The Price of Stability

Volatility and the Rise in Markups during the Great Moderation*

Friedrich Lucke[†] Giovanni Morzenti[‡] Giovanni Rizzi [§]

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Abstract

During the Great Moderation, macroeconomic volatility declined while firm markups increased. We document a causal relationship between volatility and markups due to tacit collusion. We exploit the legalisation of interstate banking as an exogenous decrease in volatility. Using an instrumental variable approach, we show that a 1% reduction in volatility causes a 19 p.p. increase in aggregate markups. The effect is due to large firms and firms operating in non-tradable industries. The changing market structure explains two-thirds of the effect, whereas reallocation only accounts for one-third. The reduction of volatility during the Great Moderation explains 31% of the markup increase between 1980 and 1997.

Keywords: Volatility, Great Moderation, Markups, Tacit Collusion

JEL codes: E32, E37, L11, L13, L16

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[†]JRC European Commission

[‡]Bocconi University

[§]Toulouse School of Economics

1 Introduction

The 1980s were a turning point for the US economy: they marked the beginning of the Great Moderation, a period of macroeconomic stability and prosperity. However, markups rose significantly during this period.[1] De Loecker et al. [2020] attribute rising markups to increasing market power. Market power harms consumer well-being, distorts economic rents, dampens labour demand and discourages innovation. Therefore, rising market power reduced the gains from the Great Moderation. Despite the substantial impact of the Great Moderation on the US economy, there is little evidence of how lower macroeconomic volatility affects markups.

In this paper, we show that the reduction of volatility had a causal impact on markups, explaining a third of their overall increase. We estimate that changes in the market structure explain two-thirds of this rise. We suggest a reduction of volatility due to the Great Moderation caused an increase in markups because it facilitated tacit collusion. [2] Volatility affects tacit collusion through two channels. Firstly, lower volatility decreases *incentives to deviate* from the collusive equilibrium. Intuitively, when firms collude using trigger strategies, incentives to deviate are strongest at business cycle peaks: the punishment phase will occur in the trough of the business cycle, which is the least profitable period.[3] Less volatile cycles imply smaller differences between peaks and troughs, thereby decreasing firms' incentives to break collusion during peaks. Secondly, lower volatility makes it easier to *monitor deviations* from the agreed upon collusive outcome. In a period of lower economic volatility, it becomes more difficult for firms to detect whether price changes are due to firm-specific shocks or a deviation from the collusive equilibrium.[4]

The main contribution of this paper is to identify the causal link between the reduction in business cycle volatility and firms' markups during the Great Moderation. We are

1 As De Loecker et al. [2020] show, average markups increased from 10% in 1980 to 60% in 2017. Other estimates point to more moderate increases of 15%, as in Farhi and Gourio [2018].

2 The finding confirms the theoretical argument that a more stable macroeconomic environment facilitates tacit collusion, as in Rotemberg and Saloner [1986] and Haltiwanger and Harrington Jr [1991].

3 See Rotemberg and Saloner [1986], Haltiwanger and Harrington Jr [1991] and Ivaldi et al. [2003].

4 See Green and Porter [1982] and Abreu et al. [1990].

the first to provide causal empirical evidence of a link between volatility and markups through the channel of tacit collusion. To do that, we exploit the legalisation of interstate banking in the US as a source of exogenous variation of state-sector business cycle volatility. Between 1978 and 1995, all 50 states and DC legalised interstate banking. This reform reduced volatility substantially, as shown in Correa and Suarez [2007]. We study the effect of volatility on markups using the legalisation of interstate banking as an instrument for volatility.

We use data on state-sector output from the Bureau of Economic Analysis to build volatility measures. This data covers 68 SIC sectors in 50 states and DC from 1963 to 1997. The disaggregation by state and sector offers sufficient variation to estimate the effects of volatility on markups. Moreover, we compute a different firm-level volatility measure using the methodology of Correa and Suarez [2007]. We use Compustat firm-level data on all publicly traded US companies to estimate markups, following the approach of De Ridder et al. [2022].

We find that a 1% reduction in volatility causes a 16-21 percentage point (p.p.) increase in aggregate markups. Firms in the top decile by market share drive this effect. The finding is consistent with tacit collusion being more prevalent between large firms. The impact of medium-term volatility is stronger than that of short-term volatility. A back-of-the-envelope calculation shows that declining volatility during the Great Moderation explains 31% of the increase in markups observed between 1980 and 1997.

Comparing the estimates of effects on sales- and input cost-weighted markups, we infer the fraction of the markup increase due to reallocation.[5] Reallocation explains away only one-third of the effect of volatility on markups. Furthermore, we consider separately tradable and non-tradable industries. We notice that the reallocation channel is active only in tradable industries. In non-tradables, the whole effect was due to changes in the market structure. Absent technological changes, this finding is consistent with an increase in tacit collusion, which is easier in non-tradables, as firms operate in geographically segmented markets. Tacit collusion in tradable industries requires that firms coordinate across larger markets and is, therefore, more dependent on firm size.

5 We follow the methodology suggested by Grassi [2017] and De Loecker et al. [2020].

Overall, this paper investigates the consequences of the Great Moderation on market structure and its policy implications. We show that there is a price to stability in the form of greater market power, which results from increased tacit collusion. The lack of competition can offset part of the gains from macroeconomic stabilisation, resulting in lower economic welfare. Therefore, policymakers should coordinate stabilisation and competition policy to reap the benefits of a dynamic economy.

Related Literature

This work contributes to the burgeoning strand of IO-to-macro literature on the rise in markups and its impact on the economy. De Loecker et al. [2020] establishes that markups rose from 10% in 1980 to 60% in 2017.[6] There are three main explanations for this rise: reallocation of market shares to higher-markup firms, technological innovations and changes in the market structure. Our work contributes to this literature by providing evidence of the importance of the market structure channel. We show that the lower volatility implied by the Great Moderation facilitated tacit collusion, thereby increasing markups and market power.[7] The finding explains why markups started to rise in the 1980s. On the other hand, De Ridder [2019] argues that technological innovations increased the share of fixed costs of firms due to investments into intangible assets. Autor et al. [2020] maintains that the rise in markups is due to the reallocation of market shares to "superstar" firms, a trend that benefits consumers and society.

Moreover, our paper fits into the empirical macro-IO literature that studies the dynamics of markups over the business cycle and analyses the macroeconomic implications of tacit collusion. We contribute to this literature by showing that the second moment of the business cycle, the variance, drives collusive behaviour. Afrouzi Khosroshahi [2016]

6 The authors attribute the bulk of the increase in markups to the reallocation of market shares to more productive firms, with entering firms having higher markups than firms exiting the market.

7 In line with this, De Loecker et al. [2021] and Gutiérrez et al. [2021] argue that higher market power resulting from higher barriers to entry explains the decline of business dynamism in the US. Higher market power favours incumbents and prevents firm entry, causing significant welfare losses.

solve a DSGE model with tacit collusion and derive the law of motion of markups to show markup dynamics are consistent with tacit collusion.[8] Moreau and Panon [2022] show that collusion is widespread and that the most productive firms are those more likely to collude.[9]

Furthermore, our analysis validates the theoretical industrial organisation models that find a negative relationship between volatility and tacit collusion. These models predict that lower volatility facilitates collusion irrespective of whether they feature pro- or counter-cyclical markups. Moreau and Panon [2022] show that narrower fluctuations in demand make collusion easier. Low profits from deviation when demand is high are not large enough to offset the low opportunity cost of colluding when demand is low. Bó [2007] shows that a less volatile discount factor increases collusive profits and prices.

This paper is structured as follows: In section 2, we describe how we construct our database and discuss the methodology by which we measure markups and volatility. In section 3, we discuss the identification strategy and estimation. Section 4 shows our results and section 5 concludes.

2 Data

This section describes the data used for the analysis. We use firm-level to estimate the production function of firms and identify markups. We use state-sector GDP data to construct measures of volatility. We use the dates of interstate banking legalisation as a policy shock that causes an exogenous variation in volatility. In the following paragraphs, we describe our methodology for estimating markups and volatility.

8 They test two alternative models explaining the relationship between the business cycle and markups: a model which relates markup cyclicality to changes in incentives to collude, and a costumer-base model in the spirit of Phelps and Winter [1970].

9 They develop a DSGE model with oligopoly and collusive firms and estimate it using data from the French competition authority.

Markup Estimation: we estimate markups at the firm level, defined as the ratio of price and marginal cost. We follow the control function approach of De Loecker and Warzynski [2012] and Ackerberg et al. [2015], commonly known as production function estimation. De Loecker et al. [2020] popularises this methodology and uses it to document the rise in US markups. Appendix A reports details on the data and methodology used to compute markups. The researcher has to make explicit assumptions about firms' production functions. We assume the production functions are Cobb-Douglas, following De Loecker et al. [2020]. We estimate the production function for firm i in sector s at time t under the assumption of constant sector-level elasticity θ_s^v of output to variable input v.[10] Building on the Hall formula, Equation 1 shows that we can compute markups as the product of this elasticity and the ratio of output to variable input.[11] This formula follows directly from the first-order condition of firms' cost minimisation:

$$\mu_{it} = \theta_s^v \times \frac{\text{Output}_{it}}{\text{Variable Input}_{it}}. \tag{1}$$

We apply this methodology to Compustat data, which covers publicly traded firms starting in 1950.[12] Although only a minority of firms operating in the US are publicly traded, they account for 41% of private sales and 29% of private employment. Moreover, collusion tends to happen between large firms, the majority of which are publicly traded.

This methodology is prone to two biases studied in detail by De Ridder et al. [2022]. Firstly, we do not have information on prices or quantities. Therefore, we need to use firm revenue to measure output and hence demand elasticity biases the estimated output elasticity θ_s^v. However, this is an average-level bias, so we can still perform a regression analysis to derive meaningful results. Secondly, the control function approach proposed by Ackerberg et al. [2015] assumes perfect competition. Relaxing this assumption can introduce a bias in our markup estimates. De Ridder et al. [2022] show that a proper first stage can account for this bias, which is small in practical applications.

10 We consider 2-digit NAICS sectors.

11 See the Appendix A and Hall [1986] and Hall [1988] for more details.

12 This is the dataset used by De Loecker et al. [2020] and Bao et al. [2022] among others. It allows us to cover many firms and industries over a long horizon, providing reliable balance sheet data.

Volatility Estimation: there is no consensus on how to measure volatility. Therefore, we construct several measures and discuss their suitability. We face the challenge of choosing a measure of volatility compatible with our identification strategy and the macroeconomic and industrial organisation aspects of our analysis. The measure of volatility must reflect both the Great Moderation and the decision-making of colluding firms.

We solve this conundrum by computing volatility using Bureau of Economic Analysis (BEA) data on output at the state-sector level for 50 states and DC and 68 SIC industries from 1963 to 1997. The approach has three advantages. Since it is easy to aggregate volatility over states and sectors, it reflects the stylised facts that the macroeconomic literature has established on the Great Moderation. At the same time, the volatility's state-sector specificity makes it relevant to the decision-making of each firm, which reflects the volatility in its own market. Finally, it is granular enough to use US states' staggered legalisation of interstate banking as an identification strategy.

