Pricing under Distress*

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Abstract

We isolate the anticipation effect of uncertainty on firms' price-setting behavior using a quasi-natural experiment: the 2019 Social Uprising in Chile. During the 31-day period following the outbreak of nationwide protests and riots, the frequency of supermarket price changes dropped by 40 to 60 percent, while the average size of adjustments rose by 40 to 50 percent. Supplier prices remained stable, and local intensity of riots did not explain the variation, suggesting a forward-looking mechanism. A menu cost model with news about future idiosyncratic demand volatility replicates these dynamics. Anticipated uncertainty amplifies the short-run real effects of monetary policy, highlighting the importance of timing.

Keywords: Menu costs, uncertainty, demand shocks, variable markups, 2019 Social Uprising in Chile, sticky prices.

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1 Introduction

The pricing decision of firms is central in macroeconomics. The frequency and size of price changes help determine the effectiveness of monetary policy on real outcomes. Price setting is inherently forward looking, as firms anticipate that the price they set today will remain in place for some time. Expectations, and more broadly uncertainty about the future, are therefore a key ingredient of the price-setting problem. Understanding how firms respond to changes in uncertainty is essential not only for interpreting micro evidence on pricing but also for evaluating how nominal shocks transmit into the real economy.

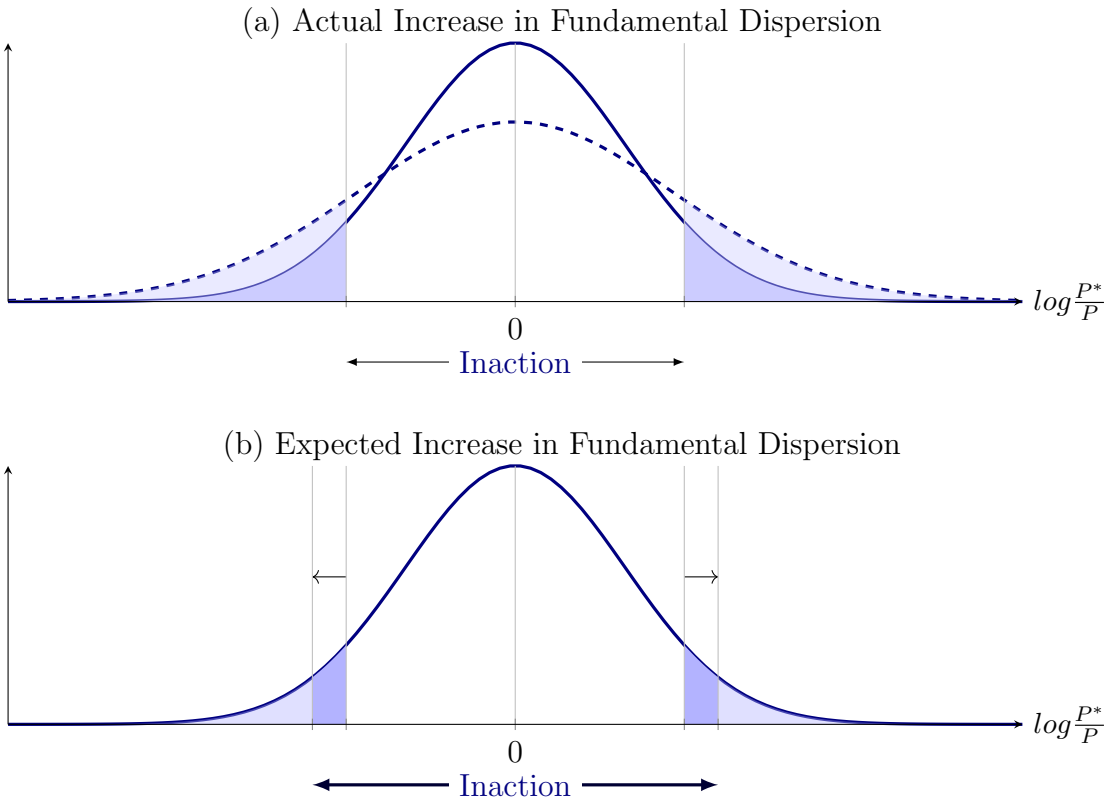
Uncertainty is typically modeled as time variation in the dispersion of a fundamental that shapes firms' decisions. In the menu-cost framework of Vavra (2014), for example, the dispersion of firm-level productivity shocks changes over time. An increase in this dispersion has two distinct effects: (i) the distribution becomes more dispersed today and (ii) it is expected to remain more dispersed tomorrow. We refer to these as the *realization* and *anticipation* effects of uncertainty, respectively.[1] Much of the existing literature, including Bloom (2009), Bloom (2014), and Vavra (2014), effectively bundles the two together, which makes it difficult to know which mechanism dominates in practice.

To demonstrate how the realization and anticipation effects affect decisions we turn to a simple and tractable model introduced by Dixit (1991). In this model, a forward-looking decision maker faces a random variable x every period, which is iid over time, following a normal distribution with zero mean and standard deviation σ. The ideal value of this variable for the decision-maker is zero and any departure from zero has the cost $f(x) = kx^2$. One can think of x as the log of a firm's lagged price relative to the aggregate price level, which moves with aggregate conditions. Ideally, the firm wants its current price to match the aggregate price level. If the firm wants to reset x to zero to do so, then they have to pay a fixed cost g. Dixit (1991) shows that the dynamic problem of the firm has the feature that if $x \in [-h, h]$, the firm keeps it unchanged; otherwise it sets $x = 0$ and pays the fixed cost. He also provides a closed-form approximation for h. More details about the model are relegated to Appendix A.

The solid-blue distribution in both panels of Figure 1 shows the log difference between P^* and P under the baseline dispersion. Price changes that fall in the blue shaded areas in the two ends of the distribution are implemented and the white part in between shows the inaction region where no price changes occur. In Panel (a), we consider a one-time, unexpected increase in the dispersion of the fundamental distribution today with no changes in the future, and the dashed-blue line shows the new distribution of desired price changes.

[1] Bloom (2009) uses the terms uncertainty and volatility effects to describe these channels.

Figure 1: Distribution of Desired Price Changes in a Stylized Menu Cost Model



Notes: Panel (a) shows the effects of an increase in the price gap distribution, going from the less dispersed solid-blue distribution to the more dispersed dashed-blue distribution. All else are held equal, including expectations about future states. The grey vertical lines represent the inaction bands. The dark-blue and light-shaded areas represent the mass of firms adjusting prices under the two distributions, respectively. Panel (b) shows an increase in future dispersion of idiosyncratic states, holding the current distribution of desired price gaps constant. This induces a wider inaction region as depicted by the shift in the grey vertical lines. The light-blue shaded areas represent the mass of firms adjusting, whereas the dark-blue shaded areas represent the mass of firms that choose not to adjust after the shock to future dispersion. The figures are drawn with: $k = 1$, $g = 0.3945$, $\sigma = 0.65$ for the solid-blue distribution and $\sigma = 0.936$ for the dashed-blue distribution. The solution in Appendix A shows that the inaction regions are $[-1, 1]$ and $[-1.2, 1.2]$.

Because the inaction bands only depend on the (unchanged) fundamental distribution in the subsequent periods, the inaction bands do not change. The wider dashed-blue distribution has more mass in the tails relative to the blue distribution, therefore, there are more price changes (the light-blue shaded area is added to the dark-blue shaded area) and the average price change is higher. This is the realization effect caused by an increase in dispersion isolated from the anticipation effect.

In Panel (b) we show what happens if the dispersion is expected to increase permanently tomorrow, with no change in today's distribution. Because the distribution does not change today, the distribution of desired price changes is also unchanged. Consider a firm whose

price was just outside the inaction bands before the change – it was willing to pay the fixed cost and change its price. After the news of an increase in future dispersion arrive, the firm takes into account that increased dispersion tomorrow may render its current price change suboptimal, leading it to pay another adjustment cost tomorrow to remedy this. As a result it chooses to postpone price adjustment until its state tomorrow is revealed. This behavior extends the inaction region, leading to fewer price changes and larger price changes conditional on adjustment. This is the effect of anticipation isolated from the realization effect.

The simple model in Figure 1 shows that the realization and anticipation effects have sharply different implications. When dispersion rises today, the distribution of desired price changes widens and more firms adjust, leading to a higher frequency of price changes and larger adjustments on average. This is the realization effect. When dispersion is instead expected to rise in the future, today's distribution of desired prices is unchanged, but firms delay adjustment to avoid paying the cost again tomorrow. The anticipation effect therefore reduces the frequency of price changes while increasing their average size. These opposite comparative statics matter for both micro evidence and macroeconomic transmission: in the realized case studied by Vavra (2014), monetary policy becomes less effective, while in the anticipation case, firms' delay in adjustment implies that monetary policy can become more powerful on impact.

The distinction between realization and anticipation is especially relevant in environments where expectations about future institutions or policies shift before any concrete reform takes place. Elections, constitutional referenda such as Brexit, regime transitions, and episodes of mass protest can all raise the perceived dispersion of future outcomes while leaving contemporaneous fundamentals largely unchanged. In these situations, firms may not know which products will be taxed, subsidized, or regulated, or even which goods will be popular with consumers. Polarization in many countries has made such forward-looking uncertainty more common, as businesses anticipate major institutional or policy shifts without knowing their precise form. These are precisely the conditions under which anticipation effects may dominate realization effects, making the distinction central for how we interpret micro evidence and assess the transmission of macroeconomic shocks.

This leads to the following natural question: can we find an example of a major event where the main effect of the change in uncertainty was anticipation and not realization? In the first part of the paper we show that the 2019 Social Uprising (*"Estallido Social"* in Spanish) in Chile provides such an example. On October 18, 2019, localized demonstrations over a small subway fare increase unexpectedly escalated into a nationwide wave of protests. In the weeks that followed, the movement broadened into a call for deep social and institu-

tional change, captured by the widespread slogan that "Chile Changed." We argue that this episode fits the definition of a quasi-natural experiment, as it was sudden and unexpected.

This leads to the following natural question: can we find a clear example of a major event where the main effect of uncertainty was anticipation rather than realization? We argue that the 2019 Social Uprising (*"Estallido Social"* in Spanish) in Chile provides such an episode. On October 18, 2019, localized demonstrations over a small subway fare increase unexpectedly escalated into a nationwide wave of protests. In the weeks that followed, the movement broadened into a call for deep social and institutional change, captured by the widespread slogan that "Chile changed." In this sense, the Social Uprising fits the definition of a quasi-natural experiment: it was sudden and unexpected. Furthermore, from the perspective of firms, this may have created heightened expectations of structural transformation in the economy and society, while leaving contemporaneous costs and fundamentals largely unchanged.

We study the price-setting behavior of supermarkets during this episode, drawing on a uniquely granular dataset built from electronic value-added tax invoices. These data allow us to observe daily prices for thousands of products across different stores and regions in Chile, and for a subset of items we can also observe the prices paid to suppliers, which lets us identify changes in costs. Two main results emerge. First, the frequency of both price increases and decreases fell sharply during the Social Uprising, while the size of price changes, conditional on adjustment, rose substantially. Second, suppliers did not alter their pricing behavior, and the geographical intensity of violent incidents associated with the Uprising was uncorrelated with supermarkets' pricing response. Together, these findings indicate that concurrent supply or local demand shocks are unlikely to account for the observed behavior. Instead, the patterns are consistent with the anticipation mechanism described in our simple model, where firms delay adjustment in the face of expected future dispersion in demand conditions.

To interpret these findings and quantify the role of anticipation, we extend the menu-cost framework of Vavra (2014). Following Aruoba et al. (2025), we introduce idiosyncratic demand shocks and allow for variable markups through a Kimball (1995) aggregator. This modification ensures that shocks to demand dispersion affect pricing decisions. We calibrate the model using our matched price and cost data, which allows us to discipline the idiosyncratic shock processes directly with micro evidence. Quantitatively we find that this anticipation channel can explain 31% of the decrease in frequency and 52% of the increase in the size of price changes observed in the data.

