings (Plot B) as well as the average number of actions these firms implemented in response (Plot A). The reported liquidity shortages are obtained as described in Section 1.2. Liquidity shortages reported for fiscal years ending between January and May are assigned to the preceding calendar year.

1.5 Conclusion

During the late-2000 financial crisis a large number of firms faced acute shortages in liquidity and, as a result, adjusted important real policies such as their employment and investment decisions. In this paper, we have investigated to what extent these crisis-related shortages were different from cases that occurred during more normal times and thus were not driven by a large financial shock. In particular, we have explored differences in the composition of the affected firms, the responses that they implemented, and the severity of the shortages they experienced.

For our analysis, we first searched 20 years worth of archived corporate filings to extract more than 1,300 narrative discussions of firm-level liquidity shortages. Then, we quantified the contents of these discussions and tested their informational content by linking them back to closely related observables. This allowed us to provide evidence for the validity of our approach by showing that its full-sample results are generally consistent with those of existing studies, and by documenting that the narratively reported decisions are in line with the firms' true behavior.

In terms of the nature of the financial crisis, we found that, while firms reporting shortages during normal times tend to be unprofitable, small and subject to large cash outflows, many of the firms that reported shortages during the crisis were relatively large and prof-

itable. We also foud that, even though the types of firms experiencing shortages during the crisis and normal times were very different, the real actions they took in response were not. Finally, we documented that, while the number of firms that experienced liquidity shortages during the crisis was very high, the shortages they experienced were not unusually severe at the level of the individual firms.

One of the main implications of our findings is that observable firm characteristics may not always be informative for identifying financially constrained firms, especially during empirically important crisis episodes. Furthermore, the findings also suggests that the role of financial constraints during a crisis may not be well captured by structural models in which a single firm characteristic such as size is the main feature that distinguishes constrained from unconstrained firms during both crises and normal times.[19] Finally, our results also highlight important ways in which liquidity shortages associated with financial crises do not appear to differ from those that occur during more normal times, even though the latter are not caused by large financial shocks and arise in very different types of firms.

1.6 Appendix

1.6.1 Descriptions and Coding Rules of the Reported Reaction Categories

General Cost Reduction Efforts This variable reflects cases in which firms disclose general cost reduction efforts without stating explicitly which types of costs they reduce.

Capital Expenditures This variable captures reductions in investments in fixed assets. For example, it reflects decreases in both initial purchases and maintenance of property, plant & equipment.

Research and Development Efforts This variable captures reductions in research and development efforts. Both the amount of money spent on these efforts and the number of employees dedicated to them are included in the definition.

Marketing and Sales Efforts This variable captures reductions in marketing, advertising and sales efforts. Both the amount of money spent on these efforts and the number of employees dedicated to them are included in the definition.

Salaries and Wages per Employee This variable captures reductions in labor cost per employee or hour worked. The types of costs reflected include salaries, wages and related items such as contributions to employee health and retirement plans. Layoffs or reductions in hours worked are not coded in this variable unless they coincide with changes labor costs per employee or hour worked. Changes in the form of payment, for example from cash to non-cash, are not coded in this variable unless they coincide with or imply changes in labor costs per employee or hours worked.

[19] An example of this is Khan and Thomas (2013).

Number of Employees This variable captures cases in which the reporting firms reduce the number of employees. It does not capture cases in which working hours per employee are reduced unless these cases are also associated with reductions in the size of the company's workforce.

Inventory Holdings This variable captures cases in which the reporting firms reduce the amount of inventories they hold.

Dividends on Common Stock This variable captures reductions in the amount of dividends the reporting firms declare on their outstanding common stock. It only reflects cases in which the value of the dividend changes. For example, changes from cash to in-kind dividends or vice versa are not coded unless the amounts or values of the respective dividends also change. Changes in dividend amounts that are not explicitly associated with preferred shares are assumed to pertain to common equity and thus reflected by this variables.

Dividends on Preferred Stock This variable captures reductions in amount of dividends the reporting firms declare on their preferred stock. It only reflects cases in which the amount or value of the dividend changes. For example, changes from cash to in-kind dividends or vice versa are not coded unless the amounts or values of the respective dividends also change. Changes in dividend amounts are only assumed to pertain to preferred shares if firms explicitly state this. Dividends not described as relating to preferred stock are assumed to refer to common stock dividends and thus not captured by this variable.

Stock Repurchases This variable captures cases in which firms reduce the amount of repurchases of their own stocks.

Payments in Equity and Related Securities This variable captures cases in which firms make payments in equity or related instruments (e.g. warrants or options) instead of paying in cash.

Payments Related to Real Transactions This variable captures cases in which firms do not make or delay payments that are related to real transactions. Examples include salaries, wages and payments to vendors. Cases in which the corresponding payments are made on time but by means other than cash are not captured by this variable.

Payments Related to Debt This variable captures cases in which firms do not make or delay payments that are related to debt or credit transactions. Examples include interest or principal payments. Cases in which the corresponding payments are made on time but not in cash are not captured by this variable.

1.6.2 Definitions of Observable Firm Characteristics

Table 10: Definitions of Observable Firm Characteristics

Firm Characteristic	Compustat Definition
Total Assets	$Total\ Assets\ (at)$
Employees	$Employees\ (emp)$
Firm Age	$Current\ fiscal\ year - first\ fiscal\ year\ in\ Compustat,\ (at\ the\ gvkey\ level)$
Firm Profitability	$\frac{Operating\ income\ before\ depreciation\ (oibdp)}{Total\ Assets\ (at)}$
Cashflow Ratio	$\frac{Cashflow\ (oibdp-xint-txt-dvc)}{Total\ Assets\ (at)}$
Cash Ratio	$\frac{Cash\ (ch)}{Total\ Assets\ (at)}$
Dividend Payer	$Indicator\ variable\ for\ positive\ common\ dividends\ in\ cash\ (dvc)$
Tobin's q	$\frac{Market\ Value\ of\ Assets\ (at+csho*prcc-ceq-txdb)}{0.9*Total\ Assets\ (at)+0.1*Market\ Value\ of\ Assets\ (at+csho*prcc-ceq-txdb)}$
Tangibility	$\frac{Property,\ Plant\ and\ Equipment\ (ppent)}{Total\ Assets\ (at)}$

Notes: The Table shows definitions of observable firm characteristics used in the main analysis. Brackets contain the Compustat item codes used to construct the respective variables. All resulting variables are winsorized at the 5% level to reduce the impact of outliers.

1.6.3 Removal of False Positives

One important step in the construction of the dataset used in this paper is the manual removal of false positives, i.e. the removal of sentences that were discovered by the automatic pre-selection algorithm even though they actually do not describe realized firm-level liquidity shortages. To illustrate the types of sentences we remove in this step, Table 11 provides some examples as well as brief discussions that explain why we classify them as false positives.

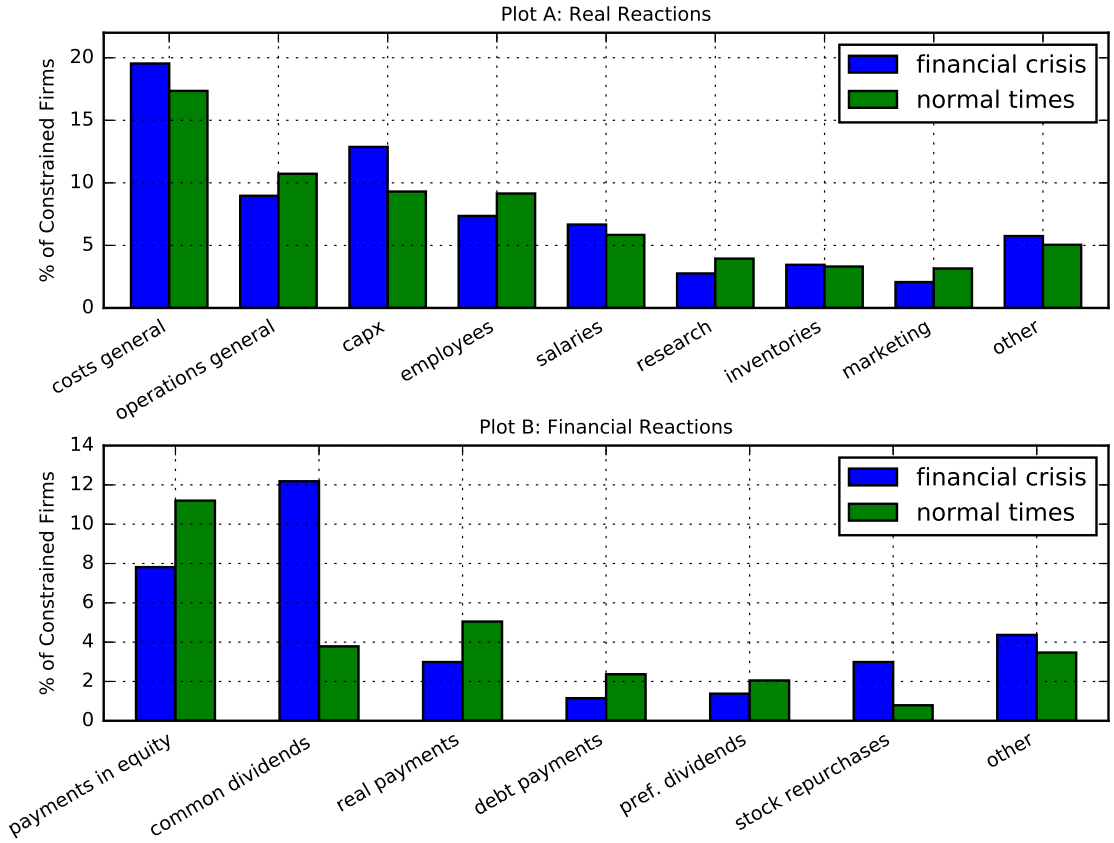
Table 11: Examples of Removed False Positives

Sample Sentence	Company Name	Reason for Exclusion
"In addition, the company can delay major capital investments or other funding requirements or pursue financing from other sources to preserve liquidity, if necessary."	American Water Works Company Inc.	The sentence describes the actions the reporting company could take in a hypothetical scenario. It does not imply that the company was experiencing a liquidity shortage in the reporting period.
"We believe we have the ability to conserve liquidity when economic conditions become less favorable through any number of strategies including curtailment of store expansion plans and cutting discretionary spending."	Tumi Holdings Inc	The sentence provides a general assessment of the reporting company's ability to preserve liquid assets. It does not imply that the company was experiencing a liquidity shortage in the reporting period.
"In addition, changes in the capital markets have resulted in a more stringent lending environment for solar and sapphire companies, which in turn has caused decreased spending within the industries we serve, as customers try to preserve their liquidity."	GT Advanced Technologies Inc	The sentence states that the customers of the reporting company were engaging in cash preservation efforts. It does not imply that the reporting company itself was also engaging in such efforts during the reporting period.

Notes: The Table shows corporate filing sentences that were extracted using the pre-selection patterns shown in Table 1 but then classified as false positives in a subsequent manual reading step.

1.6.4 Robustness: Excluding the Dotcom Episode

Figure 12: Time-Variation in Reported Reactions within the Real and Financial Categories (Dotcom Episode Excluded)



Notes: The Figure illustrates the distribution of reported real and financial reactions to liquidity shortages, distinguishing between the period of the financial crisis and normal times. Liquidity shortages that occurred during fiscal years 2001 to 2003 are excluded. The financial crisis is defined as fiscal years 2008-2010. Normal times are defined as all other fiscal years in the sample. Sample expressions and category definitions are shown in Table 4. The sentences were assigned to multiple categories whenever that best reflected their contents. The exact rules applied when coding the sentences are shown in the appendix. All sentences were manually assigned to the categories based on all information they contain.

Table 12: Firm Characteristics and Probabilities of Specific Real Reactions: Probit Regressions (Dotcom Episode Excluded)

VARIABLES	costs general		operations general		capital expenditures		employees		salaries		research	
Financial crisis	0.08	-0.13	-0.10	0.02	0.19	-0.19	-0.12	0.09	0.07	0.14	-0.16	-0.16
	(0.77)	(-1.01)	(-0.80)	(0.11)	(1.61)	(-1.25)	(-0.85)	(0.48)	(0.52)	(0.76)	(-0.89)	(-0.64)
Age		-0.01		-0.01		0.01*		-0.02**		0.00		-0.00
		(-0.79)		(-1.41)		(1.73)		(-2.44)		(0.30)		(-0.04)
Total Assets		0.00		0.00		0.00		0.00		0.00***		-0.00
		(1.40)		(1.21)		(0.10)		(1.03)		(2.93)		(-1.03)
Employees		-0.02***		0.01		0.01		0.00		-0.00		0.00
		(-2.62)		(0.87)		(1.12)		(0.46)		(-0.23)		(0.23)
CAPX ratio		-1.09		1.45*		-0.28		1.10		-4.62*		0.38
		(-1.21)		(1.91)		(-0.27)		(1.02)		(-1.83)		(0.20)
R&D ratio		-0.02*		0.00		-0.00		-0.00		0.00***		0.00
		(-1.93)		(0.35)		(-0.97)		(-0.89)		(3.73)		(0.42)
Cashflow ratio		-0.02***		0.19		2.66*		0.02		0.02**		-0.01
		(-3.11)		(0.60)		(1.87)		(0.76)		(1.97)		(-0.89)
Cash ratio		0.88***		0.24		-0.14		0.50		-0.62		0.65
		(2.59)		(0.63)		(-0.34)		(1.47)		(-1.16)		(1.54)
Profitability		0.04**		-0.18		-2.36		-0.01		0.01		0.01
		(2.29)		(-0.57)		(-1.63)		(-0.20)		(0.38)		(0.70)
Tobin's q		-0.08		-0.01		-0.08		-0.01		0.13**		0.01
		(-1.51)		(-0.12)		(-1.38)		(-0.19)		(2.25)		(0.14)
Tangibility		0.50		-0.17		0.60		-0.84		-0.30		-1.09
		(1.35)		(-0.31)		(1.51)		(-1.52)		(-0.50)		(-1.23)
Positive Dividends		0.14		-0.46*		0.25		-0.54*		-1.01*		-0.19
		(0.63)		(-1.87)		(1.14)		(-1.78)		(-1.91)		(-0.67)
Observations	1,069	680	1,069	680	1,069	680	1,069	680	1,069	680	1,069	680

The Table shows the results of multivariate probit regressions that investigate which time-periods and firm characteristics are associated with specific real reactions to liquidity shortages. The sample includes all firms matched to Compustat at the gvkey/fiscal-year level. Liquidity shortages that occurred during fiscal years 2001 to 2003 are excluded. T-statistics based on robust standard errors are reported in parentheses. *** , ** and * denote statistical significance at the 1% , 5% and 10% levels, respectively. Constants are included but not reported. All variable definitions are shown in Table 10 in the appendix. All firm characteristics are lagged by one period and winsorized at the 5% level to reduce the impact of outliers.

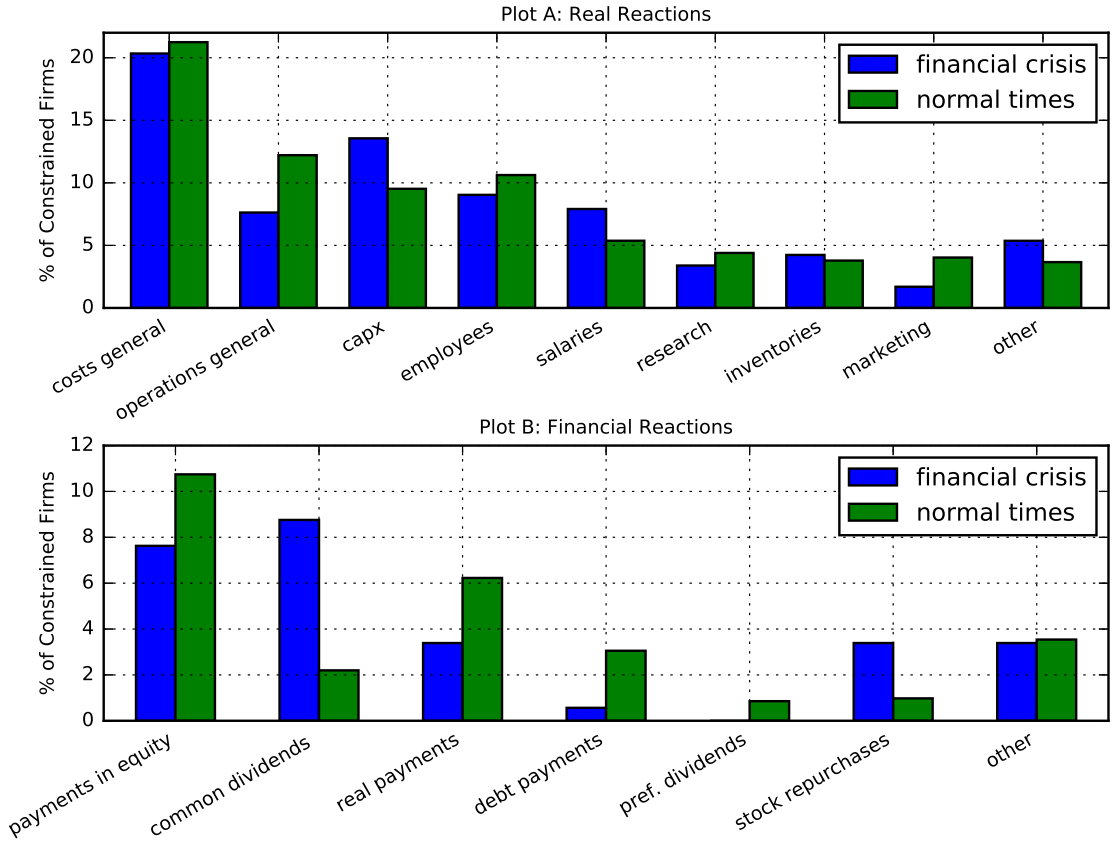
Table 13: Firm Characteristics and Probabilities of Specific Real Reactions: Probit Regressions (Dotcom Episode Excluded)

VARIABLES	Payments in Equity		Common Dividends		Real Payments	
Financial crisis	-0.20	0.09	0.61***	0.21	-0.24	-0.00
	(-1.44)	(0.45)	(4.75)	(1.01)	(-1.46)	(-0.00)
Age		-0.02**		0.01		-0.00
		(-2.21)		(1.18)		(-0.49)
Total Assets		-0.00**		-0.00		-0.00
		(-2.28)		(-1.58)		(-0.67)
Employees		0.00		-0.01		-0.00
		(0.24)		(-0.92)		(-0.20)
CAPX ratio		-1.41		0.75		0.08
		(-1.06)		(0.64)		(0.08)
R&D ratio		-0.00		-6.60		-0.19**
		(-1.23)		(-1.14)		(-2.03)
Cashflow ratio		0.06		3.61**		0.25
		(1.03)		(2.06)		(0.98)
Cash ratio		-0.75**		-4.06**		-0.34
		(-2.00)		(-2.41)		(-0.61)
Profitability		-0.05		-1.74		-0.19
		(-0.87)		(-1.07)		(-0.69)
Tobin's q		0.18***		0.07		0.12**
		(3.36)		(0.63)		(2.27)
Tangibility		0.35		-0.20		-0.26
		(0.69)		(-0.34)		(-0.57)
Positive Dividends		0.27		1.16***		-0.49
		(0.86)		(4.50)		(-1.10)
Observations	1,069	680	1,069	680	1,069	680

The Table shows the results of multivariate probit regressions that investigate which time-periods and firm characteristics are associated with specific financial reactions to liquidity shortages. The sample includes all firms matched to Compustat at the gvkey/fiscal-year level. Liquidity shortages that occurred during fiscal years 2001 to 2003 are excluded. T-statistics based on robust standard errors are reported in parentheses. *** , ** and * denote statistical significance at the 1% , 5% and 10% levels, respectively. Constants are included but not reported. All variable definitions are shown in Table 10 in the appendix. All firm characteristics are lagged by one period and winsorized at the 5% level to reduce the impact of outliers.

1.6.5 Robustness: Excluding Financial Firms

Figure 13: Time-Variation in Reported Reactions within the Real and Financial Categories (Financial Firms Excluded)



Notes: The Figure illustrates the distribution of reported real and financial reactions to liquidity shortages, distinguishing between the period of the financial crisis and normal times. Financial firms (SIC codes 6000 to 6999) are exlucded. The financial crisis is defined as fiscal years 2008-2010. Normal times are defined as all other fiscal years in the sample. Sample expressions and category definitions are shown in Table 4. The sentences were assigned to multiple categories whenever that best reflected their contents. The exact rules applied when coding the sentences are shown in the appendix. All sentences were manually assigned to the categories based on all information they contain.