To construct the volatility measures, we merge the firm-level and state-sector data. We convert the sector classifications of the firm-level data from 6-digit NAICS to 2-digit SIC and use the location of firms' headquarters to match firms to state-sectors. The reduction of specificity from 6-digit to 2-digit categories reduces the ambiguity of which sector a firm operates in.[13] To distinguish between tradable and non-tradable industries, we use the classification of Barkai and Karger [2020].[14]

The crudest measure of volatility is the variance of output growth over fixed time windows. We refer to this measure as fixed-window-volatility. We construct fixed-window-volatility for non-overlapping windows of five years each between 1968 and 1997. However, this measure has shortcomings. It reduces the variation in the data and unrealis-

13 After 1997, the BEA classifies industries according to NAICS categories. Since our instrument does not provide identification after 1997 (all states have legalised interstate banking until then), we restrict our attention to the SIC-categorised state-sector business cycles.

14 Their algorithm uses the geographical proximity of firms' establishments to customer bases, designating those which produce close to their customers as non-tradables. They perform this exercise at the 6-digit NAICS level, which we can match to the firms in our dataset. The only sources of error are firms that switch between tradable and non-tradable industries over time.

tically assumes that firms anticipate the volatility changes at the start of each five-year window but are myopic across windows. We construct a forward-looking measure that reflects the firms' decision-making to address these issues.

To capture the intertemporal incentives to deviate highlighted in Rotemberg and Saloner [1986], we are interested in a measure that captures the uncertainty firms face when forecasting the evolution of the market in each year, taking into account the business cycle and troughs that potentially last longer than one period. We construct the measure as follows: first, we fit a time-series model for each state-sector to describe its business cycle. We estimate a VAR model taking as endogenous variables state-sector output growth, aggregate state output growth and nationwide sector output growth. We add the legalisation of interstate banking as an exogenous variable. Secondly, we forecast the evolution of GDP in each state-sector, starting in 1968. We draw at random from the confidence intervals of the estimated coefficients of the VAR model and compute forecasts of GDP for horizons of one to eight years for each state-sector. We repeat this exercise 200 times, obtaining 200 forecasts of the evolution of GDP in each state-sector. Thirdly, we calculate the variance of the forecasts in each year, state-sector and horizon. Therefore, we obtain eight variances that proxy for the short- to medium-term volatility firms expect in their sector starting in a given year. We refer to these measures as forecast-volatilities. In summary, we use the uncertainty of firms' forecasts on their future sales at different horizons as a proxy for volatility.[15]

To capture the dispersion effect on monitoring in Green and Porter [1982], we calculate an instantaneous measure of volatility that follows Correa and Suarez [2007]. We refer to this measure as auxiliary-volatility as this approach relies on an auxiliary regression to estimate a firm-specific measure of the business cycle volatility firms face. We run the regression for firm i in state j in period t

$$y_{ijt} = \alpha_i + \beta_t + \gamma T_{jt} + \delta_s \ln(S_{it-1} + 1) + \delta_c \ln(C_{it-1} + 1) + v_{ijt}, \tag{2}$$

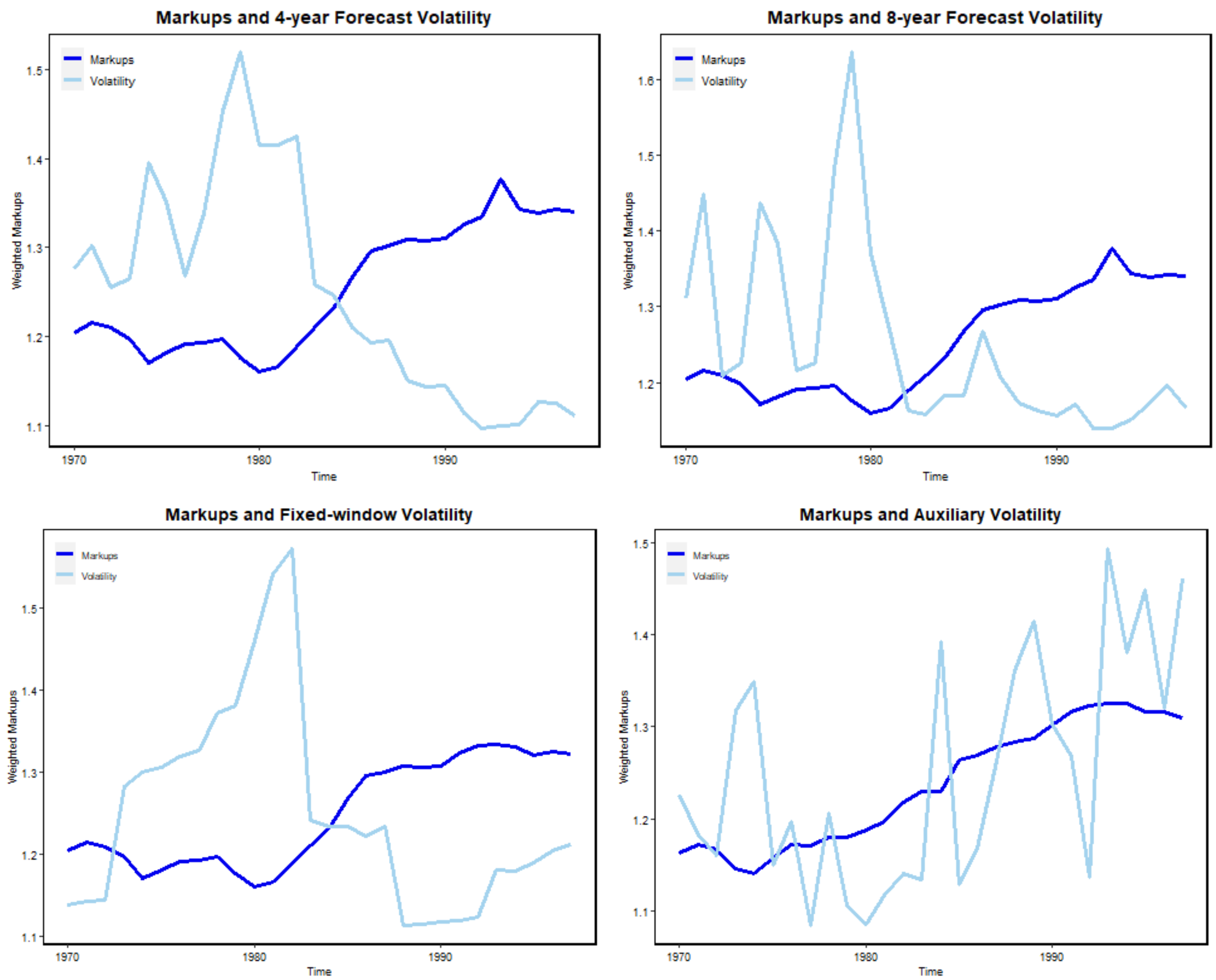
15 This approach also allows us to study the dynamic effects of volatility that firms expect at different horizons. Firms of different sizes and subject to different levels of price stickiness will forecast at different time horizons. This will affect how they respond to reductions in forecast-volatility of different horizons.

where y_{it} is the growth of sales, α_i and β_t capture firm and year fixed effects, T_{jt} is a dummy for the legalisation of interstate banking in state j, S_{it-1} are the lagged firm sales and C_{it-1} is the lagged cost of goods sold. We estimate sales volatility as $|v_{ijt}|$ and we use this measure as the dependent variable of our first stage IV, regressing it on T_{jt}.

To achieve comparability between the effects of volatility on markups, we use logs of our volatility measures, so we interpret the coefficients as semi-elasticities. We stress that only the forecast-volatility is a purely forward-looking measure. Auxiliary-volatility is a measure of instantaneous volatility that captures the difference between firm-level sales and expected sales based on past realisations of shocks. The fixed-window-volatility is forward-looking at the beginning of each new window, partially forward-looking in the middle, and backward-looking at the end. We use forecast-volatility for the benchmark analysis, as it relevant to the intertemporal aspect of colluding firms' strategies.

The graphs below compare four different measures of volatility: Figure 1 plots markups and weighted average volatilities for forecast-, fixed-window- and auxiliary-volatility. The Great Moderation is visible in both the forecast- and the fixed-window- volatility but not in the auxiliary-volatility. This finding is not surprising since the dispersion of market shares increased, as highlighted in Edmond et al. [2018]. More importantly, we see a negative relationship between markups and volatility. In the plots of the forecast-volatilities, we also see that short-lived spikes in volatility during the 1970s coincide with markup reductions. The probable causes of these spikes are oil crises. Regressing directly volatility and markups suffers from reverse causality since an increase in markups will reduce firms' pass-throughs of shocks, thereby reducing volatility. Therefore, we use the dates of the staggered legalisation of interstate banking as an exogenous variation to identify our effect, as detailed in the next section.

Figure 1: Evolution of Aggregate Markups and Volatility



This figure shows the evolution of aggregate markups (weighted by firm sales) and aggregate volatility (weighted by state-sector share in GDP). The x-axis shows the year and the y-axis measures markups and volatilities. We scale the measures of volatility to fit the graph. Their absolute quantities do not reflect percentages of the maximum volatility and are void of meaning.

3 Identification & Estimation

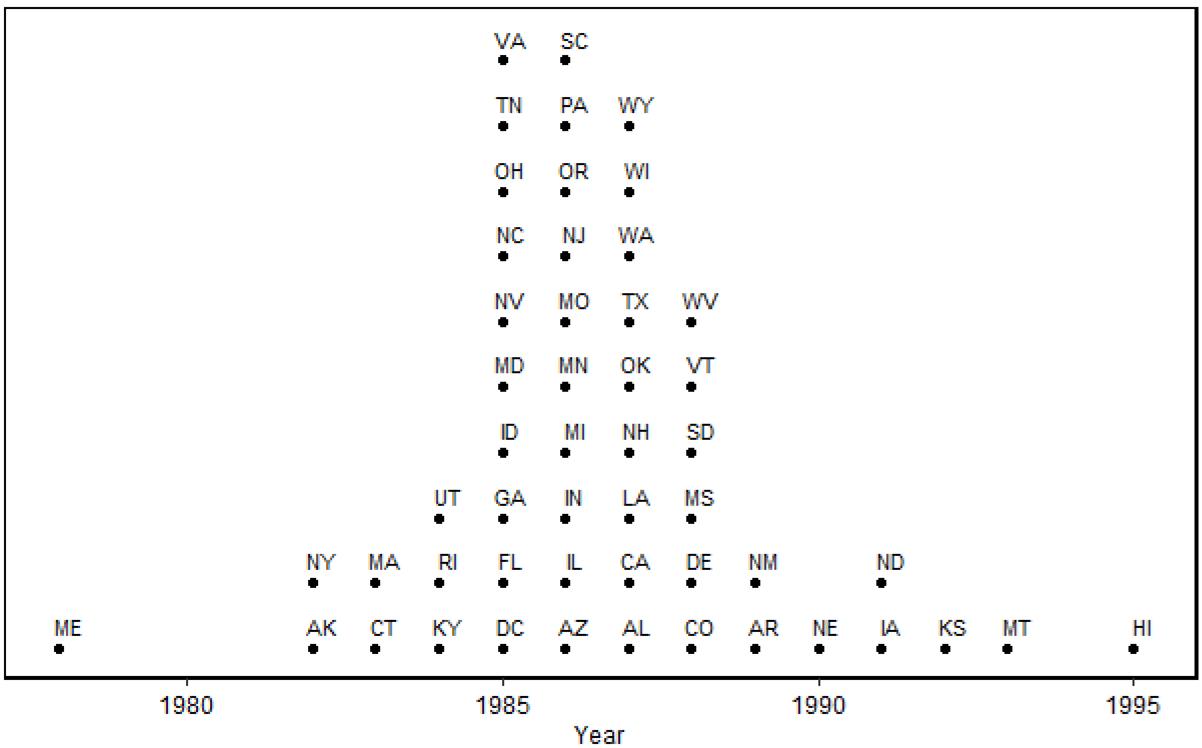
This section describes the identification strategy and discusses the underlying assumptions and possible sources of biased estimates of the impact of volatility on markups. Our estimation strategy exploits regional and sectoral variation in volatility and markups. We use the staggered legalisation of interstate banking at the beginning of the 1980s as a quasi-natural experiment to establish a causal link between volatility and markups. The Douglas Amendment to the Bank Holding Company Act of 1956 prohibited Bank Holding Companies from establishing or purchasing bank subsidiaries across state borders unless the state of the target bank authorised the transaction. From 1978 to 1995, a wave of deregulation led 50 states and DC to deregulate their banking markets by allowing interstate entry. Figure 2 shows when each state legalised interstate banking.