While the Social Uprising may have had other effects that complemented the anticipation channel (such as a decline in current demand or higher realized uncertainty), we find that

the only alternative mechanism consistent with the observed pricing dynamics is a contemporaneous and temporary increase in menu costs. The critical distinction is that a pure menu cost shock would not alter reset prices, whereas the anticipation of greater future demand dispersion would. We test this difference using markup dynamics during the Social Uprising and find that markups rose in this period. This pattern rules out menu costs as the sole driver of behavior, since a one-period increase in adjustment costs leaves desired prices and markups unchanged. We therefore conclude that while a temporary rise in menu costs may have accompanied the anticipation effect, only news about future idiosyncratic demand volatility can jointly explain all three facts: the drop in frequency, the increase in size, and the rise in markups.[2]

Turning to the effectiveness of monetary policy, our model shows that when the central bank stimulates the economy in periods where future demand dispersion is expected to increase, monetary policy is about 24% more effective on impact relative to an intervention without such expectations. Thus, this class of uncertainty amplifies the short-run potency of policy. If there is also an increase in menu costs in addition to the arrival of news, then monetary policy becomes 62% more effective on impact. By contrast, if policy is implemented only after the news has arrived and the dispersion increase has been realized, it is virtually ineffective. This is consistent with Vavra (2014), who shows that monetary policy is less effective when idiosyncratic TFP dispersion rises contemporaneously. Taken together, our results highlight that the timing of monetary interventions relative to large aggregate events is critical: policy is most effective when firms anticipate future volatility but have not yet observed it, and least effective once that volatility is realized.

Beyond the Chilean case, our analysis points to a broader lesson. In many contemporary environments, from Brexit to highly polarized elections, firms face uncertainty that is primarily about the future rather than the present. Anticipation of structural or policy changes can alter expectations about which goods will be demanded, which sectors will be regulated, or which products will be viable, even before any change is enacted. Recognizing when such anticipation effects dominate realization effects is essential for interpreting micro evidence on pricing, for evaluating macroeconomic transmission, and for understanding when monetary policy becomes more or less effective.

Literature review. Our paper contributes to several strands of the literature. A growing literature examines the macroeconomic effects of uncertainty. Bloom (2009, 2014) quantitatively show that uncertainty shocks can decrease investment and hiring due to a

[2]Our empirical results rule out a product-specific or supermarket-specific increase in menu costs. Interpreting such a change as a national shock capturing unmodeled frictions, we also consider an experiment in which an increase in menu costs is combined with the news shock, calibrated to match all three moments.

wait-and-see behavior.[3] Fernández-Villaverde et al. (2011) studies real interest rate volatility fluctuations in emerging economies while Fernández-Villaverde et al. (2015) studies fiscal uncertainty, both in the absence of changes in the means. In both cases, higher uncertainty dampens economic activity. These papers underscore that second-moment shocks, even in the absence of changes in means, can have large real effects. We build on this foundation by examining how uncertainty affects firms' pricing behavior in a setting where marginal costs are directly observed and first-moment shocks are largely absent.

An empirical challenge in this literature is to disentangle the "anticipation channel" due to higher uncertainty of future events from the "realization channel" due to higher volatility of current shocks. Berger et al. (2019) and Dew-Becker et al. (2017) find negative economic effects of increased current shocks volatility and insignificant effects of increased expected future shocks volatility. Other papers seeking to isolate the anticipation effect are Baker et al. (2016), Jurado et al. (2015) and Kumar et al. (2023). We provide a quasi-natural experiment that isolates the uncertainty channel from the realization channel.

A related body of work explores the effect of uncertainty on firms' price-setting behavior in menu-cost models.[4] Vavra (2014) shows that price dispersion increases in recessions. Interpreting this finding as counter-cyclical idiosyncratic volatility, he shows that the realization channel typically dominates the anticipation channel in a menu cost model, leading to counter-cyclical monetary policy effectiveness.[5] Drenik and Perez (2020) also empirically relate uncertainty with price dispersion. Baley and Blanco (2019) show that higher firm-level uncertainty increases the frequency and size of price changes at the micro level, but cross-sectional heterogeneity in the degree of firm-level uncertainy amplifies monetary non-neutrality. Klepacz (2021) shows that time-varying aggregate volatility affects pricing frequency and monetary transmission. We contribute to this literature by providing direct evidence of the effect of uncertainty on both timing and magnitude of price changes. Critically, we use product-level input costs in disciplining our model and also compute markups, which help provide a novel empirical test of the anticipation channel.[6]

[3]The seminal work of Bernanke (1983), Dixit (1991) and Dixit and Pindyck (1994) theoretically show that a wait-and-see behavior arises under uncertainty for irreversible decisions.

[4]Early work on menu cost models, which jointly model the extensive and intensive margins of price adjustments, are Barro (1972), Sheshinski and Weiss (1977), Caplin and Spulber (1987), Caballero and Engel (1993) and Dotsey et al. (1999) while more recent contributions are Golosov and Lucas Jr (2007), Nakamura and Steinsson (2008), Midrigan (2011), Alvarez and Lippi (2014), and Alvarez et al. (2022).

[5]Alvarez and Lippi (2022) also show this result analytically in their Appendix C.

[6]A common practice in most menu cost models is to set the marginal cost process to match the distribution of price changes. We instead discipline the process of marginal costs using merged product, retailer, location level data on selling and suppliers prices which allows to isolate the effect of uncertainty on retailers' prices from their suppliers. Eichenbaum et al. (2011) is the only paper we know using similar data – in their case, for a single supermarket in the U.S. and focusing on the pass-through of costs to prices.

From a different angle, Ilut et al. (2020) show that Knightian uncertainty can generate infrequent price adjustment behavior. Other alternative micro foundations for nominal price rigidity are customers anger (Rotemberg, 2002) or rational inattention (Maćkowiak et al., 2023). None of these papers distinguish between the anticipation and the realization channels, while our focus is squarely on the consequences of the anticipation channel.

There is also the literature on real rigidities in menu cost models. For instance, Aruoba et al. (2025) and Gagliardone et al. (2025) use menu costs augmented with Kimball (1995) preferences. Aruoba et al. (2025) shows that a simple menu cost model with demand shocks and Kimball (1995) preferences can solve the tension between price dynamics and firm-level shocks highlighted by Klenow and Willis (2016). Although our model has common features with Aruoba et al. (2025), both closely follow existing menu cost models except for the introduction of Kimball (1995) preferences and idiosyncratic demand shocks. In turn, our approach departs from Aruoba et al. (2025) in three key ways. First, we introduce leptokurtic productivity shocks. Second, we incorporate elements from the news literature, generating, for the first time in the pricing literature, a pure "wait-and-see" effect. Third, our model is directly calibrated to micro-pricing facts we obtain from our data, including costs.

A large empirical literature studies price setting during rare or disruptive events.[7] Hobijn et al. (2006) examine the introduction of the Euro, Gagnon (2009) studies exchange rate pass-through during Mexico's 1994 crisis, and Alvarez et al. (2019) analyze pricing in Argentina's large inflation swings. During the COVID-19 period, Montag and Villar (2023) and Henkel et al. (2023) document reduced stickiness, while Jaravel and O'Connell (2020a,b) report increased use of sales. In many of these papers the shocks reflect realized disruptions, supply shortages, lockdowns, or logistical bottlenecks, which increase the volatility of shocks today. In case of COVID-19, it is conceivable that some first-moment shocks are also in play. By contrast, we show that the 2019 Chilean Social Uprising produced a second-moment shock to the perceived volatility of idiosyncratic demand, without contemporaneous cost changes. This distinction allows us to isolate the anticipatory component of pricing behavior using a quasi-experimental design. Broadly consistent with our empirical findings is Cavallo et al. (2014) which documents greater nominal price rigidity after large earthquakes in Chile and Japan.

The remainder of the paper is structured as follows. Section 2 describes our unique daily pricing panel data. Section 3 describes the Chilean Social Uprising that we use as a quasi-natural experiment. Section 4 presents our empirical analysis showing the pricing effects

[7] More broadly on the effect of rare disasters, Barro (1972) and Gabaix (2012) study them theoretically and some empirical examples include Baskaya and Kalemli-Ozcan (2016) who use the 1999 earthquake in Turkey, Acemoglu et al. (2018) who use the Arab Spring in the early 2010s, Boehm et al. (2019) and Wieland (2019) who use the 2011 earthquake in Japan.

of the Social Uprising. Section 5 presents the quantitative model, and Section 6 shows its calibration, and the effects of demand uncertainty on pricing. Section 7 explores the policy implications of our findings. Finally, Section 8 concludes.

2 Data

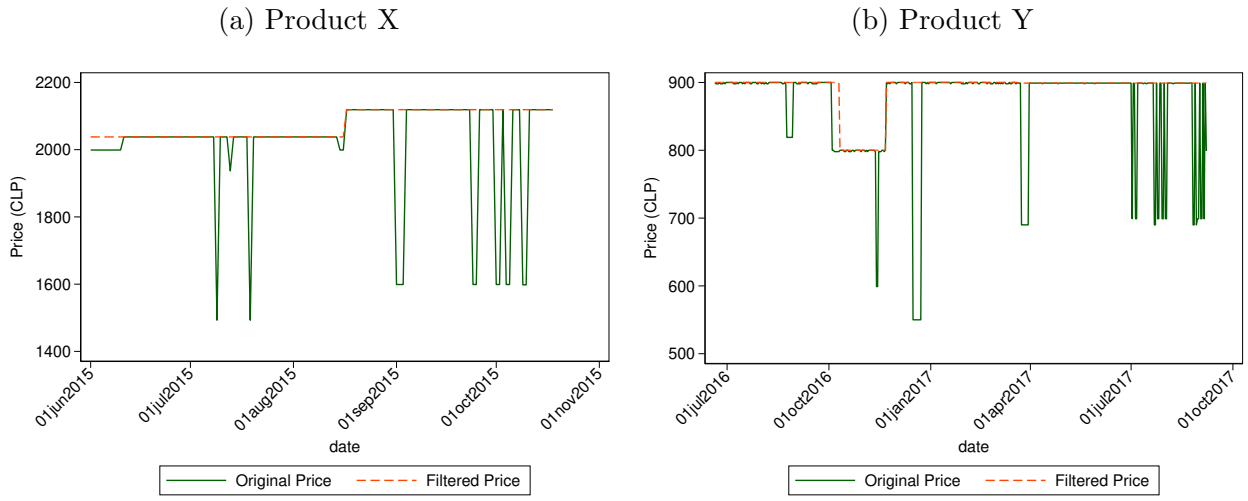
We use a business-to-business (B2B) transaction-level dataset built from electronic invoices ("Factura Electrónica" in Spanish) collected by the Chilean Tax Authority (Servicio de Impuestos Internos) and provided anonymously to the Central Bank of Chile (CBC). The coverage and granularity of the information in this dataset are unique, providing a record of the date, product description, buyer, seller, price, and quantity for the universe of B2B transactions. We focus on the period from January 2015 to November 2019 to minimize the effect of the Covid-related lockdown in March 2020. Appendix B.1 provides further details about the construction of the dataset.

Our goal in the empirical part of the paper is to examine how the pricing behavior of firms in Chile responded to the Social Uprising. We focus on transactions at supermarkets, which are a common subject in both micro and macro studies of price setting. Supermarkets are ideal for this analysis because they post uniform shelf prices for the goods they sell. Apart from occasional discounts, either general or conditional on a coupon or loyalty program, they do not price discriminate. Therefore, when we observe the maximum price for a good within a day, it typically corresponds to the posted shelf price. Since our dataset includes B2B transactions, we study the prices at which goods are purchased by firms in supermarkets. These transactions account for about 16% of total supermarket sales in Chile with the rest being recorded in the consumer-facing "Boleta Electrónica" system, which is only available since 2021. We do not rely on quantities from Factura data in our analysis. Instead, we use these detailed transaction data to observe shelf prices directly. Prices recorded in the Factura data closely match those in the consumer-facing Boleta data for overlapping periods and products.[8]

A product is defined as a unique triplet of a supermarket (seller's ID), a location ID, and a product description. Our baseline sample consists of 33.7 million invoices. We aggregate the transaction-level price data to daily frequency by taking the intra-day maximum price for each product triplet. This procedure filters out intra-day discounts that may be applied to some transactions but not others. We then apply the filter proposed by Kehoe and Midrigan

[8]In Appendix C.1.1 we show that for the year 2023, a period outside the period we study, but one where both data are available, on average 60% products in Boleta data also appear in the Factura data, and for these products the simple correlation of prices we observe is 0.98.

Figure 2: Original and Filtered Prices: Two Products in the Dataset



Notes: The figure presents the original and filtered prices of two randomly selected products in the dataset. The original price is defined as the intra-day maximum price observed at a supermarket-branch location. The filtered price applies the method in Kehoe and Midrigan (2015) to remove short-term price fluctuations.

(2015) to eliminate high-frequency variation, especially short-lived sales. Figure 2 shows the original and filtered daily prices for two randomly selected products. As expected, the filter removes transient spikes and reverts prices to their modal levels.