Table 14: Firm Characteristics and Probabilities of Specific Real Reactions: Probit Regressions (Financial Firms Excluded)

VARIABLES	costs general		operations general		capital expenditures		employees		salaries		research	
Financial crisis	-0.03	-0.20	-0.27*	-0.08	0.21*	-0.12	-0.09	0.15	0.20	0.24	-0.12	-0.05
	(-0.28)	(-1.53)	(-1.88)	(-0.52)	(1.66)	(-0.75)	(-0.64)	(0.86)	(1.42)	(1.32)	(-0.66)	(-0.22)
Age		-0.01*		-0.01		0.01**		-0.02**		0.01		-0.00
		(-1.70)		(-0.84)		(2.01)		(-2.54)		(0.82)		(-0.18)
Total Assets		0.00		-0.00		-0.00		0.00		0.00*		-0.00
		(1.02)		(-0.48)		(-0.33)		(1.46)		(1.79)		(-0.64)
Employees		-0.02**		0.01		0.01		-0.00		-0.00		-0.00
		(-2.43)		(1.27)		(1.11)		(-0.09)		(-0.36)		(-0.07)
CAPX ratio		-1.19		1.67***		-0.46		0.50		-3.44		-1.02
		(-1.48)		(2.58)		(-0.50)		(0.47)		(-1.56)		(-0.51)
R&D ratio		-0.02**		-0.00		-0.00		-0.00		0.00***		0.00
		(-2.43)		(-0.10)		(-0.95)		(-0.55)		(3.67)		(1.17)
Cashflow ratio		-0.02***		0.24		1.74		0.03		0.02**		-0.01
		(-3.06)		(0.69)		(1.39)		(0.57)		(2.57)		(-1.00)
Cash ratio		0.75**		0.32		-0.11		0.28		-0.61		1.19***
		(2.39)		(0.90)		(-0.29)		(0.89)		(-1.18)		(3.08)
Profitability		0.05**		-0.24		-1.39		-0.01		0.02		0.01
		(2.36)		(-0.66)		(-1.08)		(-0.20)		(0.70)		(0.73)
Tobin's q		-0.09*		0.01		-0.08		0.01		0.12**		0.02
		(-1.91)		(0.19)		(-1.36)		(0.27)		(2.32)		(0.43)
Tangibility		0.40		0.18		0.76**		-0.79		-0.46		-0.06
		(1.13)		(0.37)		(2.02)		(-1.57)		(-0.82)		(-0.08)
Positive Dividends		0.01		-0.48**		0.20		-0.56*		-1.13*		-0.19
		(0.04)		(-1.99)		(0.90)		(-1.89)		(-1.88)		(-0.65)
Observations	1,173	810	1,173	810	1,173	810	1,173	810	1,173	810	1,173	810

The Table shows the results of multivariate probit regressions that investigate which time-periods and firm characteristics are associated with specific real reactions to liquidity shortages. The sample includes all firms matched to Compustat at the gvkey/fiscal-year level, except for financial firms (SIC codes 6000 to 6999). T-statistics based on robust standard errors are reported in parentheses. *** , ** and * denote statistical significance at the 1% , 5% and 10% levels, respectively. Constants are included but not reported. All variable definitions are shown in Table 10 in the appendix. All firm characteristics are lagged by one period and winsorized at the 5% level to reduce the impact of outliers.

Table 15: Firm Characteristics and Probabilities of Specific Real Reactions: Probit Regressions (Financial Firms Excluded)

VARIABLES	Payments in Equity		Common Dividends		Real Payments	
Financial crisis	-0.19	0.12	0.66***	0.27	-0.29	-0.10
	(-1.25)	(0.57)	(4.00)	(1.31)	(-1.59)	(-0.45)
Age		-0.03**		0.01		0.01
		(-2.31)		(0.92)		(0.54)
Total Assets		-0.00**		-0.00		-0.00
		(-2.24)		(-0.39)		(-0.65)
Employees		0.01		-0.01		0.00
		(0.34)		(-1.01)		(0.08)
CAPX ratio		-0.96		1.27		-0.21
		(-0.87)		(1.14)		(-0.19)
R&D ratio		-0.00		-8.85		-0.19**
		(-1.43)		(-1.32)		(-2.27)
Cashflow ratio		0.10		2.02		0.33
		(0.99)		(1.12)		(1.22)
Cash ratio		-0.98**		-4.06**		-0.35
		(-2.56)		(-2.54)		(-0.70)
Profitability		-0.11		-0.43		-0.26
		(-1.00)		(-0.27)		(-0.89)
Tobin's q		0.17***		0.02		0.17***
		(3.12)		(0.13)		(3.07)
Tangibility		0.10		-0.85		0.13
		(0.23)		(-1.40)		(0.30)
Positive Dividends		0.35		1.10***		-
		(1.11)		(4.59)		-
Observations	1,173	810	1,173	810	1,173	686

The Table shows the results of multivariate probit regressions that investigate which time-periods and firm characteristics are associated with specific financial reactions to liquidity shortages. The sample includes all firms matched to Compustat at the gvkey/fiscal-year level, except for financial firms (SIC codes 6000 to 6999). T-statistics based on robust standard errors are reported in parentheses. *** , ** and * denote statistical significance at the 1% , 5% and 10% levels, respectively. Constants are included but not reported. All variable definitions are shown in Table 10 in the appendix. All firm characteristics are lagged by one period and winsorized at the 5% level to reduce the impact of outliers.

Chapter Two: Quantified Narrative Evidence on the Price-Setting Behavior of Public Companies

2.1 Introduction

Assumptions about price-setting behavior are at the heart of a large class of widely-used macroeconomic models. Among other things, they determine how well these models can fit aggregate fluctuations and whether or not they can reproduce the empirically observed delayed real effects of nominal shocks. In addition, the price-setting behavior of firms also has direct practical implications for central banks. Only if they have a good understanding of the price-setting process, can they be confident that their policy choices will actually lead to the desired outcomes. Even with the wealth of micro-level price data that has driven much of the field's recent progress, however, it has remained difficult to address some fundamentally important topics such as the main drivers of price changes and the mechanisms that can prevent firms from implementing them.

In an effort to better understand these aspects of the price-setting process, we use a type of data not previously considered by the related literature: narrative evidence contained in annual corporate reports. We exploit this evidence systematically, searching 20 years of archived documents for explicit discussions of price decisions and then manually quantifying their contents. The resulting dataset reflects 1,949 important pricing decisions taken by 983 different firms and encodes unique causal information that cannot be captured by realized prices alone. Some of our main findings include that the causes of price changes are highly asymmetric, that price-setting changes over the business cycle, and that real rigidities are an important feature of the reported pricing decisions.

The population of firms that we consider in this study includes all companies who are registered with the US Securities and Exchange Commission (SEC) and thus have to prepare the reports from which we construct the dataset. While the regulation that determines whether or not a company falls into this category is relatively complex, the general rule is that all firms whose securities are publicly traded in the US must register.[20] The firms that satisfy this criterion tend to be somewhat larger than the average US company, cover all major sectors, and account for a sizable fraction of US GDP.[21] They are also relatively international in the sense that many of them sell their products both within and outside of the US.

The types of pricing decisions that these firms discuss in their corporate reports tend to be those that had a notable impact on their overall business results in the relevant fiscal periods. In fact, much of the narrative evidence on price setting occurs in sections where firms discuss the development of important summary measures such as total revenues, sales

[20] A good starting point for readers interested in more detailed information on these registration and reporting requirements is the SEC financial reporting manual available via http://www.sec.gov/divisions/corpfin/cffinancialreportingmanual.shtml

[21] Using Compustat data of public companies, Gabaix (2011) reports that the sales of the top 50 US non-oil firms alone account for approximately 24% of US GDP.

or net income. Our results thus emphasize important price decisions taken by relatively large firms. They may therefore be particularly relevant for macroeconomists and policy makers interested in those types of changes and rigidities most likely to have notable aggregate effects.

The first specific aspect of price setting behavior that we investigate are the reasons that cause firms to change the prices of their products. Based on all discussions in which firms explicitly state at least one such reason, we discover a strong asymmetry between price increases and price decreases. While the former mostly result from increases in costs, the latter are largely driven by competitive pressures and considerations about sales volumes. Furthermore, distinguishing between different types of costs, we document that fluctuations in raw-material and commodity prices played a much larger role than variations in wages or salaries over the 20-year sample horizon.

Next, we consider the reported factors that prevented the firms from changing their prices. Here, we first document that almost all of the reported price rigidities describe cases in which firms experienced cost increases and had to decide about the extent to which they would pass them on to their customers. Our analysis suggests that the most important factor preventing them from doing so were competitive pressures, followed by weak aggregate demand conditions and pre-existing contracts. We also find that other regularly considered factors such as menu costs and customer antagonization do not appear to play an important role for the reported pricing decisions.

Given the very frequent mentions of competitive pressures in the context of both implemented price changes and price rigidities, we argue that strategic complementarities appear to be an important feature of the price-setting process. To investigate this further, we search for evidence of incomplete price adjustment after cost shocks, a phenomenon that has previously been interpreted as evidence for real rigidities.[22] This exercise reveals that approximately 51% of the reported rigidities do indeed describe situations in which firms adjusted their prices and yet describe these adjustments as incomplete relative to the increases in costs. Overall, the extracted discussions thus provide strong evidence for the presence of real rigidities.

Finally, in the last main section of the paper we link the narrative evidence to a number of closely related aggregate and firm-level observables. This allows us to assess the informational content of the narrative discussions and to establish some additional properties of the reporting firms price-setting behavior. Our first specific finding here is that the narrative evidence is closely related to the observable development of the aggregate price level. Thus, we argue that our results may be informative for central bankers targeting aggregate inflation.

Given that firms often cite costs as a driving force of pricing decisions, we also explore the co-movement between commodity prices and the numbers of extracted observations. This exercise reveals that the late-2000 commodity price boom caused an increase in both price changes and price rigidities. We interpret this as evidence of state-based pricing and

[22] For example, see Gopinath et al (2010) and Gopinath and Itskhoki (2011).

also argue that inflation would have been significantly more volatile had firms not faced the reported rigidities.[23] Furthermore, given that the numbers of both rigidities and price changes increase at the same time, we argue that price durations alone may not be an appropriate measure of rigidity.

Finally, using observable firm-level data obtained from Compustat, we also explore the extent to which narrative information is consistent with corresponding observables at the level of individual firms. In particular, we consider the reporting firms' revenues, cost of goods sold and profit margins to show that reported changes in these variables are consistent with their true behavior. Based on this finding, we then argue that the narrative information on which we base our main analysis is not only informative about the aggregate economic environment, but also about firm-specific developments that are closely related to pricing decisions.

This paper aims to contribute to a large empirical literature investigating the behavior of prices. Following the seminal work by Bils and Klenow (2004), one strand of this literature has used detailed micro-level pricing data to document important stylized facts such as the frequencies of price changes and their size distributions.[24] Much of this micro-level evidence is now regularly used to calibrate macroeconomic models, and to assess which ones are most successful at generating realistic price behavior. At a qualitative level, some of the most important findings of this literature are that prices change frequently, that both small and large price changes are common, and that price decreases occur even when aggregate inflation is strictly positive.[25]

The main conceptual difference between these studies and ours is that we do not focus on providing precise measurements of the statistical behavior of prices. Instead, by exploiting the unique type of narrative information contained in the corporate reports, our emphasis is on understanding *why* prices behave the way they do. That is, by using corporate reports to studying the factors that cause firms to change prices or prevent them from doing so, we aim to provide a clearer interpretation of the detailed stylized facts observable in micro-level price data. Of course, the narrative information contained in the corporate reports can also help better distinguish between competing price-setting models equally compatible with the raw price data.

A second strand of the empirical price setting literature that is also closely related to this paper has used surveys to directly ask firms about how they set prices. Following the influential study by Blinder (1991) and Blinder et al (1998) as well as some some

[23] A similar argument could also be made by just looking at the co-movement of raw material costs and price durations over time. However, by considering the information contained in the corporate reports, our exercise actually establishes a causal link between the two.

[24] Other influential studies using US data include Nakamura and Steinsson (2008), Klenow and Kryvtsov (2008) and Bils et al (2012). For evidence from Europe, also see Dhyne et al (2006) and Vermeulen et al (2012). Some earlier work based on less representative data includes Cecchetti (1986), Kashyap (1995), Lach and Tsiddon (1992) and Levy et al (1997). Recently, Swedish firm-level data on prices and costs has been used by Carlsson and Nordström Skans, and Cavallo (2013) introduced price data scraped from supermarket websites as an alternative to that collected by government agencies.

[25] Maćkowiak and Smets (2008) and Nakamura and Steinsson (2013) review many of the stylized facts obtained from micro-level price data and also discuss their main implications for widely used macro models.

less formal earlier work, such surveys have now been carried out for many countries.[26] Generally speaking, the fundamental motivation of these survey-based studies is very similar to the one of this paper. Both recognize that some aspects of price-setting are difficult to assess from realized prices alone, and that verbal information may be a useful alternative in those cases.

Beyond this fundamental similarity in motivation, however, the quantified narrative evidence we construct from the corporate reports is quite different from the survey data. Instead of asking companies very general questions about about how they typically set their prices, we consider only specific pricing decisions actually taken by the reporting firms. Thus, even though our dataset contains the type of verbal information previously available only from surveys, it retains the property of micro-level price data that each observation corresponds to one actual price decision. Furthermore, while survey responses are typically guided by pre-defined answer choices and research questions, the corporate reports reflect the perspective of the reporting firms, flexibly expressed in their own words. Finally, as opposed to one-off surveys conducted in a specific macroeconomic environment, the corporate report data covers more than two complete business cycles. It is therefore reflective of all business cycle stages and can even provide some insights into how price-setting behavior has varied over time.

Given its results, this paper also contributes to the ongoing discussion of strategic complementarities and their role in price-setting decisions. While it is widely recognized that such complementarities are crucial for generating a realistic amount of monetary non-neutrality in most standard monetary models, actual evidence on their existence is mixed and often indirect.[27] In particular, while some papers have argued that the empirically observed incomplete pass-through of cost shocks supports the role of real rigidities, a different group of papers has used calibrated models to argue that real rigidities reduce their ability to fit important aspects of the data.[28]

[26] Earlier survey-based studies on price-setting include Hall and Hitch (1939), Early (1956), Kaplan et al (1958), Lanzillotti (1958), Fog (1960), Haynes (1962), Lanzillotti (1964), Novotny and Walther (1978). With the exception of Lanzillotti (1958) and Novotny and Walther (1978), all of these studies and their main results are briefly summarized in Blinder et al (1998). A large number of survey-based studies on price-setting were also carried out as part of the European Central Bank's Inflation Persistence Network (IPN) and the Wage Dynamics Network (WDN). For these studies and corresponding discussions see Fabiani et al (2006), Fabiani et al (2007) and Druant et al (2012). Recent country-specific survey evidence also exist for Australia (Park et al, 2010), Canada (Amirault et al, 2006), Iceland (Ólafsson et al, 2011), Japan (Nakagawa, 2000), Norway (Langbraaten et al, 2008), Sweden (Apel et al, 2005) and the UK (Hall et al, 2000 ; Greenslade and Parker, 2012). While Blinder (1991) and Blinder et al (1998) mostly focused on theories of price stickiness, many of these later studies have also investigated the *causes* of price changes.

[27] See Ball and Romer (1990), Kimball (1995) and Basu (1995) for early theoretical contributions. Gertler and Leahy (2008) show that strategic complementarities can cause a significant degree of non-neutrality even if price-setting is assumed to be state-dependent.

[28] The existing evidence on incomplete pass-through and the link to real rigidities is discussed, for example, by Gopinath and Itskhoki (2011) and Gopinath et al (2011). On the other hand, Klenow and Willis (2006) argue that empirically observed large individual price changes are inconsistent with strong real rigidities. Burstein and Hellwig (2007) use product-level market shares and prices to assess the role of strategic complementarities in price setting. They find evidence in favor of strategic complementarities, but also document that this do not lead to notably stronger monetary non-neutrality in their specific model. Bils, Klenow and Malin (2012) argue that reset price inflation should exhibit a positive autocorrelation and low variance if real rigidities are present. They find, however, that this behavior is not borne out by

Our findings contribute to this discussion by providing three types of evidence for the existence of strategic complementarities. First, they show that firms often explicitly mention the behavior of competitors as a factor that affected their price-setting decisions. Second, they also document that incomplete pass-through of cost changes is an important feature of the data. Third, they actually establish a direct causal link between the two phenomena, documenting that firms explicitly mention competitive pressures as the main reason for most of the reported cases of incomplete pass-through.

2.2 The Dataset

As discussed above, all of the main results that we present in this study are based on narrative evidence contained in the 10-K filings of publicly traded firms. In this section, we discuss some of the main properties of these filings and why they are useful for the price-setting literature. Moreover, we also describe the construction of the dataset and provide descriptive statistics.[29]

2.2.1 Corporate Filings as a Source of Narrative Evidence on Price Setting Behavior

Governed by regulation of the US Securities and Exchange Commission, 10-K filings are one of the most important channels through which publicly listed firms inform financial market participants about the states of their businesses.[30] Combining quantitative elements such as audited balance sheets and income statements with additional verbal discussions, they provide insights about a variety of topics including the firms' profits, their financial situations, and important corporate decisions.

Especially the filings' quantitative elements are widely used for research in economics, finance and accounting. Through commercial databases such as Compustat, they are available in standardized formats that allow for detailed firm-level analyses and also facilitate combinations with other types of data. To a certain extent, existing work has also exploited the additional narrative discussions contained in the filings, typically to capture information not reflected by standard measures. At least so far, however, this has been largely limited to the accounting and finance literatures.[31]

From the perspective of the empirical price setting literature, what makes the reports particularly attractive is the fact that they sometimes contain explicit verbal discussions of cases in which firms either changed prices or were unable to do so. Regularly covering topics like the reasons for price changes and the factors that prevented the firms from

the data.

[29] Parts of this section overlap with the data description of chapter one. We include these parts here for ease of reading.

[30] See the discussion in Griffin (2003).

[31] Some examples of such studies include Kaplan and Zingales (1997), Li(2008), You and Zhang (2009), Feldman et al (2010), Hoberg and Phillips (2010), Ivashina and Scharfstein (2010), Ippolito et al (2015), Li et al (2013).

changing prices, these discussions can potentially shed light on various important issues the literature aims to understand.

Importantly, the type of information that can be extracted from the texts of corporate reports is captured neither by realized price data nor by price-setting surveys. While prices by themselves do not reveal the causal links that explain *why* firms did or did not change prices, surveys tend to provide only general descriptions of pricing behavior as opposed to information about specific decisions firms have actually taken. Furthermore, surveys are typically guided by predefined answer choices and cover only short sample horizons instead of more than two business cycles.

2.2.2 Reliability of the Narrative Evidence

Given that we base much of our analysis on narrative discussions of pricing decisions, it is generally important to assess their relibability. Unfortunately, in terms of the actual *reasons* for price changes and price rigidities, this is not easy to do because their true distributions are fundamentally unobservable. In fact, the lack of directly observable evidence on the reasons for price changes and price rigidities is what originally motivated the methodology of this study. However, there are a number of theoretical and empirical arguments suggesting that the narrative evidence is informative about the decisions we are interested in. Furthermore, in section 2.5 of this chapter we provide some direct evidence for the general informational content of the narrative discussion by linking their contents to observable variables that are closely related to the pricing decisions of firms.

The arguably most important theoretical factor suggesting that the texts contained in corporate filings are informative is that regulation enforced by the Securities and Exchange Commission legally requires the reporting firms to provide truthful and complete information.[32] Therefore, if firms are dishonest in this regard, they risk being investigated and reprimanded by the SEC. Moreover, Karpoff and Lou (2010) use a sample of 454 specific cases to show that such investigations are also severly punished by capital markets. More precisely, they document that public firms that are investigated by the SEC tend to lose approximately 20% of their market value when these investigations are announced.

The reporting behavior of firms is also disciplined by the fact that their verbal discussions are only credible to the extent that they are consistent with related observable information. Regarding discussions of price changes and rigidities, such information includes not only macroeconomic variables such as commodity prices, wages and aggregate demand, but also detailed firm-level items from the externally audited financial statements. Moreover, financial analysts that often specialize in specific sectors and follow only small numbers of firms can also relate the reported narrative information to facts that are not well known by the wider public such as entry and exit of competitors as well as their pricing policies. In the specific case of information about pricing decisions, another important factor is that it is not necessarily obvious which decisions are positive from the perspective of

[32] See the discussion in Griffin (2003).

financial markets. For example, the effects of price changes on profitability depend on elasticities and may also have complex long-run effects. Thus, managers who aim to affect the perception and valuation of their companies are unlikely to achieve this by misrepresenting their pricing decisions and might instead focus on more easily interpretable measures such as earnings forecasts and forward-looking statements.