Morgan et al. [2004b] and Holmstrom and Tirole [1997] argue that interstate banking integration can either increase or decrease volatility. If volatility is due to credit supply shocks, interstate banking integration increases risk sharing and reduces volatility. If volatility is due to collateral shocks, interstate banking integration exacerbates collateral runs and increases volatility. Collateral shocks will be a bigger determinant of volatility relative to credit supply shocks in state-sectors with smaller firms. Therefore we expect the first-stage coefficients to become more negative as we move from lower to higher deciles.

Our analysis builds on Correa and Suarez [2007], which shows that the states that legalised interstate banking experienced lower levels of volatility thanks to better risk-sharing. In our main exercise, we use this regulatory change as an instrument for volatility in a two-stage least squares estimation. While a staggered difference-in-difference approach is also possible (and is run as a robustness check), we prefer the instrumental variable strategy, as it allows us to pin down the mediating role of volatility in the regression, albeit at the cost of stronger assumptions.

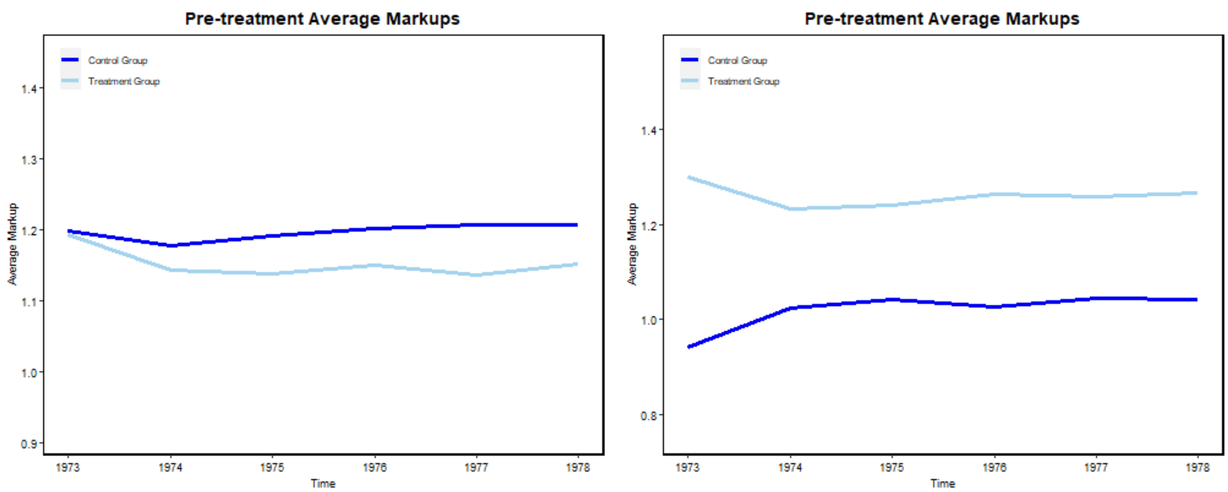
Our benchmark regression is a two-stage least squares model, in which we regress volatility on bank integration in the first stage. In the second stage, we regress markups on the instrumented volatility, using firm sales as weights. We use time- and firm-fixed

Figure 2: Interstate Banking Legalisation in US states



This figure shows when the US states and DC allowed out-of-state banks to operate in their state. The x-axis tracks time. The y-axis counts the number of states that legalised interstate banking each year.

Figure 3: Parallel Trends Exercise



This figure shows two exemplary illustrations of the parallel trends verification. The left figure assigns all states treated before 1984 to the treatment group and all others to the control group. The right figure assigns all states treated before 1987 to the treatment group and all others to the control group. The figure then shows the trends of markups before the treatment of the first state. The x-axis denotes the time and the y-axis denotes the average sales-weighted markup.

effects to remove the between-variation. Hence, only the time/firm interaction drives the results. Every regression on the entire sample shows the instrument is relevant.

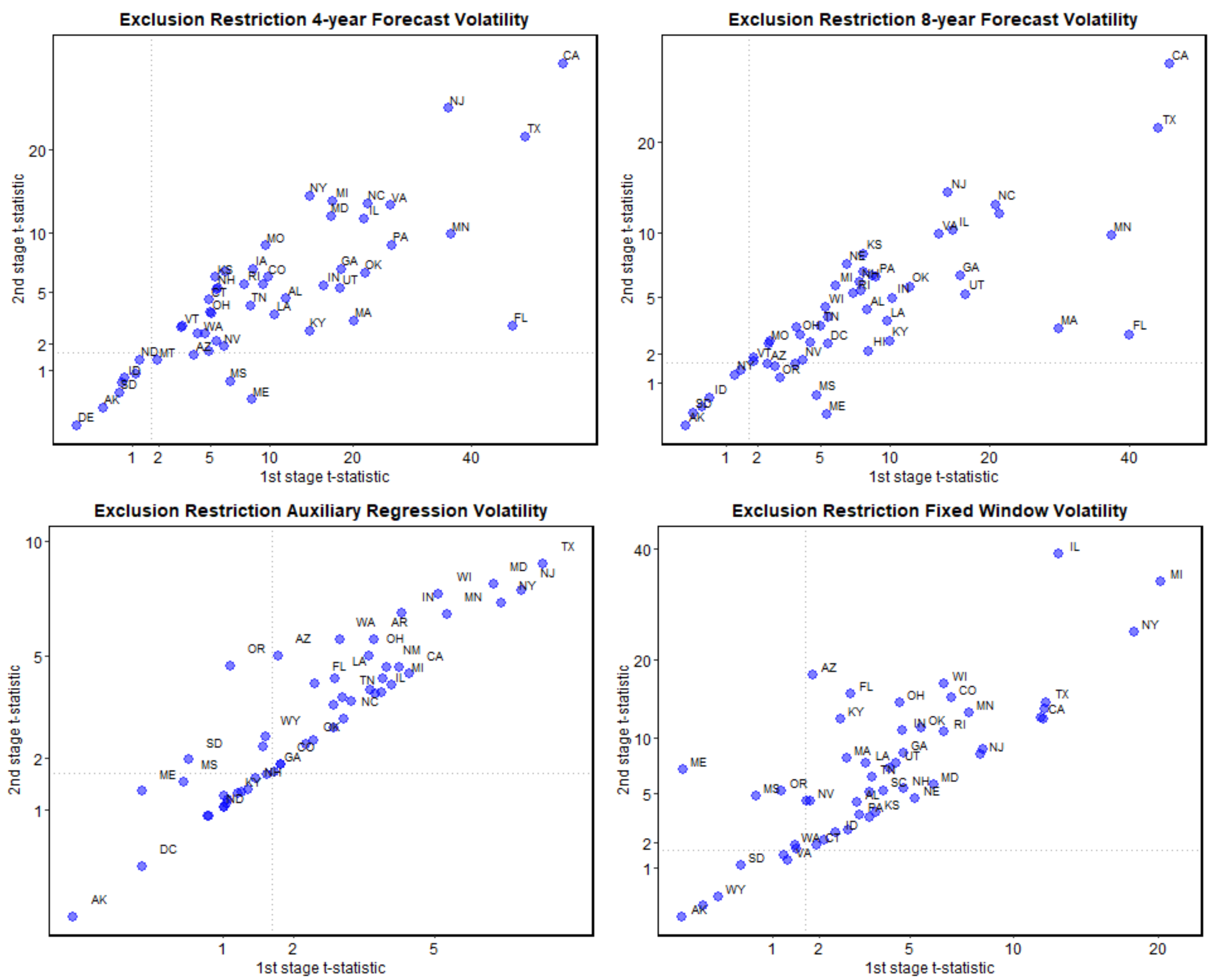
Parallel Trends: a prerequisite for identification is the assumption that the trends of markups of control and treatment groups were parallel before the treatment. The staggered legalisation of interstate banking makes it hard to illustrate parallel trends, as states effectively switch from the control to the treatment group in different periods. We perform a different robustness check that shows the parallel trends are not violated. We assign firms treated before a cutoff date to the treatment group and firms treated after to the control group. We then compare the trends in markups before 1979, the date of the first treatment. We do this for different cutoff dates and plot the results in Figure 3. We start the comparison after the first oil price shock.

We also run a formal test for parallel trends, regressing volatility on treatment, a lag and a lead. The regression shows that the lead is insignificant, which is strong evidence against treatment anticipation and confirms the parallel trends assumptions. The treatment is significant and the lag is insignificant. Interestingly the treatment coefficient is positive, indicating that the immediate impact of the reform on volatility is positive, and the insignificant coefficient of the lagged treatment is negative. The cumulative impact is negative. This dynamic suggests a humped-shape response: the immediate impact of the reform is an increase in volatility since it is a regime change; however, in longer horizons, volatility decreases as expectations increasingly reflect the less volatility market conditions.

Exclusion Restriction: we need to ensure that the instrument only affects markups through the instrumented variable, volatility. We perform a falsification test to verify if this condition holds by running the regressions state by state. We find a positive correlation between the effect of the instrument on volatility and the effect of volatility on markups. We illustrate this finding in Figure 4, where we plot the significance of the first stage and the second stage. In almost all cases in which the first stage is insignificant the second stage is also insignificant, meaning few states are in the second quadrant. When we use the benchmark four-year forecast-volatility, there are eight states where the first-stage coefficient is insignificant. In all these states the second-stage coefficient is also insignificant. This finding is evidence that the exclusion restriction holds because the absence of an effect of the instrument on volatility eliminates the effect of volatility on markups.[16] With minor exceptions this holds for the other volatility measures.

16 The eight states are: North Dakota, Idaho, Oregon, South Dakota, Montana, West Virginia, Arkansas and Delaware.

Figure 4: Exclusion Restriction



This figure plots the significance of the first stage regression (x-axis) against the significance of the second stage regression (y-axis). The dotted grey lines mark the 5% confidence level, and significance increases to the right (first-stage) and upwards (second-stage). Hence, all states in the upper right quadrant have significant effects in both stages. The higher the correlation between the two t-statistics, the stronger the evidence that the exclusion restriction holds.

4 Results

This section describes the results of the empirical analysis. We show that a decrease in volatility leads to a significant increase in markups and document the robustness of this result. Table 1 contains an overview of our main results. We then break down the results, running decile and sectoral regressions, to understand the channel through which volatility affects markups. We compare the effect in tradable and non-tradable industries, using sales-weighted and input cost-weighted markups to disentangle the mediation of reallocation and market structure. Finally, we do a back-of-the-envelope calculation suggesting that the decreasing volatility explains 35% of the rise in markups between 1980 and 1997.

4.1 Benchmark Regressions

In Table 2, we show our benchmark regressions of markups on forecast-volatility for horizons from one to eight years. In the first stage, the legalisation of interstate banking markets decreased volatility by 13-17%. These estimates are significant by at least 5% in all cases. The impact of the reform was stronger on short-term horizons. Weak instrument and Wu-Hausman tests show the instrument is strong. In the second stage, a 1% reduction in forecast-volatility increases markups by 17-21 p.p. The effect is stronger for longer horizons of volatility.[17] Except for the one-year forecast-volatility, all estimates are significant. Incidentally, the reform increased markups by 0.9-3.5 p.p overall.

