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FLEXIBLE PRICES AND LEVERAGE

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ABSTRACT

The frequency with which firms adjust output prices helps explain persistent differences in capital structure across firms. Unconditionally, the most flexible-price firms have a 19% higher long-term leverage ratio than the most sticky-price firms, controlling for known determinants of capital structure. Sticky-price firms increased leverage more than flexible-price firms following the staggered implementation of the Interstate Banking and Branching Efficiency Act across states and over time, which we use in a difference-in-differences strategy. Firms' frequency of price adjustment did not change around the deregulation.

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A online appendix is available at http://www.nber.org/data-appendix/w23066

I Introduction

Firms differ in the frequency with which they adjust output prices to aggregate and idiosyncratic shocks, and these differences are persistent across firms and over time.[1] Firms with rigid output prices are more exposed to macroeconomic shocks, making price flexibility a viable candidate to explain persistent differences in financial leverage across firms (Gorodnichenko and Weber (2016) and Weber (2015)). Moreover, managerial efficiency, customer antagonization, or slowly moving firm characteristics could also be reasons why firms adjust their output prices less frequently, which in turn might affect the leverage choices of firms (Blinder et al. (1997) and Anderson and Simester (2010)).[2]

Firms' frequency of output-price adjustment has long been a focus in Macroeconomics and Industrial Organization. In New Keynesian models, monetary policy has real effects because firms adjust product prices infrequently (Woodford (2003)). Research in Macroeconomics has studied credit constraints and price rigidity to understand aggregate fluctuations and the effectiveness of monetary policy (Bernanke, Gertler, and Gilchrist (1999)). In this paper, we provide an empirical link between these two drivers of aggregate fluctuations, and we study their effect on firms' leverage choices.

We study the differences in financial leverage across sticky- and flexible-price firms, both unconditionally and conditional on a shock to credit supply, the Interstate Banking and Branching Efficiency Act (IBBEA). The banking deregulation might result in banks with better monitoring technologies and increased geographic diversification, which would allow those banks to lend more to previously financially constrained and underleveraged firms.

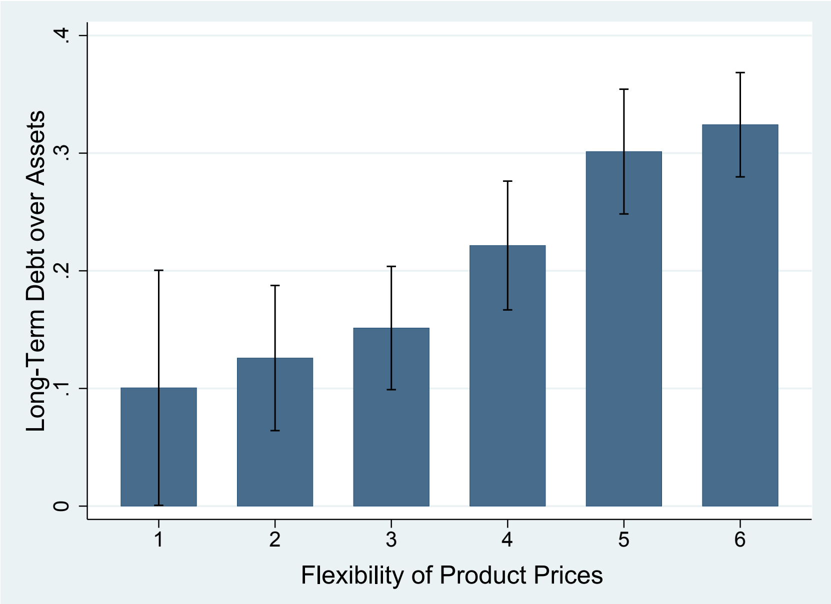
Figure 1 documents the novel stylized fact, which is the main result of the paper. We sort firms into six equally sized groups with increasing output-price flexibility. Moving from firms with the most rigid output prices to firms with the most flexible output prices increases firms' long-term leverage ratio from around 10% to over 30%.[3] We use the

[1] Alvarez, Gonzales-Rozada, Neumeyer, and Beraja (2011) show that firms' frequency of price adjustment changes little over time, even with inflation rates ranging from 0% to 16%.

[2] We discuss micro foundations of price stickiness, how they might affect leverage, and their relation to volatility and operating leverage in Section II.

[3] Heider and Ljungqvist (2015) argue firms use short-term leverage to finance working capital, and are therefore unlikely to change short-term leverage in response to changing credit supply. We therefore choose long-term leverage as the main outcome variable. Results continue to hold if we look at total leverage or net debt to assets (see Online Appendix).

Figure 1: **Flexible Prices and Financial Leverage**



This figure reports the average long-term-debt-to assets ratio (y-axis) for groups of firms with increasing output-price flexibility. We measure the flexibility of product prices at the firm level, using confidential micro data from the Bureau of Labor Statistics (see Section III.A of the paper for a detailed description). For each bin, the graph reports 95% confidence intervals around the mean leverage ratio.

confidential micro data underlying the official Producer Price Index (PPI) of the Bureau of Labor Statistics (BLS) to document this fact. We observe monthly good–level pricing data for a subsample of S&P500 firms from January 1982 to December 2014.

In the baseline empirical analysis, we find a one-standard-deviation increase in our continuous measure of price flexibility is associated with a 2.4-percentage-point-higher long-term debt-to-assets ratio, which is 11% of the average ratio in the sample (see column (1) of Table 2). We estimate these magnitudes after controlling for size, tangibility, profitability, stock-return volatility, and the book-to-market ratio. We also control for industry concentration and for firm-level measures of market power and concentration, which might be correlated with firms' price flexibility because of product-market dynamics.[4] Results are similar if we only exploit the variation in price flexibility within industries and within years. This result is important, because

[4]Ali, Klasa, and Yeung (2009) show that measures of industry concentration using only publicly listed firms are weakly correlated with concentration measures using both public and private firms. They find a strong correlation of their Census-based measure with price-to-cost margins. We add both a Compustat-based measure of industry concentration and firm-specific measures of price-to-cost margins.

product-market considerations at the industry level affect firms' demand for debt (e.g., see Maksimovic (1988) and Maksimovic (1990)). Results are also similar if we use alternative industry definitions, such as the Fama-French 48 industries, or the Hoberg-Phillips 50 industries (Hoberg and Phillips (2010), Hoberg and Phillips (2016)), which are constructed based on the distance across individual firms in the product space. The size and significance of results are unchanged when we account for measurement error using the errors-in-variables estimator based on linear cumulant equations of Erickson, Jiang, and Whited (2014).

A growing consensus in the macroeconomics literature suggests prices at the micro level are sticky (see Kehoe and Midrigan (2015)), but no consensus exists on what causes firms to have sticky prices. Potential explanations include physical costs of price adjustment, customer antagonization, pricing points, market power, and managerial inefficiencies. Blinder et al. (1997) summarize different theories and run an interview study to disentangle 12 different explanations. They find support for eight theories, and conjecture micro foundations for price stickiness might differ across industries. We do not aim to pin down the specific channels through which price stickiness affects leverage in the current paper, because the literature has not yet settled on the micro foundations of these channels. Instead, we study in detail potential determinants of price stickiness and alternative explanations for our findings, and we find none of these alternative channels explains the relationship between the frequency of price adjustment and firms' leverage choices.

An important concern is that price flexibility is a mere proxy for the volatility of cash flows. To disentangle the relationship between price flexibility and volatility, we note the association between return volatility and leverage varies widely in terms of sign and statistical significance in our baseline specifications (see Table 2), in line with the findings of Frank and Goyal (2009) and Lemmon et al. (2008). Time-varying risk aversion, fades, noise trader risk, or components potentially endogenous to leverage itself could be key drivers of total volatility and affect leverage with different signs. Once we decompose volatility into a component predicted by the frequency of price adjustment and a residual component (see Table 8), we find the predicted component of volatility is robustly negatively associated with leverage, whereas no systematic association exists between the residual component and leverage. Product price flexibility is, hence, not a simple proxy for firm-level volatility.

Price flexibility is a highly persistent characteristic of the firms in our sample, consistent with previous findings. A firm-level regression of post-1996 price flexibility onto pre-1996 price flexibility yields a slope coefficient of 93%, and we fail to reject the null that the coefficient equals 1 at any plausible level of significance. This persistence suggests we can hardly consider a shock to firm-level price flexibility for identification purposes in our sample.

The paper does not aim to test for the causal effect of price flexibility on financial leverage, which would require us to identify the persistent determinants of the price-setting strategy of firms. At the same time, sticky-price firms have lower financial leverage unconditionally and conditional on observables (Figure 1), which might indicate they are financially constrained. We therefore test whether an exogenous shock to the supply of credit affects the financial leverage of sticky-price firms more than the financial leverage of flexible-price firms. We propose a strategy inspired by the financial constraints literature. We (i) identify a positive shock to the supply of bank credit that firms can access, (ii) show sticky-price firms increase leverage more than flexible-price firms after the shock, and (iii) show the effect does not revert in the short run.

We exploit the staggered state-level implementation of the Interstate Banking and Branching Efficiency Act between 1994 and 2005 (Rice and Strahan (2010) and Favara and Imbs (2015)) as a shock to the availability of bank credit. Restrictions on U.S. banks' geographic expansion date back at least to the 1927 McFadden Act. The IBBEA of 1994 allowed bank holding companies to enter other states and operate branches across state lines, dramatically reshaping the banking landscape in affected states. The step-wise repeal of interstate bank branching restrictions increased the supply of credit. Banking deregulation resulted in lower interest rates charged (Jayaratne and Strahan (1996)), more efficient screening of borrowers (Dick and Lehnert (2010)), increased spatial diversification of borrowers (Goetz, Laeven, and Levine (2013)), higher loan volume (Amore, Schneider, and Žaldokas (2013)), more credit cards (Kozak and Sosyura (2015)), more credit lines and subsequent trade credit (Shenoy and Williams (2015)), and increased lending to riskier firms (Neuhann and Saidi (2015)).

We interpret the staggered state-level implementation of the IBBEA as a shock to financial constraints exogenous to individual firms' financial decisions. This shock allows us to test whether sticky-price firms increase their financial leverage more than flexible-price firms after the shock. One way the IBBEA may relax financial constraints is by giving firms access to banks with a better monitoring technology. These banks might be willing to lend more, consistent with the empirical evidence of Jayaratne and Strahan (1996) and Stiroh and Strahan (2003). Dick (2006) and Bushman et al. (2016) propose a slightly different view of banking deregulation. They argue the IBBEA allowed banks to lend to underleveraged borrowers, possibly due to better geographic diversification. We do not take a stance on how banking deregulation relaxes financial constraints, and focus instead on how financial constraints interact with product-price flexibility.

Our empirical design compares outcomes within firms before and after the implementation of the IBBEA in the state where the firms are headquartered, across firms in states that deregulated or not, and across flexible- and sticky-price firms. Firms in states that had not yet deregulated act as counterfactuals for the evolution of the long-term debt of treated firms absent the shock. To assess the plausibility of the required identifying assumptions, we show that before the shock, the trends of long-term debt of flexible- and sticky-price firms are parallel, and the price flexibility of firms does not change around the shock.

We find sticky-price firms increased leverage more than flexible-price firms after the deregulation. Crucially, sticky-price firms with a lower cash-to-assets ratio and a larger external finance gap, which were more likely to need external financing to fund their operations, drive the effect. The most flexible-price firms kept their leverage virtually unchanged after the deregulation. The results remain unchanged when we add interaction terms of the deregulation dummies with the Kaplan-Zingales index or stock-return volatility. In untabulated results, we find similar effects across firms with and without investment-grade bond ratings, alleviating concerns that access to the public bond market drives differences in leverage (see Faulkender and Petersen (2006)).

The availability of product-price micro data requires that we focus on large firms, but to what extent do large firms use bank credit? We use data from Sufi (2009) on credit lines, and find 94.6% of the firms in our sample have credit lines with at least one bank. The average utilization rate is above 20%, which suggests bank relationships are relevant in our sample. Moreover, both the likelihood of having credit lines and their sizes increase after the implementation of the IBBEA. After the implementation, 94.9% of the firms in our sample have a credit line, whereas the share is 93.3% before the implementation of the IBBEA. Moreover, the average credit line is $934K after the implementation of the IBBEA, compared to $543K before the implementation. Consistent with our results on leverage, sticky-price firms drive the increase in the size of credit lines. These facts are consistent with Beck, Demirgüç-Kunt, and Maksimovic (2008), who find large firms are more likely than small firms to rely on bank finance.

We assess the validity of our results with two falsification tests. We split states into early deregulators (between 1996 and 1998) and late deregulators (after 2000). In the first falsification test, we use only observations prior to 1996, when *no state* had yet deregulated. The placebo implementation date for early deregulators is 1992. We choose 1992 to have a placebo treatment of four years, the same time period between the IBBEA implementation of early and late adopters. We do not find any differences in the capital structure of sticky-price firms in early states compared to sticky-price firms in late states before and after 1992.