In addition to this filter, we impose a continuity condition that retains products exhibiting at least one uninterrupted spell of 20 consecutive weeks, during which they are sold on no fewer than three days per week. This condition ensures that each product has a sufficiently dense set of observations to allow for precise identification of both price changes and periods of price stability. Furthermore, because the COVID-19 pandemic introduced substantial disruptions to supermarket sales, we terminate the sample in November 2019 to maintain as balanced a sample as possible under the continuity requirement. These procedures yield our baseline sample, which includes 13.8 million product-day observations across 39,679 products sold in 183 supermarkets, spanning 766 locations, and covering the period from January 1, 2015, to November 17, 2019.

The granularity of the B2B invoice data allows us to complement these final prices with information on the prices charged by suppliers. We do this by examining transactions in which supermarkets appear as buyers. Because the data lacks standardized product codes (such as UPCs), we apply a fuzzy matching algorithm based on textual product descriptions. Appendix B.1 provides further details on the matching methodology. Each product in the matched sample is defined as a unique combination of a supermarket, a location, a product description, and a matched supplier ID. The resulting matched sample includes two million

Table 1: Descriptive Statistics

	Baseline sample		Matched sample		Suppliers sample	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Price setting						
Total breaks	0.0117	0.1075	0.0127	0.1118	0.0106	0.1023
Positive breaks	0.0065	0.0801	0.0067	0.0815	0.0067	0.0814
Negative breaks	0.0053	0.0723	0.0060	0.0772	0.0039	0.0625
Size, positive	0.1150	0.1289	0.1001	0.1082	0.0941	0.1173
Size, negative	0.1237	0.1391	0.1056	0.1202	0.1254	0.1528
Sample information						
Number of supermarkets	183		94		–	
Number of suppliers	–		295		295	
Number of supermarket locations	766		490		–	
Number of product IDs	39,679		6,511		2,016	
Number of product descriptions	13,682		1,920		1,923	
Number of observations	13,845,147		1,980,403		836,004	
Municipalities covered / Total	194 / 346		164 / 346		–	
Provinces covered / Total	50 / 56		48 / 56		–	
Regions covered / Total	16 / 16		16 / 16		–	

Notes: This table presents descriptive statistics for the three samples used in the analysis. The upper panel summarizes price-setting behavior, and the lower panel provides sample characteristics. The period of analysis spans January 1, 2015 through November 17, 2019.

daily price observations for 6,511 products, sold by 94 supermarkets across 490 locations, and matched to 295 distinct suppliers.

We also construct a third sample, the suppliers sample, to analyze the pricing decisions of the suppliers themselves. Here, products are defined as a triplet of supplier ID, supermarket ID (buyer), and product description. Because the location of the supermarket is not recorded in these transactions, each supplier-supermarket pair corresponds to a single product ID. This sample consists of 2,016 products priced by the 295 suppliers included in the matched sample.

Table 1 presents descriptive statistics for the two supermarket samples, the baseline sample and the matched sample, as well as for the suppliers sample. The upper panel reports statistics commonly used to characterize price-setting behavior, beginning with the share of product-day observations in which a price change occurs, which we label breaks. This is then separated into positive and negative changes. We also report the average size and standard deviation of price changes, calculated using log differences. For example, Product Y in Figure 2 exhibits one positive and one negative break. The negative break corresponds to a price change of -0.1178, computed as $\log(800) - \log(900)$.

In the baseline sample, 1.17 percent of products change prices on an average day, with

price increases occurring more frequently than price decreases. The average positive change is 11.5 percent, and the average negative change is 12.4 percent. The matched and supplier samples display similar patterns. Our dataset includes supermarkets across a broad range of regions and municipalities in Chile and geographically representative. Our baseline sample includes information across 194 municipalities out of a total of 346 (56% coverage), 50 Provinces out of a total of 56 (89% coverage), and all 16 Regions. Appendix B.2 presents additional descriptive statistics and compares the properties of the Chilean data to results in the related literature.

3 Social Uprising as a Quasi-Natural Experiment

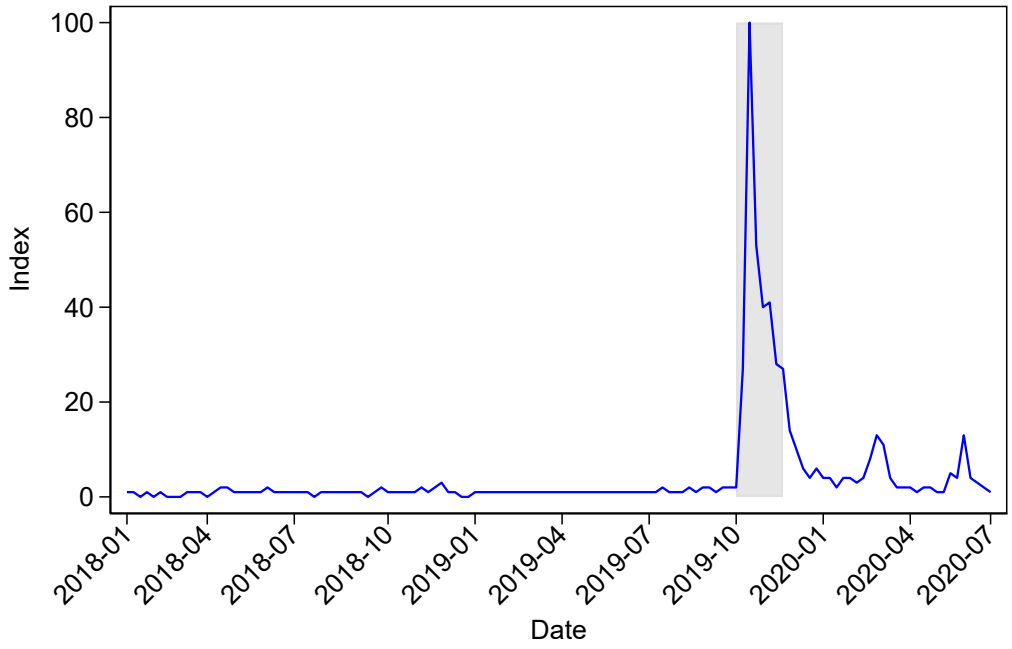
On October 6, 2019, Santiago's subway fare was raised by 4 percent, equivalent to about $0.05 USD. Students responded with peaceful demonstrations and some limited disruptions to the subway system. The situation changed on October 18, when large disturbances erupted throughout the Santiago subway network. Despite an early police response, by the morning of October 19, a significant portion of Santiago's metro infrastructure had been damaged. For the next month there were widespread riots across the country, often leading to destruction of property. On November 15, roughly one month after the onset of the Social Uprising, a broad political agreement was reached among several parties to begin the process of constitutional reform. This agreement largely brought the unrest to an end.

While a detailed account of these events is beyond the scope of this paper, scholars agree that the unrest was not only about the fare increase. Instead, it reflected a deeper and more widespread dissatisfaction in the society. The initial violence quickly evolved into a national social movement, with leaders across sectors voicing demands related to education, health care, pensions, and wages. What began as student-led demonstrations transformed into a broad call for a new social contract, captured by the slogan "Chile cambió" (Chile changed). In response, political, social, and business leaders acknowledged the need for structural reform, which generated heightened expectations and increased uncertainty about Chile's future.[9]

From the perspective of this study, the Social Uprising had three defining characteristics. First, it was fully unexpected but relatively short-lived, making it particularly suitable as a quasi-natural experiment. Figure 3 shows a daily index of Google searches for "protestas" (protests) originating in Chile. The index increases one hundred-fold between October 17

[9]See Joignant and Garrido-Vergara (2025) for a detailed account of the 2019 Uprising, including its origins in structural inequality, the range of groups involved, the role of social media, and how it ultimately led to the launch of Chile's constitutional reform process.

Figure 3: Google Trends for "Protestas" (Protests)



Notes: Figure shows a daily index of Google searches for "protestas" (protests) originating in Chile. The index is normalized so that the maximum value, on October 19, 2019, equals 100. The shaded area covers October 18 to November 17, 2019.

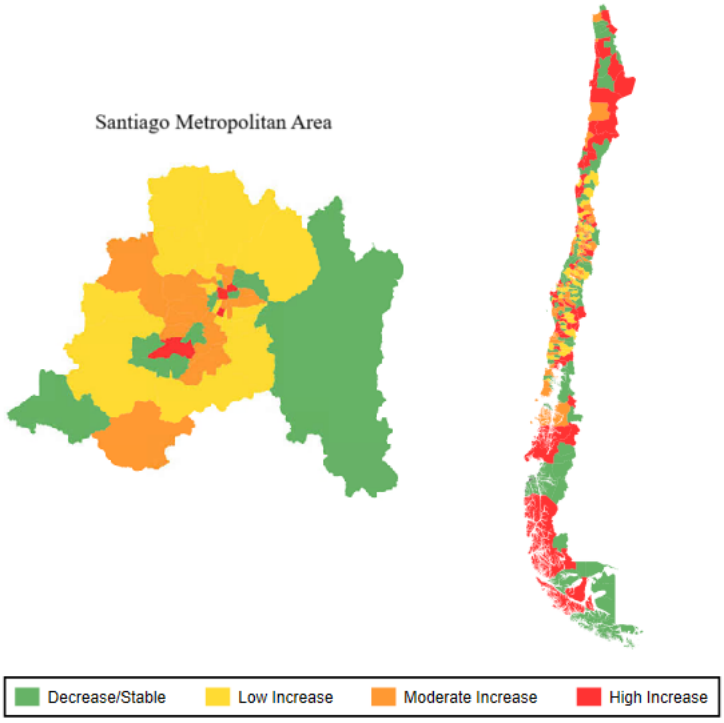
and October 19, indicating a sharp and sudden rise. By November 17, however, search volume had returned to near pre-Social Uprising levels. This also roughly coincides with the November 15 agreement. We treat this 31-day window, marked by the shaded area in the figure, as the most intense phase of the Social Uprising.

Second, the Social Uprising was widespread, but the intensity of its violent component varied significantly across locations. Figure 4 displays a municipality-level map of riot intensity across Chile, with a close-up of the Santiago Metropolitan Area. Riot intensity is measured by the increase in public disorder reports, a category that includes damage to both public and private property, relative to the same period in the previous year, adjusted for population. The map reveals substantial variation across and within regions, without a clear geographical pattern. We exploit this heterogeneity in our empirical analysis.

Third, uncertainty rose sharply in the aftermath of the Social Uprising. Figure 5 provides supporting evidence. The figure reports the standard deviation across individual forecasts in the Central Bank of Chile's Economic Expectations Survey for two questions: inflation expected by year-end, and the expected 12-month growth rate of the IMACEC index excluding mining.[10] This survey is the central source used by the Bank to gauge market expectations

[10] As discussed by Bloom (2014), dispersion in professional forecasts is often used as a proxy for firm-level

Figure 4: Geographical Distribution of Riot Intensity



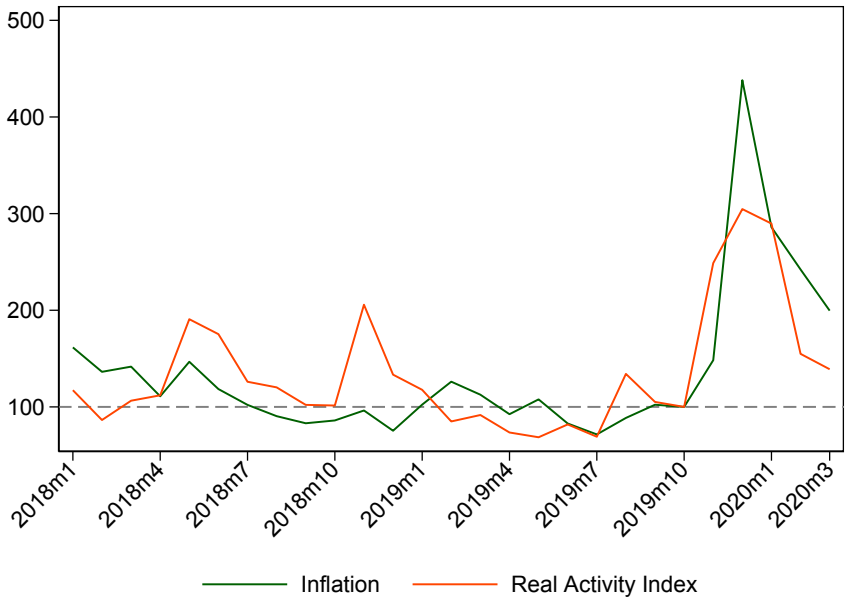
Notes: The map on the right shows riot intensity across all municipalities in Chile. The map on the left zooms into the Santiago Metropolitan Area. Riot intensity is measured as the change in the number of police reports for public disorder in October and November 2019, relative to the same period in 2018, adjusted for population. Municipalities are grouped into four categories: green indicates no increase or a decline, yellow corresponds to the lowest third of increases, orange to the middle third, and red to the top third.

and is based on input from a wide range of experts, including market participants and academics. Both measures of forecast dispersion increase substantially after October 18. For real activity, the cross-sectional standard deviation triples. For inflation, it increases nearly fivefold. These levels are without precedent in the historical series.