Finally, in addition to these theoretical arguments, there also is growing empirical support for the informational content of narrative discussions contained in corporate filings. For example, Balakrishnan et al (2010) show that they can be used to form stock portfolios that outperform the market, Li et al (2013) use them to construct a new measure of competition and show that it is positively correlated with more standard measures, and chapter one of this thesis shows that narrative corporate-filing information about real and financial firm policies is consistent the true behavior of closely related variables. Furthermore, as discussed above, this study, too, provides some related empirical evidence by linking narrative information from corporate filings to a number of observable variables.

2.2.3 Construction of the Dataset

To construct the dataset, we proceed through the following four basic steps: First, we download all annual corporate filings available in the EDGAR database maintained by the SEC.[33] This yields just above 200,000 documents filed by almost 19,000 different firms over the past 20 years. Second, we separate out the text contained in each one of these reports and break it down into sentences using a linguistig disambiguation algorithm.[34] Third, we identify those sentences that actually contain information about either implemented price changes or cases of price rigidities. Finally, we translate that information into a number of quantitative variables suitable for the subsequent analysis.

Generally speaking, the construction of the dataset is a time-consuming and computationally intensive task, because each one of the approximately 200,000 corporate reports that use contains large amounts of text and quantitative data. However, three of the four steps discussed above do not require complex discretionary decisions. In particular, because the SEC makes the relevant documents publicly available via its EDGAR web service, we are able to obtain them using a simple automated download script. Furthermore, the linguistic disambiguation technique of Kiss and Strunk (2006) that we use to break the extracted texts down into sentences mainly requires the choice of an appropriate training dataset.[35] Finally, given that the sample size turns out not to be excessively large, the translation of

[33] We consider all documents filed as 10-K and 10-K405, as well as the small business equivalents 10-KSB and 10-KSB40.

[34] Before the sentence boundary detection, we also automatically remove tables and other non-text contents such as HTML formatting instructions. We use the implementation of the Kiss and Strunk (2006) algorithm included in the Python Natural Language Toolkit (NLTK). Alternatively, we could have assumed that sentences end whenever a full stop occurs. However, this alternative approach would have lead to mistakes in cases where same symbol is used to denote abbreviations or to separate decimals from integers.

[35] The disambiguation algorithm that we use is trained on the Wall Street Journal data of the Penn Treebank. Its language is relatively similar to that used by firms in their corporate reports.

the extracted sentences into statistical variables can be based entirely on manual readings and thus achieve very high precision. The step that is significantly more elaborate than the other three, however, is the one that actually searches all of the corporate filing texts to identify only those sentences that actually talk about price changes or price rigidities. What makes this step of the data construction process particularly difficult is that the millions of sentences contained in the 200,000 reports are both too unstructured for a fully automatic classification and too numerous for a purely manual one. To tackle this challenge, we implement an approach that combines manual readings with an automatic pre-selection step. More precisely, we first search each one of the sentences contained in the annual reports for a number of verbal patterns likely to be associated with information about either price changes or price rigidities.[36] Then, out of all sentences that pass this initial screen, we keep only those for which a manual reading confirms that they do indeed contain the desired type of information.

To set up the pre-selection step, we begin by manually reading several full reports as well as large amounts of sentences that contain important basic keywords. For example, we read large numbers of sentences containing a variant of the word "price" in order to understand the wording and grammatical structure firms typically use to describe price changes and price rigidities. Then, we set up a number of general text patterns that capture these grammatical structures and can be used to identify all sentences that contain at least one of them.

Importantly, to ensure that the pre-selection step does not cause a bias with respect to the information we are interested in, we do not search for specific reasons of price changes or price rigidities. Instead, we remain agnostic and use only very general grammatical structures that can accommodate all of them without favoring any specific one. The exact patterns that we use for the pre-selection step are summarized in Table 16. Round brackets indicate that an element is optional, and square brackets denote that only one of the elements they contain are required to occur.

For those candidate sentences that are likely to contain information about implemented price changes, the patterns require that a variant of the word "price" as well as a verb referring to the concept of change occur. Furthermore, they impose that the word "we" or the expression "the company" must appear. This second condition is to ensure that the candidate sentences refer to price changes that the firms actively implemented as opposed to general market trends and pure price-taking behavior.[37] Finally, the patterns also impose some conditions on the order of the required elements and the maximum distance between them.

The patterns used to pre-select candidate sentences about price rigidities are conceptually very similar. However, instead of a reference to the concept of change, they require an

[36] The exact verbal patterns used for this step are discussed below.

[37] For example, firms who mainly sell commodities tend to have little freedom in their pricing decisions. Instead, they are largely bound to accept the development of market prices. See Amirault et al (2006).

Table 16: Verbal Patterns Used to Pre-Select Candidate Sentences About Implemented Price Changes or Price Rigidities

Pre-Selection Patterns for Sentences on Implemented Price Changes

1. "we (have) [increased/decreased/raised/reduced/lowered/adjusted/changed] (*word*)(*word*)(*word*)(*word*)(*word*) [price/prices/pricing]"
2. "the company (has) [increased/decreased/raised/reduced/lowered/adjusted/changed] (*word*)(*word*)(*word*)(*word*)(*word*) [price/prices/pricing]"

Pre-Selection Patterns for Sentences on Incomplete Pass-Through

1. "we (were) [unable to/not able to/did not/could not] (*word*)(*word*)(*word*)(*word*)(*word*) [price/prices/pricing/pass/recover/offset]"
2. "the company (was) [unable to/not able to/did not/could not] (*word*)(*word*)(*word*)(*word*)(*word*) [price/prices/pricing/pass/recover/offset]"

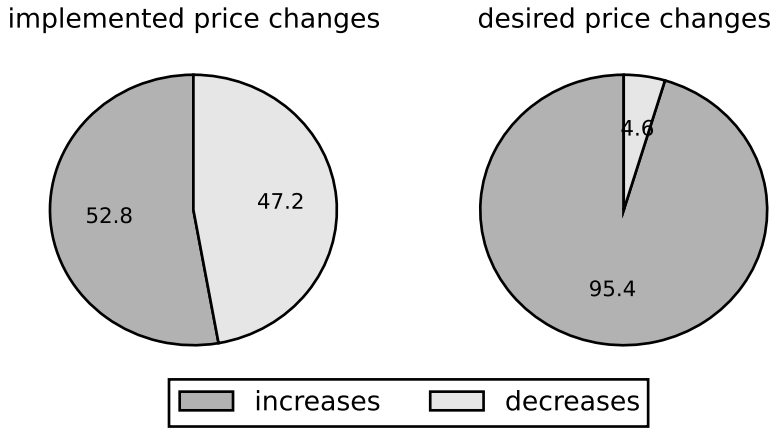
Notes: The table shows verbal patterns used to pre-select candidate sentences that are likely to contain information about implemented price changes or cases of price rigidities. Round brackets indicate optional elements. Square brackets denote that only one of the elements they contain is required to occur. The element *word* is a placeholder satisfied by any single word.

expression that denotes inability or a decision not to take a specific action. Furthermore, as an alternative to the variant of the word "price", they also accept one of several words that firms regularly use when referring to pass-through of changes in costs. The reason why we explicitly include these patterns in the search is that changes in costs are generally considered one of the main determinants of price changes and price pressures.[38]
To illustrate the kind of information that the selected sentences contain, Table 17 provides some examples for both cases of realized price changes and cases of price rigidities. All of these sentences match at least one of the patterns displayed in Table 16 and also survived the manual reading step.

2.2.4 Sample Properties

After the automatic pre-selection, the subsequent manual reading and the removal of duplicates, the final dataset contains 1,708 sentences about implemented price changes and 241 sentences about price rigidities. These observations were reported by a total of 983 different firms. In terms of the number of different companies covered, our sample is thus notably larger than those of many survey studies on price-setting behavior.[39]

Figure 14: Directions of Desired and Implemented Price Changes



Notes: The figure shows the directions of the implemented and desired price changes discussed in the extracted corporate filing sentences. Implemented price changes are those reported in the extracted price-change sentences, and desired price changes are those reported in the extracted rigidity-sentences.

To illustrate the relative importance of increases and decreases within the sample, Figure 14 displays their respective fractions for both the reported price changes and the reported cases of price rigidities. For the former, it shows that decreases account for approximately 47% of all observations. This number is slightly higher than the corresponding value

[38] All of the main results presented below are qualitatively robust to the exclusion of these patterns.
[39] For example, the survey of Blinder (1991) and Blinder et al (1998) was set up to obtain a sample size of approximately 200 firms.

Table 17: Sample Sentences About Implemented Price Changes and Price Rigidities

Sample Sentence	Category	Company Name	Fiscal Year
"This continued through the first half of 2003, and negatively impacted our gross margins, as we lowered our prices to meet competitor pricing."	Price Change	Reliance Steel & Aluminum	2003
"Effective September 2003, we increased the suggested list price for the adult somasensor and the pediatric somasensor in the united states to $11,000 and $14,000, respectively."	Price Change	Somanetics	2003
"In April 1993, the company reduced the average selling price of membership contracts even further in an attempt to increase unit volume."	Price Change	Bally Manufacturing	1993
"Due to a very competitive environment, we were unable to increase selling prices during 2010 to compensate for the increased waste paper costs and as result our operating margins were reduced."	Price Rigidity	Orchids Paper Products	2010
"The competition has put pressure on prices in the market, and we have not been able to increase the prices in some markets to the extent of our cost increases."	Price Rigidity	Devcon International	2003
"Due to existing contractual obligations, we have not raised red cell prices to market levels."	Price Rigidity	Hemacare	2002

Notes: The table shows extracted corporate report sentences about implemented price changes and price rigidities. The sentences were extracted from the corporate reports using the pre-selection patterns shown in Table 16 and a subsequent manual reading step.

documented using micro-level price data (e.g. Nakamura and Steinsson, 2008), but it is in line with the general finding that price decreases are common even when aggregate inflation is positive.

In terms of the reported price rigidities, on the other hand, we find that more than 95% of all observations describe cases in which firms faced upward pricing pressures that they did not (fully) translate into higher prices because of the reported rigidities. Thus, while it is difficult to argue conclusively what causes this uneven distribution, it does imply that our analysis of price rigidities will only be informative about cases in which they prevented firms from *raising* their prices.

Given that we are generally interested in the macroeconomic implications of price-setting behavior, another important property of our dataset is the extent to which it reflects the behavior of firms that are large enough to matter at the aggregate level. To assess this, we compare them to the general Compustat universe in terms of their total assets, sales and employee headcounts (Table 18). While we find that the mean values of these three measures are somewhat lower for the firms in our sample, the opposite is true for the corresponding medians. Thus, even though our dataset appears exclude at least some firms from the right tail of the size distribution, it is not generally biased in the sense that the typical firm it contains is unusually small.[40]

Table 18: Descriptive Statistics: Firm-Size and Report Length

Variable	Total Assets (in 1,000 USD)		Total Sales (in 1,000 USD)		Number of Employees	
Sample	Compustat	Narrative	Compustat	Narrative	Compustat	Narrative
Mean	7,141.39	4,354.36	2,100.07	1,593.06	7,899.99	5,800.52
Std. dev	69,498.66	37,819.32	10,879.88	5,663.00	36,200.80	20,175.80
Min	0.00	0.02	-15,009.33	0.00	0.00	0.00
25%	26.90	58.77	12.99	56.87	91.00	250.50
50%	189.06	277.05	94.26	250.04	516.00	1,215.00
75%	1,130.92	1,074.9	645.37	1,069.34	3,288.00	4,346.75
Max	3,771,200.00	882,547.00	470,171.00	77,349.00	2,545,209.00	400,000.00
N	165,456.00	1,565.00	161,742.00	1,554.00	137,179.00	1,496.00

Notes: The sample includes all firms matched to Compustat at the gvkey/fiscal-year level. Total assets, total sales and number of employees are the corresponding Compustat variables. Report length is measured as the number of sentences contained in the corporate filings.

2.3 Reported Causes of Price Changes

In this section we use the sentences that discuss implemented price changes to investigate what factors cause firms to change the prices of their products. We first illustrate the

[40] Furthermore, as we document below, the pricing decisions reported in the extracted sentences are closely related to the behavior of the US GDP deflator.

categories that we use to quantify the contents of these sentences and then explore the resulting distributions in the full sample, with selected sectors and in different macroeconomic environments.

2.3.1 Classification Categories and Baseline Results

To provide a quantitative look at the reported reasons for price changes and their relative frequencies, we consider all sentences in which firms explicitly state at least one such reason. We then group these reasons using the seven categories described in Table 19. The categories generally reflect the wording that firms use, but they also take their economic similarity into account. For example, closely related concepts such as sales, revenues, volume and market share all fall into one common group denoted 'Sales / Market Share'.[41] While we find that the sentences often contain specific keywords such as those stated in Table 19, we code all categories manually in order to minimize the number of errors.

[41] In cases where the reasons stated by firms correspond to more than one of the seven categories, we use multiple assignments. This causes the sample size to be slightly larger than the number of sentences that contain reasons for price changes.

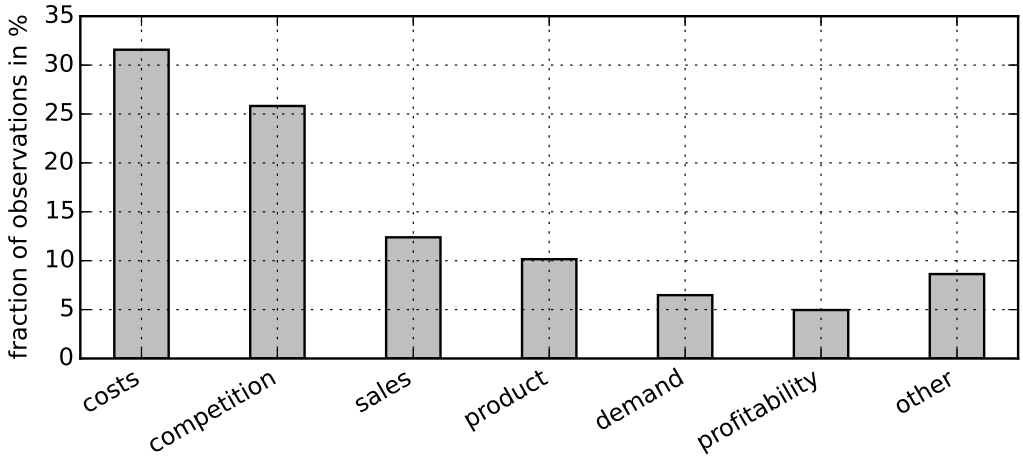
Table 19: Categories of Reasons for Price Changes

Category	Short Name	Sample Expressions
Cost Changes	Costs	'product costs' , 'manufacturing costs' , 'material costs' , 'commodity costs'
Competitive Considerations	Competition	'price competition' , 'competition in the marketplace' , 'competitive advantage ' , 'competitive pricing pressures'
Aggregate Demand / Market Conditions	Demand	'economic conditions' , 'market conditions', 'consumer demand' , 'customer demand'
Sales / Market Shares	Sales	'market share' , 'subscriber growth' , 'sales volume' , 'broader market'
Profitability and Margins	Profitability	'gross profit' , 'margin pressure' , 'gross margins' , 'profitability'
Product Features / Life Cycle	Product	'new product introductions' , 'added features' , 'feature functionality' , 'next generation'
Other	Other	-

Notes: The table shows the seven categories into which the reasons for price changes were grouped. For each one of these categories, the column 'Sample Expressions' displays a number of key-words that were used to assign specific sentences to that category. The sample expressions shown are not exhaustive. The sentences were assigned to multiple categories whenever that best reflected their content.

Figure 15 displays the results of this grouping exercise graphically. It shows that cost fluctuations and considerations related to the behavior of competitors are the most frequently reported reasons for price changes. Jointly, these two categories account for more than half of all cases in the dataset. In addition, firms also regularly report that they changed prices to influence sales volumes and to account for changes in product features. Finally, in a relatively low number of cases, they motivate price changes with demand conditions and considerations about profitability.

Figure 15: Reported Reasons of Price Changes

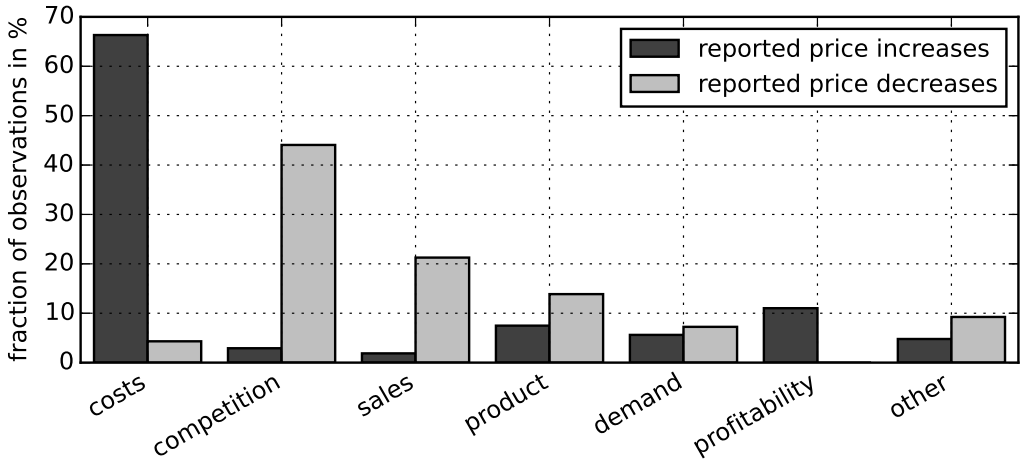


Notes: The figure illustrates the distribution of reported reasons for price changes. Sample expressions and category definitions are shown in Table 19. The sentences were assigned to multiple categories whenever that best reflected their content.

Based on the full sample of reported price changes, the above results mainly highlight the general importance of costs and competitive considerations for firm-level pricing decisions. However, some existing empirical work suggests that price increases may be driven by differen factors than price decreases. For example, Peltzman (2000), Fabiani et al (2006) and Loupias and Sevestre (2013) document that prices rise more when costs increase than they fall when costs decrease. Therefore, we next repeat the above analysis using subsamples for each of the two directions.

As Figure (16) shows, this distinction between price increases and decreases reveals that our data, too, exhibit a pronounced asymmetry. While we find that profitability and cost shocks only seem to explain price increases, competitive considerations and sales only appear to play a role in driving prices down. Thus, we confirm not only the asymmetry of price-setting in a general sense, but also the specific finding that prices appear to be more sticky after cost decreases than they are after cost increases.

Figure 16: Asymmetry in the Reasons of Price Changes



Notes: The figure illustrates the distribution of reported reasons for implemented price increases and decreases, respectively. Sample expressions and category definitions are shown in Table 19. The sentences were assigned to multiple categories whenever that best reflected their content.

One potential concern about this result is that the asymmetric role of costs may not be a structural feature of price-setting behavior and instead simply reflect a general upward trend in raw-material prices. To assess if this is the case, we perform a subsample robustness analysis using only observations from the years 1993-2002, a period during which commodity costs were largely stable. We find that the same type of asymmetry also occurs in this subsample and therefore conclude that it s not just an artifact of steadily rising raw-material prices.

Another potential concern associated with the observed asymmetry is that it may be an artifact of firms' reporting incentives. In particular, large companies might attribute price increases to rising costs instead of decreasing competition since the latter might trigger interventions by anti-trust authorities. However, such incentives are unlikely to be the cause of the asymmetry for two different reasons. First, as documented by Fabiani et al (2006), it also occurs in price-setting surveys that are not subject to the same incentive structure. Second, in an additional robustness exercise, we find that it is equally pronounced within a subsample of small firms, which are unlikely to be negatively affected by anti-trust interventions and might even benefit from them.

To complement the information obtained by grouping the reported reasons into discrete categories, we also obtain word clouds constructed directly from the extracted price-setting sentences. Based only on the word-frequencies of the explanations that firms provide, these graphical representations can serve as useful robustness checks because they do not involve any discretionary grouping decisions. As Figure 17 shows, the message of the word clouds is well in line with the distribution of the groupings shown above. While the explanations for price increases mostly contain references to changes in costs, price decreases are mostly

linked to competitive pressures. Thus, the word clouds confirm both the asymmetry between price increases and decreases, and the actual reasons for the changes.