In the second falsification test, we use only observations prior to 1996 *and* after 2000, and exclude all observations in the period 1996-2000. Before 1996, no states had yet deregulated, and after 2000, all states had deregulated. Consistent with our interpretation of the shock, sticky-price firms in both early states and late states have higher long-term debt after 2000 compared to before 1996, whereas flexible-price firms in both sets of states do not change their capital structure after 2000.

A. Related Literature

Our paper adds to a recent literature studying the macroeconomic determinants of financial leverage, default risk, and bond yields. Bhamra, Kuehn, and Strebulaev (2010) study the effect of time-varying macroeconomic conditions on firms' optimal capital structure choice. Kang and Pflueger (2015) show fear of debt deflation is an important driver of corporate bond yields. Favilukis, Lin, and Zhao (2015) document that firms in industries with higher wage rigidities have higher credit risk. Serfling (2016) finds more stringent state-level firing laws lower financial leverage of firms headquartered in the state, whereas Simintzi, Vig, and Volpin (2015) show that firms lower their financial leverage in countries passing labor-friendly law changes. Determinants of labor market frictions in this literature vary at the industry, state, or country level, and hence are unlikely to account for our findings, because we exploit variation at the firm level even within industries. In the causal test that exploits the banking deregulation shock, we can also absorb firm-level time-invariant characteristics, such as whether the firms' workforces are unionized or not, and our results do not change.

The paper also speaks to the theoretical and empirical literatures studying the effect of volume flexibility on firms' capital structure. The sign of the effect of volume flexibility on financial leverage is inconclusive. On the empirical side, MacKay (2003) finds that volume flexibility reduces financial leverage, whereas Reinartz and Schmid (2015) find the opposite using direct measures of volume flexibility for firms in the utilities sector. On the theoretical side, volume flexibility can decrease default risk (e.g., see Mauer and Triantis (1994)) and promote risk shifting and asset substitution (e.g., see Mello and Parsons (1992)), which have opposite effects on financial leverage in equilibrium. In our empirical analysis, we control for firms' price-to-cost margin, which we define as a linear transformation of operating leverage, to average out the effects of time-varying operating leverage on financial leverage.

II Hypothesis Development

In this section, we discuss the channels through which sticky-price firms might have lower financial leverage compared to firms with flexible output prices.

First, Anderson and Simester (2010) use a field experiment to document that customers dislike both positive and negative price changes, an effect they label the *customer-antagonization channel* of price stickiness. Blinder et al. (1997) find more than 50% of managers answer customer antagonization is an important reason for rigid output prices.[5] According to this channel, managers want to avoid adjusting output prices in fear of customer antagonization. They would therefore choose ex-ante lower leverage for precautionary reasons to avoid default following large cost shocks. Under this interpretation, price rigidity changes firms' demand for leverage, and lower leverage is not due to banks' decisions to restrict lending to sticky-price firms because of volatile cash flows.

Second, less efficient managers, or managers with higher attention costs, might adjust

[5]See Table 5.2 in Blinder et al. (1997).

output prices less frequently, while at the same time not equalizing the costs and benefits of financial leverage (Ellison, Snyder, and Zhang (2015)). Because firms that do not optimize their leverage choices are on average underleveraged (Graham (2000)), we would observe sticky-price firms having unconditionally lower leverage.

Third, costs of price adjustment, including menu costs, information gathering, and negotiation costs, could lead to sticky-output prices and volatile cash flows (see Gorodnichenko and Weber (2016) and Weber (2015)). Sticky-price firms might obtain less leverage due to their higher riskiness compared to flexible-price firms.

All three channels imply sticky-price firms have unconditionally lower leverage than firms with flexible prices. We therefore aim to test the following hypothesis in the data.

Hypothesis 1 *Inflexible-price firms have lower leverage than flexible-price firms.*

One might be concerned that price stickiness merely proxies for firms' cash-flow volatility or for operating leverage.

Note only the third channel we describe above operates via the riskiness of cash flows, whereas the first two channels do not necessarily imply sticky-price firms have lower leverage because of their riskier cash flows. Therefore, we do expect price stickiness helps explain financial leverage on top of measures of firm-level risk.

Moreover, output-price stickiness differs from operating leverage in several ways. First, price stickiness is the key mechanisms in New Keynesian models for the real effects of monetary policy (Woodford (2003)). If price stickiness were a mere proxy for operating leverage, monetary policy would be neutral. Second, inflexible-price firms' profits may decline both if demand turns out lower or higher than expected. This behavior differentiates price stickiness from operating leverage, which increases a firm's exposure to shocks but preserves the sign of the original exposure. Therefore, we expect that price stickiness helps explain financial leverage on top of measures of operating leverage.

Based on the first hypothesis, sticky-price firms have lower financial leverage conditional on observables, which might indicate they are financially constrained. We therefore consider the differential effect of a shock to the supply of credit for sticky-price firms and flexible-price firms. An exogenous increase in the supply of credit might change the leverage of firms through three channels.

First, banking deregulation increases competition across banks and hence the value of banking relationships. Banks might actively reach out to previously underleveraged firms in order to cater a higher supply of credit to them.

Second, banking deregulation might result in lower precautionary savings of firms, because after the deregulation, firms can access additional sources of financing more easily and faster when close to default.

Third, banking deregulation leads to banks with better monitoring technologies and better geographically diversified loan portfolios. These banks might increase lending to riskier firms after the deregulation.

Conditional on a positive shock to credit supply, we therefore expect a larger increase in financial leverage for sticky-price firms relative to firms with flexible prices. We therefore aim to test the following hypothesis in the data.

Hypothesis 2 *Following a positive shock to loan supply, inflexible-price firms increase leverage more than flexible-price firms.*

The three channels through which price stickiness might affect financial leverage have the same unconditional and conditional implications, and we do not aim to disentangle their contribution. In fact, the micro foundations of the observed degree of price stickiness are still an open question in macroeconomics.

III Data

A. Micro Pricing Data

We use the confidential micro pricing data underlying the PPI from the BLS to construct a measure of price stickiness at the firm level. We have monthly output price information for individual goods at the establishment level from 1982 to 2014. The BLS defines prices as "net revenue accruing to a specified producing establishment from a specified kind of buyer for a specified product shipped under specified transaction terms on a specified day of the month." Unlike the Consumer Price Index (CPI), the PPI measures the prices from the perspectives of producers. The PPI tracks the prices of all goods-producing industries such as mining, manufacturing, and gas and electricity, as well as the service sector.[6]

[6]The BLS started sampling prices for the service sector in 2005. The PPI covers about 75% of the service-sector output.

We focus on firms that have been part of the S&P500 during our sample period from January 1982 to December 2014 due to the availability of the PPI micro data. The S&P500 contains large U.S. firms and captures approximately 80% of the available stock market capitalization in the United States, therefore maintaining the representativeness for the whole economy in economic terms. The BLS samples establishments based on the value of shipments, and we have a larger probability of finding a link between BLS pricing data and financial data when we focus on large firms. We have 1,195 unique firms in our sample due to changes in the index composition during the sample period, out of which we were able to merge 469 with the BLS pricing data.

The BLS follows a three-stage procedure to select its sample of goods. First, it compiles a list of all firms filing with the Unemployment Insurance system to construct the universe of all establishments in the United States. Second, it probabilistically selects sample establishments based on the total value of shipments, or on the number of employees, and finally it selects goods within establishments. The final data set covers 25,000 establishments and 100,000 individual items each month. Prices are collected through a survey, which participating establishments receive via email or fax.

We first calculate the frequency of price adjustment (FPA) at the good level as the ratio of price changes to the number of sample months. For example, if an observed price path is \$4 for two months and then changes to \$5 for another three months, one price change occurs during five months, and the frequency of price adjustment is 1/5. We exclude price changes due to sales. This assumption is standard in the literature and does not affect the measure, because sales are rare in the PPI micro data (see Gorodnichenko and Weber (2016)). We then perform two layers of aggregation to create a measure of the frequency of price adjustment at the firm level. We first equally weight frequencies for all goods of a given establishment using internal identifiers from the BLS.[7] To perform the firm-level aggregation, we manually check whether establishments with the same or similar names are part of the same company. In addition, we use publicly available data to search for names of subsidiaries and name changes due to, for example, mergers, acquisitions, or restructuring occurring during the sample period for all firms in the data set.[8]

[7] Weighing good-based frequencies by the associated value of shipments does not alter our results.

[8] See Weber (2015) for a more detailed description of the data and the construction of variables. Gorodnichenko and Weber (2016) discuss in detail the number of goods and price spells used to calculate the frequencies at the firm level. The average number of products is 111 and the average number of price spells is 203. See their Table 1.

The granularity of the data at the firm level allows us to differentiate the effect of price flexibility from the effect of other industry- and firm-level characteristics.

The price flexibility of similar firms operating in the same industry can differ substantially. This difference can arise from different costs of negotiating with customers and suppliers, physical costs of changing prices, or managerial costs such as information gathering, decision making, and communication (see Zbaracki et al. (2004)). Because our results do not change when we control for firm-level market power and product-market dynamics across industries, firm-level persistent characteristics are likely to determine the within-industry variation in price flexibility across firms we exploit in the empirical analysis.

B. Financial Data

Stock returns and shares outstanding come from the monthly stock return file from the Center for Research in Security Prices (CRSP). Financial and balance-sheet variables come from Compustat.

B.1 Determinants of Financial Leverage

We define our preferred measure of leverage, *Lt2A*, as long-term debt over total assets. In the Online Appendix, we show our results are similar if we consider alternative measures of leverage, such as total debt over total assets and net debt over total assets.

We define all covariates we use in the analysis at the end the previous fiscal year. To reduce the effects of outliers, we winsorize all variables at the 1^{st} and 99^{th} percentiles. We follow Rajan and Zingales (1995), Lemmon, Roberts, and Zender (2008), and Graham, Leary, and Roberts (2015) in the choice and definition of capital-structure determinants. We define the common determinants of financial leverage as follows: *Profitability* is operating income over total assets, *Size* is the log of sales, *B-M ratio* is the book-to-market ratio, *Intangibility* is intangible assets defined as total assets minus the sum of net property, plant, and equipment; cash and short-term investments; total receivables; and total inventories to total assets. We also add stock return volatility as an additional covariate. We calculate $Total\ vol$ as annualized return volatility in the previous calendar year using daily data and idiosyncratic volatility relative to the CAPM and Fama and French three-factor model ($Idio\ vol_{CAPM}$ and $Idio\ vol_{FF3}$) following Campbell et al.

(2001). We set the volatility to missing if we have less than 60 daily return observations.

B.2 Market Power and Operating Leverage

In the analysis, we also use additional covariates that proxy for market power and operating leverage at the firm level. These controls are important, because the industrial organization literature suggests product-market considerations might affect the price-setting strategies of firms. Our preferred measure of market power at the firm level is *Price-Cost margin*, which we define as the ratio of net sales minus the cost of goods sold to net sales. This measure is equivalent to 1 minus operating leverage, and hence it also controls for time-varying changes in operating leverage at the firm level. Our results are unchanged if we control for alternative measures of operating leverage, the ratio of fixed costs over total sales, or follow Novy-Marx (2011) and define operating leverage as the ratio of cost of goods sold and selling, general, and administrative expenses to total assets.

To control for industry-level concentration, we use the Herfindahl-Hirschman index (HHI) of annual sales at the Fama-French 48-industry level. Moreover, we use the firm-level definition of concentration within the Hoberg-Phillips industries (*HP Firm-level HHI*), which are constructed based on the distance between firms in the product space, using textual analysis to assess the similarity of firms' product descriptions from the annual 10-K filings (see Hoberg and Phillips (2010), Hoberg and Phillips (2016)). These data are available from 1996 onward, which reduces the time span of our analysis. We therefore report the results for the full sample of firm-year observations, and for the restricted sample after 1996 throughout the paper.

Ali, Klasa, and Yeung (2009) show measures of industry concentration using only publicly-listed firms are weakly correlated with concentration measures using both public and private firms. They find a strong correlation of their Census-based measure with price-to-cost margins. We add both a Compustat-based measure of industry concentration and firm-specific measures of price-to-cost margins. In a robustness analysis, we also use the four-firm concentration ratio from the Bureau of Economic Analysis. This measure reports the share of sales for the four largest firms in an industry, and uses all firms, both private and public.

B.3 Alternative Definitions of Industries

Product-market considerations are likely to be most relevant across industries, as opposed to within industries. In our analysis, we focus on within-industry variation, which can hardly be driven by product-market considerations.

A growing literature in finance shows traditional definitions of industries might not capture the variety of product market spaces in which a firm operates (e.g., see Hoberg and Phillips (2010), Hoberg and Phillips (2016), and Lewellen (2012)). For these reasons, we consider two alternative industry definitions. The first definition is the Fama-French 48-industry taxonomy. The second definition is the Hoberg-Phillips set of 50 industries, based on the distance between firms in their product space (see Hoberg and Phillips (2010), Hoberg and Phillips (2016)).