Before turning to our main empirical analysis, we illustrate the effect of the Social Uprising on pricing behavior using raw daily data from the baseline sample. Figure 6 presents the daily frequency and average size of price changes throughout 2019. Panel (a) shows the share of product prices that change each day, which is our measure of the average frequency of price changes. During the 31-day period following October 18, marked by the shaded area, there is a clear and abrupt decline in the frequency of price changes, falling below

uncertainty. Extending the work by Baker et al. (2016), Ahir et al. (2022) builds a World Uncertainty Index (WUI) for a large set of countries using the frequency of the word "uncertainty" in the quarterly Economist Intelligence Unit country reports. The WUI for Chile –available on FRED with code WUICHL–shows a similar pattern during this period. It averages 0.18 for the period 2010Q1-2019Q3 and jumps to 1.07 in 2019Q4, the quarter that includes the Social Uprising, which is a 6-fold increase.

Figure 5: Uncertainty as Proxied by Standard Deviation Across Forecasters



Notes: The figure shows the monthly standard deviation of two forecasts from the "Encuesta de Expectativas Económicas" (Economic Expectations Survey) conducted by the Central Bank of Chile. The two variables are inflation expectations for December of the current year and the expected 12-month change in the IMACEC index excluding mining. The standard deviations are normalized to 100 just prior to the Social Uprising.

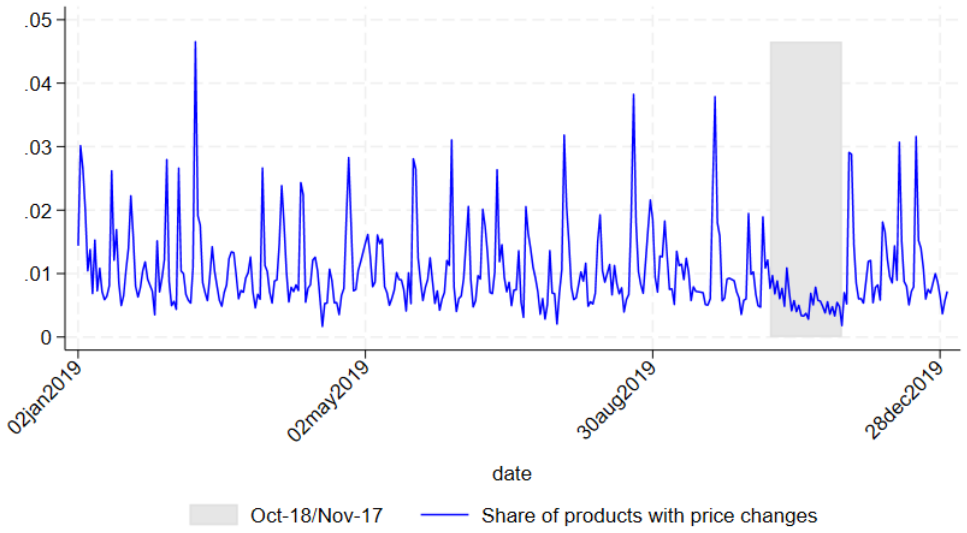
0.6 percent compared to a pre-Social Uprising average of 1.1 percent. Panel (b) shows a simultaneous increase in the average absolute size of price changes, rising from 13.6 percent to 17.4 percent, a 28 percent increase. Because of our continuity restriction, these results are based on products that remained continuously available throughout the Social Uprising. Therefore, the observed changes cannot be explained by store closures, inventory disruptions, or changes in product composition. In short, we observe a clear shift in pricing behavior along both the frequency and magnitude of adjustments during the Social Uprising period.[11]

4 Empirical Results

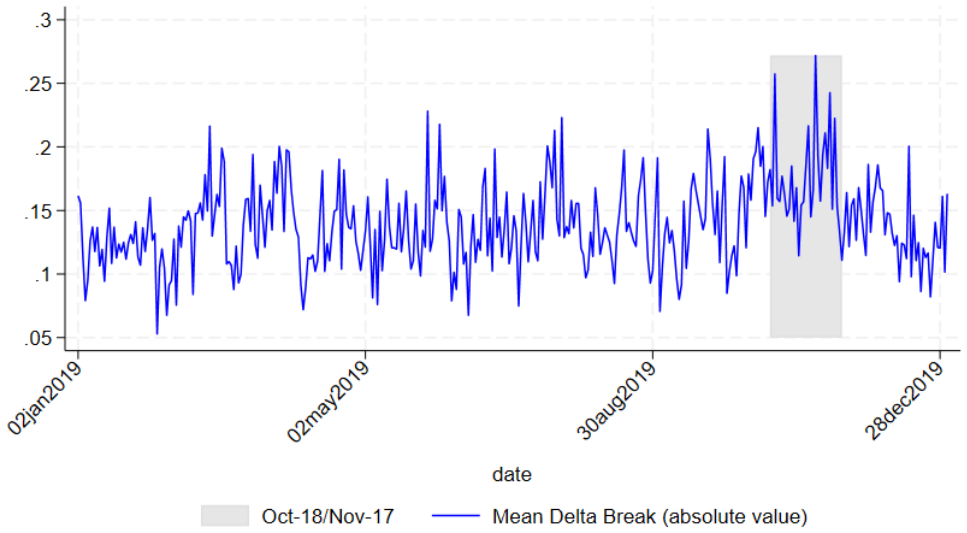
Having established that the Social Uprising satisfies the conditions of a quasi-natural experiment, we now assess its effect on the pricing behavior of supermarkets. We proceed in three steps. First, we analyze the baseline sample to examine how the frequency and size of price changes evolved during the Social Uprising period. The patterns observed in the raw data persist: the frequency of price changes declines, while the absolute size of price changes

[11]Table A-6 in Appendix C.1.2 reports the share of prices that changed during this period and the distribution of price changes across percentiles. The distribution displays noticeably fatter tails during the Social Uprising, consistent with wider inaction bands and greater selection into large adjustments.

Figure 6: Frequency and Size of Price Changes in 2019



(a) Frequency of Price Changes



(b) Size of Price Changes

Notes: Panel (a) shows the daily frequency of price changes in 2019, computed as the share of product prices that change on a given day. Panel (b) shows the average size of daily price changes in absolute value. Both panels are based on the baseline sample. The shaded area covers the period from October 18 to November 17, 2019.

increases.

Second, we investigate whether these changes could be attributed to upstream price adjustments. To do so, we analyze the pricing behavior of suppliers to the supermarkets in

our sample. We find no evidence that supplier pricing changed during the Social Uprising period.

Third, we explore whether supermarkets in areas with more intense unrest responded differently, exploiting the geographic variation in riot intensity shown in Figure 4. This allows us to test whether pricing behavior responded to localized disruptions—such as looting or transportation breakdowns—or to a national, forward-looking shift in expectations. We again find no evidence of heterogeneous responses by riot intensity.

We present these results in sequence, followed by a summary of robustness checks. We conclude the section by taking stock of what we have learned and setting up the model-based analysis in the next section.

4.1 Baseline Results for Supermarkets

Our main empirical specification is given by the following equation:

$$y_{it} = \text{Fixed Effects} + \beta D_t + \gamma_1 X_{1,it} + \gamma_2 X_{2,t} + \varepsilon^y_{it}, \tag{1}$$

where y_{it} is one of four dependent variables measured at the product-day level: a binary indicator for a price change (separately for positive and negative changes), or the absolute size of a price change (conditional on a change, again separated by sign). The variable D_t is a dummy equal to one during the 31-day Social Uprising period beginning on October 18, 2019. The vector $X_{1,it}$ includes time-varying product-level controls, and $X_{2,t}$ includes economic activity controls measured at the supermarket or national level. The coefficient of interest, β, captures the average change in pricing behavior during the Social Uprising.

The unit of observation is a product-store-day. Each product is defined as a triplet of product description, supermarket chain, and store location. For example, "milk, in branch 5 of supermarket chain X" constitutes a distinct product. We include product fixed effects, which control for all time-invariant characteristics at this level, including location, retailer identity, and persistent product characteristics. As a result, our identification of β comes from changes in pricing behavior for the same product at the same store over time—comparing before and during the Social Uprising period.

The fixed effects in (1) include: product, day of the week, week of the month, month of the year, and non-mandatory holidays. These account for regular temporal variation in pricing, such as Monday price cycles or seasonal patterns.

The vector $X_{1,it}$ includes a third-order polynomial in the number of days since the last price change, along with a count of price changes for the product in the preceding 30 days. These controls capture state-dependent pricing dynamics. The vector $X_{2,t}$ includes two

Table 2: Change in Pricing Behavior During the Social Uprising, Supermarkets (Baseline Sample)

Variables	(1) Positive Breaks	(2) Negative Breaks	(3) Size Positive	(4) Size Negative
D	-0.0026***	-0.0031***	0.0338***	0.0488***
	(0.0003)	(0.0002)	(0.0129)	(0.0097)
Observations	13,845,147	13,845,147	79,576	63,588
Adjusted R-squared	0.002	0.002	0.425	0.471
Controls	Yes	Yes	Yes	Yes
Economic Activity Controls	Yes	Yes	Yes	Yes
FE	Yes	Yes	Yes	Yes
Mean of Dependent Variable	0.0065	0.0053	0.1150	0.1237

Notes: This table reports estimates of β from Equation (1), using the baseline sample. Each column corresponds to a different dependent variable: the frequency or size of positive and negative price changes, as indicated. Standard errors are clustered at the seller-location level. *** p<0.01, ** p<0.05, * p<0.1.

measures of economic activity: (i) total monthly retail sector sales, which controls for broad market trends, and (ii) total weekly purchases by each supermarket from its suppliers, capturing store-level demand shifts. This second measure is constant across all product-store combinations within a supermarket. We cluster standard errors at the seller-location level to allow for location-specific serial correlation in pricing decisions.

Table 2 presents the regression results. Columns (1) and (2) show that the frequency of both positive and negative price changes declined significantly during the Social Uprising—by approximately 0.26 and 0.31 percentage points, respectively. These effects are economically meaningful, corresponding to reductions of more than 40 percent and 60 percent relative to their pre-Social Uprising means of 0.65 and 0.53 percent.

Columns (3) and (4) present results for the size of price changes, conditional on adjustment. During the Social Uprising period, price changes became less frequent, but the average size of those that did occur increased. The mean positive price change rose by 3.38 percentage points and the mean negative change by 4.88 percentage points. These shifts represent increases of approximately 29 and 39 percent relative to their unconditional means.

The main takeaway is that during the Social Uprising period, supermarkets adjusted prices less often, but made larger adjustments when they did. Table A-7 in Appendix C.3 shows that the effects are symmetric across price increases and decreases. This symmetry suggests that the driver of the change in price dynamics has to equally effect the supermarkets' willingness to increase and decrease prices.

Table 3: Change in Pricing Behavior During the Social Uprising, Suppliers Sample

Variables	(1) Positive Breaks	(2) Negative Breaks	(3) Size Positive	(4) Size Negative
D	-0.0004	-0.0012	0.0098	-0.0105
	(0.0007)	(0.0008)	(0.0124)	(0.0196)
Observations	836,004	836,004	5,145	2,952
Adjusted R-squared	0.028	0.028	0.354	0.364
Controls	Yes	Yes	Yes	Yes
Economic Activity Controls	Yes	Yes	Yes	Yes
FE	Yes	Yes	Yes	Yes
Mean of Dependent Variable	0.0067	0.0039	0.0941	0.1254

Notes: This table reports estimates of β from Equation (1), using the suppliers sample. The dependent variables mirror those in Table 2. The economic activity control corresponds to each supplier's total sales across all customers. Standard errors are clustered at the supplier–supermarket link level. *** p<0.01, ** p<0.05, * p<0.1.

4.2 Effect of Supply Factors on Supermarkets

One possible explanation for the baseline results is that supermarkets may have simply passed through price changes implemented by their suppliers. To test this hypothesis, we turn to the suppliers sample, which includes firms that provide the goods sold by the supermarkets in our baseline sample. We estimate (1) using this sample, with price changes defined at the supplier–supermarket–product level.[12]

As shown in Table 3, we do not detect statistically significant changes in the frequency or size of supplier price adjustments during the Social Uprising period. The point estimates are small, and standard errors are relatively large—particularly in Columns (3) and (4), where sample sizes are lower due to the infrequency of supplier-side price changes. These results suggest that the shifts observed in supermarket pricing behavior are unlikely to be driven by parallel upstream cost shocks.