Figure 17: Word Clouds of the Reported Reasons For Price Changes

Notes: The word-clouds illustrate how often firms used specific words when describing the reasons for implemented or desired price changes. The size of each displayed word reflects the number of times it occurs in the sentence fragments that contain these reasons. Only sentences that actually contain at least one reason were used. A number of very common English language words not related to the information of interest are not displayed.

The findings shown here have a number of implications for theoretical work and the conduct of monetary policy. First, they highlight that price-setting behavior of competing firms appears to be an important factor causing price changes. We interpret this as evidence for significant strategic pricing complementarities. Since such complementarities can help generate monetary non-neutrality, they are a popular ingredient of structural monetary models. However, as argued above, they are also difficult to identify using realized price data alone and have thus remained controversial from an empirical perspective.

The findings also highlight the need for theoretical mechanisms that can deliver asymmetries in price-setting behavior. Even though such asymmetries have also been documented in realized prices and surveys, they are typically not considered to be one of the main stylized facts that workhorse price-setting models should be able to reproduce.[42] Two theoretical mechanisms that may help improve the performance of models in this respect and have already been explored in existing work are positive trend inflation and consumer search. While the former achieves the desired effect by making negative cost shocks transitory and reenforcing positive ones, the latter can achieve it by making customer search after price increases more desirable than after price decreases.[43]

[42] See Peltzman (2000), Fabiani et al (2006) and Loupias and Sevestre (2013).

[43] For the work on asymmetric price setting and customer search see Cabral and Fishman (2012) and the references therein. For the relationship between positive trend inflation and asymmetric pricing behavior see Ball and Mankiw (1994).

2.3.2 Relative Importance of Different Types of Costs

Given the revealed importance of costs as a driver of both desired and implemented price changes, we also investigate exactly what types of costs matter the most for firms pricing decisions. In particular, using a number of keywords, we distinguish between raw-material costs, labor costs and a third category containing less frequent items. As above, we also again allow for multiple assignments to accurately capture the contents of the extracted sentences.

The resulting break-down into the different cost-categories is shown in Table 20, together with examples of the corresponding keywords. It suggests that raw-materials were by far the most frequent cause of cost-driven price changes, and that only a very small number of the reported observations was due to changes in labor costs. Furthermore, there appears to be no fundamental difference between the cost types relevant for the desired price changes discussed in the rigidity sentences and those that were actually implemented.

Table 20: Types of Costs and Associated Keywords

Cost Category	Sample Expressions	Fraction of Observations
Raw Materials	raw material, commodities, input, energy, fuel	83%
Labor Costs	wage, employee, personnel, labor, labour, payroll	7%
Other	operating, manufacturing, transportation, product	10%

Notes: The table shows the three cost categories used to further distinguish the cases in which implemented or desired price changes were caused by changes in costs. For each one of these categories, the column 'Sample Expressions' displays a number of key-words that were used to assign specific sentences to that category. The sample expressions shown are not exhaustive. Sentences were assigned to multiple categories whenever that best reflected their contents.

Given our methodology, this breakdown is conditional on the changes in the different cost categories that actually occurred over the sample horizon. The role of labor costs, for example, might have turned out to be larger had wages fluctuated more over the past 20 years. Nevertheless, there are two reasons to believe that the qualitative features of the distribution shown in Table 20 also generalize to other periods. First, the sample horizon covers more than two complete business cycles and thus reflects different macroeconomic environments. Second, wages were not unusually stable during the sample horizon. In fact, the period we consider contains 5 changes in the US Federal minimum wage and also exhibits a volatility of employee compensation that is very similar to that over its entire recorded history.[44]

[44]While the annualized quarterly growth rate of employee compensation had a standard deviation of 1.09% over the sample horizon, the corresponding value over its entire recorded history back to 1947 is only marginally higher at 1.16%. The time series underlying these value was obtained from the Bureau

Finally, the dominant role of raw material costs in causing price increases has two different implications worth highlighting. On the one hand, it has been shown that prices of commodities are among the first to respond to monetary policy shocks.[45] This suggests that they may be an important channel through which central banks can affect the prices of goods further down the production chain. On the other hand, however, commodity prices can also exhibit speculative behavior similar to that typically observed in financial markets. Thus, by allowing the speculative behavior to feed through to a much wider group of goods, the link documented here may also complicate the conduct of monetary policy.

2.3.3 Variation Across Sectors

One important finding of studies that have investigated the behavior of prices based on micro-level data is that price frequencies exhibit large amounts of heterogeneity across different product categories. Motivated by this stylized fact, we next explore sectoral variation in the reported causes of price changes and investigate whether price setting is also heterogeneous in this regard.

Given that most publicly traded companies are best described as multi-product firms, it is generally difficult to link our narrative evidence directly to the specific types of products defined in micro-level price data. However, we are able to form reasonably large subsamples for a number of important and economically well-defined sectors. To construct these subsamples, we use classifications from existing work by Kenneth French as well as general definitions based on SIC codes. Table 21 provides an overview of the resulting sector definitions.[46]

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of Economic Analysis, via the website of the Federal Reserve Bank of St. Louis. The corresponding BEA Account code is A576RC1. The history of changes in the US federal minimum wage was obtained from the US Department of Labor via http://www.dol.gov/whd/minwage/chart.pdf.

[45] For example, see Uhlig (2005).

[46] Add information about the Kenneth French sector codes.

Table 21: Sector Definitions

Name	Contains	Definition	Obs
Non-Durable Goods	Food, Tobacco, Textiles, Apparel, Leather, Toys	Ken French Sector - "NoDur"	226
Durable Goods	Cars, TV's, Furniture, Household Appliances, Machinery, Trucks, Planes, Chemicals, Office Furniture, Paper, Commercial Printing	Ken French Sector "Durbl" OR Ken French Sector "Manuf"	433
High-Tech Goods	Computers, Software, and Electronic Equipment (excluding services)	Ken French Sector "HiTec" AND first digit of SIC $\neq 7$	268
Healthcare Goods	Healthcare, Medical Equipment, and Drugs (excluding services)	Ken French Sector "Hlth" AND first digit of SIC $\neq 8$	172
Trade	Retail and Wholsale Trade	First digit of SIC $= 5$	257
Services	All sectors classified as services in SIC	SIC 7200 - SIC 8748	258

Notes: The table shows the sector definitions applied to form the subsamples used in Figure (18). The Ken French sector specifications and their respective descriptions are based on the 10-sector classification by Kenneth French and obtained from http://mba.tuck.dartmouth.edu/pages/faculty/ken.french/data_library.html.

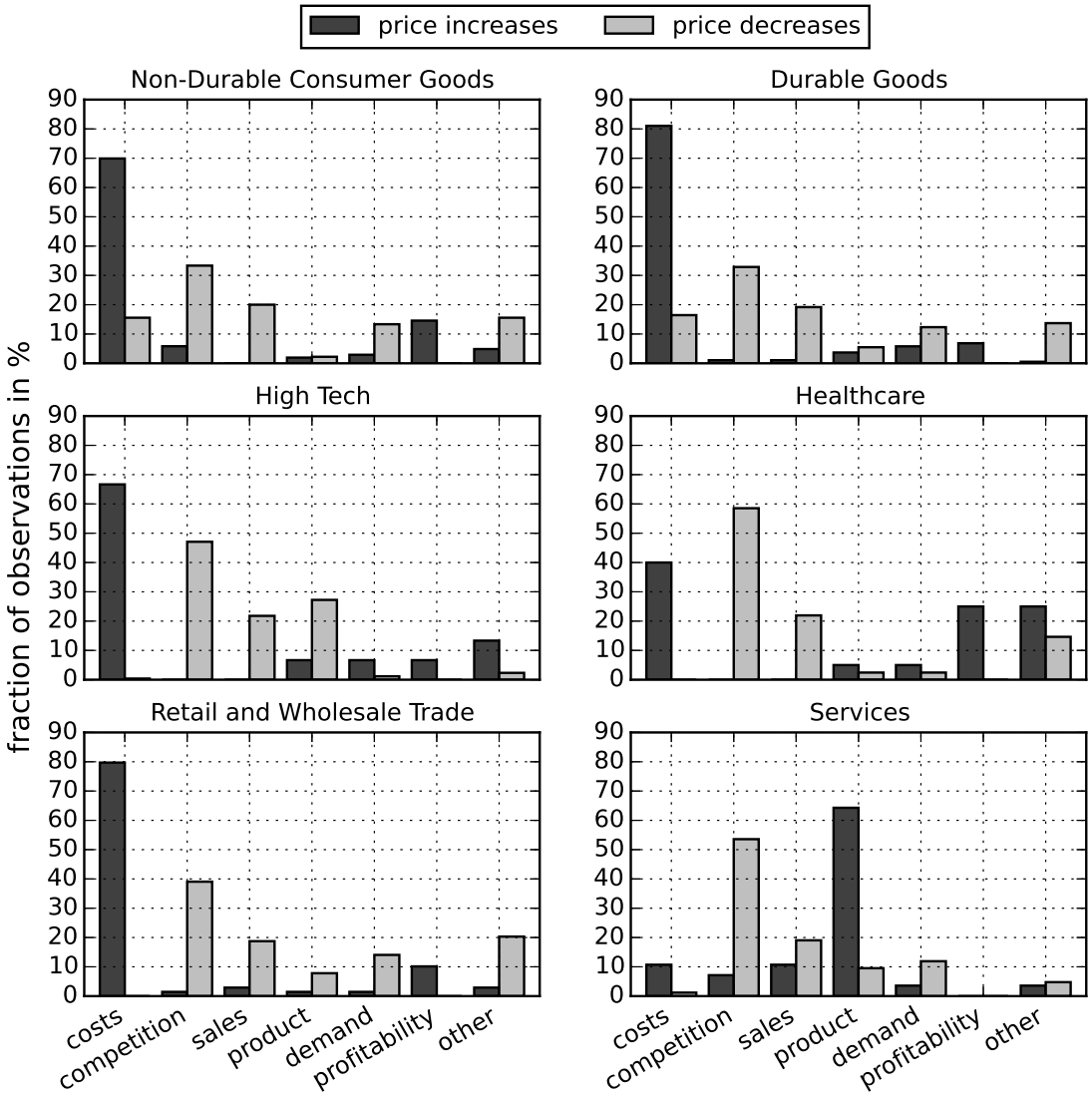
As Figure 18 shows, we find that some cross-sectoral variation in the causes of price changes exists, but not across all sectors. In particular, while the services and healthcare sectors exhibit distributions that are qualitatively different from those of the remaining ones, variation across the goods, high-tech and trade sectors are largely of a quantitative nature. Moreover, there appears to be almost no difference between the durable and non-durable goods sectors.

We also find that the cross-sectoral variation is well in line with basic structural differences between the sectors. For example, while costs play almost no role in the services sector, they are more important for the goods-producing and trade sectors. Similarly, the features of products and services are particularly important for the rapidly evolving high-tech goods as well as for services, which we find firms regularly modify or extend.

Finally, another striking result is that the asymmetry between price increases and decreases documented above is also present in every single one of the sectors we consider here. That is, while we find that costs and considerations about profitability mainly drive prices up, competitive pressures and considerations about sales volumes mainly drive prices down. Even in the services sector, where costs play only a very limited role, those observations

that are motivated by changes in costs are price increases.

Figure 18: Types of Cost Shocks That Caused Price Changes



Notes: The figure illustrates the distribution of reported causes of implemented price increases and decreases for the sectors defined in Table 21. Category definitions and corresponding sample expressions are shown in Table 19. The sentences were assigned to multiple categories whenever that best reflected their content.

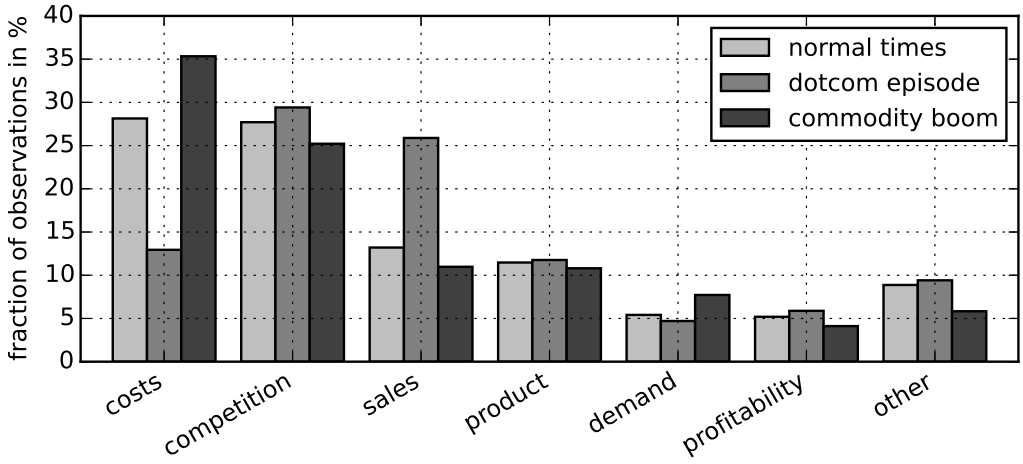
2.3.4 Variation Across Macroeconomic Regimes

Given that our dataset covers a horizon of almost 20 years, it also allows us to investigate variation in the causes of price changes over time. We do this by dividing the full sample into three time periods characterized by distinct macroeconomic conditions. First, to capture an economic downturn, we use the two years that followed the dotcom bubble. Second, to capture a period with large fluctuations in commodity prices, we use the years

2003 to 2010.[47] Finally, to capture arguably more normal economic times, we use all remaining years.

The results of this analysis are shown in Figure 19. In line with the the fact that raw-material costs were particularly volatile between 2003 and 2010, we find that firms mention them particularly often during that period. Furthermore, confirming the recessionary nature of the years after the dotcom bubble, we observe that they are characterized by an unusually large fraction of price changes motivated by a desire to increase sales. Moreover, since price changes motivated by general economic are not unusually frequent during the dotcom downturn, it appears that recessions only affect pricing decisions to the extent that they actually result in lower sales volumes.

Figure 19: Reported Causes of Price Changes Across Different Macroeconomic Regimes



Notes: The figure illustrates the distribution of reported causes of implemented price increases and decreases for three different macroeconomic regimes. The dotcom episode is defined as the years 2001 and 2002. The commodity boom is defined as the years 2003 and 2010. The normal-times regime is defined such that it contains all observations that belong to neither of the other two regimes. Reports filed in year t are assigned to calendar year $t-1$. Category definitions and corresponding sample expressions are shown in Table 19. The sentences were assigned to multiple categories whenever that best reflected their content.

2.4 Reported Causes of Price Rigidities

Having explored the reported reasons for implemented price changes, in this section we turn to the factors that can prevent firms from adjusting their prices, i.e. the reported price rigidities. For this, we investigate the contents of those extracted sentences in which firms explicitly state that they did not or could not change the prices of their products. In particular, we quantify both the factors that caused the firms' underlying desire to change their prices, and the factors that limited their ability or willingness to actually do so. Furthermore, we also investigate the frequency and specific features of cases in which the

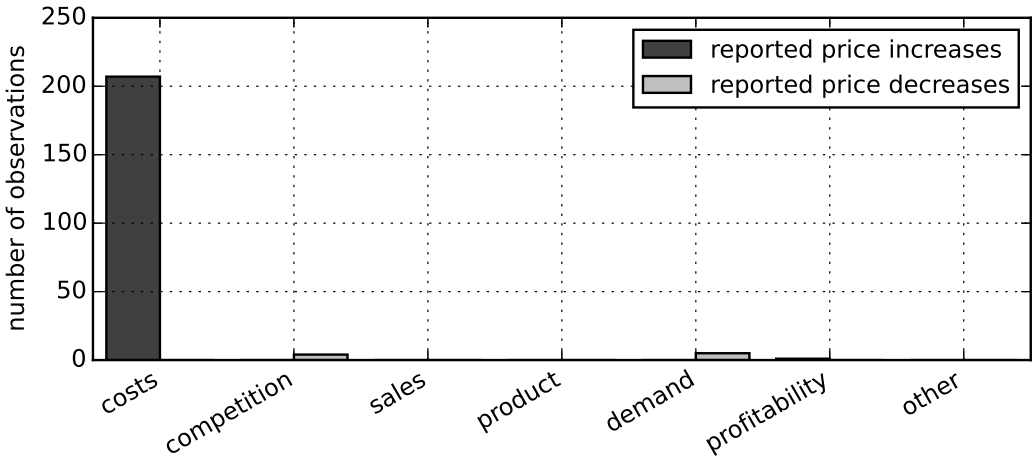
[47] This sample also includes the height of the financial crisis that occurred between 2008 and 2009. However, the results shown below are qualitatively robust to the exclusion of these two years.

rigidities did not fully prevent firms from adjusting their prices and instead only affected the size of their price adjustments.

2.4.1 Reported Causes and Directions of the Desired Price Changes

To assess the properties of the extracted sentences, we first ask why and in what directions firms would have changed their prices had they not faced the reported rigidities. Given the conceptual similarity between these desired price changes and those that firms actually implement, we do this based on the same seven categories that we also applied in the previous section. As Figure 20 shows, we find that approximately 90% of all observations reflect cases in which firms faced rising costs that they did not (fully) pass along in the form of higher prices.

Figure 20: Reported Reasons of Price Changes



Notes: The figure illustrates the distribution of reported reasons for desired price changes reported in the context of price rigidities. Sample expressions and category definitions are shown in Table 19. The sentences were assigned to multiple categories whenever that best reflected their content.

Generally speaking, it is difficult to assess why the extracted sentences contain almost no discussions about cases in which firms faced downward rigidities. While it may be the case that the reporting firms do not face such rigidities, it is in principle also possible that they simply do not report them. Therefore, while we leave it to the reader to interpret the following results in a less conservative fashion, we will assume that they mainly capture the properties of those types of rigidities that are associated with upward price pressure.

2.4.2 Classification Categories and Reported Causes of Price Rigidities

Next, to quantify the reasons that actually prevented firms from adjusting their prices in response to the rising costs, we again use a number of manually defined categories that reflect the wordings firms use and correspond to relatively well-defined economic concepts.

Interestingly, as Table 22 illustrates, we discover that most of the concepts firms use to explain why they did not change prices fit very well into some of the same categories that we used above to quantify the causes of implemented price changes. In fact the only new category that we need to define in order to accurately capture the disclosed reasons of price rigidities is one that reflects cases in which firms could not change their prices because of existing contracts.

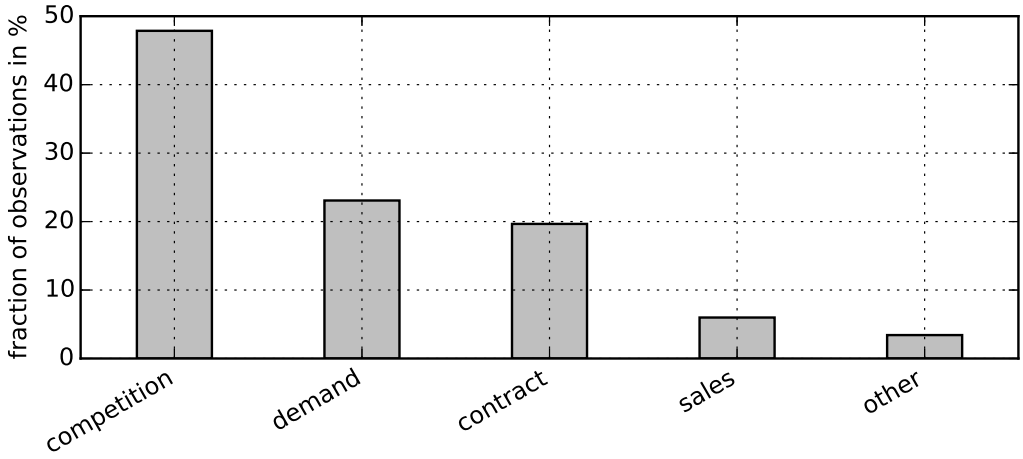
Table 22: Categories for Causes of Price Rigidities

Category	Short Name	Sample Expressions
Competitive Considerations	Competition	'intense competition' , 'competitive environment' , 'competitive pricing pressure ' , 'competitive pressure'
Aggregate Demand / Market Conditions	Demand	'challenging economic times' , 'market conditions' , 'demand environment' , 'weak demand'
Sales / Market Shares	Sales	'market share' , 'subscriber growth' , 'sales volume' , 'broader market'
Existing Contracts	Contracts	'fixed price agreement' , 'sales order backlogs', 'fixed price contracts' , 'signed sales contracts'
Other	Other	-

Notes: The table shows the five categories into which the causes of price rigidities were grouped. For each one of these categories, the column 'Sample Expressions' displays a number of key-words that were used to assign specific sentences to that category. The sample expressions shown are not exhaustive. The sentences were assigned to multiple categories whenever that best reflected their content.