C. Descriptive Statistics

Panel A of Table 1 reports descriptive statistics for our running sample. Firms in our sample do not adjust their output prices for roughly seven months ($-1/(log(1-FPA))$), with substantial variation across firms as indicated by the large standard deviation. $FPADummy$ is a dummy variable that equals 1 for the firms in the top 25% of the distribution based on price flexibility, and 0 for the firms in the bottom 25% of the distribution. The average total and idiosyncratic volatilities are 33% and 28% per year (*Total vol* and *Idio vol*). The average long-term-leverage ratio *Lt2A* is around 21%. Firms have an operative income margin (*Profitability*) of 15%. The average book-to-market ratio is 60% (*B-M ratio*), and the average firm size is USD 3.8 bn. (*Size*). Twenty-one percent of assets are intangible (*Intangibility*). The average price-to-cost margin (*Price-cost margin*) is 37%, and the average industry concentration (*HHI*) is 0.11. Panel B of Table 1 reports the pairwise unconditional correlations among the variables.

Flexible-price firms have unconditionally higher long-term leverage, and the frequency of price adjustment is unconditionally correlated with standard determinants of capital structure. The frequency of price adjustment is lower in more concentrated industries and for firms with high markups, and might, therefore, reflect more market power on the side of firms. For this reason, in our multivariate analysis, we will control for firm- and industry-level measures of market power.

IV Baseline Analysis

A. Price Flexibility and Leverage

We move on to investigate the empirical relationship between leverage and price stickiness. Heider and Ljungqvist (2015) argue firms use short-term leverage to finance working capital, and are therefore unlikely to change short-term leverage in response to changing tax benefits or credit supply. In addition, inflation is highly persistent (Atkeson and Ohanian (2001), Stock and Watson (2007)), and uncertainty about the aggregate price level increases with the forecast horizon. Price-setting frictions should therefore be most relevant for long-term leverage. For these reasons, we focus on long-term debt, as opposed to short-term debt, as the main dependent variable in our empirical analysis. In Table A.1 in the Online Appendix, we replicate all the results using total debt and net debt as our measures of leverage.[9]

We first look at the raw data, and plot the long-term-debt-to assets ratio separately for sticky- and flexible-price firms over time. In both panels of Figure 2, the blue solid lines refer to the ratio of long-term debt to assets of firms in the bottom quartile by price flexibility. The red dashed lines refer to the ratio of long-term debt over assets of firms in the top quartile by price flexibility, and the black dashed-dotted lines are the differences between the two ratios. In both panels, flexible-price firms have on average higher long-term leverage than inflexible-price firms throughout the sample period.

In the top panel of Figure 2, the red vertical line indicates 1996, which is the year the first set of U.S. states started to implement the IBBEA, an event we describe and exploit for our identification strategy below. In the bottom panel of Figure 2, the red vertical line indicates 2000, which is the year a second group of U.S. states started to implement the IBBEA. In both panels, the difference in the ratio of long-term debt to assets is stable before the deregulation, that is, to the left of the vertical lines, and it declines after the deregulation. We will exploit these events and the convergence of the ratios for the two groups of firms below to test Hypothesis 2 in Section II.

[9]Using net debt might be important because Dou and Ji (2015) argue theoretically sticky-price firms have higher precautionary cash holdings.

B. Ordinary Least-Squares Analysis

To assess the magnitude of the correlation between price flexibility and long-term debt to assets, our most general specification is the following OLS equation:

$$Lt2A_{i,t} = \alpha + \beta \times FPA_i + X'_{i,t-1} \times \gamma + \eta_t + \eta_k + \epsilon_{i,t}. \tag{1}$$

$Lt2A_{i,t}$ is long-term debt to assets of firm i in year t; FPA is the frequency of price adjustment, which is higher for firms with more flexible prices; X is a set of standard determinants of capital structure, which include size, the book-to-market ratio, profitability, intangibility, and total volatility; η_t is a set of year fixed effects, which absorbs time-varying shocks all firms face, such as changes in economy-wide interest rates; and η_k is a set of industry fixed effects, which absorbs time-invariant unobservable characteristics that differ across industries.[10]

The time period varies across specifications because of the availability of the Hoberg-Phillips data. In columns (1) and (5) of Table 2, we consider the full time span of our data from January 1982 until December 2014. In all other columns, the time period is limited from January 1996 to December 2014. This restriction reduces our sample size by about 50%.[11]

We use two definitions of industry fixed effects. The first definition allows for variation within the 48 Fama-French industries. The second definition follows the 50-industry classification of Hoberg and Phillips (2010) and Hoberg and Phillips (2016). Across all specifications, we cluster the standard errors at the firm level to allow for correlation of unknown form across the residuals of each firm over time.

In columns (1)-(4) of Table 2, FPA is the continuous measure of price flexibility. In columns (5)-(8), it is a dummy variable that equals 1 for the firms in the top 25% of the distribution based on price flexibility, and 0 for the firms in the bottom 25% of the distribution to ensure certain parts of the distribution of the frequency of price adjustment do not drive our results.

[10] Untabulated results are similar if we limit the variation within industry-years, and hence allow for different trends across industries.

[11] Note we cannot restrict the variation within firms, because the measure of frequency of price adjustment is time invariant. As we show below, even when we measure the frequency of price adjustment in different subsamples of the data, the correlation of the variables at the firm level is statistically indifferent from 1.

In column (1) of Table 2, we regress the ratio of long-term debt to assets on price flexibility and standard determinants of capital structure, as well as measures of market power at the firm level and market concentration at the industry level. Firms with more flexible output prices have a higher ratio of long-term debt to total assets. This positive association is significantly different from 0 at the 1% level of significance. A one-standard-deviation increase in price flexibility (0.14) is associated with a 2.4-percentage-point increase in the ratio of long-term debt to assets, which is 11% of the average ratio in the sample. In column (2), we add the firm-level measure of concentration within the Hoberg-Phillips industries. The baseline association between the frequency of price adjustment and long-term leverage is virtually unchanged. In columns (3)-(4), we only exploit variation in leverage and the frequency of price adjustment across firms within the same year, and across firms within the same industry. As expected, the size of the association between price flexibility and leverage decreases in the within-industry analysis, because industry-level characteristics are associated with price flexibility. The baseline association remains economically large and statistically different from 0, which suggests within-industry variation in price flexibility in also important to explain firm differences in capital structure. A t-test for whether the coefficients in columns (3)-(4) differ from the coefficient in column (1) fails to reject the null of no difference at plausible levels of significance.

In columns (5)-(8), we estimate specifications similar to equation 1, but using the indicator for firms with the most flexible prices, and look only at the most flexible firms (top 25% of the distribution by price flexibility) and the least flexible firms (bottom 25% of the distribution by price flexibility). This restriction further reduces the sample size, but the results are robust across the alternative sample cuts and we confirm the results we obtained with the continuous measure of price flexibility.[12] Being in the top quarter of the distribution of firms by price flexibility is associated with a six-percentage-point-higher ratio of long-term debt over assets. The results are qualitatively similar when we only exploit within-year and within-industry variation in price flexibility across firms.

The point estimate for some of our covariates differ from estimates in the literature. Our specific sample period from 1982 to 2014, and the fact that we focus on a set of large firms might explain these differences. In Tables A.2 and A.3, we estimate our

[12]The results are similar when we add all other firms and assign them a value of 0 for the FPA dummy measure (see Online Appendix).

baseline specification without the frequency of price adjustment and for all firms and for all firms in the S&P 500 between 1982 and 2014. Point estimates are similar to our baseline regressions, and we find large firms have higher leverage than small firms when we do not restrict the sample to S&P500 firms. The findings are consistent with Graham et al. (2015), who study the effect of balance-sheet variables on financial leverage over different subsamples. For samples of firms listed on NYSE and starting in 1980, they also do not detect any significant effect of tangibility on financial leverage, the effect of the book-to-market ratio on leverage flips sign, profitability is negatively associated with leverage, and size is uncorrelated with financial leverage in the last decade. For cash-flow volatility, Lemmon et al. (2008) do not find a significant association with book leverage, whereas Frank and Goyal (2009) show higher total stock return volatility is negatively correlated with long-term-debt-to-asset ratios, but not with total leverage or market leverage.

In untabulated results, we find the correlation between price flexibility and leverage does not change when we add other firm-level controls to equation (1), such as cash over assets (see Faulkender et al. (2012)).

C. Measurement Error

We only use a representative set of price spells at the firm level to construct our firm-specific measure of the frequency of price adjustment. We have several hundred spells per firms to construct the frequencies, but measurement error could still be a concern.

Erickson, Jiang, and Whited (2014) propose a novel methodology to account for the measurement error in explanatory variables using linear cumulant equations. They show several firm-level determinants of capital structure change sign or lose statistical significance once they allow for measurement error. We follow their methodology to assess the robustness of the association between price flexibility and long-term leverage when correcting for measurement error in key variables. Specifically, we follow Erickson et al. (2014) in assuming measurement error possibly affects two key determinants of capital structure: asset intangibility and the book-to-market ratio. In addition, we also assume the measure of price flexibility is measured with error. This assumption seems plausible, because the measure is based on the aggregation of frequencies of price adjustment at the good level based on a representative sample of goods.

In column (1) of Table 3, we report the baseline OLS estimator from column (1) of Table 2 to ease comparison across estimations. In columns (2)–(4), we report the estimated coefficients when implementing the cumulant-equation method of Erickson et al. (2014) for the third, fourth, and fifth cumulants. We do not report the results for higher-order cumulants because of the sample size. Using higher-order cumulants results in estimates of similar size and substantially lower standard errors. Comparing the estimated association of price flexibility with long-term leverage across specifications, the size and significance of the coefficients are similar in the baseline OLS specification and when we allow for measurement error in the frequency of price adjustment. The results for the other covariates are in general similar, but some lose statistical significance or switch sign, including the two covariates we also assume are measured with error (book-to-market ratio and asset intangibility).

V Banking Deregulation and Falsification Tests

To assess whether the effect of price flexibility on leverage is causal, one route would be to estimate the effect of a shock to firm-level price flexibility on leverage, or to propose an instrument for price flexibility. However, price flexibility is a highly persistent characteristic of firms. For instance, in our sample, a firm-level regression of post-1996 price flexibility onto pre-1996 price flexibility yields a slope coefficient of 93%, and we fail to reject the null that the coefficient equals 1 at any plausible level of significance.[13] This persistence suggests we can hardly consider a shock to firm-level price flexibility for identification purposes in our sample. Therefore, in this paper, we do not aim to test for the causal effect of price flexibility on financial leverage.

Instead, we test whether an exogenous shock to the supply of credit affects the financial leverage of sticky-price firms more than the financial leverage of flexible-price firms. We propose an identification strategy inspired by the financial-constraints literature. We (i) identify a positive shock to the supply of debt, (ii) show inflexible-price firms increase leverage more than flexible-price firms, and (iii) show the effect does not revert in the short run. Our strategy exploits a quasi-exogenous shock to financial constraints, and uses ex-ante unconstrained firms to assess the causal effect of financial

[13] See also Nakamura and Steinsson (2008), Golosov and Lucas (2007), and Alvarez et al. (2011).

constraints on inflexible-price firms.

To implement this strategy, we need a quasi-exogenous shock to firm-level financial constraints, as well as a viable control group of firms to assess how inflexible firms' long-term leverage would have evolved absent the shock.

The shock we use is the staggered state-level implementation of the IBBEA of 1994. The IBBEA represented a shock to the ability of banks to open branches and extend credit across state borders. This shock is relevant for the leverage of firms in our sample, because in Section V.*B.*, we find 95% of them have a credit line open with at least one bank, and all firms use such lines, especially the inflexible-price firms (see Figure A.1 in the appendix).

For the control group, we use flexible-price firms in the same states and the same years as inflexible-price firms to proxy for the behavior of inflexible-price firms absent the shock. Below, we show the pre-shock trends of long-term leverage for inflexible- and flexible-price firms are similar, which supports the parallel-trends assumption. In addition, we do not detect a change in the price flexibility of firms around the shock, lowering the likelihood that firms change leverage because their price flexibility changed.

A. Institutional Details and Interpretation

We follow the literature on banking deregulation and use the IBBEA as an exogenous shock to bank lending. Kroszner and Strahan (2014) and Rice and Strahan (2010) discuss in detail the advantages of this empirical design and the political forces driving the deregulation process. They argue technological progress, such as ATMs, accelerated deregulation, whereas the timing of implementation across different states was tied to the political process. Because of the staggered implementation, we can flexibly control for any persistent cross-state differences with state fixed effects. Time fixed-effects control flexibly for any unobservable concurrent U.S.-wide shocks, including but not limited to national changes in banking regulation and economic conditions.

Restrictions to banks' geographic expansion have a long history in the United States (Kroszner and Strahan (2014)). The McFadden Act of 1927 gave states the authority to regulate in-state branching, and most states enforced restrictions on branching well into the 1970s. In 1970, only 12 states allowed unrestricted in-state opening of branches, and 16 states prohibited banks from opening more than a single branch. In addition to branching restrictions, the Douglas Amendment to the 1956 Bank Holding Company Act effectively prohibited a bank holding company from acquiring banks outside the state where it was headquartered (Strahan (2003)).

Starting in the 1970s, the restrictions on acquiring banks across states were gradually eased. Kroszner and Strahan (1999) argue the timing of this deregulation wave relates to technological innovations, but not to time-varying local economic conditions. Instead, before the IBBEA of 1994, banks needed the target state's explicit approval to open branches across state lines.