We run three robustness checks. Firstly, in Tables 11-12, we split the US into regions and rerun the two-stage least squares regressions, showing the benchmark results hold almost everywhere.[18] Secondly, in Table 6, we use the fixed-window-volatility as the instrumented variable. The results are coherent with the benchmark, with a 1% decrease in volatility increasing markups by 16 p.p. Thirdly, Table 5 features auxiliary-volatility capturing sales' growth dispersion as the instrumented variable. The results are coherent with the benchmark as a 1% decrease in volatility increases markups by 23 p.p. All three

17 While we have not explored this result, it could reflect the fact that firms with stickier prices are both more forward-looking and more likely to collude, given that sticky prices are a form of commitment.

18 The exception is the Midwest due to Michigan. Automotive is Michigan's most important sector and witnessed a decline in markups in the 1980s due to the market entry of aggressive Japanese competitors. When we drop the automotive from the Midwest sample, the effect of volatility on markups turns negative. Furthermore, aggregate coefficients increase, with 1% decreases in volatility resulting in a 34 – 37 p.p. increase in markups.

Table 1: Summary of Results

	Dependent variable:					
	Markups					
Volatility Measure	4-year forecast	fixed-window	auxiliary	4-year forecast	fixed-window	auxiliary
Weights	sales	sales	sales	cogs	cogs	cogs
Volatility	-0.189^{**}	-0.166^{*}	-0.227^{***}	-0.194^{**}	-0.243^{**}	-0.144
	(0.083)	(0.087)	(0.083)	(0.091)	(0.102)	(0.110)
Constant	-0.005^{**}	-0.009	0.001	-0.012^{***}	-0.02^{***}	-0.006^{*}
	(0.002)	(0.006)	(0.003)	(0.003)	(0.004)	(0.004)
Weak Instruments	8871.3	1323.13	545.1	6058.59	365.26	668.86
Wu-Hausman	788.58	2.33	1498.1	1217.54	2.33	882.44
Bank Integration	-0.457^{***}	-0.170^{***}	-0.127^{***}	-0.415^{***}	-0.093^{***}	-0.142^{***}
	(0.039)	(0.028)	(0.038)	(0.035)	(0.020)	(0.024)
Constant	0.218^{***}	0.018	0.059^{***}	0.197^{***}	-0.018^{*}	0.049^{***}
	(0.016)	(0.015)	(0.014)	(0.012)	(0.009)	(0.012)
Observations	135,922	135,805	135,922	134,681	134,564	135,922
F-Statistic	$53,343.630^{***}$	$1,323.127^{***}$	56.020^{***}	$49,474.860^{***}$	365.259^{***}	90.08^{***}

This table shows the results of the main regression specification, estimated with different measures of volatilities and different weights. The first section of the Table specifies volatility measure and weights. The second and third section show the estimates of the second stage regression and its diagnostics. The fourth and fifth section show the results and diagnostics of the first stage of the 2SLS model.

robustness checks confirm that volatility negatively affects markups. This result is not driven by our choice of volatility measure nor by a subset of the market.

From the point of view of the industrial organisation literature, this result is surprising: there is a broad consensus that in a competitive setting, markups should *increase* in times of higher uncertainty. De Loecker et al. [2020] suggest competition is higher during recessions. Volatility should increase markups for two reasons: firstly, the selection effect (extensive margin) leads to an increase in the average productivity level of surviving firms and to a higher markup for every level of productivity; secondly, the price-cost markup increases on the intensive margin as well because demand is lower and more firms compete to maintain their market share. These factors should push in the opposite direction of our results, pointing to a countervailing force which makes lower volatility increase markups; we suggest that this force is tacit collusion. In the following, we conduct three empirical exercises to uphold this argument. Firstly, we run decile regressions that show large firms drive the negative impact of volatility on markups. Large firms have higher organisational know-how and can agree better on prices. Further, larger firms price less responsively to shocks, facilitating monitoring collusion. Finally, larger firms have higher returns to scale and fixed costs, which act as a commitment device.

Secondly, we disaggregate the effect by sector and we find the strongest effects are in finance, manufacturing (excluding automotive) and trade. Importantly, we also estimate separate effects for tradable and non-tradable industries, showing that the effect is more prevalent in non-tradables, in which collusion is easier due to geographically constrained markets. Both the decile regressions and the split into tradable and non-tradable industries point towards tacit collusion as the channel of the impact of volatility on markups.

Thirdly, we compare the coefficients of regressions on sales-weighted and input cost-weighted markups. The gap between the sales- and the cost-weighted coefficients suggests that volatility caused a moderate reallocation of market shares from low- to high-markup firms. However, the reduction in volatility substantially increased also cost-weighted markups. We argue that this is not due to technology, which the legalisation of interstate banking should not have affected. Since reallocation only accounts for one-third of the effect, the only explanation for the remaining two-thirds is a reduction in competition. Furthermore, reallocation fully explains the rise in markups caused by increasing auxiliary-volatility. Therefore volatility affects collusion through the intertemporal channel highlighted in Ivaldi et al. [2003] but not through the monitoring channel of Green and Porter [1982].

4.2 Disaggregation of the Effect

4.2.1 Decile Regressions

Disaggregating the regressions by decile of sales shows that the bulk of the negative effect of volatility on markups is due to firms with the largest market shares, whereas volatility increased the markups of firms in the lower deciles of sales distribution.

In the first stage, we see that the legalisation of interstate banking increased the volatility for firms in lower deciles and decreased volatility for firms in higher ones. For firms from the first to sixth decile, the reform increased volatility by 3-13%. For firms in the seventh decile, it did not significantly affect volatility. For firms from the eighth to the tenth decile, it decreased volatility by 5-16%. Weak instrument and Wu-Hausman tests show the instrument is strong. The ambiguous effect of interstate banking legalisation on volatility is consistent with Morgan et al. [2004a], which argues it mitigates volatility caused by credit supply shocks but exacerbates volatility caused by collateral shock.

In the second stage, we see that volatility affected firms differently. For firms in deciles 1-2 and 5-9, volatility did not significantly affect markups. For firms in the third and fourth decile, a 1% increase in volatility increased markups by 7-10 p.p. For firms in the tenth decile, a 1% decrease in volatility increased markups by 22 p.p.

Interestingly, the reform increased markups of both small and large firms, but for different reasons. For firms in the third and fourth decile, the positive effect of the reform on volatility and of volatility on markups increased the latter by 0.6-1 p.p. For firms in the tenth decile, the negative impact of the reform on volatility and of volatility on markups increased the latter by 3.5 p.p.

4.2.2 Sectoral Regressions

Disaggregating regressions by sector shows that finance, non-automotive manufacturing and trade drive the negative impact of volatility on markups. In the first stage, we see that the legalisation of interstate banking affects sectors differently. The reform decreased the volatility in all sectors except construction and services, for which it did not affect volatility.[19] The effect was strongest in trade, which witnessed a 25% reduction in volatility due to the reform. In the second stage, we see that a 1% lower volatility increased markups of firms in finance (94 p.p.), non-automotive manufacturing (53 p.p.) and trade (26 p.p.); by contrast, it decreased markups for firms in mining (30 p.p.). The reform differently affected firms in different sectors, increasing markups of finance (+13 p.p.), non-automotive manufacturing (+6.8 p.p.) and trade (+6.6 p.p.) and reducing markups of mining (-3.3 p.p.).

The insignificance of the second stage in the sectors in which the first stage was insignificant (construction and services) supports the exclusion restriction. The second-stage results show that the bulk of the impact of volatility on markups is due to finance and lesser so to non-automotive manufacturing. In the 80s, finance was roughly 20% of US GDP, with non-automotive manufacturing around 16%. The difference in the second-stage effects of manufacturing with and without automotive again reflects the entry of aggressive Japanese competitors in the US automotive market.

The sectoral disaggregation sheds light on the channel through which volatility affects markup, guiding policymakers on which sectors need the closest monitoring against collusion. It does not point towards a specific channel between markups and volatility. The following section divides firms into tradable and non-tradable industries. These two classifications capture radically different market structures. The differential impact of volatility on markups between these two sets of industries points to market structure as a significant channel of the effect.

19 The finding is due to construction firms typically financing themselves through collateral on the real estate they are building. Similarly, non-financial service firms are typically small and reliant on collateral. See the previous section on how collateral financing changes the impact of the reform on volatility.

4.2.3 Tradable Versus Non-tradable Industries

Firms in non-tradable industries operate in geographically confined markets and only compete with firms of the same industry and location. Hence, if tacit collusion mediates the effect of volatility on markups, we expect to find much stronger negative effects for firms in non-tradable industries. In Tables 16-21, we compare the decile regressions for sales-weighted markups of tradables and non-tradables.

In the first stage, we see that interstate banking integration affected tradable and non-tradable firms almost identically, with results similar to the decile regression in Table 9. Hence, any difference in the overall effect must come from the relationship between volatility and markups. In the second stage, volatility affected tradable and non-tradable firms differently. We notice that lower volatility decreased markups for tradable firms and increased them for non-tradable firms of all deciles. Specifically, for tradable firms from the first to the fifth decile, a 1% reduction in volatility reduced markups by 13-35 p.p. For non-tradable firms in the same deciles, a 1% reduction in volatility increased markups by 2.4-7 p.p. For firms from the sixth to the ninth decile, volatility did not affect markups. For firms in the tenth decile, a 1% reduction in volatility reduced markups of tradable firms by 35 p.p. and increased markups of non-tradable firms by 46 p.p.

The reform affected tradable firms of different sizes oppositely than it did non-tradable firms. For firms from the first to the fifth decile, it increased markups of tradable firms by 1.2-2.3 p.p. and decreased markups of non-tradable firms by 1-2.4 p.p. The reform did not affect firms from the sixth to the ninth decile. Considering firms in the tenth decile, the reform decreased markups of tradable firms by 5.8. p.p. and increased markups of non-tradable firms by 9 p.p.

Collusion in tradable industries requires broader geographic coordination and organisational capabilities. Therefore, only large firms can collude. The geographical constraints of markets reduce the coordination problem enough that firms of all sizes can collude. The fact that the effect is negative in non-tradable industries, even for the lower deciles, and positive for the higher deciles of tradable industries is evidence in favour of the tacit collusion story. Having established that market structure is a relevant channel, we now quantify its impact by measuring the other main channel through which volatility affects markups, namely reallocation.

4.2.4 Sales Versus Cost Weights

Following Grassi [2017], Edmond et al. [2018] and De Loecker et al. [2020], we study the difference in the estimated effects when using sales-weighted markups and input cost-weighted markups. The reallocation of market shares to more productive, higher-markup firms increases sales-weighted markups, leaving cost-weighted markups unaffected. This way, we can disentangle markup increases due to reallocation and changes in the market structure. As before, we compare the effects for each decile to understand the aggregate effects. Table 16-21 compare by decile the effect of volatility on sales-weighted and cost-weighted markups for firms in tradable and non-tradable industries.

In the regressions on the entire sample, we see that when we use cost weights, coefficients are less negative than we use sales weights by roughly one-fourth. Consequently, the reallocation of market share to firms with higher markups, following the increase in volatility, explains between a third and a quarter of the effect of volatility on markups increase in markups. In the auxiliary regression, the effect disappears when we take cost-aggregate markups. This finding suggests dispersion of firm growth rates affects markups entirely through reallocation. Therefore, volatility affects collusion only through the intertemporal considerations highlighted in Ivaldi et al. [2003] and not through the monitoring concerns suggested by Green and Porter [1982].