Plotting the distribution that results from this grouping exercise, Figure 21 illustrates that the most frequently reported cause of price rigidities are competitive considerations, followed by demand conditions and pre-existing nominal contracts. Thus, apart from rigidities explained by existing contracts, the factors that prevent firms from raising their prices are remarkably similar to those that actually cause price reductions. This is especially true for competitive pricing pressures, which turns out to be the frequently reported factor for both implemented price decreases and reported upward rigidities.

Figure 21: Reported Causes of Price Rigidities



Notes: The figure illustrates the distribution of reported causes of price rigidities. Sample expressions and category definitions are shown in Table 22. The sentences were assigned to multiple categories whenever that best reflected their content.

Using the word-frequencies of the explanations given by firms, we also again obtain a corresponding graphical representation in the form of a word cloud. Since almost all of the desired price changes are increases, however, we do not distinguish between the two directions. As Figure 22 shows, the general picture obtained from the grouping exercise is again confirmed by the word frequencies. That is, the most common terms used in the explanations are those related to competition, contracts and the general demand environment.

Figure 22: Word Cloud of the Reported Reasons of Price Rigidities

Notes: The word-cloud illustrates how often firms used specific words when describing the causes of price rigidities they experienced. The size of each displayed word reflects the number of times it occurs in the sentence fragments that contain these reasons. Only sentences that actually contain at least one reason were used. A number of very common English language words not related to the information of interest are not displayed.

The frequent mentions of competitive pressures as a reported cause of price rigidity are generally well in line with the findings from the previous section. They, too, can be interpreted as evidence in favor of strategic pricing complementarities between competing firms. This further validates the use of such complementarities as a mechanism that can increase the degree of monetary non-neutrality in structural models. In addition, the sentences shown here explicitly document that these complementarities work not only by inducing price decreases, but also by actually preventing firms from raising their prices.
Another interesting aspect of the reported rigidities is the fact that firms appear to be less able or willing to pass along cost increases when aggregate demand is weak. This suggests that they may face a counter-cyclical elasticity of demand and thus have incentives to set markups in a pro-cyclical fashion. While this type of behavior is at odds with a large class of models in which counter-cyclical markups are important for monetary transmission, it is in line with the recent empirical evidence provided by Nekarda and Ramey (2013).
Finally, it is also worth noting that a number alternative causes of rigidities regularly considered in existing work are practically absent from the extracted narrative discussions. For example, the firms do not emphasize classical nominal frictions such as menu costs. Fixed-price contracts arguably come closest to such nominal frictions, but they are endogenous in the sense that they result from the interaction between the firms and their customers. A second notably absent force is fear of customer anatagonization. This factor first emerged in an open question of the survey by Blinder (1991) and Blinder et al (1998), and it was also confirmed as very important in several of the subsequent survey studies. For example, in their review of the IPN evidence, Fabiani et al (2006) relate that "the fear

of antagonizing customers with frequent price changes seems to be the most important explanation for price stickiness in the euro area" (page 6).[48]

One potential explanation for the absence of these factors is based on the fact that the pricing decisions discussed in the corporate reports are likely to be the relatively large and important ones. More precisely, in principle both menu costs and fear of customer antagonization may well be strong enough to prevent firms from implementing minor or temporary price changes. However, they may not be sufficient to deter firms from implementing those price changes large enough to affect their overall business results and thus warrant verbal discussions in the filings.[49]

2.4.3 Relative Importance of Different Types of Costs

Given that almost all of the reported rigidities turn out to reflect cases in which firms faced rising costs, we also again investigate the relative importance of different types of costs (Table 23). Using the same categories that we applied in the previous section, we again find that raw-material costs are much more frequently mentioned by the firms than changes in labor-related costs. Thus, raw material prices appear to explain not only a large number of implemented price changes, but also much of the upward price pressure firms experienced in the context of the reported rigidities.

Table 23: Types of Costs and Associated Keywords

Cost Category	Sample Expressions	Fraction of Observations
Raw Materials	raw material, commodities, input, energy, fuel	61%
Labor Costs	wage, employee, personnel, labor, labour, payroll	8%
Other	operating, manufacturing, transportation, product	31%

Notes: The table shows the three cost categories used to further distinguish the cases in which desired price changes were caused by changes in costs. For each one of these categories, the column 'Sample Expressions' displays a number of key-words that were used to assign specific sentences to that category. The sample expressions shown are not exhaustive. Sentences were assigned to multiple categories whenever that best reflected their contents.

2.4.4 Cases of Partial Price Adjustment

Finally, the extracted sentences also allow us to distinguish between cases in which rigidities fully prevent price changes and cases in which adjustment does occur but is incomplete. For the price-setting literature, this distinction is particularly important because under incomplete adjustment prices can be rigid even if their durations are low. This, in turn, can

[48] Also see Rotemberg (2005), who explores the role of customer antagonization in a theoretical setting.

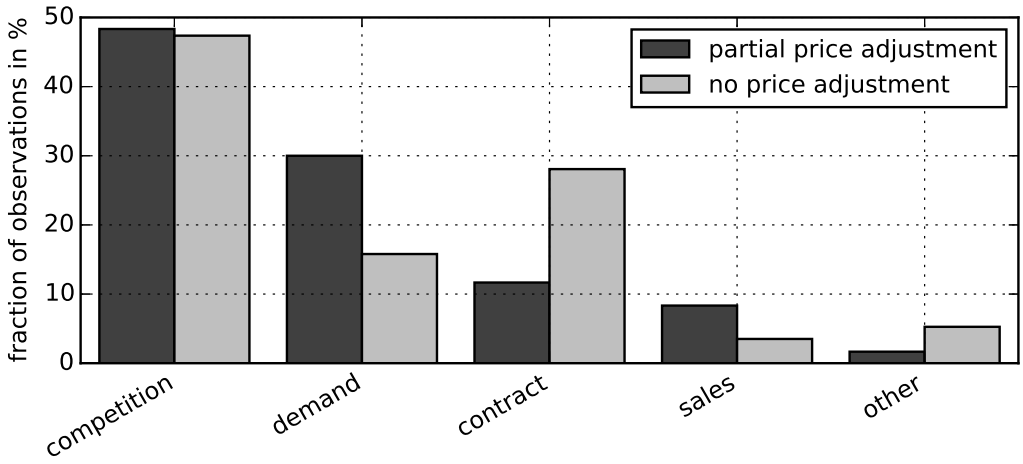
[49] For example, if these costs are independent of the size of the nominal adjustment, only relatively large shocks would cause firms to leave the inaction region and actually change their prices.

invalidate approaches that calibrate price rigidity based only on observable price-change frequencies. Furthermore, because they condition on nominal prices changing, cases of incomplete price adjustment can also be interpreted as evidence for rigidities beyond fixed adjustment costs.

To quantitatively assess the incidence of incomplete price adjustment in the narrative data, we consider all of the extracted rigidity sentences and code a new dummy variable. This variable captures cases in which firms explicitly state that the reported rigidities only caused them to limit the size of their price changes instead of fully preventing them from occurring. In other words, it reflect cases in which the reporting firms themselves perceive their price adjustments as incomplete. Furthermore, given that almost all of the reported rigidities occurred in the context of changing costs, they are best interpreted as cases of incomplete cost-pass through.

Overall, we find that incomplete price adjustment is very frequent at the firm-level, accounting for slightly more than half of the extracted rigidity sentences. Breaking the observations down according to their causes as shown in Figure 23, we also find that those rigidities that led to incomplete adjustment do not appear to be fundamentally different from the ones that fully prevented price changes. Both types are mainly driven by competitive considerations and, to some extent, by aggregate demand conditions and exciting contracts.[50]

Figure 23: Causes of Price Rigidities: Incomplete Adjustment vs. No Adjustment



Notes: The figure illustrates the distribution of reported causes of price rigidities. It distinguishes between those rigidities that caused the desired price adjustment to be incomplete, and those that fully prevented it from occurring. Sample expressions and category definitions are shown in Table 22. The sentences were assigned to multiple categories whenever that best reflected their content.

The frequent occurrences of incomplete adjustment suggest that firms face rigidities be-

[50]One exception to this is the fact that the order of demand conditions and pre-existing contracts is reversed. However, given the small sample size, it is difficult to assess if this is indeed due to fundamental differences between the two groups.

yond fixed costs of nominal adjustment. This is generally well in line with the above interpretation that price-setting is affected by real rigidities. Interestingly, though, the behavior documented here is not just driven by the strategic pricing complementarities and can instead also arise because of weak aggregate demand conditions and, to a certain extent, even because of binding nominal contracts.

Given that firms' desired price levels are not observable in realized price data, the existing price-setting literature has mainly assessed incomplete adjustment by examining the extent to which real and nominal exchange rates co-move.[51] Our findings generally corroborate the ones of that literature at the firm-level and without relying on the same assumptions, but they also establish a more direct link between incomplete adjustment and its underlying causes.[52] In the future, surveys may provide further evidence on the issue, but existing studies have not emphasized it.

2.5 Linking the Narrative Evidence to Closely-Related Observable Variables

Up to this point, we have mainly analyzed the narrative corporate filing information in separation and without linking it to other types of data. However, much of it is in fact closely related to observable variables. In the following, we explore these relationships empirically. This allows us to assess the general reliability of the narrative evidence by investigating to what extent it is consistent with the related observables. Furthermore, as we show below, it provides some additional insights on the price-setting behavior of the reporting firms.

2.5.1 Reported Price Changes and Aggregate Inflation

We begin by linking the narrative evidence on reported price changes to the behavior of aggregate inflation. More precisely, we investigate whether an index constructed from the numbers of reported price increases and decreases co-moves with the US GDP deflator. Because publicly traded firms tend to be relatively large and account for a sizable fraction of USD GDP, we would generally expect this to be the case if the reported price changes are approximately representative of actual price changes.

Since firms typically do not disclose the exact size of the price changes they implement, we cannot calculate a quantitative measure of reported inflation. However, we can exploit that changes in the aggregate price level P_t must be proportional to the net fraction of price increases in the economy, assuming that price increases have the same average size as price decreases and that all goods contribute to GDP with equal weights. To see that this is true, first assume that the GDP contributions of all goods in the economy are

[51] See Gopinath et al (2011).

[52] One of the main assumptions under which incomplete exchange rate pass-through can be interpreted as evidence of real rigidities is that exchange rate fluctuations are exogenous. See Golosov (2011) for a discussion.

equal. This implies that changes in the aggregate price level can be expressed as the simple average of all individual price changes:

$$\Delta P_t = \tilde{\Delta p}_t \tag{1}$$

Next, let N_t^+ and N_t^- be the total numbers of individual price increases and decreases, respectively. The fraction of price increases is then given by $\alpha = \frac{N_t^+}{N_t^+ + N_t^-}$ and can be used to rewrite rewrite (1) as

$$\Delta P_t = \alpha \tilde{\Delta p_{t,+}} + (1 - \alpha) \tilde{\Delta p_{t,-}} \tag{2}$$

Here, $\tilde{\Delta p_{t,+}}$ and $\tilde{\Delta p_t}, -$ denote the average sizes of individual price increases and decreases, respectively. Finally, assuming that $\tilde{\Delta p_{t,+}} = -\tilde{\Delta p_{t,-}^-}$ equation (2) simplifies to

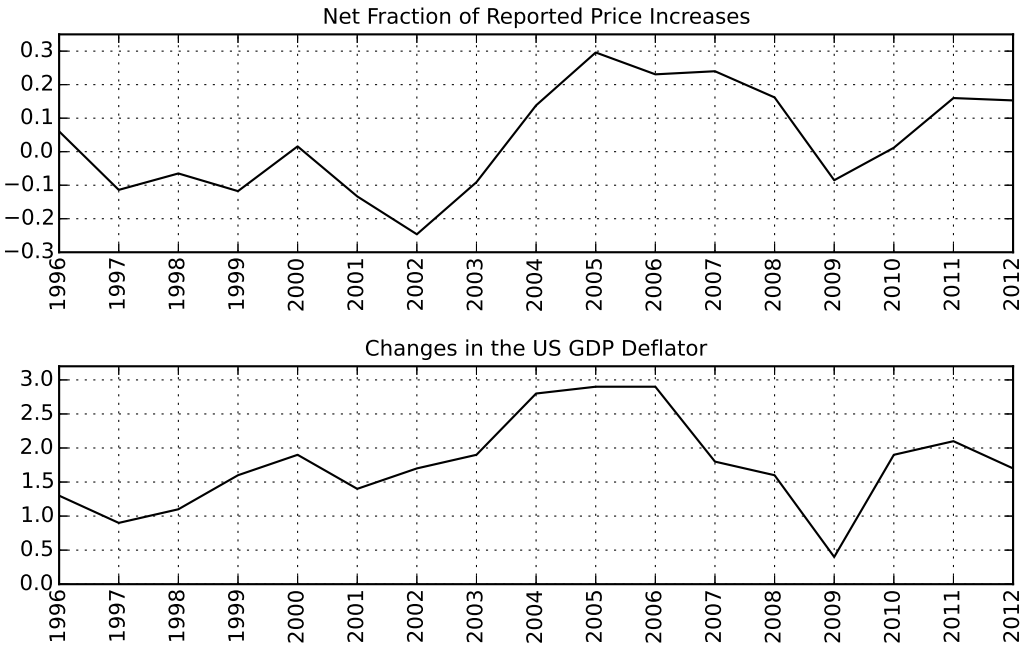
$$\Delta P_t = (2\alpha - 1) \tilde{\Delta p_{t,+}} \tag{3}$$

Using the above definition of α, this implies that the change in the aggregate price level is proportional to the net fraction of price increases in the economy:

$$\Delta P_t \propto \frac{N_t^+ - N_t^-}{N_t^+ + N_t^-} \tag{4}$$

Motivated by this relationship, Figure 24 plots data equivalents of both sides of equation (4). While we define ΔP_t as the change in the US GDP deflator, we measure N_t^+ and N_t^- as the numbers of price increases and decreases extracted from the corporate reports. The plots illustrate that the two measures are indeed positively correlated as suggested by equation (4). We interpret this finding as evidence for the (approximate) representativeness of the reported price changes, because it documents that they are indeed closely related to aggregate inflation. Furthermore, it suggests that our main analysis may be particularly informative for central bankers who aim to understand and possibly influence those types of pricing decisions that are most important for the development of the aggregate price level.

Figure 24: Reported Price Changes and Changes in the GDP Deflator



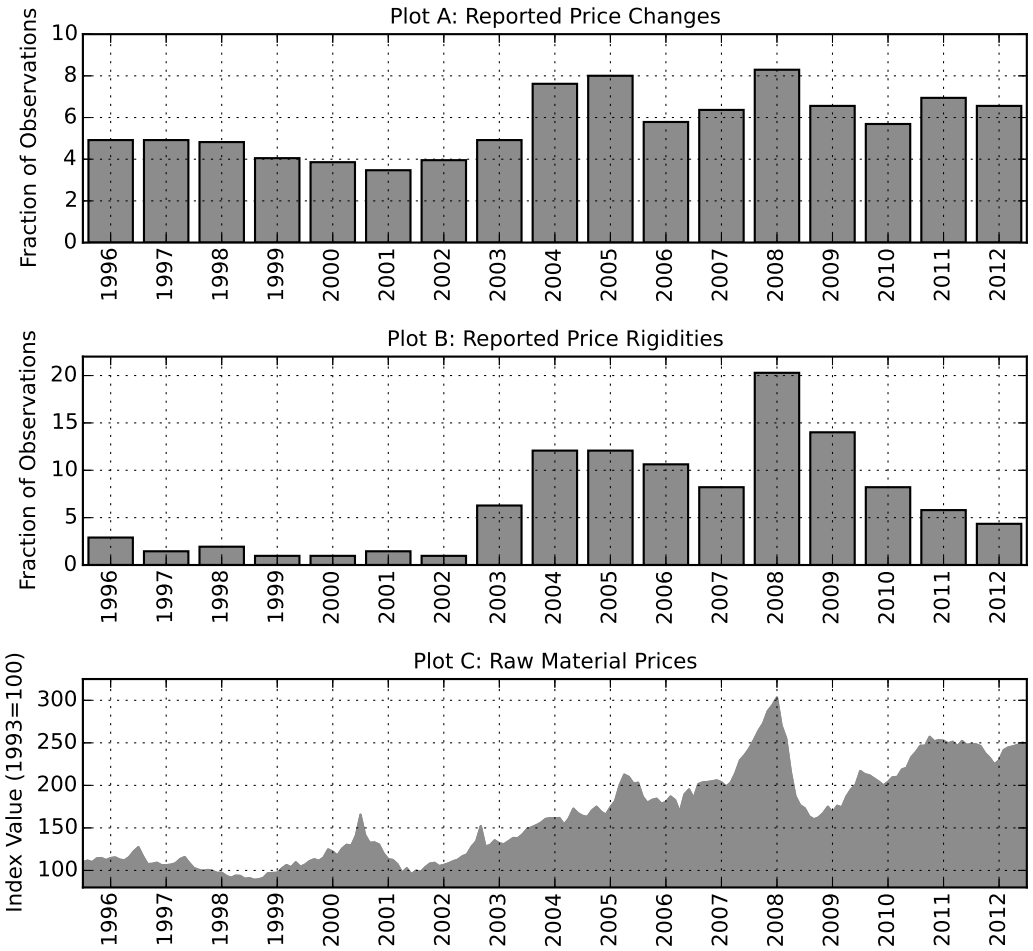
Notes: The figure shows changes in the net fraction of reported price increases and changes in the us GDP deflator. The net fraction of price changes is calculated as $\frac{N_t^+ - N_t^-}{N_t^+ + N_t^-}$, where N_t^+ and N_t^- are measured as the numbers of price increases and price decreases extracted from the corporate reports. Reports filed in year t are assigned to calendar year $t-1$. The US GDP deflator series (gdpdef) was obtained from the Federal Reserve Bank of St. Louis.

2.5.2 Reported Pricing Decisions and Raw-Material Costs

The second observable variable that we link to the narrative corporate-filing information is an index of raw material prices. The motivation for this is that we already know from the above analysis that changes in the costs of raw materials are very frequently reported as causes of both implemented and desired price changes. Therefore, if this aspect of the narrative information is accurate, we would generally expect to see more observations of both types when commodity-price fluctuations are particularly large.

To assess the extent to which this is true in the data, we plot the development of raw material prices together with the numbers of implemented price changes and reported rigidities for each one of the sample years (Figure 25). We find that both reported price changes (Plot A) and reported rigidities (Plot B) are indeed more frequent during the late-2000 commodity-price boom. Moreover, it appears that firms were generally able to change their prices by the desired amounts until approximately 2003. Once raw-material prices started to rise at a faster rate, however, the reported rigidities prevented at least some of the firms from passing along these changes in costs.

Figure 25: Reported Pricing Decisions and Raw-Material Costs



Notes: The figure illustrates the development of raw-material prices over time together with the distributions of reported pricing decisions. For each reporting year, Plot A shows the number of extracted price-change sentences, and plot B displays the number of extracted price-rigidity sentences. Reports filed in year t are assigned to calendar year $t-1$. Plot C displays raw material prices as measured by the Crude Materials for Further Processing Producer Price Index (PPICRM). This series was obtained from the Federal Reserve Bank of St. Louis and normalized to set its value for January 1 1993 equal to 100.