The approval of IBBEA was a watershed event for interstate banking, but did not immediately lead to nationwide branching in all states. The law permitted states to (a) require a minimum age of the acquired institution, (b) restrict *de novo* interstate branching, (c) disallow the acquisition of individual branches without acquiring the entire bank, and (d) impose statewide deposit caps. We use Rice and Strahan's (2010) time-varying index for regulatory constraints between 1994 and 2005 to construct a dummy variable that equals 1 in the year the state lifted at least one of the restrictions (a) through (d), and in all the subsequent years. In the following sections, a state is deregulated when this dummy variable equals 1, and it is not deregulated otherwise.[14] We map our firms to states based on the location of the firm's headquarters. For both external financing decisions and the management of internal capital markets, CFOs are crucial (Graham and Harvey (2002)), which is why our empirical analysis identifies the company's headquarters as the relevant geographic unit for financial leverage choices.

B. Financial Dependence and Bank Debt

Our sample includes firms in the S&P500 from January 1982 to December 2014, for which we can observe the micro-pricing data. Our empirical design exploits a shock to bank-level debt, and hence we first need to verify that the firms in our sample depend on bank debt rather than only public bond markets. Colla et al. (2013) report that bank loans and credit lines jointly account for at least 30% of the leverage for the largest Compustat firms. This fact suggests bank debt is an important source of financing for firms with similar characteristics to the ones in our sample.

[14]No states reinstated any restriction they had already lifted. Several states lifted the restrictions (a) through (d) in different years from 1996 until 2002.

To assess whether the firms in our sample depend on bank debt, we use the data on credit lines collected by Sufi (2009).[15] These data allow us to observe an extensive margin of credit lines—whether firms have an active credit line or not—and an intensive margin of credit lines—the share of the line that has been used at each point in time. We can construct the extensive margin for all the firm-year observations in our sample, whereas the intensive margin is only available for those firms that match with the 5% random sample of Compustat firms constructed by Sufi (2009).

As for the extensive margin, the vast majority of the firm-year observations in our sample have a credit line open with at least one bank (94.6%). Flexible-price firms are more likely to have a credit line (97.3%) than inflexible-price firms (93.6%), and a t-test for whether these ratios are equal rejects the null at the 1% level of significance. Moving on to the intensive margin, we find the usage rate of credit lines for firms in our sample is 24.8%. An economically significant difference exists in the usage rate across inflexible-price firms (28.1%) and flexible-price firms (15.6%). A t-test for whether these ratios are equal rejects the null at the 5% level of significance. In Figure A.1 of the Online Appendix, we plot the density of the usage ratio for the two groups of firms. The full distribution of the usage ratio for inflexible-price firms lies to the right of the distribution for flexible-price firms. Although inflexible-price firms are less likely to have a credit line with banks, they are more likely to draw down the credit line, indicating they might be more credit constrained than flexible-price firms.

C. Triple-Differences Strategy

We propose a triple-differences strategy exploiting the time variation in the implementation of the IBBEA. Moreover, we use flexible-price firms as counterfactual for the evolution of long-term debt of inflexible-price firms absent the deregulation shock. The idea is that, for several reasons, flexible-price firms were not borrowing constrained before 1996, as we discuss in Section II.

[15] In contrast to Capital IQ, Sufi (2009) has comprehensive coverage starting in 1996 and information on drawn and undrawn credit lines.

C.1 Parallel-Trends Assumption

A necessary condition for identification is the *parallel-trends assumption*, which states that the evolution of long-term debt of flexible- and inflexible-price firms would have followed common trends across states before *and* after the shock, had the shock not happened. The potential outcome absent the shock is unobservable, and hence we cannot test this assumption directly. At the same time, we can assess the extent to which the trends of long-term leverage across flexible- and inflexible-price firms are parallel before the shock. If we are convinced the pre-trends are parallel, our identifying assumption would be that any divergence in the trends after the shock is due to the shock itself, and not to other possible concurrent shocks or alternative explanations. Under this identifying assumption, the evolution of long-term debt of flexible-price firms represents a valid counterfactual to the evolution of long-term debt of inflexible-price firms had they not been exposed to the deregulation.

Figure 3 proposes a visual assessment for whether the trends in long-term leverage are parallel across flexible- and inflexible-price firms in the years before the first states implement the IBBEA in 1996. Figure 3 plots the estimated coefficients, $\hat{\beta}_t$, and the 95% confidence intervals from the following OLS specification:

$$Lt2A_{i,t} = \alpha + \sum_{t=1983}^{1996} \beta_t \times FPA_i + \delta_1 \times FPA_i + \eta_t + \epsilon_{i,t}, \tag{2}$$

which estimates year-specific coefficients of FPA for the years before the first IBBEA implementations (1996). The excluded year is 1982, and we can interpret β_t as the change in the effect of price stickiness on firms' leverage from 1982 to year t. The estimated coefficient $\hat{\delta}_1$ equals 0.092 (t-stat 5.54), and statistical inference is based on standard errors clustered at the firm level. The sizes of the confidence intervals are similar if we allow for correlation of unknown form across observations in the same state. We fail to reject the null hypothesis that the effect of price flexibility is equal to that in the baseline year for all years before the first implementations of IBBEA except 1995. The estimated year-specific effect for 1995 is positive rather than negative, which decreases the likelihood that pre-trends drive our result.

C.2 Price Flexibility around the Shock

A large literature in macroeconomics finds price flexibility is a highly persistent feature of firms (e.g., see Alvarez et al. (2011) and Nakamura, Steinsson, Sun, and Villar (2016)). We verify in our sample firm-level that price stickiness is extremely persistent before and after the banking deregulation shock. This evidence alleviates concerns that banking deregulation affects price flexibility. Ideally, we would like to test formally that the firm-level frequency of price adjustment did not change over time, and the bank-deregulation shock did not affect the frequencies. We cannot compute yearly values, because to construct a meaningful measure, we need several price spells for a given good.

We therefore proceed as follows. We identify the firms in our sample for which we can observe monthly price spells for the three years before and after 1996. We construct a measure of price flexibility before 1996, based on the monthly spells in the period 1993-1995, and a measure of price flexibility after 1996, based on the monthly spells in the period 1996-1998. We then regress the post-1996 measure on the pre-1996 measure and a constant. Our null hypothesis is that the regression coefficient equals 1; that is, the pre-1996 measure is perfectly correlated with the post-1996 measure. Our estimated coefficient equals 0.93, and we cannot reject the null that this coefficient differs from 1 at any plausible level of significance. The 95% confidence interval around the point estimate is (0.73; 1.12). We truncate price spells by only focusing on a three-year period, and hence we introduce noise into our measures. The almost perfect correlation in the frequency of price adjustment before and after 1996 is therefore hardly consistent with the notion that firm-level price flexibility changed around the implementation of the IBBEA.

C.3 Triple-Differences Specification

To implement our strategy, we estimate the following specification:

$$\begin{aligned} Lt2A_{i,t} &= \alpha + \beta \times FPA_i \times Deregulated_{i,t} \\ &\quad + \delta_1 \times FPA_i + \delta_2 \times Deregulated_{i,t} + \eta_t + \eta_k + \epsilon_{i,t}, \end{aligned} \tag{3}$$

where $Deregulated_{i,t}$ is an indicator that equals 1 if firm i is headquartered in a state that had implemented the deregulation in or before year t, and 0 otherwise; η_k and η_t are a full set of industry and year effects. Alternatively, we can also include a full set of firm fixed effects (η_f), because variation exists in the interaction between price flexibility and deregulation within firms over time. When included, firm fixed effects absorb industry fixed effects and the frequency of price adjustment. All the results are similar if we also add the full set of controls in equation (1) (see Table A.4 in the Online Appendix).

Equation (3) compares the long-term debt-to-assets ratio within firms before and after their state implemented the deregulation, across firms in deregulated and regulated states, and across flexible- and inflexible-price firms. We label our specification a triple-differences specification to emphasize these three dimensions we use to compare the firms in the sample, but note our specification only exploits one exogenous shock, captured by the deregulation dummy.

Based on the predictions we described in Section II, we expect the following regarding the coefficients of equation (3): $\delta_1 > 0$ because, on average, higher price flexibility leads to more long-term debt; and $\delta_2 \geq 0$, because firms have more funds available to borrow after the 1994 deregulation shock, which could be 0 because flexible-price firms were unlikely to be financially constrained before the shock. The crucial prediction of our strategy is that $\beta < 0$, because the most inflexible-price firms obtain disproportionally more funds after the deregulation compared to the most flexible-price firms.

For the purposes of statistical inference, we cluster standard errors at the firm level. All t-statistics are higher if we instead cluster standard errors at the state level, which is the level of the treatment. We only observe firms in 42 states. The low number of clusters likely explains why standard errors are lower when we cluster at the state level as compared to the firm level.

Table 4 reports the estimates for the coefficients in equation (3). In columns (1)-(4), FPA is the continuous measure of price flexibility; in columns (5)-(8), it is the dummy that equals 1 for firms in the top 25% of the distribution based on price flexibility, and 0 for those in the bottom 25% of the distribution.

For both sets of results, the first column reports estimates for the baseline specification. In the second column, we add year fixed effects and the 48 industry-level dummies for the Fama-French industry taxonomy. In the third column, we add year fixed effects and the 50 industry-level dummies for the Hoberg-Phillips industry classification. In the fourth column, we add year fixed effects and firm fixed effects.

Across all specifications, the sign of the estimated coefficients are in line with

Hypothesis 1 and Hypothesis 2 in Section II. Firms with higher price flexibility have higher long-term debt on average ($\hat{\delta}_1 > 0$). More importantly, across all specifications, we find flexible-price firms increase their leverage less than inflexible-price firms after the state-level implementation of the deregulation ($\hat{\beta} < 0$). The effect of price flexibility post-deregulation ($\hat{\beta} + \hat{\delta}_1$) is close to zero across all specifications. Comparing column (1) with columns (2)-(4), and column (5) with columns (6)-(8), we see the size of the estimated interaction effect does not change when we only exploit within-industry variation. Therefore, whereas industry-level effects explain about half of the size of the baseline effect of price flexibility on leverage, the variation across firms within the same industries explains the full size of the effect of financial constraints across flexible- and inflexible-price firms. This result survives when we only exploit variation within firms, and hence we absorb any time-invariant determinant of financial leverage at the firm level.

Tables A.4 and A.5 show our triple-difference design when we add all the covariates from the baseline OLS analysis, as well as state fixed effects. State fixed effects control flexibly for unobserved heterogeneity across states, such as differential growth paths, which might affect demand for goods, investment prospects, and ultimately external finance demands.

Table A.7 in the Online Appendix shows the results are largely unchanged when we exclude financial firms and utilities. Tables A.8 and A.9 in the Online Appendix, instead, run our triple-differences identification design interacting the deregulation dummy also with firm volatility and the Kaplan-Zingales index at the firm level, whereas Tables A.10 and A.11 reports the specification for volatility and the Kaplan-Zingales index without the frequency of price adjustment. We do not detect any systematic interaction effect across specifications, whereas our baseline results continue to hold: unconditionally, flexible price firms have higher financial leverage, but the firms with less flexible output prices are the ones that increase leverage more following the bank branching deregulation.

C.4 Effect on Impact and Over Time

Our tests so far have used observations for a same firm in different years, both before and after the implementation of the IBBEA. Bertrand et al. (2004) show the autocorrelation between observations of a same unit over time might understate dramatically the size of the standard errors in difference-in-differences research designs. We tackle this issue in

Table 5. First, we estimate equation (3) using only two data points for each firm. We only keep firm-level observations in the year before the deregulation and the year after the deregulation is implemented in their state. This test aims to estimate the effect of the shock on impact, that is, around the year in which the shock happened. We report the results for this test in column (1) of Table 5. We only have 599 observations compared to 9,119 in our baseline sample. The coefficient on the frequency of price adjustment is almost identical to the estimates in Table 4. The estimated coefficient on the interaction term between the frequency with the deregulation dummy is negative. The size of the coefficient is about half the size of the corresponding coefficient in column (1) of Table 4.

In columns (2)-(5) of Table 5, we report the results for estimating equation (3) in periods of different lengths. In column (2), we only use observations from 1994 until 2002, which include the years in which the first and the last state implemented the IBBEA (1996 and 2001, respectively). In each of columns (3)-(5), we enlarge the time period by three years going backward and forward. Qualitatively, our results are similar across these different time periods. Interestingly, the size of the interaction between price flexibility and the IBBEA implementation increases monotonically in absolute value when we add observations in later years. At the same time, the baseline effect of price flexibility on leverage stays identical across sub-periods. These results are consistent with the idea that it took time for banks to expand across state borders and for firms to adjust their leverage ratios. Diverging trends between flexible- and inflexible-price firms before the shock cannot drive these results, because we find parallel trends before the shock in Figure 3.