In the decile regression comparing tradables and non-tradables, we see that changing from sales to cost weights decreased coefficients from the first to the fifth decile and increased coefficients in the tenth decile. This widening wedge reflects the differential impact of reallocation, which increases the markups of large firms and decreases that of small firms. Besides this, changing the weights did not significantly affect the coefficient on non-tradables and the effects remained significant. At the same time, changing the weights flips the coefficient on tradable firms in the tenth decile from positive to negative, with a difference of 84 p.p. This large wedge suggests that the positive relationship between volatility and markups in the sales-weighted regressions for tradables is entirely due to reallocation. Incidentally, the reform increased cost-weighted markups of tradable firms in the tenth decile by 8.5 p.p. Reallocation of market shares from small to large tradable firms more than offset this increase, reducing markups of 17.5 p.p. Excluding reallocation effects, lower volatility increases markups also for tradable firms in the tenth decile.

In contrast, the insignificant changes in coefficients of non-tradable industries imply that reallocation explains only a fraction of the impact of volatility on markups. Absent technological change, the reduction of competition due to an increase in collusion explains the effect of volatility on markups.

4.3 The Contribution of Volatility to the Markup Increase

We do a back-of-the-envelope calculation to see what fraction of the overall markup increase during the Great Moderation is caused by lower volatility. We multiply the effect of volatility on markups by the overall reduction in volatility between 1980 and 1997. This computation gives a crude estimate of how much markups would have increased only because of the reduction in volatility, excluding non-linearities. We find that volatility explains 28%-38% of the overall markup increase over the period. The effect was stronger in non-tradable industries, for which the reduction in volatility explains 46-66% of the markup increase. The magnitude of this effect is notable, given the countervailing forces we have outlined.

5 Conclusion

In this paper, we documented a causal relationship between volatility and markups. We showed that this relationship is robust and explains a large fraction of the markup increase between 1980 and 1997. We disaggregated the effects by firm-size and across industries. We used our results to argue that tacit collusion is the main channel through which volatility negatively affects markups. The fact that lower volatility has a stronger impact on the markups of large firms and firms in non-tradable industries supports this finding. The discovery of the volatility/markup relation entails significant policy considerations. For competition policy it suggests that vigilance should increase during periods of low volatility. Concerning monetary and fiscal stabilisation policy, it documents a trade-off between stability and competition. Therefore, this paper suggests that policy-makers should harmonise competition and macroeconomic policy.

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A Markup Estimation

De Loecker and Warzynski [2012] estimate markups at the firm level using the financial data and the cost minimisation problem of the firm, without imposing any assumption on the demand system. In particular, the researcher models the production function of the firm:

$$Q_{it} = Q_{it}(\Omega_{it}, V_{it}, K_{it}).$$

Where Q are sales (SALE in Compustat), V is a vector of variable inputs (COGS in Compustat) and K stands for capital (PPEGT in Compustat). All variables are deflated using appropriate deflators. The index i represents firms and t stands for time. Then, given the minimisation problem faced by the firm:

$$\mathcal{L}(V_{it}, K_{it}, \lambda_{it}) = P_{it}^{V} V_{it} + r_{it} K_{it} - \lambda_{it}(Q(.) - \bar{Q}_{it}).$$

One can note that the lagrangian multiplier λ_{it} represents the marginal cost faced by the firm, and thus it is possible to derive the Hall [1986] expression for the markup:

$$\mu_{it} = \frac{P_{it}}{\lambda_{it}} = \theta_{it}^{v} \frac{P_{it} Q_{it}}{P_{it}^{V} V_{it}}.$$

Where θ_{it}^{v} is the elasticity between output Q and variabe input V. This elasticity can be computed at the sector level (in this work 2-digit NAICS) by running sector-specific panel regressions with variables in logs:

$$q_{it} = \theta_s^{v} v_{it} + \theta_s^{k} k_{it} + \omega_{it} + \epsilon_{it}.$$

The variable ω_{it} represents an unobserved productivity shock. This productivity can be estimated by running a non-parametric regression

$$q_{it} = \phi(v_{it}, k_{it}) + \epsilon_{it}.$$

This regression is commonly referred to as the first stage. Then, one can define $\omega_{it} = \phi_{it} - (\theta_s^{v} v_{it} + \theta_s^{k} k_{it})$. The process for productivity ω_{it} is modeled as an AR(1):

$$\omega_{it}(\theta_s^{v}) = \alpha \omega_{it-1}(\theta_s^{v}) + \xi_{it}.$$

Where ξ_{it} is an unexpected shock to productivity to which firms can react by adjusting only variable input v_{it}. Therefore, we consider the input v_{it} to be "static", as it is determined in static cost minimisation. On the other hand, the capital input k_{it} is called "dynamic", as it is set in the previous period and it cannot be adjusted. As a consequence, one can impose that variable input responds to current productivity shocks, but lagged variable input does not. Together with the condition that capital does not respond to current shocks, this gives moment conditions to identify the desired elasticity θ_s^v :

$$\mathbb{E}\left[\xi_{it}(\theta_s^v)\begin{bmatrix} v_{it-1} \\ k_{it} \end{bmatrix}\right] = 0.$$

Once the sector level elasticity θ_s^v is computed, one can obtain firm i markup for every period t.

B Regressions

B.1 Forecast-volatility

Table 2: Benchmark IV Forecast-volatility (sales weights)

	Dependent variable:							
	Markup							
Volatility Measure	1-year	2-year	3-year	4-year	5-year	6-year	7-year	8-year
Forecast-volatility	−0.119	−0.177**	−0.179**	−0.189**	−0.192**	−0.206**	−0.204**	−0.215**
	(0.076)	(0.079)	(0.078)	(0.083)	(0.080)	(0.082)	(0.079)	(0.087)
Constant	−0.004*	−0.004**	−0.004**	−0.005**	−0.006***	−0.007***	−0.007***	−0.008***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Weak Instruments	12395.82	11923.77	10951.82	8871.3	6827.12	4841.45	4027.28	3044.56
Wu-Hausman	814.82	760.07	764.28	788.58	854.42	916.57	949.4	994.36
Bank Integration	−0.168***	−0.159***	−0.462***	−0.457***	−0.147***	−0.137***	−0.138***	−0.131**
	(0.035)	(0.032)	(0.042)	(0.039)	(0.033)	(0.039)	(0.046)	(0.053)
Constant	0.049***	0.045***	0.222***	0.218***	0.031*	0.025	0.024	0.020
	(0.019)	(0.017)	(0.018)	(0.016)	(0.018)	(0.021)	(0.025)	(0.028)
Observations	135,922	135,922	135,922	135,922	135,922	135,922	135,922	135,922
F-Statistic	12,395.820***	11,923.770***	58,598.030***	53,343.630***	6,827.120***	4,841.449***	4,027.283***	3,044.558***

This table shows the results of the benchmark regressions, using forecast-volatility measure as an independent variable, instrumented by the bank integration dummy. Markups and volatility are demeaned by firm and year instead of using similar fixed effects. In these regressions we use sales weights.

Table 3: Benchmark IV Forecast-volatility (COGS weights)

	Dependent variable:							
	Markup							
Volatility Measure	1-year	2-year	3-year	4-year	5-year	6-year	7-year	8-year
Forecast-volatility	−0.188*	−0.200**	−0.196**	−0.194**	−0.190**	−0.196*	−0.190*	−0.196*
	(0.099)	(0.102)	(0.093)	(0.091)	(0.095)	(0.105)	(0.107)	(0.118)
Constant	−0.011***	−0.011***	−0.012***	−0.012***	−0.012***	−0.013***	−0.013***	−0.013***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)
Weak Instruments	7156.79	6649.74	6523.02	6058.59	5109.86	3980.13	3372.53	2654.79
Wu-Hausman	1140.43	1145.14	1184.39	1217.54	1316.97	1409.12	1451.25	1519.55
Bank Integration	−0.120***	−0.113***	−0.410***	−0.415***	−0.119***	−0.115***	−0.119***	−0.115***
	(0.033)	(0.028)	(0.038)	(0.035)	(0.027)	(0.030)	(0.035)	(0.038)
Constant	0.026	0.023	0.195***	0.197***	0.019	0.016	0.016	0.013
	(0.017)	(0.015)	(0.014)	(0.012)	(0.014)	(0.016)	(0.017)	(0.019)
Observations	134,681	134,681	134,681	134,681	134,681	134,681	134,681	134,681
F Statistic	7,156.788***	6,649.738***	51,370.010***	49,474.860***	5,109.861***	3,980.134***	3,372.531***	2,654.793***

This table shows the results of the benchmark regressions, using forecast-volatility measure as an independent variable, instrumented by the bank integration dummy. Markups and volatility are demeaned by firm and year instead of using similar fixed effects. In these regressions we use COGS weights.

Table 4: Benchmark IV Excluding Automotive Sector

	Dependent variable:							
	Markup							
Volatility Measure	1-year	2-year	3-year	4-year	5-year	6-year	7-year	8-year
Forecast-volatility	−0.348***	−0.360***	−0.357***	−0.367***	−0.361***	−0.375**	−0.364**	−0.373**
	(0.130)	(0.130)	(0.122)	(0.118)	(0.130)	(0.151)	(0.161)	(0.183)
Constant	−0.009**	−0.009**	−0.010***	−0.011***	−0.012***	−0.013**	−0.013**	−0.014*
	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.006)	(0.007)	(0.007)
Weak Instruments	10622.32	10543.28	10302.03	9066.67	7674.15	5953.94	5112.68	4068.05
Wu-Hausman	3472.15	3459.27	3575.84	3738.28	4033.19	4309.88	4459.27	4629.25
Bank Integration	−0.153***	−0.147***	−0.149***	−0.145***	−0.147***	−0.142***	−0.146***	−0.142***
	(0.040)	(0.036)	(0.033)	(0.030)	(0.035)	(0.040)	(0.046)	(0.052)
Constant	0.042*	0.039*	0.038**	0.033**	0.032	0.028	0.028	0.026
	(0.022)	(0.020)	(0.019)	(0.017)	(0.020)	(0.022)	(0.025)	(0.028)
Observations	133,962	133,962	133,962	133,962	133,962	133,962	133,962	133,962
F-Statistic	10,622.320***	10,543.280***	10,302.030***	9,066.669***	7,674.151***	5,953.938***	5,112.681***	4,068.048***

B.2 Auxiliary-volatility

Table 5: IV Regressions Auxiliary-volatility

	Dependent variable:					
	Markup					
Sample	full	full	1^{st} quartile	2^{nd} quartile	3^{rd} quartile	4^{th} quartile
Auxilary-volatility	-0.227^{***}	-0.144	1.138	-0.660	-5.433	-1.176
	(0.083)	(0.110)	(9.848)	(1.056)	(136.855)	(3.088)
Constant	0.001	-0.006^{*}	-0.050	0.008	-0.007	-0.007
	(0.003)	(0.004)	(0.192)	(0.015)	(0.570)	(0.006)
Weak Instruments	545.1	668.86	0.1	0.55	0.01	1.42
Wu-Hausman	1498.1	882.44	2.44	13.26	16.29	170.04
Bank Integration	-0.127^{***}	-0.142^{***}	-0.003	-0.008	0.001	-0.012
	(0.024)	(0.022)	(0.025)	(0.011)	(0.023)	(0.034)
Constant	0.059^{***}	0.049^{***}	0.022	-0.010	-0.005	0.006
	(0.014)	(0.012)	(0.018)	(0.006)	(0.016)	(0.019)
Weights	sales	cogs	sales	sales	sales	sales
Observations	135,922	135,922	33,981	33,980	33,980	33,981

Note: *p<0.1; **p<0.05; ***p<0.01

This table shows the results of the regressions when using the volatility measure from the auxiliary regression of Correa and Suarez [2007] as an independent variable, instrumented by the bank integration dummy. Markups and volatility are demeaned by firm and year instead of using similar fixed effects.