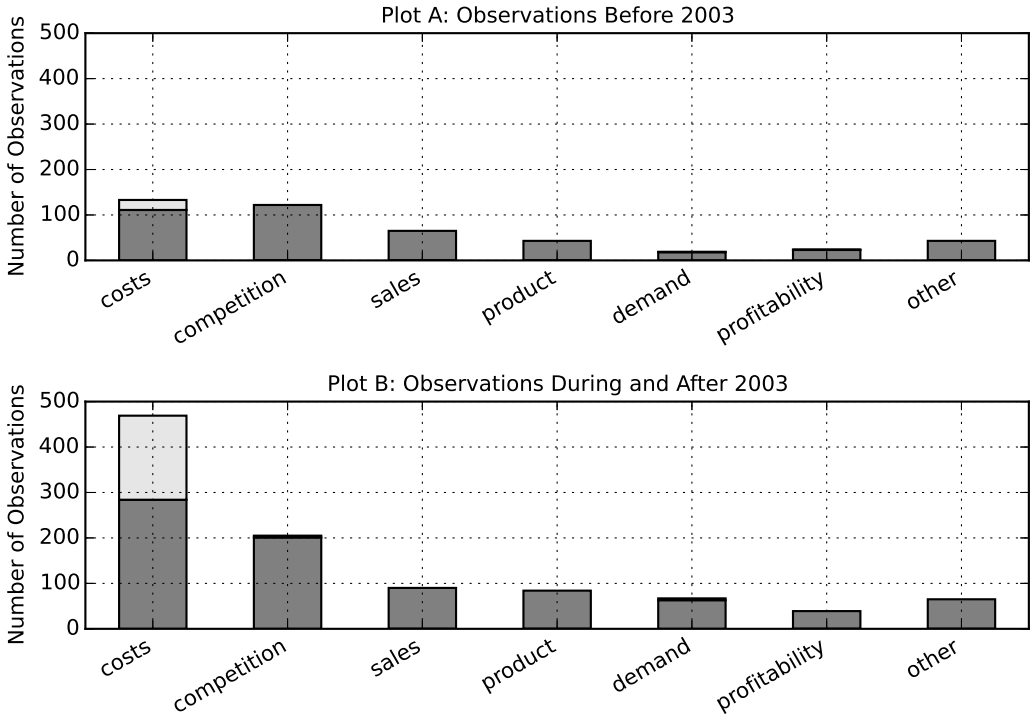
From a macroeconomic perspective, this suggests that the reported upward rigidities may have played an important role in keeping inflation relatively stable during the late-2000 commodity price boom. Especially when considering that the reported rigidities probably emphasis cases in which the desired price changes were large enough to have a noticeable impact on the firms' overall financial results, they may have also mattered for the economy as a whole.[53]

[53]Such a relationship would also be qualitatively in line with the results presented in Cecioni (2010). Based on an estimated New Keynesian model with endogenous firm entry, they suggest that a 10% increase in the number of firms in the US economy would reduce inflation by 1.4% in the short run. Also see Gust et al (2010), who argue that the empirically observed decline in exchange rate pass-through over the last two decades may be driven by increased trade integration and a correspondingly higher responsiveness to

The evolution of price changes and rigidities shown in Figure 25 also illustrates that price change frequencies by themselves are an incomplete measures of price flexibility. While an isolated look at the increasing number of price changes over the sample horizon might have suggested that rigidities became less important, the interpretation changes once we condition on the arrival of cost shocks. Indeed, an alternative story is consistent with state-dependent pricing and suggests that it was an increase in cost shocks that caused an increase in the number of price changes, while also bringing firms closer to a situation in which the upward rigidities began to bind.

This alternative story is also supported by Figure 26. Distinguishing between observations that occurred before and after 2003, it highlights that firms would have implemented approximately 50% more cost-motivated price changes during the later part of the sample had they not faced the reported rigidities.[54]

Figure 26: Causes of Implemented and Desired Price Changes Before and After 2003



Notes: The figure shows the distribution of the causes of implemented and desired price changes. Schedule A shows only observations reported before 2003. Schedule B shows only observations reported during and after 2003. Reports filed in year t are assigned to calendar year $t-1$. Implemented price changes are those reported in the extracted price-change sentences, and desired price changes are those reported in the extracted rigidity-sentences. Sample expressions and category definitions are shown in Table 19. The sentences were assigned to multiple categories whenever that best reflected their content.

competitors' pricing choices.

[54] Our choice of 2004 as the breakpoint is motivated by the fact that it marks the beginnings of the commodity-price boom, as well as the observed increase in reported price rigidities.

Table 24: Observable Firm-Level Variables: Definitions and Corresponding Verbal Patterns

Economic Concept	Verbal Patterns	Compustat Variables
Total Revenues	revenues [increased/rose/decreased/fell]	*Total Revenues* (*revt*)
Cost of Goods Sold	[cost/costs] of goods sold [increased/rose/decreased/fell]	*Cost of goods sold* (*cogs*)
Gross Profit Margin	[margin/margins] [increased/rose/decreased/fell]"	$\frac{Sales\ (sale) - Cost\ of\ goods\ sold\ (cogs)}{Sales\ (sale)}$

Notes: The table shows a number of firm-level observables in terms of their formal definition and corresponding verbal patterns. The verbal patterns are used to identify and count cases in which firms report increases or decreases in the respective variables. Square brackets denote that only one of the elements they contain is required to occur. The verbal patterns are applied such that they result in separate counts of increases and decreases, respectively.

2.5.3 Observable and Narratively Reported Changes in Firm-Level Variables

Finally, in addition to these aggregate-level comparisons, we also investigate the extent to which observable and narrative information are consistent at the level of individual firms. For this, we first construct a second dataset that contains a number of firm-level variables together with corresponding narrative information. Then, using this dataset, we assess the validity of the narrative information by correlating it with the respective variables' true values.

To construct the dataset, we again search the texts of the annual corporate filings for a number of pre-defined verbal patterns. However, the patterns that we apply here are not designed to capture complete discussions about pricing decisions. Instead, they simply aim to detect cases in which firms report increases or decreases in specific observable variables. Table 24 contains an overview of these patterns, together with definitions of the corresponding observables.

The variables that we consider here are the reporting firms' total revenues, their costs of goods sold and their gross profit margins. We choose these variables for two main reasons. First, unlike most of the general concepts firms mention in their narrative discussions of pricing decisions, all of them have corresponding entries in the firms' externally audited financial statements. This allows us to establish a clean link between narrative and verified observable information. Second, all of them are closely related to firms pricing decisions. For the actual analysis, we first search all corporate filings in our database for occurrences of the patterns shown in Table 24. Distinguishing between the directions of the reported

changes, this allows us to calculate the net number of verbally reported increases for each filing and each one of the variables. Then, we assess the relationship between this narrative measure and the variables' true changes by running regressions of the following form:

$$\Delta y_t^i = \alpha + \beta NNI_{y,t}^i + \Theta X^i + \Phi X_t + \epsilon_t^i \tag{5}$$

Here, Δy_t^i denotes the percentage change of observable variable y that firm i exhibited in fiscal year t. $NNI_{y,t}^i$ denotes the corresponding net number of reported increases in the same variable. Furthermore, X^i is an optional set of firm-fixed effects defined at the gvkey-level and X_t denotes optional time-fixed effects defined at the level of fiscal years.[55] The coefficient of interest is β. If the narrative information about changes in variables is informative about the respective variables true behavior, this coefficient should be positive.

Table 25: Large-Sample Regressions: Narratively Reported and Observable Changes in Selected Variables

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Change in Profit Margin		Change in Cost of Goods Sold		Change in Revenues	
Net # of Narratively Reported Increases in the Dependent Variable	0.53%***	0.53%***	6.14%***	4.85%***	2.41%***	1.96%***
	(42.25)	(20.69)	(24.54)	(10.09)	(91.24)	(31.81)
Fixed Effects	✗	✓	✗	✓	✗	✓
Time-Fixed Effects	✗	✓	✗	✓	✗	✓
Observations	515,874	515,874	520,840	520,840	539,335	539,335
R-squared	0.00	0.00	0.00	0.05	0.02	0.06
Number of Companies	15,286	15,286	15,425	15,425	15,468	15,468

Notes: In all specifications, the net number of reported increases is defined as the number of verbally reported increased minus the number of verbally reported decreases in the respective dependent variable. These measures are obtained by searching the corporate filings for the verbal patterns shown in Table 24. Parentheses contain t-statistics based on robust standard errors. *** , ** and * denote statistical significance at the 1% , 5% and 10% levels, respectively. Fixed effects are applied at the level of companies as defined by their gvkeys. Constants are included where applicable but not reported. Time-fixed effects are applied at the level of fiscal years. All dependent variables are winsorized at the 1% level.

The results of these regressions are shown in Table 25. For all three of the variables, they suggest that the respective net number of reported increases is positively correlated

[55] Mention the robustness when using calendar years instead.

to the variables true changes. Furthermore, they document that this relationship holds regardless of whether firm- and time-fixed effects are included. This suggests that narrative information about changes in the observable variables is informative about their true changes, and that its information is more complex than general variation in growth rates over time or across firms.

2.6 Conclusion

In this paper, we have quantified narrative evidence from 20 years of archived corporate reports to construct a novel dataset on the price setting behavior of publicly traded companies. This dataset reflects the properties of 1,949 pricing decisions taken by 983 different firms and encodes unique causal information that cannot be inferred from realized prices alone. The information it contains also differs from that used in survey studies because it avoids the use of pre-defined questions, because its observations reflect specific pricing decisions, and because it covers more than 20 years instead of a specific business cycle stage.

Using the dataset, we have then explored a number of important aspects of the price setting behavior of publicly traded firms. In particular, we have investigated the causes of price changes and price rigidities, cases of incomplete adjustment, and variation across both time-periods and sectors. Furthermore, by linking the extracted narrative discussions to corresponding observables, we have provided empirical evidence for their informational content.

Our main findings can be summarized in terms of four results. First, the causes of price changes are highly asymmetric. While price increases are mainly driven by increases in raw-material costs, price decreases mainly result from competitive pricing pressures. Second, pressures resulting from the pricing behavior of competitors are also the most important factor preventing firms from raising their prices, followed by considerations about weak aggregate demand and existing contracts. Third, the presence of real rigidities is not just born out by frequent references to the behavior of competitors, but also by a large number of cases in which firms adjust prices but explicitly describe these price changes as incomplete. Fourth, as evident from the late-2000 commodity price boom, a larger number of price changes does not necessarily imply that rigidities are weaker or less binding.

These results highlight that price-setting models should generally be able to reproduce asymmetric behavior between price increases and decreases, as well as variation in price setting over the business cycle. They also lend support to state-dependent pricing and suggest that the degree of price rigidity should not be calibrated based only on observed price durations. Instead, measures of price rigidity should take both the realized price durations and the properties of their underlying shocks into account. Finally, the various types of evidence in favor of strategic pricing complementarities suggest that real rigidities may indeed be a valid mechanism of generating monetary non-neutrality in structural

models.

Chapter Three: Using Financial Markets to Estimate the Macro Effects of Monetary Policy: An Impact-Identified FAVAR[56]

3.1 Introduction

> "If all goes as planned, the changes in financial asset prices and returns induced by the actions of monetary policymakers lead to the changes in economic behavior that the policy was trying to achieve".
>
> Ben S. Bernanke, London School of Economics Public Lecture (2003)

It is widely believed that monetary policy affects both the real and financial sides of the economy, but actual estimation of these effects must carefully take the endogeneity of policy decisions into account. I approach this identification problem in a FAVAR framework, using a small number of contemporaneous restrictions imposed exclusively on financial market variables. These restrictions are motivated by separate high-frequency estimates and carry over to lower frequencies by the logic of the efficient market hypothesis (EMH) as defined in Fama (1970).[57] In terms of results, I find strong delayed effects on real variables such as housing starts, employment and industrial production. Consumer prices, on the other hand, appear to be almost unaffected by monetary policy shocks. Furthermore, unlike the benchmark recursive FAVAR estimated as in Bernanke et al (2005), the method proposed in this paper delivers financial market responses that are compatible with the EMH.

The fundamental motivation for this paper stems from two main points. First, credibly causal estimates of financial market responses to monetary policy can be obtained by considering only those very short time-periods at which monetary policy shocks are actually known to arrive. Second, at least some of the fundamental underlying forces that drive the dynamics of financial markets may also matter for the real side of the economy.[58] Together, these two points suggest that response estimates of low-frequency macro variables may benefit from the information contained in their financial-market counterparts. In other words, if some fundamental factors matter for both financial and real variables, it should in principle be possible to exploit that overlap.

[56] A slightly earlier version of this chapter was circulated as Sveriges Riksbank Working Paper No. 267.

[57] More precisely, I will argue below that unless market participants can regularly earn risk-adjusted excess returns by timing the market, there is a tight correspondence between daily and monthly responses to monetary policy shocks. A recent review of the empirical EMH literature is provided in Lim and Brooks (2011).

[58] For example, such factors may include both realizations and expectations of general economic activity, unemployment, real rates and inflation.

Existing work provides strong empirical support for such a close relationship between the real and financial sides of the economy: First, a large high-frequency literature shows that financial market variables immediately react to the arrival of monetary policy and other macro news.[59] Second, it has been shown that daily financial market data can be used to improve the quality of macro forecasts.[60] Third, a number of studies have even shown which financial instruments contain information about which macro economic concepts.[61] In spite of this clear picture, only relatively few empirical studies on the real effects of monetary policy exploit the additional information provided by financial markets. Three of the earliest and probably most important examples of this group of papers are Bagliano and Favero (1999), Faust et al (2003) and Faust et al (2004). All of these studies use high-frequency data to construct financial market shocks outside of the VAR framework. Then, in a second step, they use this information within a vector autoregression. In related work, Cochrane and Piazzesi (2002) construct monetary policy shocks from high-frequency data and carefully compare them to those obtained using traditional VAR methods. D'Amico and Farka (2011) use high-frequency data to allow for contemporaneous feedback between monetary policy and the stock market in an otherwise recursive VAR.[62] Francis et al (2011) use a MIDAS VAR framework to estimate structural impulse responses to monetary policy shocks. Finally, in contemporaneous work, Gertler and Karadi (2015) identify the monetary policy shock in a low-frequency VAR using high-frequency surprises as instruments.

The work presented in this paper is closely related to the above studies in that it explicitly recognizes and exploits the link between high-frequency financial market data and the real economy. However, instead of estimating small-scale VARs such as those in Bagliano and Favero (1999), D'Amico and Farka (2011), and Gertler and Karadi (2015), it uses a number of high-frequency estimates to identify the monetary policy shock in a 133 variable FAVAR. One advantage of the FAVAR framework is that it can include and thus restrict a relatively large number of financial variables without causing degrees of freedom issues. This allows me to exploit that differet financial variables contain information about different underlying economic factors. In addition, the use of several financial market restrictions implies that each one of them need only be implemented in a relatively weak manner. Instead of exact point restrictions, sign or range requirements generally turn out to be sufficient for identification. This makes the resulting structural FAVAR relatively robust to uncertainty in the high-frequency estimates. As a positive side effect, the approach used in this paper also allows for the inclusion of standard theory-based

[59] Some important papers of this very large literature are Cook and Hahn (1989), Kuttner (2001), Rigobon and Sack (2004), Bernanke and Kuttner (2005), Faust et al (2007), Beechey and Wright (2009) and Ammer and Wongswan (2010).

[60] Andreou et al (2013).

[61] For example, Estrella and Hardouvelis (1991) document that the term structure predicts real activity, Gürkaynak et al (2010) extract inflation expectations from the TIPS yield curve, and Kueng (2012) shows that municipal bond spreads contain information about actual and expected federal taxes.

[62] The work of Rigobon and Sack (2003, 2004) provides strong support for such a bidirectional relationship. In an alternative approach, Bjornland and Leitemo (2009) impose a combination of long- and short-run restrictions to allow for a similar relationship without actually using high-frequency data.

restrictions on macro variables and inherits the general advantages of the FAVAR framework.[63] It yields impulse responses for all 133 series included in the model and alleviates the omitted variables bias that can occur in non-factor VARs. Crucially, the financial market restrictions forming the backbone of the proposed identification scheme are supported by separate high-frequency estimates and basic economic theory.

3.2 Framework and Identification

The estimation and identification of the monetary-policy FAVAR proceeds in the following three steps:

1. Estimate the responses of financial market variables to monetary policy shocks using high-frequency data.

2. Estimate a reduced form monthly FAVAR that contains the same financial market variables.

3. Use the financial-market properties obtained in step 1 to identify the monetary policy shock in the reduced form FAVAR

In this section, I explain and discuss each one of these steps in detail.

3.2.1 High-Frequency Estimation

This first step of the estimation procedure exploits high-frequency identification methods to establish how financial market variables react to monetary policy surprises. The results obtained here form the basis of the restrictions used to identify the FAVAR below. Methodologically, the estimation closely follows a large established literature on the financial market effects of monetary policy.[64] First, I choose the high-frequency sample such that it only includes short time periods around FOMC meetings. This ensures that much of the variation in the sample is indeed driven by shocks to monetary policy. Second, I use survey data to disentangle expected from unexpected policy actions and keep only the latter as my explanatory variable. One important reason for this step is that unexpected policy components are unlikely to be correlated with potentially omitted variables.[65] Assuming that the chosen time-interval around the FOMC policy meetings spans exactly one day, the main high-frequency regression can then be written as

$$r_d^i = \alpha^i + \beta^i s_d + \epsilon_d^i \tag{6}$$

Here, r_d^i is the return of asset i on day d, s_d is the corresponding policy surprise, and

[63] See Bernanke et al (2005) and Boivin et al (2009) for general discussions of the FAVAR framework.

[64] For example Cook and Hahn (1989), Kuttner (2001), Rigobon and Sack (2004), Bernanke and Kuttner (2005), Faust et al (2007), Beechey and Wright (2009) and Ammer and Wongswan (2010).

[65] Ehrmann and Fratzscher (2004) provide evidence for this.

β^i is asset i's monetary policy reaction coefficient.[66] As mentioned above, only days on which FOMC meetings took place are actually included in the sample. While the existing literature uses many closely related variations of this regression, the resulting estimates are generally very similar across different specifications.

3.2.2 FAVAR Estimation

The second main element of the approach presented in this paper is the reduced-form FAVAR. Using the financial estimates from above, this FAVAR will be given a structural interpretation in the third and last step of the estimation procedure. Since the econometric framework is standard, I present only a brief review. Readers interested in a more detailed discussion may want to refer to original papers of Bernanke et al (2005) and Boivin et al (2009). First, assume that the economy is fundamentally driven by the observable monetary policy rate I_t and a relatively small number K of unobserved factors F_t. Then, let the joint law of motion of these factors be given by the following reduced form VAR

$$C_t = \Phi(L)C_{t-1} + v_t \tag{7}$$

, with

$$C_t = \begin{bmatrix} F_t \\ I_t \end{bmatrix} \tag{8}$$

and v_t being i.i.d zero-mean innovations with covariance matrix Ω. Finally, assume that all of the variables contained in the FAVAR dataset X are related to the factors as follows:

$$X_t = \Lambda C_t + e_t \tag{9}$$

Here, Λ is an N by $(K+1)$ matrix of factor loadings and e_t is a vector of series specific disturbances. In terms of interpretation, ΛC_t denotes what is typically called the *common component* of X_t, whereas e_t represents the *series-specific component*. To obtain an estimate $\hat{F}_t$ of the unobserved factors F_t, I regress the full dataset X_t on the monetary policy instrument I_t and then extract the first K principal components from the resulting residual series.[67] Using this factor estimate $\hat{F}_t$, the transition equation (7) can be estimated by OLS. To take estimation uncertainty in the factors and the VAR into account, I perform the same two-stage bootstrap procedure used in Boivin et al (2009). Finally, for a direct comparison to the standard recursive FAVAR identification scheme, I also compute alternative factors following Bernanke et al (2005).[68]

[66] The policy surprise is defined as the difference between actual and expected policy actions. Expectations of monetary policy are derived either from surveys or from Eurodollar futures as discussed below. For the estimation, I chose a SUR framework to exploit cross-equation correlations between the error terms in (1).

[67] All series in X_t are transformed for stationarity as documented in the appendix and initially normalized to have a standard deviation of 1.

[68] The classification of variables into the slow- and fast-moving categories is reported in the appendix.

3.2.3 FAVAR Identification

Having estimated both the high-frequency responses of financial market variables and the reduced-form FAVAR, I combine the two in order to identify the monthly-frequency monetary policy shock. This step exploits the knowledge gained from high-frequency data to estimate structural responses of lower-frequency macro variables. I begin by imposing that the FAVAR and the high-frequency responses of financial variables are generally consistent. For example, if the identified high-frequency response of the S&P500 to a monetary tightening is a quick and permanent drop, the corresponding FAVAR response should arguably not be flat or positive. Then, as a second step, I can exploit the fact that the FAVAR's underlying factors govern not only the dynamics of the financial series, but also those of the low-frequency real variables. Using equation (9) to link high-frequency and low-frequency variables allows me to recover indirect restrictions on the real side that are implied by the separately observed high-frequency restrictions on the financial side. Thus, if one is confident about the financial variable restrictions, one can also be confident about the resulting responses of the real variables. In terms of technical implementation, I use a factor generalization of the standard sign/range restriction approach to impose the desired restrictions on the financial variables.[69] The exact procedure is described in algorithm 1 and performed on each one of the reduced-form bootstrap draws. To obtain impulse response quantiles, I follow the conventional approach of sorting the obtained IRFs at each horizon and then selecting the desired quantiles.[70]

Algorithm 1 Imposing High-Frequency Restrictions on the FAVAR

1. Orthogonalize the estimated VAR in the factors denoting the structural matrix $\hat{\phi}$.
2. Obtain a quadratic matrix Z of dimension $(K+1)$, where each element $Z_{i,j}$ is drawn from an independent standard normal distribution.
3. Obtain the QR decomposition of Z such that $Z = QR$ and $Q'Q = I_{K+1}$
4. Obtain the contemporaneous factor impulse responses for the current identification draw Q as $Q\hat{\phi}$.
5. Calculate contemporaneous impulse responses of the restricted variables using observation equation (9).
6. Accept the current draw of Q if and only if these impulse responses satisfy the desired restrictions. Otherwise go back to step 1.