C.5 Effect by dependence on external financing

To corroborate the interpretation of the deregulation shock, we exploit cross-sectional variation in terms of the financial dependence. If the deregulation shock is truly driving the interaction effect, then inflexible-price firms that depend more on external finance should drive this effect. We thus estimate the specification in equation (3) separately for firms in the top tercile of cash-to-assets and for other firms and firms in the top tercile of the external finance gap and other firms. We follow Demirgüç-Kunt and Maksimovic (2002a) to calculate the external finance need of firms in our sample, using the average sales growth over the last three years, and subtract the sum of cash, total debt, and equity.

We scale the difference by total assets to arrive at the external finance gap. The rationale is that inflexible-price firms with high cash-to-assets ratios and low external finance gaps will not depend much on external financing. The deregulation shock should instead affect inflexible-price firms with lower cash-to-assets ratios and high external finance gaps. Consistent with this interpretation, Table 6 shows the effect of deregulation on firms' leverage is driven by inflexible-price firms with low cash-to-assets ratios and high external finance gaps (columns (1) and (4)), as opposed to those with high cash-to-assets ratios and low external finance gaps (columns (2) and (3)). In the Online Appendix, we introduce triple interactions between the frequency of price adjustment, the deregulation dummy, and the cash-to-assets ratio and find sticky-price firms with a higher cash-to-assets ratio increase their leverage less after the deregulation compared to sticky-price firms with low cash on hand (see Table A.12). We do not detect similar effects for triple interactions with total or idiosyncratic volatility or the KZ index.

D. Falsification Tests

To further assess the validity and interpretation of our triple-differences results, we propose an empirical setup that allows the design of two falsification tests (Roberts and Whited, 2013). We exploit the fact that the state-level implementation of the IBBEA was not only staggered over time, but also clustered in two periods. The majority of U.S. states implemented the deregulation between 1996 and 1998. The second group of states only implemented the deregulation after 2000. We call the first group of states "early states," and the second group, "late states." This setup allows us to construct three tests across three groups of years. Before 1996, no state had implemented the deregulation yet. Between 1996 and 2000, firms in early states were exposed to the deregulation, but firms in late states were not. After 2000, all firms were in deregulated states.

We consider the following specification:

$$\begin{aligned} Lt2A_{i,t} = {} & \alpha + \beta \times FPA_i \times After1996_{i,t} \times Early_i + \delta_1 \times FPA_i \times After1996_{i,t} \\ & + \delta_2 \times FPA_i \times Early_i + \delta_3 \times After1996_{i,t} \times Early_i + \gamma_1 \times FPA_i \\ & + \gamma_2 \times After1996_{i,t} + \gamma_3 \times Early_i + X'_{i,t} \times \zeta + \epsilon_{i,t}. \end{aligned} \tag{4}$$

Panel A of Figure 4 sketches our predictions for the specification in equation (4). It compares outcomes within firms before and after 1996, across firms before and after 1996, across firms in early and late states, and across flexible- and inflexible-price firms. To corroborate our triple-differences results in this alternative setup, we estimate equation (4) using only firm-level observations up to 2000. The rationale is that firms in early states were exposed to the deregulation between 1996 and 2000, whereas firms in late states were not. Flexible- and inflexible-price firms in late states thus represent the control group for the differential evolution of long-term debt in flexible- and inflexible-price firms in early states, had they not been exposed to the deregulation shock.

Our prediction is that $\beta < 0$, $\delta_1 = 0$, and $\gamma_1 > 0$; that is, flexible-price firms have higher leverage on average, and after the deregulation, only inflexible-price firms in early states increase their leverage compared to flexible-price firms in early states. The baseline effect of price flexibility on leverage should not change after 1996 for firms in late states.

The estimates in column (1) of Table 7 support our predictions. In columns (2)-(3) of Table 7, we repeat the analysis separately for firms with low and high cash-to-assets ratios. Similar to our earlier results, the subsample of firms with cash-to-assets ratios drive the effects.

We then proceed to assess the validity of our designs by constructing two falsification tests. Panel B of Figure 4 sketches our predictions for the first falsification test. We build on the specification in equation (4), but we limit our estimation to observations before 1996. This limitation implies that no firms, neither in early nor in late states, were exposed to the deregulation shock. Because in the baseline analysis we use a treatment period of four years for early states, from 1996 to 2000, we assign 1992 as a placebo deregulation year to observations in early states. We thus replace the dummy $After1996_{i,t}$ in equation (4) with the dummy $After1992_{i,t}$, which equals 1 for all firm-level observations after 1992. Our falsification test consists of comparing flexible- and inflexible-price firms in early and late states after 1992, and before the deregulation happened. If our earlier test was invalid, and our baseline results captured the effect of state-level characteristics differently across early and late states, but unrelated to the deregulation event, we should reject the null hypothesis that $\beta = 0$. Column (4) of Table 7 shows that, instead, we fail to reject this null hypothesis at a plausible level of significance. As expected, we find flexible-price firms have higher leverage on average, irrespective of the states where they are located.

We sketch the predictions for the second falsification test in Panel C of Figure 4. For this test, we exclude all firm-level observations between 1996 and 2000. This limitation implies that in each year, the observations in early and late years are either not exposed to the deregulation shock (before 1996), or they are all exposed to the deregulation shock (after 2000). We thus estimate the same specification in equation (4), but the new setup implies different predictions from those discussed above. On the one hand, we should not be able to reject the null that $\beta = 0$, because early and late states are exposed to the deregulation in the same years. On the other hand, we now do expect $\delta_1 < 0$ and $\gamma_1 > 0$, because flexible-price firms in both early and late states should have on average higher leverage, and should react less than inflexible-price firms to the deregulation shock. We find evidence consistent with these predictions in column (5) of Table 7.

VI Robustness

A. Price Flexibility, Volatility, and Leverage

Gorodnichenko and Weber (2016) and Weber (2015) argue sticky-prices firms are riskier and have higher idiosyncratic and total return volatility. Higher volatility and risk might result in lower leverage, but earlier literature on return volatility and financial leverage finds ambiguous results. Frank and Goyal (2009) document a negative relationship between total volatility and long-term book leverage, whereas Lemmon et al. (2008) do not detect a significant association between cash-flow volatility and book leverage. Higher volatility can lead to higher or lower financial leverage depending on the specifications also in our sample. In Table 2, total volatility is only weakly associated with financial leverage, and the association flips sign based on the variation we exploit, in line with the literature. Tables A.13 and A.14 in the Online Appendix document similar results for idiosyncratic volatility with respect to the CAPM and to the Fama and French three-factor model.

Several factors influence stock return volatility, and these factors could affect financial leverage differently. To study whether we can reconcile our findings with those of Gorodnichenko and Weber (2016) and Weber (2015), we decompose stock return volatility into a part predicted by the FPA and a residual. Table 8 shows higher predicted volatility by the frequency of price adjustment is negatively associated with financial leverage across specifications. The residual part of volatility orthogonal to the frequency of price adjustment, instead, does not show any robust association with financial leverage.

B. Additional Controls

The frequency of price adjustments depends on a number of factors that determine the benefits and costs of price adjustment, such as the curvature of the profit function, operating leverage, the volatility of demand, and marginal costs. In Table 9, we add a wide range of controls to further disentangle the effect of price stickiness from potentially confounding firm- and industry-level factors.

In the first column, we repeat the baseline regression with year and industry fixed effects at the Fama and French 48-industry level.

In our baseline specification, we already control for market power at the firm and industry levels using the price-to-cost margin and the Herfindahl index in annual sales at the Fama and French 48-industry level. Both of these measures have potential shortcomings, because they are only based on publicly-listed firms or might be mismeasured at the firm level. In column (2), we add the share of output accounted for by the largest four firms within an industry. This measure has the advantage of measuring concentration at the industry level for all firms using data from the economic census. The concentration ratio does not affect our baseline conclusion.

The volatility of demand might affect the frequency with which firms adjust their output prices, or affect the stability of firms' margins and hence optimal leverage choices. To study this alternative channel, we explicitly control for the durability of output in columns (3) and (4) using the classifications of Gomes, Kogan, and Yogo (2009) and Bils, Klenow, and Malin (2012), respectively. The demand for durable goods is particularly volatile over the business cycle, and consumers can easily shift the timing of their purchases, thus making their price sensitivity especially high (see, e.g., D'Acunto, Hoang, and Weber (2016)). Controlling for the cyclicality of demand has little impact on the association between the frequency of price adjustment and financial leverage.

Some heterogeneity of stickiness in output prices may reflect differences in the stickiness of input prices. For instance, firms with inflexible output prices might also have inflexible input prices, leading to stable profit margins. We show results are robust to controlling for input-price stickiness at the industry level. Unfortunately, the BLS micro data do not allow us to construct analogous measures of input-price stickiness at the firms level, because the data do not contain the identity of buyers. We proxy for input-price stickiness with the frequency of wage adjustment at the industry level from Barattieri, Basu, and Gottschalk (2014) in column (5). We indeed find firms in industries with more flexible wages tend to have higher financial leverage, but controlling for input-price stickiness has little effect on the association between the frequency of price adjustment and financial leverage.

Column (6) adds Engel curve slopes from Bils et al. (2012) to control for differences in income elasticities, column (7) includes the Kaplan - Zingales index (excluding leverage) to investigate the impact of financial constraints, column (8), the S&P long-term issuer rating, and columns (9) and (10) include the ratio of fixed costs to sales and the ratio of costs of goods sold and selling, general, and administrative expenses to total assets (Novy-Marx (2011)) as alternative proxies for operating leverage. Firms with higher ratings and lower operating leverage have higher financial leverage, whereas income elasticities have no systematic association with financial leverage. Controlling for the additional variables, however, has no impact on our estimate of price flexibility on financial leverage.

Column (11) adds all covariates jointly. Whereas some of the covariates now lose statistical significance or switch signs, the frequency of price adjustment is robustly associated with higher financial leverage.

Table A.1 in the Online Appendix shows our results do not change when we consider two alternative definitions of financial leverage as our main outcome variable: total debt over total assets and net debt over total assets. In Table A.15, we also find the baseline results are virtually identical when we exclude financial firms and utilities from the sample. In unreported results, we find similar effects when restricting the variation to within industries $\times$ year combinations, both in terms of size and statistical significance. Industry$\times$year fixed effects control for industry-specific trends in leverage over time.

The frequency of price adjustment varies at the firm level. In Table A.16 in the Online Appendix, we show our results are economically and statistically similar if we collapse our data at the firm level and run a single cross-sectional regression. Price stickiness explains 10% of the cross-sectional variation in leverage across firms. Size, volatility, intangibility, the price-to-cost margin or industry concentration all explain less of the cross-sectional variation (see Table A.17 in the Online Appendix).

VII Conclusion

We show that firms with inflexible output prices have lower leverage relative to firms with flexible prices, after controlling for standard determinants of capital structure. Using the staggered implementation of the 1994 Interstate Bank Branching Efficiency Act across states, we test whether a larger supply of bank debt increases the financial leverage of sticky-price firms more compared to flexible-price firms in a triple-differences strategy, and find empirical support.

These results suggest price flexibility is an important determinant of firms' capital structure. Because firm-level price flexibility is highly persistent over time, these results also suggest price flexibility might help us understand the origin of persistent differences in financial leverage across firms as documented by Lemmon, Roberts, and Zender (2008).

Price rigidity has a long tradition in research across fields as different as Marketing, Industrial Organization, and Macroeconomics. Our results open up exciting avenues for future research at the intersection of Corporate Finance, Macroeconomics, and Industrial Organization.