B.3 Fixed-window-volatility

Table 6: Fixed-window-volatility (levels)

	Dependent variable:				
	Markups				
Sample	full	1^{st} quartile	2^{nd} quartile	3^{rd} quartile	4^{th} quartile
Fixed-window-volatility	-0.166^{*}	-0.033	0.080	-0.147	-0.107^{***}
(levels)	(0.087)	(0.041)	(0.064)	(0.115)	(0.042)
Constant	-0.009	-0.027^{***}	0.017^{***}	0.017^{***}	-0.012^{***}
	(0.006)	(0.002)	(0.002)	(0.001)	(0.001)
Weak Instruments	1323.13	159.27	56.91	15.64	203.44
Wu-Hausman	2.33	2.33	14.43	14.45	149.47
Bank Integration	-0.170^{***}	0.111^{**}	0.065^{*}	0.035	-0.136^{***}
	(0.028)	(0.052)	(0.036)	(0.029)	(0.020)
Constant	0.018	-0.028	-0.029	-0.017	0.017
	(0.015)	(0.040)	(0.024)	(0.020)	(0.013)
Observations	135,805	33,935	33,965	33,945	33,960
F-Statistic	$1{,}323.127^{***}$	159.269^{***}	56.910^{***}	15.643^{***}	203.435^{***}

Note: *p<0.1; **p<0.05; ***p<0.01

Table 7: Fixed-window-volatility (growth rates)

	Dependent variable:				
	Markup				
Sample	full	1^{st} quartile	2^{nd} quartile	3^{rd} quartile	4^{th} quartile
Fixed-window-volatility	−0.131	−0.083	0.177	0.234	-0.236^{*}
(differences)	(0.080)	(0.093)	(0.185)	(0.500)	(0.128)
Constant	0.002	-0.028^{***}	0.016^{***}	0.016^{***}	-0.007^{***}
	(0.002)	(0.001)	(0.003)	(0.002)	(0.001)
Weak Instruments	1688.33	22.5	10.68	5.51	32.38
Wu-Hausman	2.77	2.77	13.07	16.46	165
Observations	136,390	34,024	34,118	34,093	34,155
Bank Integration	-0.216^{***}	0.045	0.029	−0.022	-0.062^{**}
	(0.066)	(0.030)	(0.031)	(0.030)	(0.031)
Constant	0.109^{***}	−0.034	−0.010	0.013	0.029^{*}
	(0.035)	(0.024)	(0.023)	(0.020)	(0.016)
Observations	135,777	33,928	33,956	33,932	33,961
F-Statistic	$1{,}688.326^{***}$	22.498^{***}	10.684^{***}	5.508^{**}	32.376^{***}

This table shows the results of the IV regressions, computing volatility from fixed windows of the state-sector GDP growth rates. Each window is five years long and the windows do not overlap. This measure is used as an explanatory variable, instrumented by the bank integration dummy. Markups and volatility are demeaned by firm and year instead of using similar fixed effects.

Table 8: Fixed-window-volatility COGS weights (levels)

	Dependent variable:				
	Markups				
	(1)	(2)	(3)	(4)	(5)
Fixed-window-volatility	−0.243**	−0.033	0.080	−0.147	−0.091**
	(0.102)	(0.041)	(0.064)	(0.115)	(0.042)
Constant	−0.020***	−0.027***	0.017***	0.017***	−0.013***
	(0.004)	(0.002)	(0.002)	(0.001)	(0.001)
Weak Instruments	365.26	159.27	56.91	15.64	185.61
Wu-Hausman	2.33	2.33	14.43	14.45	103.66
Bank Integration	−0.093***	0.111**	0.065*	0.035	−0.133***
	(0.020)	(0.052)	(0.036)	(0.029)	(0.019)
Constant	−0.018*	−0.028	−0.029	−0.017	0.015
	(0.009)	(0.040)	(0.024)	(0.020)	(0.013)
Observations	134,564	33,935	33,965	33,945	32,719
F Statistic	365.259***	159.269***	56.910***	15.643***	185.613***

B.4 Decile Regressions

Table 9: Forecast-volatility Deciles 1-5

	Dependent variable:				
	Markup				
Decile	1^{st}	2^{nd}	3^{rd}	4^{th}	5^{th}
4-year Forecast-volatility	−0.118	0.038	0.098^{***}	0.070^{**}	0.031
	(0.093)	(0.040)	(0.016)	(0.031)	(0.048)
Constant	-0.033^{***}	-0.032^{***}	-0.005^{**}	0.017^{***}	0.025^{***}
	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)
Weak Instruments	681.85	594.85	426.83	380.88	218.26
Wu-Hausman	11.08	9.69	31.77	21.68	4.05
Bank Integration	0.134^{***}	0.118^{***}	0.097^{***}	0.091^{***}	0.069^{***}
	(0.010)	(0.014)	(0.016)	(0.012)	(0.011)
Constant	-0.073^{***}	-0.056^{***}	-0.042^{***}	-0.041^{***}	-0.026^{***}
	(0.009)	(0.017)	(0.013)	(0.014)	(0.010)
Observations	13,593	13,592	13,592	13,592	13,592
F-Statistic	681.847^{***}	594.853^{***}	426.827^{***}	380.883^{***}	218.264^{***}

This table shows the results of the decile (1-5) regressions, using the 4-year forecast-volatility measures as independent variables, instrumented by the bank integration dummy. Markups and volatility are demeaned by firm and year instead of using similar fixed effects.

Table 10: Forecast-volatility Deciles 6-10

	Dependent variable:				
	Markup				
Decile	6^{th}	7^{th}	8^{th}	9^{th}	10^{th}
4-year Forecast-volatility	−0.178	3.015	−0.007	0.003	-0.224^{**}
	(0.120)	(19.454)	(0.060)	(0.042)	(0.089)
Constant	0.025^{***}	0.003	0.001	-0.012^{***}	-0.014^{***}
	(0.003)	(0.077)	(0.003)	(0.001)	(0.004)
Weak Instruments	43.36	0.3	121.48	317.09	1163.68
Wu-Hausman	5.14	13.91	0.74	1.78	242.82
Bank Integration	0.030^{*}	−0.003	-0.051^{***}	-0.083^{***}	-0.157^{***}
	(0.017)	(0.014)	(0.014)	(0.021)	(0.028)
Constant	−0.008	0.005	0.014	0.011	0.032^{**}
	(0.014)	(0.009)	(0.010)	(0.010)	(0.015)
Observations	13,592	13,592	13,592	13,592	13,593
F-Statistic	43.357^{***}	0.299	121.481^{***}	317.089^{***}	$1{,}163.675^{***}$

This table shows the results of the decile (6-10) regressions, using the 4-year forecast-volatility measures as independent variables, instrumented by the bank integration dummy. Markups and volatility are demeaned by firm and year instead of using similar fixed effects.

B.5 Geographic Subsample Regressions

Table 11: Regional IV Forecast-volatility I

	Dependent variable:					
	Markup					
	West	Great Plains	Midwest	Northeast	South	Midwest (w/o automotive)
4-year Forecast-volatility	−0.180***	−0.512***	0.420***	−0.854***	−0.137***	−0.437***
	(0.017)	(0.115)	(0.053)	(0.215)	(0.046)	(0.015)
Constant	−0.009***	−0.013**	0.021***	−0.032***	−0.008***	−0.009***
	(0.002)	(0.006)	(0.004)	(0.008)	(0.001)	(0.002)
Weak Instruments	5480.35	862.18	752.7	1313.38	2620.43	551.44
Wu-Hausman	500.52	870.84	512.06	1837.54	272.11	505.67
Bank Integration	−0.228***	−0.143***	−0.128***	−0.093**	−0.177***	−0.096**
	(0.024)	(0.025)	(0.043)	(0.037)	(0.010)	(0.042)
Constant	0.075***	0.018	0.022	0.006	0.047***	0.005
	(0.009)	(0.017)	(0.018)	(0.023)	(0.006)	(0.016)
Observations	39,892	10,152	21,268	41,486	21,951	20,134
F-Statistic	5,480.347***	862.181***	752.696***	1,313.382***	2,620.427***	551.435***

This table shows the results for regional subsamples, using the 4-year forecast-volatility measures as independent variables, instrumented by the bank integration dummy. Markups and volatility are demeaned by firm and year instead of using similar fixed effects.

Table 12: Regional IV Forecast-Volatility II

	Dependent variable:							
	Markup							
	Far West	Rocky Mountains	Southwest	Plains	Great Lakes	Mideast	New England	Southeast
4-year Forecast-volatility	-0.293^{***}	0.062	-0.133^{***}	-0.627^{***}	0.420^{***}	-0.973^{***}	-0.268^{***}	-0.137^{***}
	(0.009)	(0.193)	(0.002)	(0.096)	(0.057)	(0.287)	(0.048)	(0.046)
Constant	-0.010^{***}	-0.018^{***}	-0.007^{***}	-0.019^{**}	0.021^{***}	-0.036^{***}	-0.005^{***}	-0.008^{***}
	(0.001)	(0.004)	(0.0002)	(0.009)	(0.004)	(0.012)	(0.001)	(0.001)
Weak Instruments	2672.19	250.2	2592.39	598.13	747.26	1103.76	204.45	2628.23
Wu-Hausman	555.29	3.68	133.29	824.61	506.79	1723.21	37.45	272.5
Bank Integration	-0.205^{***}	-0.125^{*}	-0.263^{***}	-0.121^{***}	-0.128^{***}	-0.094^{*}	-0.091^{***}	-0.177^{***}
	(0.024)	(0.070)	(0.019)	(0.012)	(0.045)	(0.049)	(0.014)	(0.010)
Constant	0.075^{***}	0.022	0.082^{***}	0.005	0.022	0.006	0.005	0.047^{***}
	(0.010)	(0.030)	(0.005)	(0.018)	(0.019)	(0.029)	(0.007)	(0.006)
Observations	19,779	5,572	16,188	8,754	21,043	29,901	11,585	22,176
F-Statistic	$2,672.192^{***}$	250.201^{***}	$2,592.393^{***}$	598.129^{***}	747.257^{***}	$1,103.762^{***}$	204.450^{***}	$2,628.231^{***}$

This table shows the results for regional subsamples, using the 4-year forecast-volatility measures as independent variables, instrumented by the bank integration dummy. Markups and volatility are demeaned by firm and year instead of using similar fixed effects.

B.6 Regression with different weights

B.7 Sectoral regressions

Table 13: Sectoral IV Forecast-volatility (Sales Weights)

	Dependent variable:							
	Markup							
	Mining	Construction	Manufacturing	Trade	Transportation	Finances	Services	Manufacturing w/o automotive
4-year Forecast-volatility	0.304**	1.475	−0.093	−0.264**	−0.005	−0.943**	0.301	−0.528***
	(0.122)	(6.265)	(0.235)	(0.119)	(0.068)	(0.372)	(0.542)	(0.165)
Constant	−0.001	0.061	0.0002	−0.023**	0.004***	−0.024	−0.003	−0.016***
	(0.002)	(0.299)	(0.012)	(0.010)	(0.001)	(0.015)	(0.017)	(0.003)
Weak Instruments	140.59	1.52	2906.08	3979.46	1074.49	1330.03	201.09	2773.89
Wu-Hausman	207.02	70.59	80.53	406.81	13.87	720.63	17.46	6470.4
Bank Integration treat	−0.112***	−0.017	−0.142***	−0.251***	−0.134***	−0.142***	−0.050	−0.130***
	(0.042)	(0.080)	(0.030)	(0.041)	(0.029)	(0.045)	(0.033)	(0.040)
Constant	0.077***	−0.038	0.024*	0.084***	0.024	0.040	0.002	0.018
Observations	7,142	2,147	59,601	15,843	16,158	15,313	19,718	57,641
F-Statistic	25.442***	24.150***	809.881***	2,169.303***	389.309***	197.540***	169.641***	1,393.969***

This table shows the results for sector subsamples, using the 4-year forecast-volatility measures as independent variables, instrumented by the bank integration dummy. Markups and volatility are demeaned by firm and year instead of using similar fixed effects.