While we cannot observe contract terms directly, one plausible explanation is that supermarkets and suppliers operate under long-term pricing agreements. These arrangements may be subject to periodic renegotiation but are unlikely to change rapidly in response to events such as the Social Uprising. Supplier prices represent the key marginal cost input for supermarkets, and we do not find significant changes in these costs during the Social Uprising period.

Taken together, the evidence from the supplier sample indicates that the supermarket-level pricing response is not mechanically driven by upstream price increases, but more likely

[12]One may wonder whether our baseline results hold in the matched sample. Table A-8 in Appendix C shows that this is indeed the case.

reflects changes in retail pricing behavior.

4.3 Local Intensity of Social Uprising: Is it about Now?

Having ruled out supplier pricing as the primary driver of the changes observed during the Social Uprising, we now turn to the question of whether variation in local riot intensity helps explain the shift in supermarket pricing behavior. If the disruptions caused by the riots—such as store damage, transportation issues, or staffing challenges—were concentrated in specific areas, we might expect supermarkets in those areas to change their prices more sharply than those in less affected regions. This analysis allows us to test whether the observed pricing response was driven by localized contemporaneous shocks or by a broader, forward-looking shift in expectations, which would imply a more uniform response across locations.

We exploit the geographic variation in riot intensity shown in Figure 4, grouping municipalities based on the severity of the unrest. We construct two proxies for riot intensity. The first is a continuous measure, defined as the change in police reports for public disorder in October and November 2019 relative to the same period in 2018, adjusted for population. This is the measure shown in Figure 4. The second is a binary indicator equal to one if the municipality experienced a disorder increase above the national median.

To estimate whether the pricing response varied by local riot intensity, we interact each proxy with the Social Uprising period dummy and estimate the following regression:

$$y_{it} = \text{Fixed Effects} + \beta D_t + \theta D_t \times \text{Intensity} + \gamma_1 X_{1,it} + \gamma_2 X_{2,t} + \varepsilon^y_{it}, \tag{2}$$

where the variable *Intensity* is either the continuous or binary measure. Note that the direct effect of *Intensity* is absorbed by the product fixed effect, since product is defined at the product–supermarket–location level.

Table 4 presents the results. Panel A reports estimates using the continuous proxy, and Panel B uses the binary version. In both cases, the coefficient on the Social Uprising period dummy (β) remains significant and similar in magnitude to the estimates in Table 2, consistent with a 40–60 percent decline in the frequency of price changes and a 30–40 percent increase in their size.

The interaction terms (θ) are statistically insignificant across all specifications, and the point estimates are very small relative to the corresponding β and the unconditional mean of the dependent variables with one exception. This suggests that riot severity at the local level did not materially alter the pricing response. They are consistent with a nationally shared response—potentially reflecting an anticipatory shift in expectations rather than direct operational disruptions at the local level.

Table 4: Supermarket Analysis: Baseline Sample and Riot Intensity

Variables	(1) Positive Breaks	(2) Negative Breaks	(3) Size Positive	(4) Size Negative
Panel A: continuous riots intensity measure				
D	-0.0033***	-0.0032***	0.0281*	0.0464***
	(0.0005)	(0.0004)	(0.0154)	(0.0142)
D * intensity	2.79e-05	5.34e-06	0.0002	9.31e-05
	(1.76e-05)	(9.47e-06)	(0.0005)	(0.0004)
Observations	13,845,147	13,845,147	79,576	63,588
Adjusted R-squared	0.002	0.002	0.425	0.471
Controls	Yes	Yes	Yes	Yes
Economic Activity Controls	Yes	Yes	Yes	Yes
FE	Yes	Yes	Yes	Yes
Mean of Dependent Variable	0.0065	0.0053	0.1150	0.1237
Panel B: dummy riots intensity measure				
D	-0.0026***	-0.0023***	0.0343**	0.0721***
	(0.0006)	(0.0005)	(0.0159)	(0.0265)
D * intensity	3.60e-05	-0.0009	-0.0005	-0.0272
	(0.0007)	(0.0006)	(0.0213)	(0.0286)
Observations	13,845,147	13,845,147	79,576	63,588
Adjusted R-squared	0.002	0.002	0.425	0.471
Controls	Yes	Yes	Yes	Yes
Economic Activity Controls	Yes	Yes	Yes	Yes
FE	Yes	Yes	Yes	Yes
Mean of Dependent Variable	0.0065	0.0053	0.1150	0.1237

Notes: This table reports the estimated coefficients β and θ from Equation (2), using two alternative proxies for riot intensity. The continuous proxy is the change in public disorder reports in October–November 2019 relative to the same period in 2018, adjusted for population. The binary proxy equals one if this change is above the national median. Standard errors are clustered at the seller-location level. *** p<0.01, ** p<0.05, * p<0.1.

4.4 Robustness and Extensions

In addition to providing further descriptive statistics, Appendix C presents a series of robustness checks and extensions to our main empirical findings reported in Table 2. These tests serve two purposes: to evaluate whether the effects we estimate are sensitive to specification choices, and to explore whether alternative mechanisms—such as unmodeled costs or omitted shocks—could plausibly account for the results.

Raw data and controls. As shown in Figure 6, the drop in price change frequency and the rise in average size are visually apparent in the raw daily data. To assess the role of our control variables, we re-estimate Equation (1) while progressively removing controls. Tables A-9, A-10, and A-11 show that the main results are robust: frequency effects remain similar, and size effects are even slightly stronger when controls are excluded. In particular, dropping all controls increases the estimated size effect to 4.6 percentage points for positive changes and 6.2 percentage points for negative changes.

Pooled direction of price changes. Our baseline regressions separate price increases and decreases. In Table A-12, we pool all price changes and find results consistent with the separate regressions: the frequency of price changes falls by 0.57 percentage points, and the average size increases by 3.8 percentage points. Figure A-1, which plots residuals from this pooled regression (excluding the Social Uprising dummy), visually confirms the shift during the Social Uprising period.

Filtering. One concern is that our application of the Midrigan (2011) filter may understate true pricing activity if supermarkets responded to the Social Uprising with short-lived price changes (e.g., promotions or rapid reversals). To evaluate this, Table A-13 presents results using unfiltered (raw) prices. We find that the estimated decline in frequency is even larger—1.2 to 1.6 percentage points, nearly five times the size of the filtered estimate. The size effect is somewhat smaller (2.6 percentage points) but remains statistically significant. These results suggest that our filtering approach is, if anything, conservative. Another robustness check we considered was using a balanced panel of products that pass our continuity restriction for every week in the period September 23, 2019 to November 11, 2019. Table A-14 shows the results for 6,973 products that satisfy this criterion. The decline in frequency for these products is about 30% smaller than the baseline results, though still highly significant and the change in size of prices very similar.

Fixed effects and supermarket-wide pricing. Our baseline specification includes product-level fixed effects (defined at the product–store–chain level). One might worry, however, that if supermarket chains coordinate pricing across stores, as documented by DellaVigna and Gentzkow (2019), these fixed effects could miss higher-level policies. Table A-15 replaces product fixed effects with chain-level fixed effects. The results remain robust, and the estimated size effects are slightly larger. In Table A-16, we also interact the Social Uprising dummy with an indicator for supermarket size (based on monthly sales at supermarket-location level) and find no evidence of differential effects between large and small chains.

Geographic aggregation. In Section 4.3, we analyze riot intensity at the municipality level. To address the concern raised above, that supermarkets may choose price policies centrally, we repeat the same analysis using supermarket-level exposure to unrest, defined as a weighted average of riot intensity across municipalities where the supermarket operates. Table A-17 presents these results, which largely replicate those in Table 4: local intensity is not significantly associated with differences in pricing behavior, reinforcing the view that the pricing response reflects a nationally shared shift in expectations.

Additional controls. Finally, we augment our baseline regressions with several new control variables. Table A-18 adds the daily CLP/USD exchange rate; Table A-19 includes each supermarket's monthly wage bill; and Table A-20 adds the most recent price change by the product's supplier (in the matched sample). Across all three specifications, the coefficient on the Social Uprising dummy remains stable and statistically significant. These findings suggest that the estimated effects are not confounded by exchange rate pass-through, labor cost pressures, or contemporaneous changes in restocking costs—echoing the supplier-side results in Table 3.

Taken together, these robustness checks reinforce the conclusion that the observed drop in frequency and rise in size of price changes during the Social Uprising are not an artifact of control variables, fixed effects structure, filtering, or compositional changes in the data.

4.5 Summary of Empirical Results

Our results show that during the 31-day period following the outbreak of the Social Uprising on October 18, 2019, supermarkets in Chile changed the way they set prices. The frequency of price changes declined by between 40 and 60 percent, while the size of price changes, conditional on an adjustment, increased by between 29 and 39 percent compared to the pre-Social Uprising period. Due to the continuity requirement we used in constructing our data, these results are identified by products that are sold both during and before the Social Uprising and are not influenced by store closures during the Social Uprising.

In contrast, the firms that supply goods to supermarkets show no change in their pricing behavior. We enhanced these results by directly controlling for other costs of supermarkets and the results are very similar to the baseline results. These findings suggest that the shift in supermarket behavior was not driven by cost pass-through from suppliers, or other supply-side concerns, but instead by factors on the demand side.

Moreover, we find no relationship between the local intensity of unrest and the change in pricing behavior. This implies that the changes were not driven by direct operational disruptions but by concerns shared by all supermarkets. In other words, it seems like the supermarkets were not reacting to what they see in front of their door, but to what they see on the news. The changes in pricing behavior we identified are also symmetric across price increases and decreases, indicating that they could not be driven by concerns about drawing criticism for increasing prices during the Social Uprising which was triggered by a price increase. Taken together with the sharp rise in uncertainty documented in the months following the Social Uprising—as measured by forecast disagreement in the Central Bank of Chile's survey—these findings point to an anticipatory mechanism.

We interpret these results as consistent with a scenario in which firms anticipated greater

dispersion in future demand but did not yet observe it. While other channels may also have played a role, our findings support the plausibility of a forward-looking pricing response to anticipated uncertainty. In the next section, we show that such a response can be generated within a quantitative model, and we explore its implications for monetary policy. We also consider a range of other explanations within the context of the model and conclude that the evidence in the data necessarily points to an anticipation effect.

5 Quantitative Model

In the rest of the paper, we turn to a calibrated quantitative model to accomplish two tasks. First, we use the model to demonstrate that *news* about a possible increase in the dispersion of future idiosyncratic demand—i.e., an anticipation effect—can generate the empirical results we documented during the Chilean Social Uprising, and that alternative explanations by themselves cannot. Second, we investigate how the arrival and timing of such news influence the effectiveness of monetary policy.

The model builds on the menu cost framework of Vavra (2014), which was the first to show that uncertainty—modeled as a contemporaneous increase in idiosyncratic dispersion—can reduce the effectiveness of monetary policy by increasing the frequency of price adjustment. In contrast, we study an *anticipation shock*, in which dispersion is expected to rise in the future but remains unchanged today. This distinction reverses Vavra's conclusion: when firms expect greater volatility ahead, they defer price adjustment, widening the inaction region and increasing the real effects of monetary policy. Thus, while we adopt his quantitative structure, we show that the implications of uncertainty depend critically on its timing.

Our model also closely follows Aruoba et al. (2025), which introduces two necessary ingredients. First, we include idiosyncratic demand shocks in addition to productivity shocks. In normal times, which we use to calibrate the model, these follow a standard AR(1) process with constant innovation variance. Second, we replace the standard constant elasticity of substitution (CES) aggregator with the Kimball (1995) aggregator, which generates variable markups and strategic complementarities in price-setting.[13] This step is essential: under CES, idiosyncratic demand shocks do not affect optimal prices, precluding any anticipation effect.

A final distinguishing feature of our approach is empirical: unlike Vavra (2014), who infers costs indirectly, we observe both prices and input costs at the product level and directly

[13]Non-CES aggregation such as Kimball is commonly used in international finance and trade, where constant markups and complete pass-through of costs are inconsistent with the data. See Gopinath and Itskhoki (2010) and Amiti et al. (2019) for recent applications.

calibrate the model to micro data from Chilean supermarkets and their suppliers. This allows us to match key pricing moments—such as cost pass-through and markup behavior—and to identify the model more precisely. Crucially, it also enables us to empirically distinguish between competing mechanisms: while both menu cost and news shocks can explain changes in frequency and size, only the news channel affects reset prices relative to cost, a difference we capture using markup dynamics in the data.