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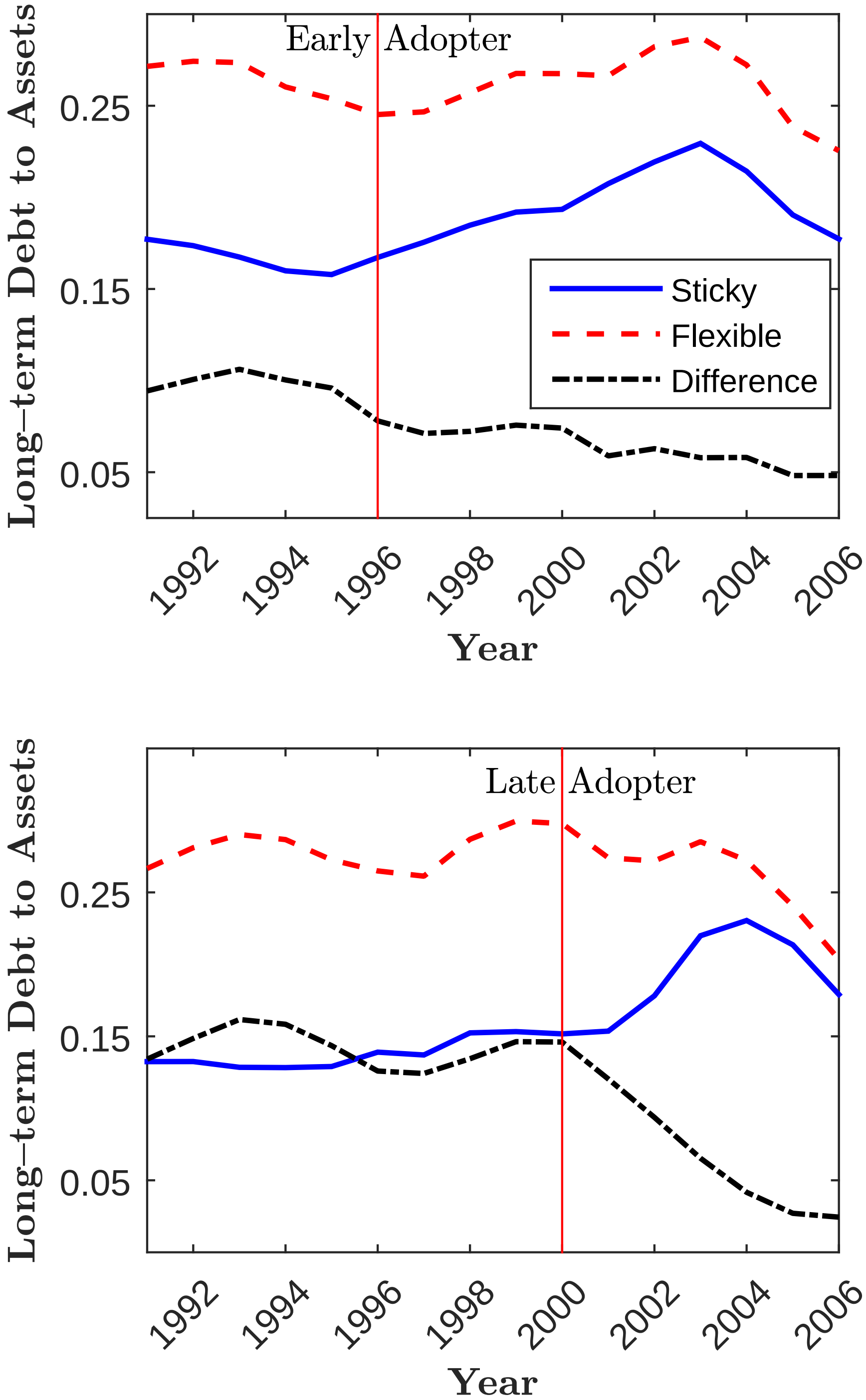
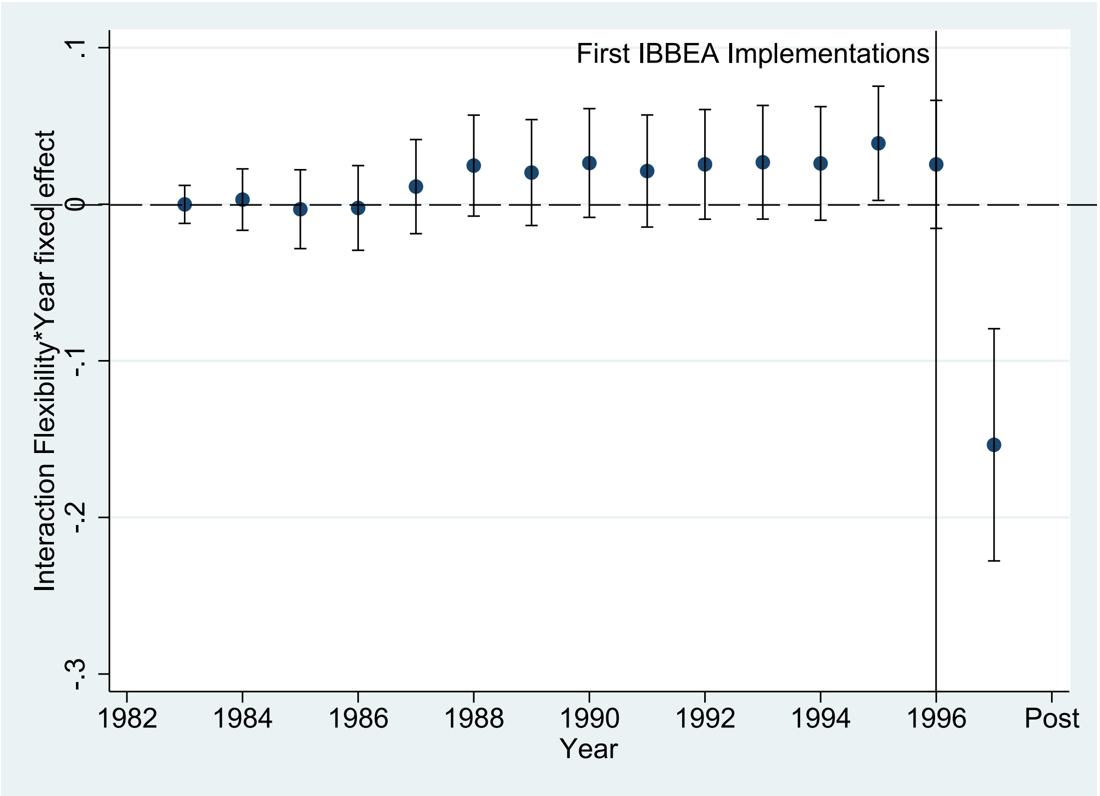
Figure 2: **Long-Term Debt and Price Flexibility**



This figure plots the ratio of long-term debt to total assets for different percentiles of the frequency of price adjustment distribution. Sticky-price firms are firms in the bottom quartile of the distribution. Flexible-price firms are firms in the top quartile of the distribution. The sample period is January 1982 to December 2014. Equally-weighted probabilities of price adjustments are calculated at the firm level using the micro-data underlying the Producer Price Index constructed by the Bureau of Labor Statistics.

Figure 3: **Parallel Trends Assumption: Assessment of Pre-Trends**



This figure plots the estimated coefficients $\hat{\beta}_t$ and the 95% confidence intervals from the following linear equation:

$$Lt2A_{i,t} = \alpha + \sum_{t=1983}^{1996} \beta_t \times FPA_i + \delta_1 \times FPA_i + \eta_t + \epsilon_{i,t},$$

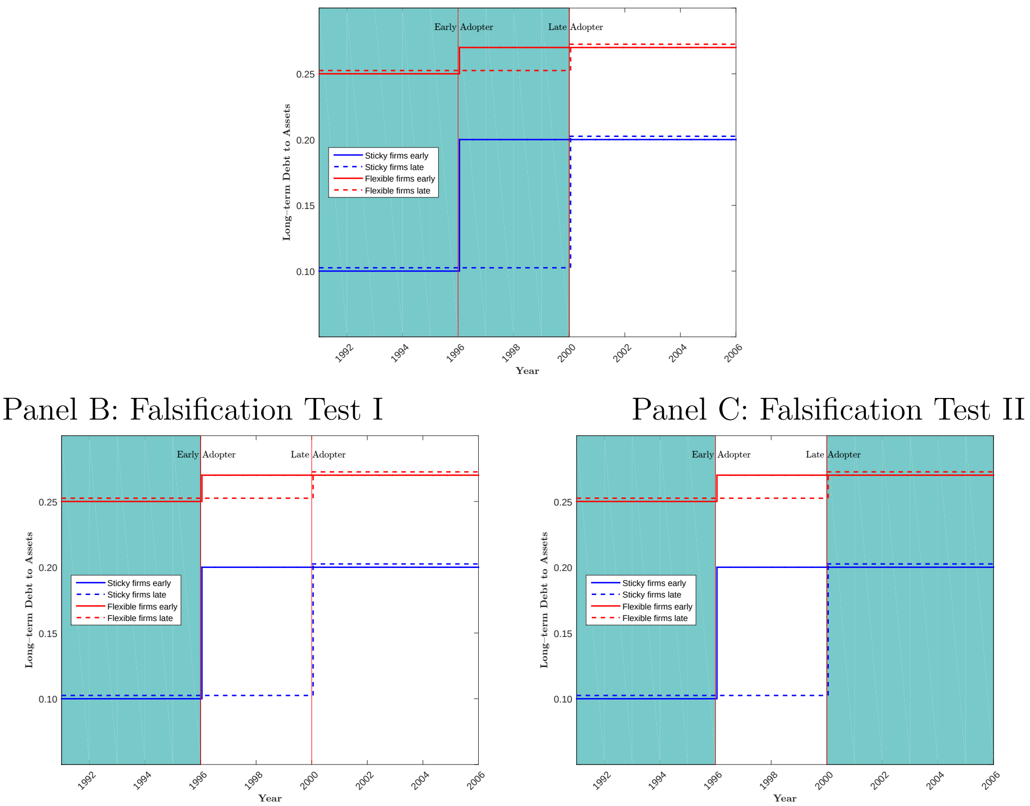
which includes a set of leads of the interactions between price flexibility and year fixed effects for the years before the first IBBEA implementations (1996). The excluded year is 1982. The estimated coefficient $\hat{\delta}_1$ equals 0.092 (t-stat 5.54). The sample period is January 1982 to December 2014. Standard errors are clustered at the firm level.

Figure 4: **Falsification Tests**

Panel A: Quadruple-differences

Panel B: Falsification Test I

Panel C: Falsification Test II



This figure describes our falsification framework (Panel A) and two falsification tests (Panels B and C). The shaded areas represent the years whose observations we exploit in each test. In each Panel, the two bottom lines refer to inflexible-price firms in early states that implemented the deregulation of interstate branching between 1996 and 1998 (blue, solid), and in late states that implemented the deregulation after 2000 (blue, dashed). The two top lines refer to flexible-price firms in early states (red, solid) and late states (red, dashed). In each type of state, the increase in the ratio of long-term debt to assets increases more for inflexible-price firms than for flexible-price firms after the deregulation. In Panel A, we only use observations up to 2000. In this setup, we therefore compare financial leverage within firms before and after 1996, across firms before and after 1996, between early and late states, and between flexible- and inflexible-price firms. Hypothesis 2 in section II states that firms in early states increase their financial leverage in 1996, whereas firms in late states do not. Moreover, sticky-price firms in early states increase their financial leverage more than flexible-price firms in 1996. In Panel B, we depict the first falsification test, in which we only use observations up to 1996. Before 1996, no firm was exposed to the deregulation, and hence we should see no differences in financial leverage across firms in early and late states. Instead, we should detect the unconditional difference in leverage between flexible- and inflexible-price firms, irrespective of their location. In Panel C, we depict the second falsification test, in which we use only observations before 1996 and after 2000, and hence we exclude the period 1996-2000. In this case, either all firms are in deregulated states, or they are all in regulated states. Thus, we should detect no differences in the change in leverage across firms in early and late states. Instead, we should detect the baseline difference in leverage across flexible- and inflexible-price firms, as well as the larger increase in leverage for inflexible-price firms after the deregulation.

Table 1: **Summary Statistics**

This table reports descriptive statistics for the variables used in the empirical analysis in Panel A, and correlations across variables in Panel B. FPA measures the frequency of price adjustment. Equally-weighted probabilities of price adjustments are calculated at the firm level using the micro-data underlying the Producer Price Index constructed by the Bureau of Labor Statistics. FPA Dummy is a dummy that equals 1 for firms in the top 25% of the distribution by the frequency of price adjustment and 0 for the bottom 25%. Total vol is the annual total stock return volatility, Idio vol$_{CAPM}$ *and Idio vol*$_{FF3}$ *are idiosyncratic volatility with respect to the CAPM and Fama and French 3-factor model, Lt2A is long-term debt to total assets, Profitability is operating income over total assets, Size is the logarithm of sales, B-M ratio is the book-to-market ratio, Intangibility is intangible assets to total assets, Price-cost margin is the price-to-cost margin, and HHI is the Herfindahl-Hirschman index of sales at the Fama and French 48-industry level. The sample period is January 1982 to December 2014.*

Panel A. Summary Statistics

	FPA (1)	FPA Dummy (2)	Total vol (3)	Idio vol$_{CAPM}$ (4)	Idio vol$_{FF3}$ (5)	Lt2A (6)	Prof (7)	Size (8)	BM (9)	It2A (10)	PCM (11)	HHI (12)
Mean	0.14	0.25	0.33	0.28	0.28	0.21	0.15	8.25	0.60	0.26	0.37	0.10
Median	0.07	0.00	0.29	0.25	0.24	0.20	0.15	8.29	0.49	0.23	0.34	0.08
Std	0.14	0.43	0.16	0.14	0.14	0.13	0.08	1.39	0.41	0.17	0.18	0.10
Min	0.00	0.00	0.02	0.02	0.02	0.00	−0.47	4.16	0.05	0.01	0.05	0.01
Max	0.71	1.00	2.04	1.94	1.88	0.62	0.97	11.62	2.23	0.74	0.83	0.93
Nobs	9,133	9,133	9,130	9,130	9,130	9,119	9,125	9,133	8,997	9,048	9,133	9,133