[69] See Canova and De Nicoló (2002), Uhlig (2005), Rubio-Ramírez et al (2010), Fry and Pagan (2011). Also see Ahmadi and Uhlig (2009) for a Bayesian FAVAR with sign restrictions.

[70] Fry and Pagan (2011) argue that this approach is somewhat problematic since there may in fact not be one single identification draw and thus underlying model that yields this specific quantile. Their argument is theoretically valid, but Canova and Paustian (2011) also show that the measure of Fry and Pagan (2011) does not generally perform better in recovering the true impulse responses. I decide to use the standard approach for reasons of comparability and in order to be able to focus on the effects of the high-frequency identifying restrictions.

3.2.4 Identifying Assumptions

The identification scheme proposed in this paper is valid if the following two conditions are satisfied: First, the estimated high frequency responses must correctly reflect those features of their true counterparts that are used as FAVAR restrictions. Second, these features must also hold at the monthly frequency. In the following, I briefly discuss both of these conditions and why they are likely to hold.

Condition 1: Similarity Between the True and Estimated High-Frequency Responses

Since the identification approach outlined above does not use exact high-frequency point estimates, it also does not require them to be exactly identical to their true values. Instead, the estimated responses need only be correct in terms of those features that are actually used as restrictions in the FAVAR identification. For example, if we estimate a negative stock market response to a monetary tightening and impose this property in terms of a sign restriction in the FAVAR, then the approach merely requires that the true high-frequency response indeed be negative. To ensure that condition 1 holds, the implementation below uses only high-frequency restrictions that are robust to variations in data frequency, surprise measures and identifying assumptions.[71] In addition, as discussed in section 3.4.2, all of the restrictions used to identify the FAVAR below are also supported by economic theory.

Condition 2: Similarity Between the High-Frequency and Monthly-Frequency Responses

The second main assumption of the proposed identification scheme is that the properties derived from high-frequency estimates carry over to monthly-frequency data. To see the correspondence between monthly and daily responses formally, consider the financial market series X^i and its month-t realization X^i_t. This realization can be expressed in terms of the previous month's value and the daily returns during the month:

$$X^i_t = X^i_{t-1} * \prod_{d=1}^{D} R^i_d \tag{10}$$

Here, R^i_d is the daily return of series X^i_t on day d of month t, and D is the last day of month t. Taking the monthly return in the form of log-differences and considering the daily log-return on $d = k$ separately then yields

$$\Delta x^i_t = \sum_{d=1}^{k-1} r^i_d + r^i_k + \sum_{d=k+1}^{D} r^i_d \tag{11}$$

Now assume that a monetary policy shock arrives on day k. Clearly, this shock cannot affect any of the returns r^i_d with $d < k$, because they are already fixed at the time of its arrival. Furthermore, if the EMH holds, new information is immediately priced in and

[71] For example, Bernanke and Kuttner (2005) perform event-study regressions, whereas Rigobon and Sack (2004) employ identification through heteroskedasticity. The identifying assumptions of these two studies are fundamentally different, but the results are not.

thus delayed reactions on later dates $d > k$ are also ruled out.[72] Therefore, the monthly return reaction must occur on the day of the monetary policy shock itself, via r_k^i. It is this same-day reaction that the high-frequency regressions given by equation (6) capture and that forms the basis of the monthly-frequency restrictions. The restrictions used for the FAVAR below are, however, even weaker than exact equalities in the sense that they are robust to the existence of reasonably small differences between the reactions of Δx_t^i and r_k^i. For example, mild delayed financial market responses or reversals on $d > k$ that may have occurred in the specific sample we use would not invalidate the proposed identification scheme. Similarly, general low-frequency price predictabilities such as those discussed by Cochrane (2007) are accommodated by the restrictions chosen below.[73] Section 3.4.2 discusses each one of the restrictions used in this paper to further clarify the argument.

3.2.5 Some Differences to Alternative Identification Schemes

First, compared to the standard identification approaches used in many (FA)VARs, this study avoids restrictions that are exclusively based on economic theory and not supported by additional high-frequency evidence. For example, it does not impose a recursive ordering and also does not rely on sign-restrictions for macro variables. This allows us to estimate responses of macro variables without prior assumptions about their behavior.[74] Second, compared to existing studies that use single high-frequency point estimates to identify non-factor VARs, this paper imposes several high-frequency restrictions at a weaker level. This can make the identification remain valid even if the high-frequency policy shocks are not *exactly* identical to their lower-frequency counterparts.[75] Moreover, the fact that several different financial variables can be restricted also implies that several different kinds of identifying information can be exploited. Of course, how much the imposed financial market restrictions say about the responses of different macro variables to monetary policy is directly revealed by the FAVAR results. For example, if the financial market restrictions only have power for a subset of variables, only the responses of these variables will exhibit tight confidence bands. Finally, the approach proposed in this paper also differs from the mixed-frequency method of Francis et al (2011), who focus primarily on the *timing* of policy rate innovations within the month and how these innovations can be optimally aggregated. In contrast to their approach, my method sidesteps time aggregation issues by using only general properties of financial market responses that would hold under any timing and aggregation scheme. To see this, assume as discussed above

[72] See Fama (1970). If systematic delayed reactions did exist, market participants would be able to trade on them and regularly earn risk-adjusted excess returns. That, however would make them disappear.

[73] For example, D'Amico and Farka (2011).

[74] For example, consider the restriction of Ahmadi and Uhlig (2009) that prices cannot increase following a monetary tightening. Even though this no-price-puzzle restriction is in line with many standard macro models, it is at odds with the Barth and Ramey (2011) and Chowdhury et al (2006) cost-channel of monetary transmission.

[75] Considering that (FA)VAR expectations are a function of monthly data whereas the high-frequency expectations condition on intra-month information, there is no obvious reason to believe and thus impose that the two should be *exactly* the same.

that monetary policy shocks have highly persistent level effects on financial markets that materialize within the day. Then, the timing of these shocks within the month will not affect the monthly level difference caused by the shock.

3.3 The Dataset

The High-Frequency Dataset The high-frequency dataset spans the period 1994-2008 and contains daily observations of monetary policy surprises as well as a number of financial market variables.[76] As discussed above, only observations for policy days are actually used in the regressions. The main monetary policy surprise variable is defined as the difference between survey expectations and the actually realized monetary policy action.[77] For robustness, I also construct an alternative surprise measure that extracts expectations from a spliced front-month Eurodollar futures rate with a maturity of 3 months.[78] Finally, the dataset also includes the spread between the 1-year and 10-year treasury rates, the S&P 500 return, and returns on a number stock market sector series. I obtain these variables from the FRED database and the website of Kenneth French, respectively.[79]

The FAVAR Dataset The appendix contains a detailed overview of all FAVAR series and the transformations I apply for stationarity. The sample is of monthly frequency and covers the years 1973-2011. Thus, while I include the outbreak of the recent financial crisis, I exclude those periods during which central banks increasingly used non-conventional policy measures. The main reason for this is that such non-convential tools are difficult to capture using short-term rates. To ensure that my results are comparable to those of Bernanke et al (2005) and Boivin et al (2009), I take their datasets as starting points. The largest part of the macro series are taken from the DRI basic economics database that is also used by the authors of these two studies. While Boivin et al (2009) add several hundred disaggregated price series to their core macro dataset, I focus on financial markets. My additional variables are 30 stock market sector returns, 3 excess return variables between different stock sectors, and the 3 main measures of bank credit from the FRB H8 dataset.[80] In terms of data timing, I use monthly averages for all financial variables. This is important to ensure that the impulse responses of financial variables to monetary policy shocks have the correct magnitude. To see this, consider that a monetary policy shock occurring in the middle of the month will increase the *average* fed funds rate only by half of the shock size. If stock prices are measured in terms of end-of-the-month

[76] I choose the starting date in line with the existing literature and the fact that the FOMC started openly announcing its policy decisions in 1994. The dataset does not continue beyond 2008, because my survey series ends in that year.

[77] This variable is taken directly from Kilian and Vega (2011).

[78] This second series is based on CME data and obtained via Datastream.

[79] The FRED data is available via http://research.stlouisfed.org/fred2/ and the Kenneth French data library can be accessed via http://mba.tuck.dartmouth.edu/pages/faculty/ken.french/data_library.html .

[80] It has repeatedly been argued that bank credit is an important indicator taken into consideration by the FED in its policy decisions. If that is the case, it should be included in the dataset.

values, however, they will reflect the response to the full size of the shock and thus appear larger than they actually should. Using both variables in the same timing convention eliminates this inconsistency.

3.4 Empirical Results

3.4.1 High-Frequency Results

Table 26 summarizes the estimated high-frequency responses of a number of financial market variables to a 100 basis points monetary policy shock. These responses form the basis of the restrictions imposed below to identify the monetary policy shock in the FAVAR. The regressions confirm the well-established result that financial market variables react quickly and significantly to unexpected monetary policy actions. For the stock market, they suggest a significant drop, and that the healthcare, food and utilities sectors react less than the auto sector. Furthermore, the slope of the yield-curve decreases in response to a monetary tightening. I also report the reaction coefficient of the USD/Pound exchange rate from Faust et al (2007). Their intraday analysis shows that the US Dollar appreciates relative to the Pound Sterling.[81]

[81] Since exchange rate markets are particularly noisy, the use of intraday data is important for obtaining precise estimates. My high-frequency dataset is only at daily frequency, however, so I report this particular result from Faust et al (2007).

Table 26: The High-Frequency Response of Financial Variables to Monetary Policy Shocks

VARIABLES	(Stock Index) S&P500	(Stock Sectors) Food-Cars	(Stock Sectors) Healthcare-Cars	(Stock Sectors) Utilities-Cars	(Yield Curve) Term Spread	(Exchange Rate) Pound/USD
Survey Shocks						
Coefficient	-7.15%***	10.33%***	8.179%***	10.22%***	-0.357***	-
Z-Statistic	(-6.176)	(7.618)	(6.264)	(6.637)	(-4.622)	-
R-squared	0.244	0.330	0.250	0.272	0.153	-
Futures Shocks						
Coefficient	-5.56%***	8.974%***	8.128%***	8.266%***	-0.353***	1.68**
Z-Statistic	(-3.505)	(4.723)	(4.694)	(3.886)	(-3.538)	n/a
R-squared	0.094	0.159	0.157	0.113	0.096	n/a
Observations	118	118	118	118	118	n/a

SUR estimates. Constant terms included but not reported. Survey Shocks defined as the difference between survey expectations and policy decisions. Futures shocks defined as changes in spliced 3-month Eurodollar futures. Term spread defined as the difference between the 10-year and 1-year treasuries. Regressions contain only one of the shock measures at a time. Exchange rate coefficient from Faust et al (2007). *** p<0.01, ** p<0.05, * p<0.1

3.4.2 Derived FAVAR Restrictions

Based on the high-frequency results reported in Table 26, I derive the following FAVAR restrictions that are also summarized in Table 27. Since financial market variables react immediately to the arrival of monetary policy news, I impose all restrictions exclusively on impact.

i) Strong Negative Response of the S&P500

First, I require that the S&P500 drops by at least 2% in response to an exogenous 100 basis points monetary tightening. While my own point estimates and those of other high-frequency studies suggest that the actual reaction is much stronger, I choose to remain relatively conservative.[82] A negative response of the stock market to an increase in interest rates is also supported by theory. Discounted dividend pricing suggests a decrease in value as the discount rate increases.[83] If future dividends also decrease in response to monetary tightening, this effect becomes even stronger. Alternatively, we can think of bonds and stocks as substitutes. Then, as the monetary tightening increases the real rate on bonds, stocks should immediately become less attractive and thus exhibit a drop in prices.

ii) Strong Relative Response of the Automotive Sector

Second, I restrict the responses of the healthcare, food and utilities sectors to be less pronounced than that of the automotive industry. Again, this restriction is not only supported by the above regressions, but it is also well in line with basic economic theory. If we consider cars to be the archetypical debt-financed durable good, higher real rates should decrease their demand and thus the market valuation of the sector. On the opposite side of the spectrum, the healthcare, utilities and food sectors do not rely on debt financing in a similar fashion and should therefore not react as much. In addition, if cars are luxury goods, their sector should react more to a monetary tightening than basic goods sectors.

iii) Decrease in the Slope of the Yield Curve

Third, I impose that the slope of the yield curve decreases following a monetary tightening. To see that this restriction is also supported by theory, consider the expectation hypothesis of the term structure.[84] Abstracting from risk premia, one can think of rates at the long end of the term-structure as expectations of average future short-term rates. Then, knowing that short-term rates are not perfectly persistent, the long end of the yield curve should always move less than one-for-one with monetary policy shocks occurring at the short end.[85]

iv) Appreciation of the Dollar

[82] Given their smaller windows around the monetary policy announcement, the confidence bands of intraday high-frequency studies are less affected by noise and therefore typically even tighter than the ones reported here.

[83] Gordon (1959).

[84] See Lutz (1940) , Campbell (1986) and Cook and Hahn (1989).

[85] As discussed in Ellingsen and Söderström (2001), deflationary effects of monetary policy may even cause long-run rates to decrease after a monetary tightening. Such an inverse relationship is a particularly strong case of the restriction imposed here and therefore compatible with it. Also, any systematic effects that monetary policy may have on risk-premia along the yield curve are unlikely to be large enough on average to cause a violation of the imposed restriction.

Fourth, I impose that the USD appreciates relative to the Pound Sterling following a monetary tightening in the US. This restriction derives additional theoretical support from theories of interest rate parity. Intuitively, if the nominal rate earned on US debt securities increases with the Federal Funds shock, then the exchange rate must slowly depreciate over time to offset this effect. Therefore, an initial appreciation must occur. Also, even if the uncovered interest rate parity relationship were not to hold in the data, an appreciation could still be explained. In the absence of a slow reversal as predicted by the UIP, it would simply be rational to invest in the currency that offers an increased return after the shock.

v) Federal Funds Range Restriction

Finally, I impose that the contemporaneous impact of a 100 basis points federal funds shock on the federal funds rate itself lies between 80 and 120 basis points. To understand this restriction, assume that the FOMC decides to increase the policy rate by 100 basis points at one of its meetings. What I require, then, is that contemporaneous relationships in the economy cannot *systematically* cause the committee to adjust this initial decision by more than 20 basis points at a later date within the month. There are two reasons for this restriction. First, the institutional setup of the FOMC dictates that decisions typically occur only once a month. Thus, strong systematic reversals or increases following the original decision are very unlikely. While unscheduled intra-meeting decisions do in general allow for rate changes within the month, these would still have to be strong and systematic enough to lead to a violation of the +/- 20 basis points interval that I impose. Second, if a given interest rate decision were *generally* followed by a strong second-round increase or decrease within the month, the FOMC should become aware of this mechanism after some time and take it into account when making regular future interest rate decisions. This should then eliminate or at least strongly dampen the effect. In addition, the above argument that market participants would be able to trade against any truly systematic pattern also applies here.

vi) Optional Sign Restrictions on CPI and Industrial Production

As discussed above, one of the properties of the identification approach presented in this paper is that the financial market impact restrictions can easily be combined with other assumptions researchers may be interested in. I make use of this option and also include additional sign restrictions on consumer prices and industrial production in a alternative identifying specification. Canova and Paustian (2011) show that these two restrictions are consistent with a large class of standard macroeconomic models, and it may therefore be interesting to see how they interact with the high-frequency properties derived from the financial market estimates.[86]

[86]It should be noted, though, that these two additional restrictions do not form part of the novel FAVAR identification scheme proposed in this paper. There is no direct high-frequency evidence for them.

Table 27: Summary of Impact Restrictions for a 100bps Shock in the Federal Funds Rate

	(1) high-frequency restrictions	(2) high-frequency and robust sign restrictions
S&P 500	¡ -2%	¡ -2%
Healthcare - Cars	positive	positive
Term Spread	negative	negative
USD/Pound	negative	negative
Federal Funds	80 bps ¡β¡ 120 bps	80 bps ¡β¡ 120 bps
CPI	-	negative
Industrial Production	-	negative

3.4.3 FAVAR Results

For the reduced form FAVAR, I set the number of unobservable factors to K=5 as in Bernanke et al (2005) and Boivin et al (2009). Increasing the number of factors to 6 and 7 leaves my results qualitatively unchanged and quantitatively very similar to the baseline case. The lag length of the FAVAR is set to 5, and the reported confidence bands cover the 68% range around the median at each time horizon.[87] For comparison, I also report impulse responses obtained using the standard recursive identification scheme of Bernanke et al (2005).[88] As noted above, it is generally possible that the resulting confidence areas also include responses to other structural shocks. However, this does not change the interpretation that the monetary policy responses lie within the same area at the reported confidence level.

FAVAR Responses of Financial Variables For each of the 3 different identification schemes, Figure 27 shows the impulse responses of 5 key financial variables to a 100 basis points shock in the federal funds rate. While the reactions of the federal funds rate and the term spread are very similar across the different identification schemes, the stock market responses clearly differ between the recursive and impact-identified versions. Instead of the instantaneous stock market reactions implied by the high-frequency results, the recursive identification shows only small delayed responses. As argued above, however, such a systematic delayed reaction contradicts the efficient market hypothesis. If it really were the true pattern, market participants could make risk-adjusted profits by trading on it. That, however, should make the pattern itself disappear.[89] The responses obtained with

[87] While both Bernanke et al (2005) and Boivin et al (2009) use 13 lags, the Hannan-Quinn and Schwarz information criteria suggest that fewer lags are optimal.

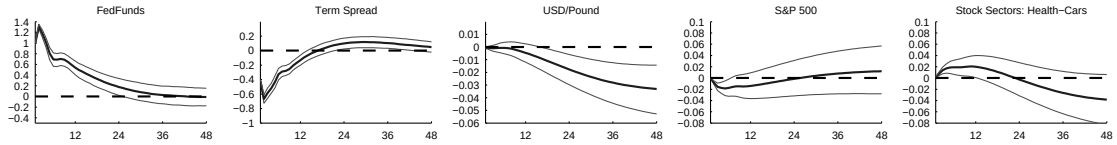
[88] The classification of variables into slow and fast groups is reported in the appendix. The results are, however, very robust to using the same factor specification in both cases.

[89] Note that the high-frequency identified stock market responses also exhibit a delay before they reach their maximum. However, this is an artefact of the financial series being expressed in terms of monthly averages. As explained above, in averaged data, any permanent shock not happening at the very beginning of the month will only reach its full level in the month that follows.

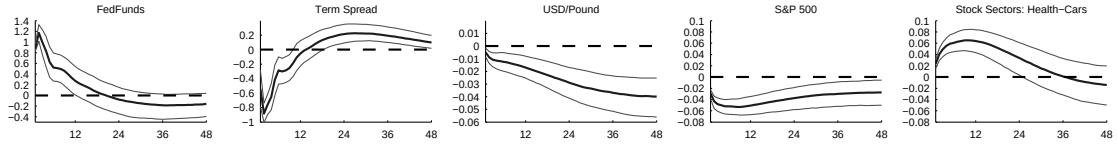
the identification scheme proposed in this paper, on the other hand, are well in line with the EMH: The initial reaction occurs contemporaneously and its effect is highly persistent. Even 4 years after the initial shock, the response remains significant. Importantly, this feature is a result of the identification scheme and not imposed as a restriction.

Figure 27: Monthly Financial Responses to a 100 Basis Points Monetary Policy Shock

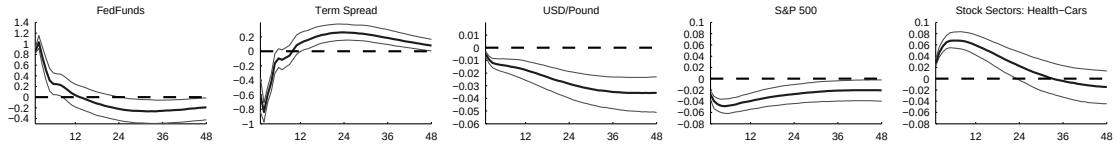
Schedule A: Standard Recursive Identification



Schedule B: Financial Market Identification



Schedule C: Financial Market and Robust Sign Restrictions



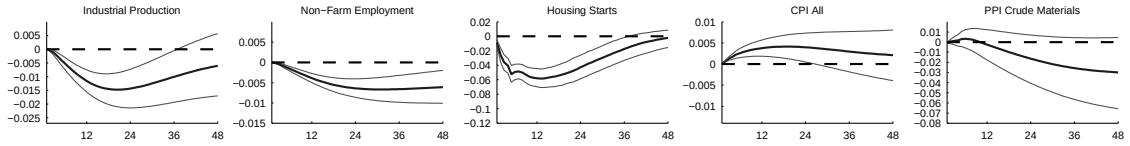
68% confidence bands based on 10 000 bootstrap draws. For schedules B and C, the bands reflect both estimation and identification uncertainty.