Table 14: Sectoral IV Forecast-volatility (COGS Weights)

	Dependent variable:							
	Markup							
	Mining	Construction	Manufacturing	Trade	Transportation	Finances	Services	Manufacturing w/o automotive
4-year Forecast-volatility	0.342**	1.121	−0.038	−0.236**	0.024	−0.846**	0.002	−0.537***
	(0.135)	(4.008)	(0.288)	(0.110)	(0.046)	(0.423)	(0.249)	(0.156)
Constant	−0.004*	0.050	−0.001	−0.026**	0.002**	−0.025	−0.021**	−0.018***
	(0.002)	(0.209)	(0.013)	(0.011)	(0.001)	(0.015)	(0.010)	(0.003)
Weak Instruments	148.96	2.1	2907.4	3873.45	1019.26	1086.83	286.41	2739.6
Wu-Hausman	322.36	54.97	5.75	375.74	43.3	642.06	4.85	6833.03
Bank Integration	−0.115***	−0.021	−0.143***	−0.250***	−0.131***	−0.126**	−0.060*	−0.128***
	(0.045)	(0.082)	(0.029)	(0.039)	(0.028)	(0.051)	(0.034)	(0.042)
Constant	0.076***	−0.041	0.026*	0.080***	0.020	0.038	0.005	0.019
	(0.025)	(0.042)	(0.014)	(0.018)	(0.017)	(0.033)	(0.025)	(0.020)
Observations	7,142	2,147	59,601	15,843	16,158	15,313	19,718	57,641
F-Statistic	148.956***	2.098	2,907.397***	3,873.447***	1,019.259***	1,086.828***	286.409***	2,739.597***

Table 15: Tradables versus Non-tradables IV Forecast-volatility

	Dependent variable:							
	Markup							
	tradables	tradables	non-tradables	non-tradables	tradables	tradables	non-tradables	non-tradables
4-year Forecast-volatility	−0.522***	−0.526***	−0.402***	−0.331***				
	(0.137)	(0.134)	(0.131)	(0.122)				
8-year forecast-volatility					−0.541	−0.541	−0.453**	−0.366**
					(0.348)	(0.349)	(0.206)	(0.186)
Constant	−0.009***	−0.012***	−0.016***	−0.021***	−0.012	−0.014	−0.020**	−0.025***
	(0.003)	(0.002)	(0.005)	(0.005)	(0.011)	(0.010)	(0.009)	(0.008)
Weak Instruments	2618.79	2591.63	5170.97	4941.32	1079.2	1063.43	1939.27	1901.86
Wu-Hausman	6404.1	6680.23	920.97	764.51	7327.89	7639.67	1371.25	1152.72
Bank Integration	−0.137***	−0.136***	−0.160***	−0.157***	−0.132	−0.132	−0.142***	−0.142***
	(0.033)	(0.035)	(0.030)	(0.030)	(0.083)	(0.086)	(0.043)	(0.045)
Constant	0.034*	0.035*	0.048***	0.045***	0.028	0.029	0.033*	0.032
	(0.019)	(0.019)	(0.015)	(0.015)	(0.043)	(0.043)	(0.020)	(0.021)
Weights	sales	cogs	sales	cogs	sales	cogs	sales	cogs
Observations	55,065	55,065	52,529	52,529	55,065	55,065	52,529	52,529
F-Statistic	2,618.785***	2,591.632***	5,170.967***	4,941.319***	1,079.202***	1,063.429***	1,939.266***	1,901.860***

This table shows the results of the regressions when firms are classified into tradable and non-tradable goods. These regressions use the 4-year Forecast-volatility measures as independent variables, instrumented by the bank integration dummy. Markups and volatility are demeaned by firm and year instead of using similar fixed effects.

B.8 Decile Regressions Tradables versus Non-tradables

Table 16: Deciles 1-5 Tradables (Sales Weights)

	Dependent variable:				
	Markups				
Decile	1^{st}	2^{nd}	3^{rd}	4^{th}	5^{th}
4-year Forecast-volatility	-0.052	0.128^{***}	0.308^{***}	0.348^{***}	0.347^{***}
	(0.038)	(0.035)	(0.097)	(0.048)	(0.128)
Constant	-0.045^{***}	-0.031^{***}	-0.007^{**}	0.012^{***}	0.021^{***}
	(0.005)	(0.003)	(0.003)	(0.002)	(0.003)
Weak Instruments	330.36	187.96	65.69	73.11	59.26
Wu-Hausman	2.97	18.62	41.54	63.77	63.25
Observations	7,445	6,713	6,084	5,742	5,701
Bank Integration	0.136^{***}	0.104^{***}	0.063^{***}	0.068^{***}	0.063^{***}
	(0.007)	(0.012)	(0.022)	(0.015)	(0.013)
Constant	-0.069^{***}	-0.050^{***}	-0.033^{***}	-0.037^{***}	-0.033^{***}
	(0.011)	(0.012)	(0.013)	(0.014)	(0.010)
Observations	7,445	6,713	6,084	5,742	5,701
F-Statistic	330.355^{***}	187.960^{***}	65.686^{***}	73.111^{***}	59.264^{***}

Table 17: Deciles 6-10 Tradables (Sales Weights)

	Dependent variable:				
	Markup				
Decile	6^{th}	7^{th}	8^{th}	9^{th}	10^{th}
4-year Forecast-volatility	−0.052	0.128	0.308	0.348	0.347***
	(0.511)	(5.980)	(0.718)	(0.296)	(0.120)
Constant	−0.045***	−0.031	−0.007	0.012	0.021***
	(0.002)	(0.056)	(0.009)	(0.014)	(0.002)
Weak Instruments	10	0.5	8.42	21.66	332.31
Wu-Hausman	43.47	33.2	58.18	54.05	797.59
Bank Integration	0.026	−0.006	−0.026*	−0.044*	−0.172***
	(0.025)	(0.011)	(0.014)	(0.022)	(0.040)
Constant	−0.019	−0.006	0.0001	−0.019	0.047**
	(0.019)	(0.007)	(0.011)	(0.015)	(0.020)
Observations	5,373	4,835	4,442	4,292	4,438
F-Statistic	9.998***	0.496	8.418***	21.656***	332.313***

Table 18: Deciles 1-5 Non-tradables (Sales Weights)

	Dependent variable:				
	Markup				
Decile	1^{st}	2^{nd}	3^{rd}	4^{th}	5^{th}
4-year Forecast-volatility	-0.235^{***}	0.036	-0.082^{*}	-0.114	-0.336^{***}
	(0.080)	(0.083)	(0.047)	(0.076)	(0.121)
Constant	-0.053^{***}	-0.032^{***}	0.005	0.026^{***}	0.043^{***}
	(0.004)	(0.006)	(0.004)	(0.004)	(0.004)
Weak Instruments	192.44	279.77	293.87	159.62	54.96
Wu-Hausman	8.77	5.93	0.05	0.25	17.85
Bank Integration	0.109^{***}	0.126^{***}	0.119^{***}	0.088^{***}	0.051^{***}
	(0.017)	(0.017)	(0.015)	(0.020)	(0.019)
Constant	-0.047^{***}	-0.047^{**}	-0.030^{*}	-0.010	0.015
	(0.014)	(0.021)	(0.017)	(0.017)	(0.013)
Observations	4,239	4,828	5,291	5,494	5,262
F-Statistic	192.438^{***}	279.766^{***}	293.871^{***}	159.622^{***}	54.962^{***}

Table 19: Deciles 6-10 Non-tradables (Sales Weights)

	Dependent variable:				
	Markup				
Decile	6^{th}	7^{th}	8^{th}	9^{th}	10^{th}
4-year Forecast-volatility	-1.195^{*}	-3.469	0.104	0.016	-0.459^{***}
	(0.627)	(12.056)	(0.163)	(0.047)	(0.147)
Constant	0.073^{**}	0.086	-0.007^{*}	-0.026^{***}	-0.019^{**}
	(0.031)	(0.257)	(0.004)	(0.003)	(0.008)
Weak Instruments	12.23	0.55	64.53	231.44	765.76
Wu-Hausman	64.25	20.46	6.64	1.48	142.14
Bank Integration	0.024	0.005	-0.053^{***}	-0.106^{***}	-0.196^{***}
	(0.015)	(0.019)	(0.016)	(0.013)	(0.040)
Constant	0.026^{**}	0.019^{*}	0.020^{**}	0.025^{***}	0.059^{***}
	(0.013)	(0.010)	(0.010)	(0.008)	(0.019)
Observations	5,203	5,963	5,887	5,366	4,996
F-Statistic	12.228^{***}	0.548	64.535^{***}	231.439^{***}	765.755^{***}

Table 20: Deciles 1-5 Tradables (COGS Weights)

	Dependent variable:				
	Markup				
Decile	1^{st}	2^{nd}	3^{rd}	4^{th}	5^{th}
4-year Forecast-volatility	0.193^{**}	0.146^{***}	0.358^{**}	0.353^{***}	0.302^{**}
	(0.096)	(0.039)	(0.146)	(0.056)	(0.149)
Constant	-0.080^{***}	-0.037^{***}	-0.007^{**}	0.013^{***}	0.018^{***}
	(0.011)	(0.005)	(0.003)	(0.003)	(0.003)
Weak Instruments	549.87	230.26	72.07	72.87	56.42
Wu-Hausman	47.09	27.92	60.08	67.15	52.9
Bank Integration	0.201^{***}	0.117^{***}	0.067^{***}	0.067^{***}	0.061^{***}
	(0.011)	(0.011)	(0.017)	(0.014)	(0.014)
Constant	-0.086^{***}	-0.055^{***}	-0.040^{***}	-0.047^{***}	-0.039^{***}
	(0.013)	(0.013)	(0.008)	(0.012)	(0.009)
Observations	7,445	6,713	6,084	5,742	5,701
F-Statistic	549.872^{***}	230.264^{***}	72.069^{***}	72.866^{***}	56.417^{***}

Table 21: Deciles 6-10 Tradables (COGS Weights)

	Dependent variable:				
	Markup				
Decile	6^{th}	7^{th}	8^{th}	9^{th}	10^{th}
4-year Forecast-volatility	0.632	−25.059	−1.365	−0.558	-0.503^{***}
	(0.413)	(402.726)	(0.954)	(0.399)	(0.117)
Constant	0.022^{***}	−0.281	−0.012	-0.026^{*}	-0.011^{***}
	(0.002)	(4.682)	(0.013)	(0.016)	(0.002)
Weak Instruments	12.48	0.01	3.29	14.86	328.06
Wu-Hausman	52.89	37.51	50.24	46.93	812.13
Bank Integration	0.029	−0.001	−0.016	−0.036	-0.170^{***}
	(0.022)	(0.012)	(0.011)	(0.023)	(0.042)
Constant	−0.024	−0.011	−0.005	−0.021	0.048^{**}
	(0.017)	(0.008)	(0.009)	(0.013)	(0.020)
Observations	5,373	4,835	4,442	4,292	4,438
F-Statistic	12.480^{***}	0.007	3.295^{*}	14.856^{***}	328.059^{***}