Panel B. Correlations

	FPA (1)	FPA Dummy (2)	Total vol (3)	Idio vol$_{CAPM}$ (4)	Idio vol$_{FF3}$ (5)	Lt2A (6)	Prof (7)	Size (8)	BM (9)	It2A (10)	PCM (11)
FPA Dummy	0.869***										
Total vol	−0.0636***	−0.0415***									
Idio vol$_{CAPM}$	−0.0538***	−0.0295**	0.953***								
Idio vol$_{FF3}$	−0.0617***	−0.0358***	0.948***	0.998***							
Lt2A	0.249***	0.200***	−0.0727***	−0.0567***	−0.0579***						
Profitability	−0.143***	−0.115***	−0.0932***	−0.0975***	−0.0987***	−0.284***					
Size	0.129***	0.117***	−0.191***	−0.272***	−0.279***	0.121***	−0.0650***				
B-M ratio	0.342***	0.274***	−0.0276**	0.0117	0.0116	0.258***	−0.456***	−0.0253*			
Intangibility	−0.224***	−0.187***	−0.0505***	−0.0923***	−0.0898***	0.109***	−0.132***	0.297***	−0.192***		
Price-Cost margin	−0.211***	−0.194***	0.0131	0.003	0.00291	−0.167***	0.461***	−0.190***	−0.383***	0.141***	
HHI	−0.0924***	−0.0976***	0.0474***	0.0270*	0.0297**	−0.0647***	0.133***	0.133***	−0.164***	0.136***	0.0580***

t-stats in parentheses
$*p < 0.10, **p < 0.05, ***p < 0.01$

Table 2: **Panel Regressions of Leverage on Price Flexibility**

This table reports the results for estimating the following linear equation:

$$Lt2A_{i,t} = \alpha + \beta \times FPA_i + X'_{i,t-1} \times \gamma + \eta_t + \eta_k + \epsilon_{i,t},$$

where Lt2A is long-term debt to total assets, FPA is the frequency of price adjustment, and $X'_{i,t-1}$ a vector of additional controls (see Table 1 for a detailed description). HP Firm-level HHI is the firm-level measure of product-space concentration based on the Hoberg & Phillips 300 industries. Fama-French 48 FE is a set of 48 dummies that capture the Fama and French 48-industries. Hoberg-Phillips 50 FE is a set of 50 dummies that capture the Hoberg and Phillips 50 industries. The sample period is January 1982 to December 2014 in columns (1) and (5). The sample is restricted to the period January 1996 to December 2014 in all other columns, due to the availability of the Hoberg-Phillips data. Standard errors are clustered at the firm level. Columns (1) - (4) use the continuous measure of the frequency of price adjustment, and columns (5) - (8) use a dummy that equals 1 if the firm is in the top quartile of the frequency of price adjustment distribution and zero if the firm is in the bottom quartile. Equally-weighted probabilities of price adjustments are calculated at the firm level using the micro-data underlying the Producer Price Index constructed by the Bureau of Labor Statistics.

	FPA continuous				FPA dummy			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
FPA	0.17***	0.15***	0.12***	0.09**	0.06***	0.06***	0.04**	0.04**
	(4.95)	(3.54)	(3.07)	(2.09)	(3.92)	(2.80)	(2.15)	(2.30)
Total vol	−0.02	−0.03	0.05**	0.07**	−0.05**	−0.05*	0.03	0.01
	(−1.26)	(−1.46)	(2.19)	(2.56)	(−2.13)	(−1.97)	(1.05)	(0.48)
Profitability	−0.23***	−0.11	−0.20***	−0.22***	−0.22***	−0.05	−0.11	−0.13
	(−3.15)	(−1.29)	(−2.77)	(−2.79)	(−2.61)	(−0.46)	(−1.13)	(−1.33)
Size	0.00	−0.00	−0.00	−0.00	−0.00	−0.01**	−0.01	−0.01
	(1.16)	(−0.81)	(−0.93)	(−0.71)	(−0.31)	(−2.01)	(−1.35)	(−1.65)
B-M ratio	0.05***	0.03**	−0.01	0.00	0.06***	0.04**	0.01	0.01
	(5.30)	(2.53)	(−0.55)	(0.16)	(5.29)	(2.51)	(0.62)	(0.99)
Intangibility	0.11***	0.10***	0.14***	0.10***	0.14***	0.13**	0.17***	0.13***
	(3.79)	(2.90)	(3.93)	(2.96)	(3.16)	(2.44)	(3.43)	(3.13)
Price-Cost margin	−0.00	−0.06*	0.04	0.04	0.02	−0.05	0.06	0.07*
	(−0.13)	(−1.84)	(0.98)	(1.01)	(0.46)	(−1.13)	(1.13)	(1.68)
HHi	−0.03	0.05	0.06	0.00	−0.02	0.03	0.06	−0.15**
	(−0.66)	(0.98)	(1.65)	(0.06)	(−0.35)	(0.41)	(0.93)	(−1.99)
HP Firm-level HHI		−0.04	0.03	0.03		−0.04	−0.03	0.01
		(−1.27)	(1.06)	(0.98)		(−1.03)	(−0.66)	(0.20)
Constant	0.13***	0.24***	0.18***	0.19***	0.17***	0.31***	0.20***	0.23***
	(3.73)	(4.64)	(3.53)	(3.69)	(4.12)	(4.67)	(2.92)	(3.89)
Year FE			X	X			X	X
Fama-French 48 FE			X				X	
Hoberg-Phillips 50 FE				X				X
Nobs	8,821	4,706	4,706	4,671	4,406	2,265	2,265	2,256
Adjusted R^2	0.16	0.09	0.29	0.24	0.18	0.12	0.33	0.32

t-stats in parentheses
$*p < 0.10, **p < 0.05, ***p < 0.01$

Table 3: **Panel Regressions of Leverage on Price Flexibility (Errors-in-Variables)**

This table reports the results of regressing long-term debt to total assets (Lt2A) on the frequency of price adjustment, FPA, and a vector of additional controls (see Table 1 for a detailed description) using the linear cumulant equations methodology of Erickson, Jiang, and Whited (2014). We assume FPA, B-M ratio, and Intangibility are measured with error. The sample period is January 1982 to December 2014. Standard errors are clustered at the firm level. All columns use the continuous measure of the FPA. Equally-weighted probabilities of price adjustments are calculated at the firm level using the micro-data underlying the Producer Price Index constructed by the Bureau of Labor Statistics.

	OLS	3rd cum	4th cum	5th cum
	(1)	(2)	(3)	(4)
FPA	0.17^{***}	0.27^{***}	0.30^{***}	0.06^{***}
	(4.95)	(3.65)	(6.05)	(3.27)
Total vol	-0.02	-0.03	0.01	-0.02
	(-1.26)	(-1.07)	(0.29)	(-0.95)
Profitability	-0.23^{***}	0.03	0.58^{***}	-0.00
	(-3.15)	(0.12)	(6.69)	(-0.06)
Size	0.00	0.02^{**}	0.02^{***}	0.03^{***}
	(1.16)	(2.36)	(3.79)	(6.31)
B-M ratio	0.05^{***}	-0.02	0.04^{***}	0.04^{***}
	(5.30)	(-0.66)	(2.65)	(4.38)
Intangibility	0.11^{***}	-0.25^{**}	-0.28^{***}	-0.40^{***}
	(3.79)	(-2.06)	(-4.91)	(-16.01)
Price-Cost margin	-0.00	-0.04	-0.08^{**}	0.02
	(-0.13)	(-0.55)	(-2.07)	(0.57)
HHI	-0.03	-0.02	-0.03	-0.00
	(-0.66)	(-0.42)	(-0.57)	(-0.01)
Constant	0.13^{***}	0.13^{**}	-0.01	0.06^{*}
	(3.73)	(2.24)	(-0.22)	(1.66)
Nobs		8,821		
Adjusted R^2		0.16		

t-stats in parentheses

$*p < 0.10, **p < 0.05, ***p < 0.01$

Table 4: **Triple Differences: Interstate Bank Branching Efficiency Act, Price Flexibility, and Leverage**

This table reports the results for estimating the following linear specification:

$$\begin{aligned} Lt2A_{i,t} &= \alpha + \beta \times FPA_i \times Deregulated_{i,t} \\ &+ \delta_1 \times FPA_i + \delta_2 \times Deregulated_{i,t} + \eta_t + \eta_k + \epsilon_{i,t}, \end{aligned}$$

where Lt2A is long-term debt to assets, FPA is the frequency of price adjustment, and $Deregulated_{i,t}$ is an indicator that equals 1 if firm i is in a state that had implemented the deregulation in or before year t, and 0 otherwise. η_t and η_k are a full set of year and industry fixed effects. Fama-French 48 FE is a set of forty-eight dummies that capture the Fama and French 48 industries. Hoberg-Phillips 50 FE is a set of fifty dummies that capture the Hoberg and Phillips 50 industries. Firm FE is a set of firm-level fixed effects, which absorbs the measures of price flexibility in columns (4) and (8). The sample period is January 1982 to December 2014 except from columns (3) and (7), in which the sample period is January 1996 to December 2014, due to the availability of the Hoberg-Phillips data. Standard errors are clustered at the firm level. Columns (1) - (4) use the continuous measure of the frequency of price adjustment and columns (5) - (8) use a dummy that equals 1 if the firm is in the top quartile of the frequency of price adjustment distribution and zero if the firm is in the bottom quartile. Equally-weighted probabilities of price adjustments are calculated at the firm level using the micro-data underlying the Producer Price Index constructed by the Bureau of Labor Statistics.

	FPA				FPA Dummy			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
FPA × Deregulated	-0.15^{***}	-0.16^{***}	-0.25^{***}	-0.16^{***}	-0.04^{**}	-0.04^{***}	-0.08^{***}	-0.05^{***}
	(-4.08)	(-4.64)	(-4.60)	(-4.56)	(-2.41)	(-2.71)	(-3.30)	(-3.05)
FPA	0.30^{***}	0.17^{***}	0.28^{***}		0.09^{***}	0.06^{***}	0.10^{***}	
	(8.04)	(5.16)	(4.40)		(5.77)	(3.58)	(3.46)	
Deregulated	0.05^{***}	0.03^{**}	0.04^{**}	0.02	0.04^{***}	0.01	0.02	0.02
	(5.75)	(2.15)	(2.42)	(1.43)	(3.39)	(0.78)	(1.15)	(1.45)
Constant	0.15^{***}	0.15^{***}	0.19^{***}	0.18^{***}	0.16^{***}	0.16^{***}	0.15^{***}	0.19^{***}
	(19.75)	(17.84)	(9.91)	(28.36)	(15.67)	(14.42)	(8.94)	(22.20)
Year FE		X	X	X		X	X	X
Fama-French 48 FE		X				X		
Hoberg-Phillips 50 FE			X				X	
Firm FE				X				X
Nobs	9,119	9,119	4,843	9,119	4,558	4,558	2,354	4,558
Adjusted R^2	0.08	0.27	0.21	0.58	0.09	0.30	0.26	0.55

t-stats in parentheses

$*p < 0.10, **p < 0.05, ***p < 0.01$

Table 5: **Triple Differences: Effect Before/After and at Alternative Horizons**

This table reports the results for estimating the following linear specification:

$$Lt2A_{i,t} = \alpha + \beta \times FPA_i \times Deregulated_{i,t} + \delta_1 \times FPA_i + \delta_2 \times Deregulated_{i,t} + \eta_t + \eta_k + \epsilon_{i,t},$$

where $Lt2A$ is long-term debt to assets, FPA is the frequency of price adjustment, and $Deregulated_{i,t}$ is an indicator that equals 1 if firm i is in a state that had implemented the deregulation in or before year t, and 0 otherwise. The sample period is January 1982 to December 2014. Standard errors are clustered at the firm level. In column (1), the sample only includes firm-level observations in the year before and after the implementation of the interstate bank branching deregulation in the state where the firm is headquartered. In columns (2)-(5), the sample period is indicated at the top of each column. All columns use the continuous measure of the frequency of price adjustment. Equally-weighted probabilities of price adjustments are calculated at the firm level using the micro-data underlying the Producer Price Index constructed by the Bureau of Labor Statistics.