FAVAR Responses of Macro Variables Considering the responses of a number of macro variables, I find that central features obtained with the standard recursive Bernanke et al (2005) approach carry over to the fundamentally different financial market identification method. For intermediate time horizons, both approaches suggest that industrial production, housing starts and employment decrease. These effects come out as long-lasting and only fade away after approximately 4 years. For Consumer prices, both the recursive and financial-market FAVARs suggest a weak delayed price puzzle whereas crude material prices exhibit a slow and marginally significant decrease in both identification schemes.[90] How do these results change when we also exclude the contemporaneous price puzzle and and a positive effect on industrial production? As schedule C of Figure 28 shows, the intermediate effects on employment, housing starts and industrial production remain unchanged, but we now observe continuously decreasing consumer and crude material prices.

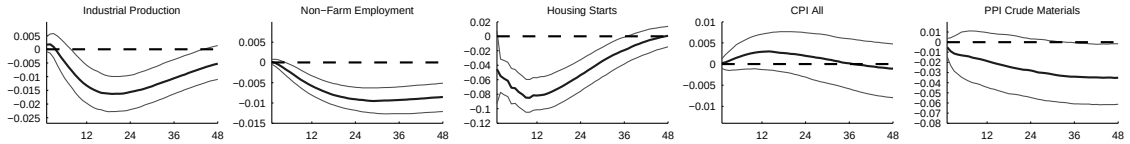
[90]Bernanke et al (2005) and Boivin et al (2009) find no price puzzles. Limiting the sample period to that of Boivin et al (2009) and increasing the number of lags to 13 as in their estimates somewhat weakens the puzzle.

Figure 28: Monthly Macro Responses to a 100 Basis Points Monetary Policy Shock

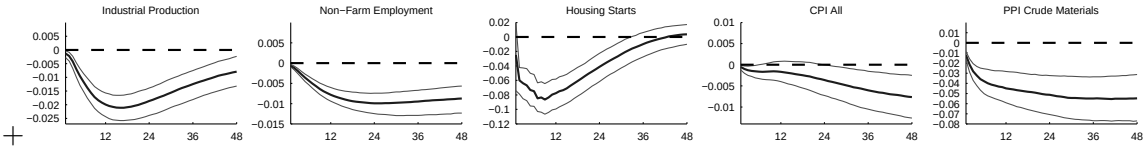
Schedule A: Standard Recursive Identification



Schedule B: Financial Market Identification



Schedule C: Financial Market and Robust Sign Restrictions



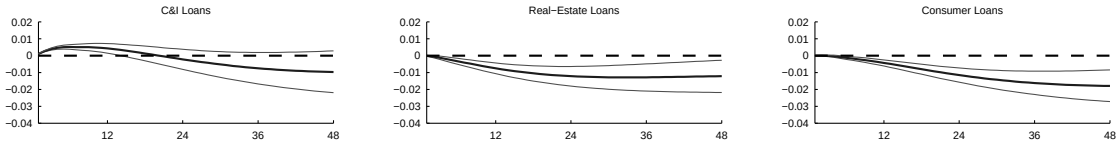
68% confidence bands based on 10 000 bootstrap draws. For schedules B and C, the bands reflect both estimation and identification uncertainty.

FAVAR Responses of FRB H8 Loan Volumes In the wake of the recent financial crisis, the effect of monetary policy on bank lending volumes has received particularly much attention. To shed light on this issue, I report the responses of the 3 main measures of the H8 dataset on bank lending. For all three identification schemes, my results suggest that commercial and industrial (C&I) loans increase following a monetary tightening whereas real estate and consumer loans do not. At longer horizons, all three loan measures show significant negative responses. Given how robust the initial increase in C&I loans appears to be, further work may have to address the question how it can be reconciled with standard models of the bank lending channel.[91]

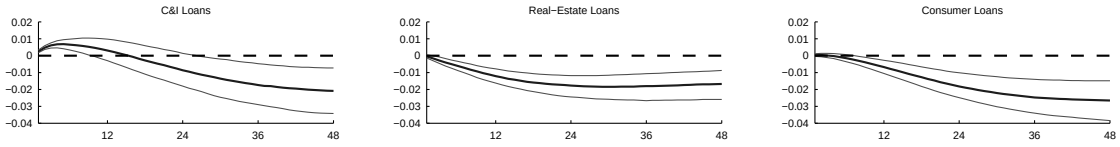
[91] Den Haan et al (2007) obtain a similar result and discuss some potential explanations.

Figure 29: FRB H8 Loan Volume Responses to a 100 Basis Points Monetary Policy Shock

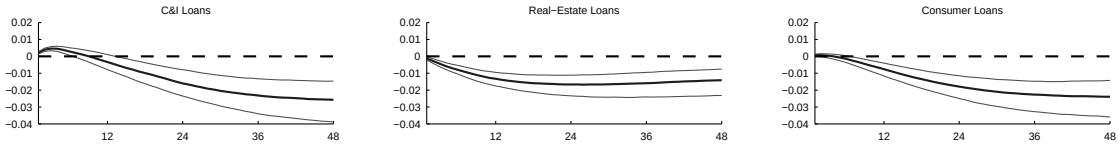
Schedule A: Standard Recursive Identification



Schedule B: Financial Market Identification



Schedule C: Financial Market and Robust Sign Restrictions



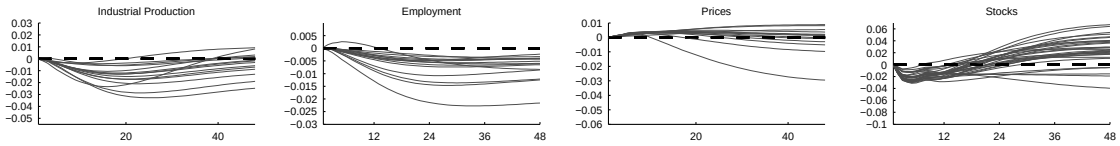
68% confidence bands based on 10 000 bootstrap draws. For schedules B and C, the bands reflect both estimation and identification uncertainty.

Response Dispersion Finally, working in the FAVAR framework, we can also look at responses of more disaggregated data series. Figure 30 shows sector responses of industrial production, employment, prices and stocks under all 3 identification schemes. The main result here is that the above findings also hold at this less aggregated level and are not just driven by single sectors. Industrial production and employment show persistent decreases in all 3 cases, and prices only exhibit very weak responses. As to the stock market, the sectoral responses show very large amounts of dispersion. This is consistent with existing sector-level high-frequency evidence.[92] However, under the recursive identification scheme every single stock sector shows a delayed response. As argued above, this contradicts both theory and high-frequency estimates.

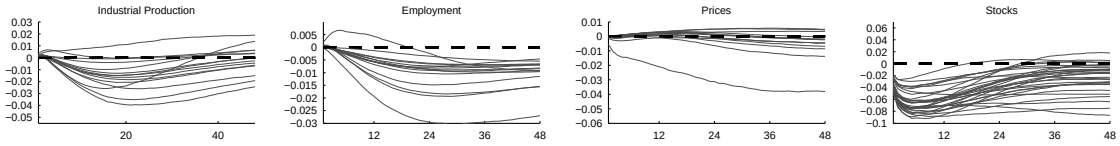
[92]For example, see Ehrmann and Fratzscher (2004).

Figure 30: Sectoral Dispersion in Monthly Responses to a 100 Basis Points Monetary Policy Shock

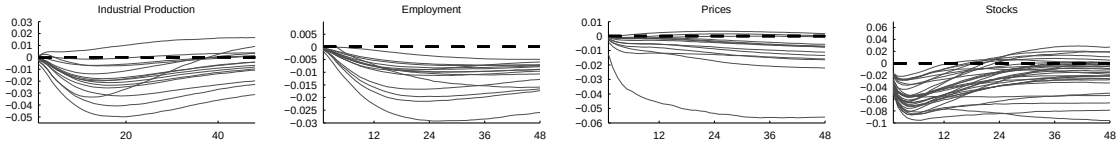
Schedule A: Standard Recursive Identification



Schedule B: Financial Market Identification



Schedule C: Financial Market and Robust Sign Restrictions



Estimates based on 10 000 bootstrap draws. Only Medians reported.

3.5 Conclusion

This paper illustrates how high-frequency financial market estimates can be used to identify structural monetary policy responses in a non-recursive FAVAR. Exploiting the economy's underlying factor structure as the link between low- and high-frequency data, the proposed method confirms key results of a benchmark recursive FAVAR. More precisely, it suggests that monetary policy has significant effects on real activity and only a very limited impact on consumer prices. These findings are obtained without falling back on any of the classical VAR and FAVAR identifying assumptions. In terms of financial market responses, the proposed method improves upon the benchmark recursive FAVAR in so far as it delivers responses that do not contradict the efficient market hypothesis. In the future, the method presented here may also be augmented with information from additional high-frequency variables. For example, as the time-series for TIPS bonds gets long enough to be included in a FAVAR, one may hope to capture inflation expectations even more precisely, and to further narrow down the identification uncertainty in low-frequency estimates.

3.6 Appendix

This appendix contains further information on the dataset used in the estimation of the FAVAR. In particular, it contains the variable descriptions as well as corresponding names

that uniquely identify the series in the underlying data sources discussed above. Moreover, for each data series it also contains information on the transformations applied before the estimation of the FAVAR, as well as an indicator reflecting whether or not the respective variable is considered to be fast moving in the Bernanke et al (2005) type factor estimation.

Description	Name	Transformation	Fast
CONSUMER CREDIT OUTSTANDING - NONREVOLVING(G19)	CCINRV	log differences	Yes
EMPLOYEES, NONFARM - TOTAL NONFARM	CES001	log differences	No
EMPLOYEES, NONFARM - TOTAL PRIVATE	CES002	log differences	No
EMPLOYEES, NONFARM - GOODS-PRODUCING	CES003	log differences	No
EMPLOYEES, NONFARM - NATURAL RESOURCES & MINING	CES004	log differences	No
EMPLOYEES, NONFARM - CONSTRUCTION	CES011	log differences	No
EMPLOYEES, NONFARM - MANUFACTURING	CES015	log differences	No
EMPLOYEES, NONFARM - DURABLE GOODS	CES017	log differences	No
EMPLOYEES, NONFARM - NONDURABLE GOODS	CES033	log differences	No
EMPLOYEES, NONFARM - SERVICE-PROVIDING	CES046	log differences	No
EMPLOYEES, NONFARM - PRIVATE SERVICE-PROVIDING	CES047	log differences	No
EMPLOYEES, NONFARM - TRADE, TRANSPORT, UTILITIES	CES048	log differences	No
EMPLOYEES, NONFARM - WHOLESALE TRADE	CES049	log differences	No
EMPLOYEES, NONFARM - RETAIL TRADE	CES053	log differences	No
EMPLOYEES, NONFARM - FINANCIAL ACTIVITIES	CES088	log differences	No
EMPLOYEES, NONFARM - GOVERNMENT	CES140	log differences	No
AVERAGE WEEKLY HOURS, PRODUCTS WRKRS, NONFARM - TOTAL PRIVATE	CES150	as is	No
AVERAGE WEEKLY OVERTIME HOURS, PRODUCTS WRKRS, NONFARM - MANUFACTURING	CES155	as is	No
AVERAGE HRLY EARNINGS, PRODUCTS WRKRS, NONFARM - CONSTRUCTION	CES277	log differences	No
AVERAGE HRLY EARNINGS, PRODUCTS WRKRS, NONFARM - MANUFACTURING	CES278	log differences	No
MOBILE HOMES: MANUFACTURERS' SHIPMENTS (THOUS.OF UNITS,SAAR)	HMOB	logs	Yes
HOUSING AUTHORIZED: TOTAL NEW PRIV HOUSING UNITS (THOUS.,SAAR)	HSBR	logs	Yes
HOUSING STARTS:NONFARM(1947-58);TOTAL FARM&NONFARM(1959-)(THOUS.,SA)	HSFR	logs	Yes
HOUSING STARTS:MIDWEST(THOUS.U.)S.A.	HSMW	logs	Yes
HOUSING STARTS:NORTHEAST (THOUS.U.)S.A.	HSNE	logs	Yes
HOUSING STARTS:SOUTH (THOUS.U.)S.A.	HSSOU	logs	Yes
HOUSING STARTS:WEST (THOUS.U.)S.A.	HSWST	logs	Yes

INDUSTRIAL PRODUCTION INDEX - TOTAL INDEX	IPS10	log differences	No
INDUSTRIAL PRODUCTION INDEX - PRODUCTS, TOTAL	IPS11	log differences	No
INDUSTRIAL PRODUCTION INDEX - CONSUMER GOODS	IPS12	log differences	No
INDUSTRIAL PRODUCTION INDEX - DURABLE CONSUMER GOODS	IPS13	log differences	No
INDUSTRIAL PRODUCTION INDEX - NONDURABLE CONSUMER GOODS	IPS18	log differences	No
INDUSTRIAL PRODUCTION INDEX - BUSINESS EQUIPMENT	IPS25	log differences	No
INDUSTRIAL PRODUCTION INDEX - FINAL PRODUCTS	IPS299	log differences	No
INDUSTRIAL PRODUCTION INDEX - RESIDENTIAL UTILITIES	IPS307	log differences	No
INDUSTRIAL PRODUCTION INDEX - BASIC METALS	IPS316	log differences	No
INDUSTRIAL PRODUCTION INDEX - MATERIALS	IPS32	log differences	No
INDUSTRIAL PRODUCTION INDEX - DURABLE GOODS MATERIALS	IPS34	log differences	No
INDUSTRIAL PRODUCTION INDEX - NONDURABLE GOODS MATERIALS	IPS38	log differences	No
INDUSTRIAL PRODUCTION INDEX - MANUFACTURING (SIC)	IPS43	log differences	No
INDUSTRIAL PRODUCTION INDEX - MINING NAICS=21	IPS67	log differences	No
INDUSTRIAL PRODUCTION INDEX - ELECTRIC AND GAS UTILITIES	IPS68	log differences	No
CIVILIAN LABOR FORCE: EMPLOYED, TOTAL (THOUS.,SA)	LHEM	log differences	No
CIVILIAN LABOR FORCE: EMPLOYED, NONAGRIC.INDUSTRIES (THOUS.,SA)	LHNAG	log differences	No
UNEMPLOY.BY DURATION: PERSONS UNEMPL.5 TO 14 WKS (THOUS.,SA)	LHU14	as is	No
UNEMPLOY.BY DURATION: PERSONS UNEMPL.15 WKS + (THOUS.,SA)	LHU15	as is	No
UNEMPLOY.BY DURATION: PERSONS UNEMPL.15 TO 26 WKS (THOUS.,SA)	LHU26	as is	No
UNEMPLOY.BY DURATION: PERSONS UNEMPL.LESS THAN 5 WKS (THOUS.,SA)	LHU5	as is	No
UNEMPLOY.BY DURATION: AVERAGE(MEAN)DURATION IN WEEKS (SA)	LHU680	as is	No
UNEMPLOYMENT RATE: ALL WORKERS, 16 YEARS & OVER (%,SA)	LHUR	as is	No
NEW ORDERS (NET) - CONSUMER GOODS & MATERIALS, 1996 DOLLARS (BCI)	MOCMQ	log differences	Yes
NEW ORDERS, NONDEFENSE CAPITAL GOODS, IN 1996 DOLLARS (BCI)	MSONDQ	log differences	Yes
NAPM COMMODITY PRICES INDEX (PERCENT)	PMCP	as is	Yes
NAPM VENDOR DELIVERIES INDEX (PERCENT)	PMDEL	as is	Yes

NAPM EMPLOYMENT INDEX (PERCENT)	PMEMP	as is	No
PURCHASING MANAGERS' INDEX (SA)	PMI	log differences	No
NAPM NEW ORDERS INDEX (PERCENT)	PMNO	as is	Yes
NAPM INVENTORIES INDEX (PERCENT)	PMNV	as is	Yes
NAPM PRODUCTION INDEX (PERCENT)	PMP	log differences	No
CPI-U: APPAREL & UPKEEP (82-84=100,SA)	PU83	log differences	No
CPI-U: TRANSPORTATION (82-84=100,SA)	PU84	log differences	No
CPI-U: MEDICAL CARE (82-84=100,SA)	PU85	log differences	No
CPI-U: COMMODITIES (82-84=100,SA)	PUC	log differences	No
CPI-U: DURABLES (82-84=100,SA)	PUCD	log differences	No
CPI-U: ALL ITEMS (82-84=100,SA)	PUNEW	log differences	No
CPI-U: ALL ITEMS LESS FOOD (82-84=100,SA)	PUXF	log differences	No
CPI-U: ALL ITEMS LESS SHELTER (82-84=100,SA)	PUXHS	log differences	No
CPI-U: ALL ITEMS LESS MIDICAL CARE (82-84=100,SA)	PUXM	log differences	No
PRODUCER PRICE INDEX:CRUDE MATERIALS (82=100,SA)	PWCMSA	log differences	No
PRODUCER PRICE INDEX:FINISHED CONSUMER GOODS (82=100,SA)	PWFCSA	log differences	No
PRODUCER PRICE INDEX: FINISHED GOODS (82=100,SA)	PWFSA	log differences	No
PRODUCER PRICE INDEX:INTERMED MAT.SUPPLIES & COMPONENTS(82=100,SA)	PWIMSA	log differences	No
PERSONAL INCOME (CHAINED) (BIL2000$,SAAR)	YPR	log differences	No
MONEY STOCK: M1(CURR,TRAV.CKS,DEM DEP,OTHER CK'ABLE DEP)(BIL$,SA)	FM1	log differences	Yes
MONEY STOCK:M2(M1+O'NITE RPS,EURO$,G/P&B/D MMMFS&SAV&SM TIME DEP(BIL$,	FM2	log differences	Yes
MONETARY BASE, ADJ FOR RESERVE REQUIREMENT CHANGES(MIL$,SA)	FMFBA	log differences	Yes
DEPOSITORY INST RESERVES:TOTAL,ADJ FOR RESERVE REQ CHGS(MIL$,SA)	FMRRA	log differences	Yes
FOREIGN EXCHANGE RATE: CANADA (CANADIAN $ PER U.S.$)	DEXCAUS	log differences	Yes
FOREIGN EXCHANGE RATE: JAPAN (YEN PER U.S.$)	DEXJPUS	log differences	Yes
FOREIGN EXCHANGE RATE: SWITZERLAND (SWISS FRANC PER U.S.$)	DEXSZUS	log differences	Yes
FOREIGN EXCHANGE RATE: UNITED KINGDOM (CENTS PER POUND)	DEXUSUK	log differences	Yes
INTEREST RATE: FEDERAL FUNDS (EFFECTIVE) (% PER ANNUM,NSA)	DFF	as is	Yes
INTEREST RATE: U.S.TREASURY CONST MATURITIES,1-YR.(% PER ANN,NSA)	DGS1	as is	Yes
INTEREST RATE: U.S.TREASURY CONST MATURITIES,10-YR.(% PER ANN,NSA)	DGS10	as is	Yes

INTEREST RATE: U.S.TREASURY CONST MATURITIES,5-YR.(% PER ANN,NSA)	DGS5	as is	Yes
INTEREST RATE: U.S.TREASURY BILLS,SEC MKT,3-MO.(% PER ANN,NSA)	DTB3	as is	Yes
INTEREST RATE: U.S.TREASURY BILLS,SEC MKT,6-MO.(% PER ANN,NSA)	DTB6	as is	Yes
S&P'S COMMON STOCK PRICE INDEX: COMPOSITE (1941-43=10)	SP500	log differences	Yes
Spread DGS1 - DFF	SPRDGS1	as is	Yes
Spread DGS10 - DFF	SPRDGS10	as is	Yes
Spread DGS5 - DFF	SPRDGS5	as is	Yes
Spread TB3M - DFF	SPRTB3	as is	Yes
Spread TB6M - DFF	SPRTB6	as is	Yes

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