Throughout this section, we use x, x' and x_{-1} to denote the value of a generic variable in the current, next and previous periods, respectively. Much of the detail is relegated to Appendix E, including the household's problem, which is entirely standard.[14]

5.1 Final Good Producer

A representative firm aggregates intermediate varieties y^i into a final good Y using the implicit aggregator introduced by Kimball (1995), defined as

$$\int_0^1 G\left(\frac{n^i y^i}{Y}\right) di = 1, \tag{3}$$

where n^i is a variety-specific demand shifter that scales consumer preferences for good i. As in Aruoba et al. (2025), we allow n^i to evolve stochastically and to affect not only the level but also the elasticity of demand. This feature is essential for capturing variation in markups in response to changes in local demand conditions.

We adopt the functional form for $G(\cdot)$ used in Dotsey and King (2005) and Harding et al. (2021)

$$G\left(\frac{n^i y^i}{Y}\right) = \frac{\omega}{1+\omega\psi}\left[(1+\psi)\frac{n^i y^i}{Y} - \psi\right]^{\frac{1+\omega\psi}{\omega(1+\psi)}} + 1 - \frac{\omega}{1+\omega\psi}, \tag{4}$$

where ω governs the baseline price elasticity of demand in the symmetric equilibrium, and $\psi \leq 0$ controls how that elasticity varies with quantity (i.e., the super-elasticity). The steady-state markup is jointly determined by both ω and ψ.

Given a vector of prices $\mathbf{p} = \{p^i\}_{i\in[0,1]}$, price for the final good P and total final output Y, the final-good producer chooses intermediate inputs $\{y^i\}$ to maximize profits

$$\max_{\{y^i\}} \quad 1 - \int_0^1 \frac{p^i y^i}{PY} di \quad \text{subject to} \quad \int_0^1 G\left(\frac{n^i y^i}{Y}\right) di = 1. \tag{5}$$

The solution to this problem yields a system of implicit demand functions for each variety,

[14] A representative household supplies labor to firms in exchange for wage payments, trades a complete set of Arrow-Debreu securities, and consumes a final good. It also owns all firms in the economy and receives all accrued profits.

$y^i = y(p^i, n^i; P, Y, \lambda)$ where the unit cost index $P(\mathbf{p})$ and the Lagrange multiplier $\lambda(\mathbf{p})$, both of which depend on the full vector of prices.[15]

When $\psi = 0$, the aggregator collapses to the familiar CES case. In this limit, n^i enters demand purely as a multiplicative taste shifter: it affects quantities but not markups. This is exactly the structure analyzed in Redding and Weinstein (2020), where demand shocks scale revenue but leave pricing incentives unchanged. With $\psi < 0$, by contrast, the elasticity of demand decreases in n^i—so that firms facing higher demand optimally charge higher markups. This feature is crucial for generating a pricing response to demand shocks in our model. Aruoba et al. (2025) provides a detailed description of how the Kimball (1995) aggregator compares to the more standard CES aggregator, in particular under idiosyncratic demand shocks.

5.2 Intermediate Producers

A continuum of intermediate producers, indexed by $i \in [0, 1]$, each produce a differentiated variety using linear technology: $y^i = z^i h^i$, where z^i is idiosyncratic productivity and h^i is labor input. Since all producers are ex-ante identical and we focus on a symmetric equilibrium, we omit the i superscript where convenient.

Idiosyncratic productivity z evolves according to a Poisson AR(1) process, as in Midrigan (2011) and Vavra (2014)

$$\log z = \begin{cases} \rho_z \log z_{-1} + \sigma_z \epsilon^z, \quad \epsilon^z \sim \mathcal{N}(0,1) & \text{with probability } p_z, \\ \log z_{-1} & \text{with probability } 1 - p_z. \end{cases} \tag{6}$$

Idiosyncratic demand n follows a standard AR(1) process

$$\log n = \rho_n \log n_{-1} + \sigma_n \epsilon^n, \quad \epsilon^n \sim \mathcal{N}(0,1). \tag{7}$$

Following Aruoba et al. (2025), we separate the firm's problem into two stages. First, the firm chooses its nominal price p, taking as given the price of the final good P, aggregate output Y, multiplier for the final-good problem λ, and the implied demand function $y(p, n; P, Y, \lambda)$. In solving this problem, the firm faces a fixed cost f expressed in terms of labor units to change prices. Thus at the start of each period, the firm decides whether to re-optimize and pay f, or keep $p = p_{-1}$. Second, conditional on the resulting quantity y, it hires labor to meet demand following $h = y/z$, which is the solution to the cost minimization

[15] Under the Kimball specification, a single scalar price index does not suffice to summarize cross-variety interactions; see Matsuyama (2025) for a detailed discussion.

problem.

The effect of demand n on pricing operates through the Kimball aggregator discussed earlier: firms with higher n face less elastic demand and charge higher markups. Under CES, this channel would be absent—prices would depend only on cost. Under Kimball, both cost and demand fundamentals shape pricing, leading to incomplete pass-through and markup variation.[16] This variable-elasticity structure also implies strategic complementarities in price-setting. Deviating from the average price becomes more costly when demand is less elastic, steepening the profit function near the optimal price. These interactions are central to the model's predictions about inertia and the effects of policy.

5.3 Equilibrium

We focus on a symmetric stationary equilibrium, as the model features no aggregate risk. Aggregate nominal spending is defined as $S \equiv PY$, where P is the price of the final good. We assume that S grows deterministically at a constant rate μ, such that $\log(S) = \mu + \log(S_{-1})$. Given this deterministic growth path, equilibrium consists of a time-invariant distribution over firm states (n, z, p) and associated pricing decisions that jointly satisfy firms' optimality, the aggregator constraint, and market clearing.

After calibrating the model to match the stationary steady state, we use it to study a series of experiments in which firms receive news about one-time structural changes—such as shocks to demand dispersion or monetary conditions—which in turn affect S in the current period. A formal definition of equilibrium is provided in Appendix D.4, and the full solution algorithm for the steady state is described in Appendix D.5.1.

6 Quantitative Results

We now bring the model to the data in two steps. First, we calibrate the model using Chilean microdata that includes both retail and supplier prices, allowing us to directly discipline both cost-side and pricing behavior. Second, we use the calibrated model to explore how anticipated volatility shocks—such as those triggered by the Chilean Social Uprising—affect firms' pricing decisions and the transmission of monetary policy. The next subsection details our calibration strategy.

[16] Unlike CES, there may be states in which the firm finds no profitable price that yields positive sales. In such cases, the firm remains dormant and earns zero profits. That is, if for a given (n, z) no interior solution exists that yields $y > 0$, the firm sets $y = 0$. Following Aruoba et al. (2025), we treat this as a dormancy state. For our calibration, dormancy occurs with very low probability and has negligible impact on aggregate outcomes.

Table 5: External Calibration

Parameter	Description	Value	Source
β	Discount Rate	0.997	Vavra (2014)
μ	Trend Inflation	0.37%	Nominal and Real GDP Growth
p_z	Prob. change in idio. TFP	0.19	Supplier price change frequency
ρ_z	Idio. TFP persistence	0.33	Supplier price AR(1) estimate
σ_z	Idio. TFP innovation	0.10	Supplier price AR(1) estimate
ξ	Labor disutility	1.0	Normalization

Notes: This table reports externally calibrated parameters and the empirical source or identifying moment used for each.

6.1 Calibration

We now turn to exploring the quantitative implications of our model. Our calibration strategy takes full advantage of the rich Chilean microdata, especially the matched supplier-supermarket price data. This allows us to go beyond the standard practice of targeting only pricing moments, and instead discipline the stochastic processes using direct information on both prices and costs. This approach constitutes a contribution to the quantitative menu cost literature. Consistent with standard practice, we set the model frequency to monthly. When necessary, we aggregate our daily dataset to the monthly level to construct the calibration targets.

We begin by externally calibrating a subset of parameters shown in Table 5. The discount factor is set to $\beta = 0.997$, consistent with a 3.7% annual real interest rate, and the labor disutility parameter ξ is normalized to unity. We set the trend growth rate of nominal expenditure μ to 0.37%, based on the average monthly difference between nominal and real GDP growth in Chile from 1996 to 2021.

We use the matched supermarket-supplier data to discipline the idiosyncratic productivity process. Assuming that the marginal cost of a retailer equals the supplier price of the product, we treat the inverse of the supplier price as the firm's TFP. The probability of receiving a productivity shock, p_z, is calibrated to match the observed monthly probability of a supplier price change (0.19). Conditional on a price change, we estimate the AR(1) parameters of the productivity process by regressing the log inverse of supplier prices on its lag using the Arellano and Bond (1991) panel estimator with supplier fixed effects. This yields $\rho_z = 0.33$ and $\sigma_z = 0.10$.

We then jointly calibrate the remaining parameters $(\omega, \psi, \rho_n, \sigma_n, f)$ to match five empirical moments: the average markup, cost pass-through, the fraction of price changes that are increases, the average size of price changes, and the monthly frequency of price changes. The calibrated values and moment matches are shown in Table 6. The model reproduces all five

Table 6: Internal Calibration

Parameter	Description	Value	Target Moment	Model	Data
ω	Kimball elasticity	1.32	Avg. Markup	0.37	0.37
ψ	Super-elasticity	-1.68	Cost Pass-through	0.31	0.31
ρ_n	Demand AR(1) persistence	0.76	Fraction up	0.53	0.53
σ_n	Demand AR(1) innovation	0.088	Size of change	0.11	0.11
f	Menu Cost	0.042	Frequency	0.26	0.26

Notes: The table reports internally calibrated parameters along with the model and empirical values of their associated target moments.

targeted moments exactly up to two decimal points.

The Kimball parameters (ω, ψ) control the curvature of demand and thus shape both markups and pass-through. We compute the average markup as the ratio between the observed supermarket price and the supplier price for matched products. To avoid unit mismeasurement (e.g., bulk vs. per-unit pricing), we exclude cases where the supermarket price is below the supplier price. The average markup is 37% in the data.

To estimate pass-through, we regress changes in log supermarket prices on changes in log supplier prices, conditional on observing a price change:

$$\Delta \log\left(p_t^i\right) = \beta \cdot \Delta \log\left(c_t^i\right) + \text{Product FE}_i + \epsilon_t^i, \tag{8}$$

where c_t^i is the matched supplier price. We find $\beta = 0.31$, consistent with short-run pass-through estimates in the literature (e.g., Burstein and Gopinath (2014)). The model, via $\psi < 0$, replicates this incomplete pass-through exactly.

The menu cost f is pinned down by the frequency of price changes, which is 26% per month in the data. Finally, the idiosyncratic demand parameters (ρ_n, σ_n) are jointly chosen to match the average size of price changes (11%) and the fraction of increases (53%). With $(\rho_n, \sigma_n) = (0.76, 0.088)$, the model hits both targets precisely.

6.2 Social Uprising as a News Shock

We now use the model to evaluate the impact of an uncertainty shock similar to the one we argue occurred during the Chilean Social Uprising. Our baseline experiment introduces an unanticipated, one-time news shock about a potential increase in the dispersion of future idiosyncratic demand. Specifically, at time $t = 0$, firms are told that with probability $\mathcal{P}$, the standard deviation of demand innovations in the next period ($t = 1$) will increase by a factor of D. Regardless of whether this increase materializes, the shock is known to be transitory: from $t = 2$ onward, the innovation standard deviation returns to its baseline value σ_n with

certainty.

Formally, idiosyncratic demand in $t = 1$ evolves as

$$\log(n_1) = \rho_n \cdot \log(n_0) + v \cdot \sigma_n \cdot \epsilon_1^n, \text{ where } v = \begin{cases} D & \text{with prob. } \mathcal{P}, \\ 1 & \text{with prob. } 1 - \mathcal{P}, \end{cases} \tag{9}$$

and from $t = 2$ onward, the process reverts to the baseline AR(1) form in equation (7).

Although the shock affects the economy only through beliefs in $t = 0$, it creates a departure from the stationary distribution that persists as the economy converges back. To evaluate its effects, we solve for the full transition path beginning in the steady state. Because the shock may or may not be realized, we compute the dynamics under both scenarios. We use a shooting algorithm to solve for the transitional dynamics of the full economy. Further details are provided in Appendix D.5.2.