	Before/After (1)	1994-2002 (2)	1991-2005 (3)	1988-2008 (4)	1985-2011 (5)
FPA × Deregulated	-0.07^{**}	-0.10^{**}	-0.11^{***}	-0.12^{***}	-0.14^{***}
	(-2.22)	(-2.37)	(-3.08)	(-3.34)	(-3.81)
FPA	0.28^{***}	0.31^{***}	0.30^{***}	0.29^{***}	0.29^{***}
	(5.18)	(6.31)	(6.70)	(6.78)	(7.45)
Deregulated	0.03^{***}	0.04^{***}	0.04^{***}	0.04^{***}	0.04^{***}
	(4.00)	(4.45)	(4.61)	(4.36)	(5.03)
Constant	0.17^{***}	0.16^{***}	0.16^{***}	0.16^{***}	0.16^{***}
	(16.24)	(16.64)	(18.24)	(18.82)	(19.35)
Nobs	599	2,795	4,605	6,286	7,857
Adjusted R^2	0.08	0.08	0.08	0.07	0.07

t-stats in parentheses

$*p < 0.10, **p < 0.05, ***p < 0.01$

Table 6: **Triple Differences: Heterogeneous Effect by Dependence on External Financing**

This table reports the results for estimating the following linear specification:

$$Lt2A_{i,t} = \alpha + \beta \times FPA_i \times Deregulated_{i,t} + \delta_1 \times FPA_i + \delta_2 \times Deregulated_{i,t} + \eta_t + \epsilon_{i,t},$$

where Lt2A is long-term debt to assets, FPA is frequency of price adjustment, and $Deregulated_{i,t}$ *is an indicator that equals 1 if firm i is in a state that had implemented the deregulation in or before year t, and 0 otherwise.* η_t *are a full set of year fixed effects. The sample period is January 1982 to December 2014. Standard errors are clustered at the firm level. Columns (1) and (2) split the sample based on the cash-to-asset ratios and columns (3) and (4) based on the external finance gap defined as in Demirgüç-Kunt and Maksimovic (2002b). Equally-weighted probabilities of price adjustments are calculated at the firm level using the micro-data underlying the Producer Price Index constructed by the Bureau of Labor Statistics.*

	Low Cash (1)	High Cash (2)	Low External Finance Gap (3)	High External Finance Gap (4)
FPA × Deregulated	-0.18^{***}	-0.06	-0.06	-0.19^{***}
	(-4.37)	(-0.85)	(-1.10)	(-4.54)
FPA	0.26^{***}	0.14^{***}	0.29^{***}	0.31^{***}
	(7.31)	(2.71)	(7.72)	(7.58)
Deregulated	0.03^{*}	0.04^{**}	0.02	0.02
	(1.95)	(2.12)	(1.34)	(1.06)
Constant	0.18^{***}	0.08^{***}	0.08^{***}	0.16^{***}
	(17.49)	(7.59)	(6.00)	(15.77)
Year FE	X	X	X	X
Nobs	6,006	3,042	2,888	5,871
Adjusted R^2	0.08	0.08	0.13	0.09

t-stats in parentheses
$*p < 0.10, **p < 0.05, ***p < 0.01$

Table 7: **Falsification Tests: Early vs. Late Deregulating States**

This table reports the results for estimating the following linear specification:

$$
\begin{aligned}
Lt2A_{i,t} &= \alpha + \beta \times FPA_i \times After1996_{i,t} \times Early_i + \delta_1 \times FPA_i \times After1996_{i,t} \\
&+ \delta_2 \times FPA_i \times Early_i + \delta_3 \times After1996_{i,t} \times Early_i + \gamma_1 \times FPA_i \\
&+ \gamma_2 \times After1996_{i,t} + \gamma_3 \times Early_i + X'_{i,t} \times \zeta + \epsilon_{i,t}.
\end{aligned}
$$

where $Lt2A$ is long-term debt to assets, FPA is the frequency of price adjustment, and $Deregulated_{i,t}$ is an indicator that equals 1 if firm i is in a state that had implemented the deregulation in or before year t, and 0 otherwise. $After1996$ is an indicator that equals 1 in years after 1996. Early is an indicator that equals 1 for firms headquartered in states that implemented the interstate bank branching deregulation in the first wave, between 1996 and 1998. The sample period is January 1982 to December 2014. Standard errors are clustered at the firm level. In columns (1)-(3), the sample period is January 1982 to December 1999. In the first falsification test of column (4), an indicator that equals 1 for years after 1992, $After1992$, replaces $After1996$. In column (5), the sample period is January 1982 to December 1995. In column (5), it is January 1982 to December 1995 and January 2001 to December 2014. Equally-weighted probabilities of price adjustments are calculated at the firm level using the micro-data underlying the Producer Price Index constructed by the Bureau of Labor Statistics.

	All (1)	Low Cash (2)	High Cash (3)	Falsification Test 1 (4)	Falsification Test 2 (5)
FPA × After1996 × Early	−0.17**	−0.16*	0.21		−0.01
	(−2.00)	(−1.78)	(0.84)		(−0.09)
FPA × After1996	0.08	0.08	−0.23		−0.14*
	(0.99)	(0.95)	(−0.95)		(−1.89)
FPA × Early	0.01	0.00	−0.06	0.04	0.01
	(0.16)	(−0.02)	(−0.44)	(0.52)	(0.16)
After1996 × Early	0.02	0.00	0.00		0.00
	(0.88)	(0.17)	(−0.09)		(0.12)
FPA	0.28***	0.27***	0.18	0.27***	0.28***
	(4.37)	(3.83)	(1.62)	(3.81)	(4.37)
After1996	0.02	0.02	0.02		0.05**
	(0.87)	(0.89)	(0.59)		(2.40)
Early	0.00	0.03	−0.02	0.00	0.00
	(0.20)	(1.24)	(−0.86)		(0.20)
FPA × After1992 × Early				−0.12	
				(−1.39)	
FPA × After1992				0.06	
				(0.73)	
After1992 × Early				0.02	
				(0.88)	
After1992				−0.01	
				(−0.48)	
Constant	0.15***	0.16***	0.11***	0.15***	0.15***
	(7.27)	(6.96)	(7.07)	(6.76)	(7.27)
Nobs	5,376	3,796	1,580	4,110	7,549
Adjusted R^2	0.10	0.10	0.02	0.10	0.08

t-stats in parentheses
$*p < 0.10, **p < 0.05, ***p < 0.01$

-

Table 8: **Panel Regressions of Leverage on Price Flexibility with Total Volatility Decomposition**

This table reports the results for estimating the following linear equation:

$$Lt2A_{i,t} = \alpha + \beta_1 \times Predicted\ Total\ vol_{i,t} + \beta_2 \times Residual\ Total\ vol_{i,t} + X'_{i,t-1} \times \gamma + \eta_t + \eta_k + \epsilon_{i,t},$$

where Lt2A is long-term debt to assets, Predicted Total vol and Residual Total vol are the predicted part of a regression of total volatility on the frequency of price adjustment and the residual, respectively, and $X'_{i,t-1}$ is a vector of additional controls (see Table 1 for a detailed description). HP Firm-level HHI is the firm-level measure of product-space concentration based on the Hoberg & Phillips 300 industries. Fama-French 48 FE is a set of 48 dummies that capture the Fama and French 48 industries. Hoberg-Phillips 50 FE is a set of 50 dummies that capture the Hoberg and Phillips 50 industries. The sample period is January 1982 to December 2014 in columns (1). The sample is restricted to the period January 1996 to December 2014 in all other columns, due to the availability of the Hoberg-Phillips data. Standard errors are clustered at the firm level. All columns use the continuous measure of the frequency of price adjustment. Equally-weighted probabilities of price adjustments are calculated at the firm level using the micro-data underlying the Producer Price Index constructed by the Bureau of Labor Statistics.

	(1)	(2)	(3)	(4)
Predicted Total vol	-2.77^{***}	-2.44^{***}	-1.83^{***}	-1.27^{**}
	(-5.00)	(-3.59)	(-3.00)	(-2.00)
Residual Total vol	-0.02	-0.03	0.05^{**}	0.07^{**}
	(-1.26)	(-1.46)	(2.19)	(2.56)
Profitability	-0.23^{***}	-0.11	-0.20^{***}	-0.22^{***}
	(-3.15)	(-1.29)	(-2.77)	(-2.79)
Size	0.00	-0.00	-0.00	-0.00
	(1.16)	(-0.81)	(-0.93)	(-0.71)
B-M ratio	0.05^{***}	0.03^{**}	-0.01	0.00
	(5.30)	(2.53)	(-0.55)	(0.16)
Intangibility	0.11^{***}	0.10^{***}	0.14^{***}	0.10^{***}
	(3.79)	(2.90)	(3.93)	(2.96)
Price-Cost margin	-0.00	-0.06^{*}	0.04	0.04
	(-0.13)	(-1.84)	(0.98)	(1.01)
HHi	-0.03	0.05	0.07	0.00
	(-0.66)	(0.98)	(1.65)	(0.06)
HP Firm-level HHI		-0.04	0.03	0.03
		(-1.27)	(1.06)	(0.98)
Constant	1.05^{***}	1.04^{***}	0.81^{***}	0.64^{***}
	(5.64)	(4.55)	(3.98)	(2.97)
Year FE			X	X
Fama-French 48 FE			X	
Hoberg-Phillips 50 FE				X
Nobs	8,821	4,706	4,706	4,671
Adjusted R^2	0.16	0.09	0.29	0.24

t-stats in parentheses
$*p < 0.10, **p < 0.05, ***p < 0.01$

Table 9: **Panel Regressions of Leverage on Price Flexibility: additional controls**

This table reports the results for estimating the following linear equation:

$$Lt2A_{i,t} = \alpha + \beta \times FPA_i + X'_{i,t-1} \times \gamma + \eta_t + \eta_k + \epsilon_{i,t},$$

where Lt2A is long-term debt to assets, FPA is the frequency of price adjustment, and $X'_{i,t-1}$ a vector of additional controls (see Table 1 and Section VI B. for a detailed description). Fama-French 48 FE is a set of 48 dummies that capture the Fama and French 48 industries. The sample period is January 1982 to December 2014. Standard errors are clustered at the firm level. All columns use the continuous measure of the frequency of price adjustment. Equally-weighted probabilities of price adjustments are calculated at the firm level using the micro-data underlying the Producer Price Index constructed by the Bureau of Labor Statistics.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
FPA	0.09***	0.08***	0.09***	0.10***	0.09***	0.09***	0.09***	0.09***	0.10***	0.08***	0.11***
	(3.12)	(2.60)	(3.03)	(3.11)	(3.13)	(2.96)	(3.08)	(3.22)	(2.75)	(2.70)	(2.65)
Total vol	0.06***	0.06***	0.06***	0.05**	0.06***	0.05**	0.06***	0.07***	0.08***	0.07***	0.09***
	(2.84)	(2.62)	(2.81)	(2.05)	(2.59)	(2.01)	(2.87)	(3.33)	(3.59)	(2.91)	(3.48)
Profitability	−0.30***	−0.29***	−0.30***	−0.32***	−0.30***	−0.32***	−0.29***	−0.29***	−0.44***	−0.27***	−0.43***
	(−5.53)	(−5.07)	(−5.48)	(−5.57)	(−5.49)	(−5.58)	(−5.31)	(−4.99)	(−6.35)	(−5.02)	(−5.62)
Size	0.00	0.01*	0.00	0.00	0.00	0.00	0.00	−0.00	0.00	0.00	−0.00
	(0.76)	(1.66)	(0.83)	(0.87)	(1.14)	(0.79)	(0.83)	(−0.56)	(0.95)	(0.94)	(−0.29)
B-M ratio	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.02*	0.00	0.01	−0.00
	(1.56)	(1.29)	(1.51)	(0.74)	(1.62)	(0.70)	(1.37)	(1.86)	(0.41)	(1.06)	(−0.34)
Intangibility	0.14***	0.16***	0.14***	0.15***	0.15***	0.16***	0.15***	0.14***	0.15***	0.12***	0.14***
	(5.29)	(5.76)	(5.27)	(5.63)	(5.39)	(5.69)	(5.49)	(5.09)	(5.22)	(4.62)	(4.72)
Price-Cost margin	0.06**	0.04	0.06*	0.07**	0.05	0.07**	0.07**	0.06**	0.22***	0.00	0.17**
	(2.00)	(1.19)	(1.91)	(2.06)	(1.61)	(1.99)	(2.05)	(1.98)	(4.39)	(0.10)	(2.41)
HHi	0.06	0.03	0.07	0.09	0.06	0.09	0.06	0.07	0.04	0.05	0.00
	(1.36)	(0.76)	(1.41)	(1.59)	(1.34)	(1.51)	(1.31)	(1.42)	(0.74)	(1.12)	(0.07)
4F Concentration ratio		0.04									−0.00
		(0.61)									(−0.06)
Nondur			0.01								0.05
			(0.28)								(0.21)
Serv			−0.09**								−0.06
			(−2.30)								(−0.92)
Invest			0.02								−0.00
			(1.16)								(−0.18)
Giv			0.01								−0.03
			(0.54)								(−1.38)
NX			0.02								−0.00
			(0.84)								(−1.19)
Durability BKM				−0.01							−0.02
				(−0.36)							(−1.17)
FWA					1.86**						−0.48
					(2.22)						(−0.43)
Engel curve slopes						−0.08					−0.04
						(−0.88)					(−0.47)
KZ Index w/o Lev							−0.02				−0.01
							(−1.65)				(−0.56)
S&P rating								0.01***			0.02***
								(3.90)			(4.80)
Fixed Costs to Sales									−0.24***		−0.20***
									(−4.57)		(−3.25)
Operating Leverage$_{NovyMarx}$										−0.04***	−0.03***
										(−4.46)	(−2.80)
Constant	0.12***	0.10***	0.12***	0.13***	−0.17	0.19**	0.12***	0.15***	0.12***	0.18***	0.33*
	(3.55)	(2.93)	(3.22)	(3.58)	(−1.29)	(2.19)	(3.48)	(4.21)	(3.13)	(5.19)	(1.78)
Year FE	X	X	X	X	X	X	X	X	X	X	X
FF 48 FE	X	X	X	X	X	X	X	X	X	X	X
Nobs	8,821	8,093	8,821	7,999	8,648	7,999	8,819	8,486	7,585	8,749	5,906
Adjusted R^2	0.34	0.35	0.34	0.35	0.35	0.35	0.34	0.35	0.29	0.36	0.29

t-stats in parentheses
$*p < 0.10, **p < 0.05, ***p < 0.01$