Table 22: Deciles 1-5 Non-tradables (COGS Weights)

	Dependent variable:				
	Markup				
Decile	1^{st}	2^{nd}	3^{rd}	4^{th}	5^{th}
4-year Forecast-volatility	−0.194	0.048	−0.031	−0.166**	−0.407***
	(0.159)	(0.065)	(0.034)	(0.080)	(0.153)
Constant	−0.052***	−0.027***	0.008*	0.027***	0.039***
	(0.013)	(0.007)	(0.004)	(0.004)	(0.004)
Weak Instruments	152.56	284.2	298.39	117.29	47.65
Wu-Hausman	7.17	5.6	0.98	3.76	32.44
Bank Integration	0.097***	0.127***	0.120***	0.075***	0.048**
	(0.019)	(0.018)	(0.017)	(0.018)	(0.020)
Constant	−0.027*	−0.050**	−0.043**	−0.012	0.010
	(0.015)	(0.021)	(0.018)	(0.014)	(0.014)
Observations	4,239	4,828	5,291	5,494	5,262
F-Statistic	152.561***	284.196***	298.386***	117.291***	47.649***

Table 23: Deciles 6-10 Non-tradables (COGS Weights)

	Dependent variable:				
	Markup				
Decile	6^{th}	7^{th}	8^{th}	9^{th}	10^{th}
4-year Forecast-volatility	-1.418^{**}	3.831	0.208	0.088^{*}	-0.393^{***}
	(0.609)	(15.793)	(0.151)	(0.045)	(0.142)
Constant	0.066^{**}	−0.071	-0.013^{***}	-0.027^{***}	-0.025^{***}
	(0.029)	(0.302)	(0.004)	(0.003)	(0.008)
Weak Instruments	10.92	0.67	68.07	219.9	714.67
Wu-Hausman	106.03	41.35	23.45	12.97	128.52
Bank Integration	0.023^{*}	−0.005	-0.055^{***}	-0.103^{***}	-0.191^{***}
	(0.012)	(0.019)	(0.017)	(0.012)	(0.039)
Constant	0.022^{*}	0.022^{**}	0.022^{**}	0.023^{***}	0.055^{***}
	(0.012)	(0.010)	(0.010)	(0.008)	(0.020)
Observations	5,203	5,963	5,887	5,366	4,996
F-Statistic	10.922^{***}	0.669	68.065^{***}	219.902^{***}	714.672^{***}

B.9 Parallel Trends Test

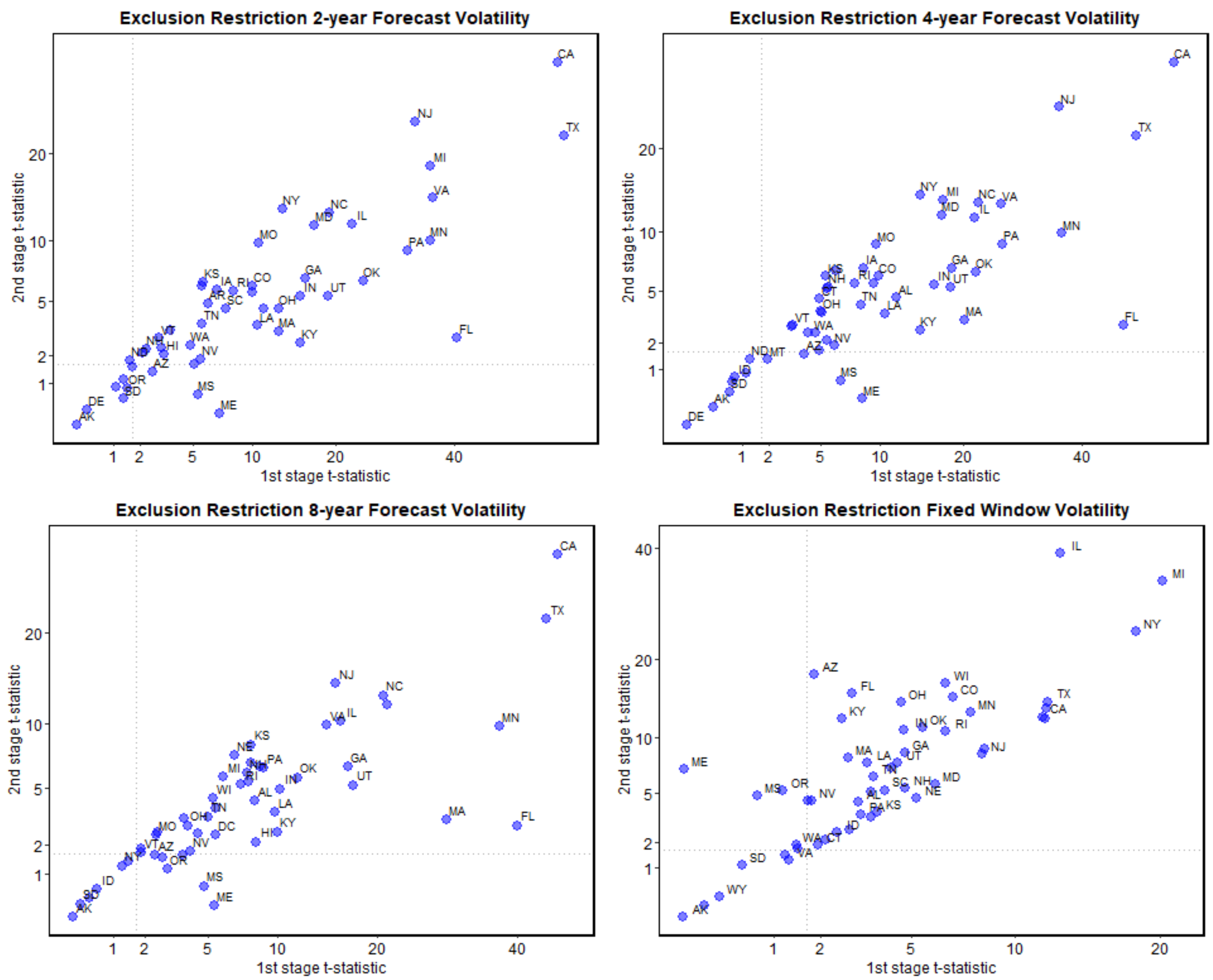
Table 24: Parallel Trends Test

	Dependent variable:		
Volatility-forecast	2-year	4-year	8-year
Banking Integration	0.008	29.102*	12,603,630.000
	(0.041)	(16.375)	(8,315,709.000)
Lag 1	-0.103	-35.015	-11,047,377.000
	(0.073)	(24.040)	(7,884,092.000)
Lead 1	-0.020	-11.877	-5,088,035.000
	(0.050)	(7.545)	(3,692,217.000)
Constant	0.052**	7.012	1,294,904.000
	(0.023)	(6.301)	(1,314,409.000)
Observations	135,922	135,922	135,922
Note:			*p<0.1; **p<0.05; ***p<0.01

B.10 Local Projections

We also estimate local projections following Jordà [2005]. This provides further insights into how the effect of a shock to volatility affects markups over time. The benchmark in Figure 6 shows that the aggregate effect is negative and stays does not fade out quickly. In fact, the point estimate stays negative for more than 8 years - only its precision fades so that it is no longer significant at this point. If we break down the effect into quartiles as in Figure 7 we see a similar pattern: the fact that the point estimate is increasing even many years after the shock suggests that there may be a feedback loop between markups and volatility that exacerbates the effect.

Figure 5: Test of the Exclusion Restriction



This figure shows the results of falsification tests that illustrate that the exclusion restriction is not violated. In each graph, the x-axis plots the t-statistic of the Bank Integration effect in the first stage regression, the y-axis measures the t-statistic of the effect of volatility in the second stage.

Figure 6: Local Projection Full Sample

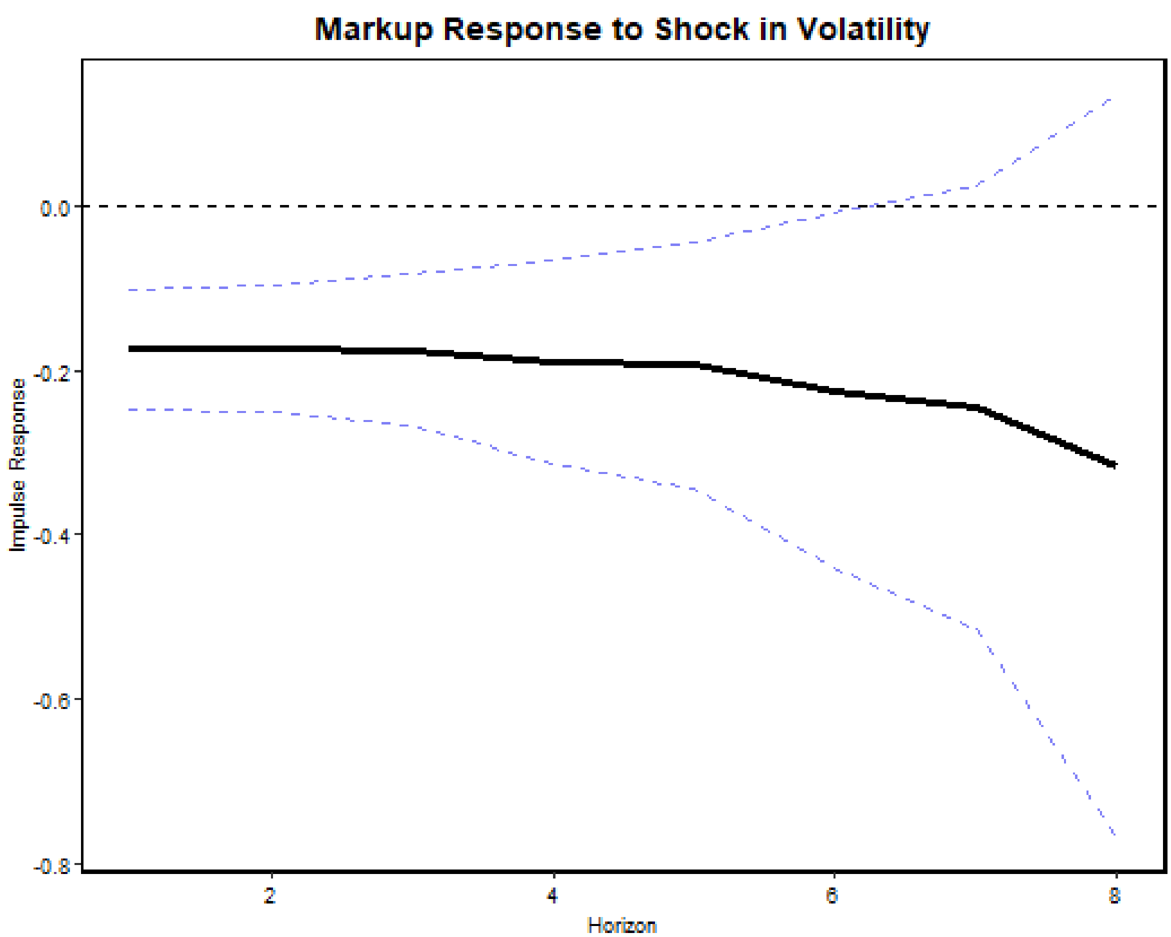


Figure 7: Local Projections by Quartile