To conduct a quantitatively meaningful experiment, we must choose values for $(D, \mathcal{P})$. Our calibration approach is guided by external evidence, not by directly targeting of the pricing moments we seek to explain. Using the monthly version of our data, we find that during the Social Uprising: (i) the frequency of price changes fell by 13.5 percentage points, and (ii) the average (absolute) size of price changes rose by 2.1 percentage points—both highly significant.[17] While no direct firm-level survey of uncertainty exists for the Social Uprising period, Figure 5 shows that professional forecasters' uncertainty measures rose by a factor of 3 to 5, suggesting a substantial shift in expectations.[18]

We set $D = 4$, the midpoint of the observed uncertainty increase, and $\mathcal{P} = 0.75$. At these values, the model predicts a 4.2 percentage point decline in the frequency of price changes and a 1.1 percentage point increase in average size—capturing 31% and 52% of the empirical changes, respectively.[19] The mechanism is intuitive: when firms expect greater future volatility in their demand, the range of likely optimal prices widens. As a result, they become more cautious about adjusting prices today, leading to a larger inaction region. In other words, heightened uncertainty makes firms more likely to wait and see.

Appendix E.1 explores a range of $(D, \mathcal{P})$ combinations and yields three insights. First,

[17] The monthly data aggregates prices by keeping the observation from the first Friday of each month, or the nearest weekday if that Friday is a holiday.

[18] Although the available uncertainty measures reflect forecast disagreement about aggregate variables (such as inflation and GDP), existing evidence suggests that firm-level uncertainty comoves strongly with such macro-level dispersion. Ropele et al. (2024) show that firms with greater disagreement about aggregate inflation also report higher uncertainty about their own pricing and demand conditions. This supports our modeling assumption that increased macro forecast dispersion can be interpreted as a rise in perceived idiosyncratic volatility.

[19] Model outcomes are computed as the difference in mean outcomes across two simulations of 100,000 firms each—with and without the news shock.

all combinations considered produce qualitative patterns consistent with the data. Second, the results depend primarily on the product $\mathcal{P} \cdot D$: different $(D, \mathcal{P})$ pairs with the same product yield similar outcomes. Third, the effect on the frequency of price changes levels off with increasing $\mathcal{P} \cdot D$—the maximum decline the model can generate through this news channel alone is approximately 6 percentage points.

6.3 Alternative Explanations

In the previous section, we showed that the anticipation effect triggered by a news shock to future idiosyncratic demand volatility can explain 30–50% of the pricing behavior observed during the Social Uprising. In this section, we consider alternative explanations with two objectives. First, we assess whether any of these mechanisms could by themselves account for the empirical results and thus potentially overturn our interpretation. Second, we evaluate whether any might serve as complementary channels, helping to explain the residual variance left by the news shock alone. We take up the latter in the next section and focus here on ruling out substitutes.

We treat the parameters $(\beta, \mu, \xi, \omega, \psi)$ as structural and fixed throughout. This leaves the menu cost f and the stochastic properties of idiosyncratic shocks as plausible channels through which the Social Uprising could affect pricing. We consider both contemporaneous changes at $t = 0$, as well as news about changes arriving at $t = 0$ but taking effect in $t = 1$, in line with the structure used in the main experiment.

Specifically, we examine five contemporaneous experiments: increases in $\mathbb{E}[z]$, $\mathbb{E}[n]$, $\text{Var}[n]$, $\text{Var}[z]$, and f, and four forward-looking experiments with a news structure: increases in $\mathbb{E}[z]$, $\mathbb{E}[n]$, $\text{Var}[z]$, and f. That is, we consider the possibility that the Social Uprising affected the mean or dispersion of productivity or demand shocks, or altered the fixed cost of price adjustment. All changes we consider, whether contemporaneous or in the future, last for one period and they revert to their baseline value in the following period.[20]

Table 7 summarizes the findings. The first row replicates the direction of the observed changes during the Social Uprising: a decline in frequency and an increase in size of price changes. Each subsequent row reports the effect of a given shock relative to the baseline.

[20] For increases in the expected value, we consider a temporary, one-period introduction of an intercept vector proportional to the steady-state standard deviation of the relevant shock parameter. This is equivalent to a temporary, one-time change in the mean of the relevant shock parameter. For increases in the variance, we use a temporary, one-period increase in the standard deviation of the relevant innovation process. For contemporaneous changes at $t = 0$, the increase in the moment of the shock parameter is 50% (or by 50% of the baseline value for f). For forward-looking news experiments, we assume a one-period shock at $t = 1$ that occurs with certainty, with a 100% increase in moment of the shock parameter (or 50% for f). Results are based on simulations with 100,000 firms. We use a significance threshold of 0.1%; changes with higher p-values are labeled "0" (insignificant).

Table 7: Alternative Explanations

	Change in Frequency	Change in Size
Data	−	+
Change Today ($t = 0$)		
Increase in $\mathbb{E}[z]$	+	+
Increase in $\mathbb{E}[n]$	+	+
Increase in $\text{Var}[n]$	+	+
Increase in $\text{Var}[z]$	+	+
Increase in f	−	+
News arriving in $t = 0$ **about Tomorrow** ($t = 1$)		
Increase in $\mathbb{E}[z]$	0	0
Increase in $\mathbb{E}[n]$	0	0
Increase in $\text{Var}[z]$	0	0
Increase in f	+	−

Notes: See footnote 20 for simulation details."0" indicates statistically insignificant changes, "+" denotes a significant increase, and "−" a significant decrease.

Three main results emerge. First, all contemporaneous changes—except an increase in f—lead to a rise in both the frequency and the size of price changes. This is intuitive: shocks to the mean or variance of fundamentals make existing prices suboptimal, triggering more frequent and larger adjustments.[21]

Second, among forward-looking experiments, only a future increase in the menu cost generates significant effects. In principle, a future increase in demand or TFP dispersion could also trigger inaction via a wait-and-see effect. But in our model, TFP shocks affect firm states only when a new draw arrives, which occurs with probability $p_z = 0.23$. This limits the scope for anticipated TFP shocks to influence behavior. We verified that increasing p_z or ρ_z generates a stronger anticipation effect–but only at the cost of violating calibration targets.[22]

Third, an increase in f today, or an anticipated decrease in f tomorrow, match the empirical pattern: frequency falls and size increases. The mechanism is straightforward: if price changes become more costly today, but the cost goes back to normal tomorrow, firms are more likely to postpone adjustment, widening the inaction band. Similarly, if firms expect adjustment costs to fall tomorrow, with a reveral in the next period, they may delay costly

[21] We also explored scenarios combining an anticipation shock with a small realization shock in $t = 0$. For example, a 10% contemporaneous increase in dispersion is enough to overturn the decline in frequency produced by our baseline news shock (with $D = 4$ and $\mathcal{P} = 0.75$), consistent with the idea that realization effects counteract anticipation effects.

[22] News about future changes in the expected level of demand or productivity shifts the firm's optimal price path but does not introduce uncertainty about what that path will be. As a result, there is no "wait-and-see" value: firms either adjust today if the expected future gain justifies the menu cost, or they wait to adjust tomorrow. In contrast, news about future dispersion increases uncertainty, which can expand the inaction region and reduce adjustment today.

changes until the cheaper future arrives. However, we find the latter explanation implausible in the context of the Social Uprising. Given the widespread social unrest, operational uncertainty, and potential for logistical disruption, firms would have little reason to anticipate a spontaneous, temporary *decline* in adjustment frictions. If anything, the environment was more likely to raise perceived costs of decision-making or divert managerial attention from routine pricing.

Before concluding, we revisit the plausibility of interpreting the observed changes in pricing behavior as the result of a sudden and temporary increase in the menu cost f. In Section 4.3, we showed that these changes were not correlated with the local intensity of the riots. If higher f reflected physical or logistical disruptions—such as staffing shortages, delivery delays, or local security concerns—we would expect firms in more affected municipalities to behave differently, which they do not. Moreover, we find no change in supplier pricing behavior over the same period, suggesting that upstream operational frictions are unlikely to explain the shift.

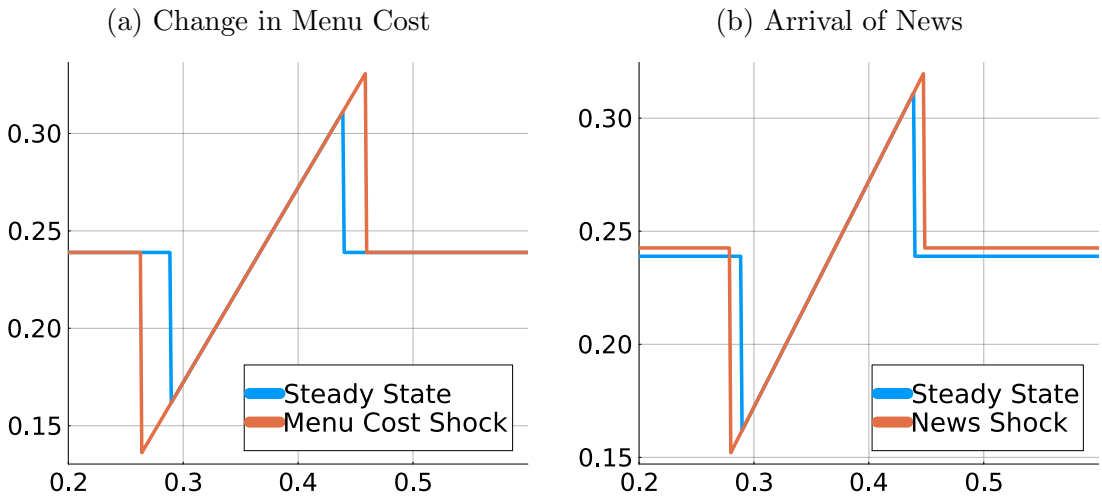
We find little support for the idea that firms avoided price changes for political or reputational reasons. If that were the case, we would expect an asymmetric response, fewer price increases but continued or even increased price cuts. Instead, the decline in adjustment frequency is symmetric. More importantly, the Social Uprising quickly evolved from a protest over subway fares into a nationwide crisis centered on inequality, institutional reform, and the legitimacy of Chile's development model.[23]

While we cannot rule out a temporary rise in the effective cost of price adjustment, we interpret it more broadly: not as a literal increase in technical or logistical costs, but as a reduced-form representation of forces outside the model's structure. In periods of widespread uncertainty and social stress, firms may reallocate managerial attention toward more immediate operational concerns—such as staff safety, inventory control, or damage prevention—delaying routine decisions like pricing unless deviations become unusually large.[24] This raises the effective inaction threshold, even if the physical or financial cost of repricing remains unchanged. Taken together, these considerations suggest that a literal menu cost shock is unlikely to be the primary driver of the observed behavior. However, to give the model full flexibility to match the data, we allow for a temporary increase in f as a complementary channel to the news shock. We interpret this as a catch-all mechanism: a reduced-form proxy for behavioral frictions, reoptimization costs, or other omitted mecha-

[23] See Joignant and Garrido-Vergara (2025) for a comprehensive review. They document how the protests reflected deep structural discontent across education, pensions, health, and wages, ultimately catalyzing a process of constitutional reform.

[24] Although not focusing on uncertainty and specific tasks, Turen (2023) proposes a model with infrequent price adjustments and state-dependent allocation of firms' attention.

Figure 7: Decision Rule Comparison: Menu Cost versus News



Notes: The plotted decision rule is the optimal price $p(p_{-1}, n, z)$ given p_{-1}, with p and p_{-1} in logs. State variables are fixed at $n = 1.042$ and $z = 0.880$. Panel (a): increase in menu cost by 60%. Panel (b): arrival of news with $(D, \mathcal{P}) = (4, 0.75)$ as in Section 6.2.

nisms that make firms more reluctant to adjust prices during turbulent periods. In the next section, we formalize this interpretation by combining the news shock with a one-period increase in f and evaluating their joint explanatory power.

6.4 News versus Change in Menu Cost

Given the analysis so far—and as previewed in the simple model in the Introduction—only two mechanisms emerge as viable candidates to explain the pricing behavior observed during the Social Uprising: (i) the arrival of news about future idiosyncratic demand volatility, and (ii) a contemporaneous, temporary increase in the fixed cost of adjusting prices. In Section 6.2, we showed that the former mechanism accounts for approximately 30% of the drop in the frequency of price changes and 50% of the increase in the size of changes. In this section, we evaluate the second mechanism. We begin by showing that although both mechanisms generate similar effects on frequency and size, they have sharply different implications for a third moment: the reset price. We then introduce empirical evidence on markups to evaluate which mechanism better fits the data. Finally, we show that combining both channels allows the model to match all three moments more closely.

