

The Role of Local Price Stickiness in Monetary Policy Transmission

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October 6, 2026

Abstract

New Keynesian models predict stronger real effects of monetary policy when prices are stickier. Using U.S. retail scanner data, we construct a time-varying measure of county price stickiness from national price-adjustment frequencies and local expenditure shares. We show that price stickiness amplifies monetary transmission: following a 25-basis-point shock, the employment response is 2.0 percentage points stronger in the stickiest counties than in the most price-flexible counties. This differential is driven predominantly by contractionary shocks, consistent with a sticky-price mechanism. We provide the first empirical evidence linking local price stickiness to monetary transmission, identifying a distinct source of geographic heterogeneity.

Keywords: price stickiness; monetary policy transmission; retail scanner data; local employment

JEL Codes: E31, E52

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We are grateful to participants at the 2026 Western Economic Association International Annual Conference, and to Aeimit Lakdawala and Matthew Schaffer for helpful comments. All remaining errors are our own.

1 Introduction

The idea that nominal price stickiness is central to the real effects of demand shocks dates back at least to [Keynes \(1936\)](#) and [Hicks \(1937\)](#) and remains central to modern New Keynesian models. In these models, slower price adjustment causes a larger share of a monetary policy shock to be absorbed through changes in real activity rather than prices, making price stickiness a key determinant of monetary policy transmission. A large literature has explored the role of nominal price stickiness in monetary policy transmission across products, firms, households, and industries. Yet, to our knowledge, there is no direct empirical evidence on whether monetary policy has stronger real effects in places with stickier prices. We fill this gap by documenting such a relationship, providing direct empirical support of a central mechanism in New Keynesian models.

The geographic dimension provides a natural setting for such a test. Monetary policy shocks are common across U.S. counties, while differences in local expenditure shares generate variation in the degree of price stickiness across places. This setting allows us to test a central prediction of sticky-price models: if price stickiness is an important source of monetary non-neutrality, a common monetary policy shock should have larger real effects in counties whose expenditure shares are more heavily weighted toward goods whose prices adjust infrequently.

To implement this empirical test, we construct a time-varying measure of nominal price stickiness for 2,454 U.S. counties using retail scanner data from 2006 to 2019. We first estimate a single, location-invariant measure of price stickiness for each product using its national price history over the sample period. We then combine these product-level measures with monthly county-specific expenditure shares to construct an expenditure-weighted measure of local price stickiness. Counties whose expenditure shares are more heavily weighted toward products that adjust prices infrequently are therefore classified as having stickier prices. Because each product is assigned the same price-stickiness measure regardless of location, variation in local price stickiness reflects differences in expenditure shares rather

than differences in local pricing behavior or local shocks.

We first document substantial variation in price stickiness across counties. We then test whether this variation translates into differences in the local real effects of monetary policy. We divide counties into groups based on their degree of price stickiness at the time of a monetary policy shock and estimate impulse response functions of county-level employment using panel local projections. Our baseline monetary policy shocks are the high-frequency identified shocks of [Bauer and Swanson \(2023\)](#), aggregated to the monthly level. These shocks are based on changes in futures contracts around FOMC announcements and are orthogonalized with respect to a set of financial and macroeconomic variables. We find that monetary policy has substantially larger real effects in places with stickier prices. Following a 25-basis-point (bps) monetary policy shock, employment responds 2.0 percentage points more after 16 months in counties in the top quintile of price stickiness than in counties in the bottom quintile.¹

We subject our main result to a series of robustness tests. The differential employment response remains when we include state-by-month fixed effects to account for state-specific time-varying shocks, calculate price stickiness using stricter criteria for identifying price changes, use an alternative monetary policy shock measure that accounts for the central bank information effect, use different county-level employment data, and vary the sample period. Across these specifications, the magnitude and dynamics of the differential response remain qualitatively similar.²

We next show that our results are not explained by several alternative county-level factors associated with monetary policy transmission that may be correlated with local price stickiness. To do so, we re-estimate our baseline panel local projections while controlling, one at a time, for these alternative factors and their interactions with the monetary policy shock

¹We show that, on average, employment changes by 3.6% 16 months after a 25bps monetary policy shock.