To illustrate the distinction between the two mechanisms, Figure 7 compares firms' price-setting rules under a higher menu cost (panel a) and under a news shock (panel b), holding (n, z) fixed and varying p_{-1}. The 45-degree segments show the inaction region; the flat portions correspond to reset prices when the firm adjusts. In both cases, the inaction region

Table 8: Empirical vs. Model-Implied Changes During the Social Uprising

	Frequency	Size	Log Gross Markup (standardized)
Data	-0.135^{***}	0.021^{**}	0.025^{***}
	(0.034)	(0.010)	(0.008)
News Only	-0.043^{***}	0.010^{***}	0.032^{**}
	(0.001)	(0.000)	(0.007)
News + Menu Cost	-0.135^{***}	0.028^{***}	0.027^{**}
	(0.001)	(0.000)	(0.010)

Notes: The first row reports regression estimates from the data using an indicator for the Social Uprising period, conditional on price changes. The second and third rows report model-based regressions using simulated data for 100,000 firms. See Appendix E.1 for simulation details. *** p<0.01, ** p<0.05, * p<0.1.

expands, as firms become less likely to adjust prices. However, only the news shock leads to a change in the reset price itself.

This distinction is intuitive. A higher menu cost today with no change tomorrow, makes price adjustment less likely today by widening the inaction region, but conditional on adjusting, firms choose the same reset price as in the baseline. This is because the fixed cost drops out of the within-period optimization problem once the firm decides to adjust. By contrast, when firms anticipate greater future dispersion in idiosyncratic demand, they factor that into their price-setting decision today. As a result, those firms that do adjust choose a different reset price—one that reflects the expected volatility ahead. The news shock therefore affects both the width of the inaction band and the level of the reset price.[25]

This observation motivates the use of a third empirical moment—markup behavior—to distinguish the two channels. In our data, we observe both supermarket prices and supplier prices for the same products. As discussed in Section 6.1, we treat the supplier price as a proxy for marginal cost, allowing us to compute the log gross markup, $\mathcal{M} = \log(p) + \log(z)$, where z is proportional to the inverse of the supplier price. Since z is held fixed across our simulations, any changes in markups reflect changes in reset prices.

To test this implication, we estimate the effect of the Social Uprising on log gross markup at the product level, conditional on a price change occurring. This regression uses the same set of fixed effects in (6).[26]

[25]In general, reset prices in our model respond to the arrival of news shocks, though the direction and magnitude depend on the state variables. By contrast, reset prices are invariant to temporary changes in menu costs: once the firm decides to adjust, the fixed cost does not influence the optimal price.

[26]Running the markup regression conditional on a price change isolates the firm's reset price decision, holding marginal cost fixed. A significant increase in markup during the Social Uprising indicates that firms chose systematically different reset prices.

Table 8 summarizes the empirical and model-implied changes in frequency, size, and markup. The news-only version accounts for roughly one-third of the drop in frequency and half of the increase in size, and predicts a 3.2 percentage point increase in markups—slightly higher than observed, but qualitatively consistent. A model without the news channel cannot replicate this markup effect, because menu cost shocks leave reset prices unchanged.[27]

The third row of Table 8 shows the results from combining the news shock with a modest increase in the menu cost. The increase in f—calibrated at 54%—is chosen to close the gap on frequency and size while preserving the markup result. The combined model matches all three moments closely. Details of the experiment are provided in Appendix E.1.

Taken together, these results suggest that the news channel is essential for understanding the firm-level response to the Social Uprising. While a menu cost shock can serve as a reduced-form proxy for additional frictions and help align the model with the data, it cannot explain the observed change in markups on its own. Only the anticipation of future volatility can rationalize the shift in reset prices relative to cost, as revealed by the markup evidence.

7 Policy Implications

Prior work, notably Vavra (2014), shows that increases in idiosyncratic volatility make prices more flexible by raising the frequency of adjustment, thereby dampening the real effects of monetary shocks. This logic is intuitive: when shocks become more dispersed today, more firms find themselves far from their optimal price and choose to adjust. In fact, we replicated this mechanism in Table 7, where a contemporaneous increase in dispersion leads to more frequent and larger price changes. In this section, we ask a complementary question: what happens when firms receive news today about an increase in dispersion that may occur tomorrow? We show that such anticipation generates a wait-and-see response—firms delay price changes, which raises nominal rigidity and amplifies the real effects of monetary policy. This is not a contradiction of Vavra (2014)'s results, but a reflection of the fact that the implications of uncertainty depend on both its timing and its definition.

This section quantifies those policy implications. First, we show that the arrival of a news shock makes monetary interventions more powerful in the short run, precisely because firms are less willing to adjust their prices. Second, we show that the ultimate effectiveness of the intervention depends on whether the volatility shock is realized: if dispersion rises, many firms adjust and the nominal shock is absorbed; if not, real output remains elevated

[27] In simulations with a pure menu cost increase, the model can match the changes in frequency and size, but predicts no change in markup. Since the reset price is unaffected by f, there is no mechanism to alter markups conditional on cost.

Table 9: Output Response to MP Shock

	$t=0$	$t=1$	CIR
No News or Change in Menu Cost	0.459	0.234	0.884
News-Only (realized)	0.570	0.008	0.556
News-Only (not realized)	0.570	0.271	1.035
News + Menu Cost (realized)	0.743	0.016	0.764
News + Menu Cost (not realized)	0.743	0.318	1.307

Notes: Output is expressed as a fraction of the nominal expenditure shock. "Realized" means the news about higher dispersion is materialized in $t = 1$; "not realized" means it is not. The News-Only case uses $(D, \mathcal{P}) = (4, 0.75)$. The News + Menu Cost case adds a 54% increase in f, calibrated to match the observed decline in the frequency of price changes.

for longer.

We operationalize monetary policy as a one-time, unanticipated shock to nominal aggregate expenditure in period $t = 0$.[28] Output response is measured as a fraction of the nominal expenditure shock. If prices are fully flexible, the entire shock is absorbed by P and C does not change. If prices are fully rigid, the entire shock is passed through to C. Recall that in the model $S = P \cdot C$, where P is the aggregate price level and C is consumption. Previously, S grew deterministically at rate μ, so that $\log(S) = \log(S_{-1}) + \mu$. Here we introduce a one-time increase in S_0 equal to $3 \times \mu$ (about 1.11%), mimicking a monetary expansion. From $t = 1$ onward, S resumes its deterministic path. The policy shock pushes the economy out of its steady state, and we use a shooting algorithm to solve for the full transition, as described in Appendix D.5.3.

We report three output responses: (i) the immediate response in $t = 0$, (ii) the response in $t = 1$, and (iii) the cumulative impulse response (CIR), which aggregates the effect over the full adjustment path. Since our model has no explicit propagation mechanism, the impact response at $t = 0$ also represents the peak effect. The impact response and the CIR offer tractable and interpretable measures of rigidity for the magnitude and duration of the effect, respectively.

Table 9 presents the results. The first row shows the baseline case with no news or menu cost shock—a textbook monetary expansion. The output response peaks at 0.46 and dissipates quickly, with a CIR of 0.88. These magnitudes are broadly consistent with prior

[28] Following the menu cost literature, for example Golosov and Lucas Jr (2007), Midrigan (2011), and Alvarez and Lippi (2014), this way to implement monetary policy shocks avoids the need to model the nominal interest rate directly as a policy instrument. In practice, central banks adjust a short-term policy rate, such as the federal funds rate, in order to influence credit conditions and demand. In our framework, we capture the effect of such interventions as an exogenous change in nominal expenditure. Given the simplicity of the model, it is not possible to distinguish between a monetary policy shock and other shocks to aggregate nominal expenditures.

estimates, though care is warranted in comparing across countries.[29]

The next two rows introduce a news shock in period $t = 0$, keeping monetary policy timing unchanged. Because the news induces caution, firms delay adjustment, increasing the initial effectiveness of the policy. The impact response rises to 0.57—a 24% increase. If the dispersion shock is not realized in $t = 1$, firms remain reluctant to adjust, and the CIR rises to 1.04. But if the shock is realized, 63% of firms adjust in $t = 1$ (versus the steady state of 26%), absorbing more of the nominal stimulus and reversing some of the initial output gains. As a result, output falls below baseline in later periods and the CIR drops to 0.56—lower than in the benchmark case without news. This reflects a temporary amplification followed by rapid neutralization and even slight reversal of the policy's real effects.

The final two rows repeat the exercise under the News + Menu Cost calibration from Section 6.4. Since this version better matches the drop in frequency during the Social Uprising, it generates larger output responses across the board. The $t = 0$ response jumps to 0.74, and the CIR in the non-realized case exceeds 1.3. These magnitudes reflect the combined force of heightened perceived volatility and increased inaction, and reinforce the broader point from the News-Only calibration: even modest changes in expectations about future dispersion can significantly amplify the short-run effectiveness of monetary policy.

Appendix E.2 explores a related experiment in which the monetary expansion occurs not at the time of the news shock ($t = 0$), but one period later ($t = 1$). In that case, if the news is realized, monetary policy becomes virtually ineffective. This aligns with the result in Vavra (2014): realized dispersion increases price flexibility and neutralizes the real effects of policy. But if the news is not realized, the intervention retains strong effects—mirroring the findings in the contemporaneous case.

Two lessons emerge. First, monetary policy is more effective when uncertainty about the future leads firms to delay price adjustments—temporarily increasing nominal rigidity. Second, the ultimate real effect depends on whether the anticipated volatility materializes. If it does, firms reoptimize more aggressively, absorbing a larger share of the nominal stimulus and possibly reversing the initial gains in real activity. If not, rigidity persists and output gains are larger.

In sum, the anticipation of higher future volatility increases the short-run potency of monetary policy by inducing a temporary slowdown in price adjustment. But the durability of those effects hinges on what happens next. The implication for policymakers is clear: when

[29] For example, Alvarez et al. (2016) show that the real effect of monetary shocks is roughly proportional to the ratio of the kurtosis of price changes to the frequency of price adjustment. In our Chilean data, frequency is higher than in U.S. data (0.26 versus 0.11 as reported, for example, in Vavra (2014)) and kurtosis is similar to the values obtained in the U.S. data, implying a smaller CIR under standard calibrations.

facing news shocks that raise uncertainty, timing matters. Acting too late—once volatility is realized—can dramatically reduce the effectiveness of intervention.

8 Conclusion

Periods of heightened uncertainty often generate large changes in firm behavior, but distinguishing between the effects of realized volatility and anticipated volatility is empirically difficult. In this paper, we exploit a unique quasi-natural experiment—the October 2019 Social Uprising in Chile—and a high-frequency dataset of supermarket prices and costs to isolate the anticipation channel. We show that in the one-month period following the start of the Social Uprising, the frequency of price changes fell by nearly 50%, while the average size of those changes increased by roughly 20%. Importantly, these shifts occurred only on the supermarket (retail) side of the price chain; suppliers did not alter their pricing behavior. Moreover, we find no relationship between a store's change in pricing behavior and the local intensity of the Social Uprising. Together, these facts point to an anticipatory shift in retailer behavior—motivated not by direct physical disruptions, but by perceived changes in the economic environment and future consumer demand.

The context supports this interpretation. What began as riots quickly evolved into the broader Social Uprising, which was widely perceived as a political and institutional rupture that raised the prospect of constitutional overhaul, redistribution, and a restructuring of economic policy. While difficult to measure directly, these developments plausibly increased firms' uncertainty about future demand conditions, even if immediate circumstances remained unchanged.

To formalize this idea, we embed anticipated volatility into an otherwise standard menu cost model, allowing for future increases in the dispersion of idiosyncratic demand shocks. To ensure that these shocks affect pricing behavior, we adopt the Kimball aggregator, which generates variable markups and strategic complementarities in price-setting. In this framework, the arrival of news about future demand volatility induces firms to wait—expanding the inaction region and reducing the frequency of price changes. Calibrated to our Chilean data, the model can explain roughly 25% of the observed decline in frequency and 40% of the increase in the size of price changes. Because the model abstracts from realized disruptions, these effects isolate the role of the anticipation channel.

We then use the model to evaluate the implications for monetary policy. Contrary to the standard result that volatility reduces the effectiveness of nominal shocks—as in Vavra (2014)—we show that anticipated volatility can temporarily amplify the real effects of policy. Firms become less responsive to current shocks while they wait to learn more about

the future. However, once dispersion is realized, price flexibility rises and monetary policy becomes less effective. The result is a time-dependent policy environment: interventions are most potent when uncertainty is high but unrealized, and weakest once volatility arrives.

These results underscore the importance of distinguishing between the anticipation and realization of uncertainty. Policymakers responding to large-scale events—whether political, financial, or social—should consider not only the current volatility in fundamentals, but also how firms perceive and internalize future risks. The effectiveness of monetary policy may be amplified or attenuated depending not just on how uncertain the world is, but on how that uncertainty is expected to evolve.

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