²In the Appendix, we also show that our results are significantly different than those from a placebo-timing test where we randomly reassign the timing of the monetary policy shocks and re-estimate the local projections 1,000 times.

and its lags. These factors include the intertemporal marginal propensity to consume, the share of employment in non-tradable sectors, average establishment size, median household income, the share of households with high housing costs, the homeownership rate, population density, the rural population share, and average temperature. Our estimated differential employment response by price stickiness is virtually unchanged across these specifications, suggesting that the main results are not driven by these alternative factors.

Finally, when we split shocks into contractionary and expansionary shocks, we find that the differential response by price stickiness is driven predominantly by contractionary shocks. Consistent with this pattern, the empirical monetary policy literature has commonly found that contractionary monetary shocks have significantly larger real effects than expansionary shocks.³ Menu-cost models such as [Ball and Mankiw \(1992\)](#) or [Alvarez and Lippi \(2014\)](#) are one class of models that can generate this asymmetry. In these models, prices adjust less readily in response to negative demand shocks than to positive demand shocks, leading negative shocks to have larger effects on real activity. In response to a contractionary monetary shock, we find that employment falls significantly and persistently more in sticky-price than in flexible-price counties; however, following expansionary monetary shocks, the difference in employment responses between sticky- and flexible-price counties is statistically insignificant at nearly all horizons. This asymmetry provides additional evidence that the differential employment response operates through a price-stickiness mechanism, as it is consistent with common sticky-price models.

Taken together, our findings make three main contributions. First, we develop a new time-varying measure of price stickiness for U.S. counties, bringing the measurement of nominal price adjustment to the level of local economies and documenting substantial geographic variation in a mechanism that is central to New Keynesian models. Second, we provide direct evidence that this variation has important real consequences for monetary

³See the discussion of the literature in [Section 5.1](#) below.

policy transmission: places with stickier prices experience substantially larger employment responses to monetary policy shocks. This provides a geographic test of the sticky-price mechanism and extends existing evidence on the role of nominal rigidities from products, firms, industries, and aggregate time periods to local economies. Third, we identify price stickiness as a distinct source of heterogeneity in the local effects of monetary policy. The relationship remains largely unchanged after accounting for a range of other determinants of local monetary transmission, while the substantially stronger differential response to contractionary than to expansionary shocks is consistent with the predictions of sticky-price models. Together, these results provide evidence that nominal price stickiness helps explain not only why monetary policy has real effects, but also why those effects differ across places.

Related Literature

Our paper contributes to several existing literatures. First, we build on the literature using micro price data to measure the frequency and duration of price adjustment. Seminal contributions by [Bils and Klenow \(2004\)](#), [Klenow and Kryvtsov \(2008\)](#), and [Nakamura and Steinsson \(2008\)](#) document substantial heterogeneity in price adjustment across goods. More closely related to our approach, [Cravino et al. \(2020\)](#) combine product-level differences in price stickiness with heterogeneity in consumption baskets to study price stickiness across the income distribution. [Kim \(2019\)](#) similarly documents systematic differences in price stickiness across the income distribution arising from differences in the quality composition of consumption, while [Lan et al. \(2026\)](#) extend this approach to other dimensions of household heterogeneity. We follow a similar approach for the geographic dimension by combining product-level price adjustment with local expenditure shares to construct time-varying measures of price stickiness across U.S. counties.

Second, our paper contributes to a growing empirical literature examining whether nominal price stickiness shapes the real effects of monetary policy. At the product level, [Boivin](#)

et al. (2009) find that monetary shocks induce larger quantity declines for product categories whose prices are least responsive to the shock, while Gorodnichenko and Weber (2016) provide related firm-level evidence. At the industry level, Henkel (2020) and Ho (2025) find larger output responses to monetary policy in sectors with stickier prices. Hong et al. (2023) find that the frequency of price changes is the pricing moment most strongly associated with monetary non-neutrality across sectors. Relatedly, Alvarez et al. (2025) show that sectoral monetary non-neutrality varies with the frequency and kurtosis of price changes in ways consistent with sticky-price models. At the national level, Seiler (2025) finds stronger real effects of monetary policy during periods of greater price stickiness.⁴ The present paper extends this literature by bringing the measurement of price stickiness to the county level, allowing us to test whether local price stickiness helps explain monetary policy transmission across local economies.

Third, our paper contributes to the literature on geographic heterogeneity in monetary policy transmission. Early work by Carlino and DeFina (1998) documents substantial differences in the effects of monetary policy across U.S. regions and states. More recent work identifies several mechanisms underlying this geographic heterogeneity. Bellifemine et al. (2023) emphasize county-level intertemporal marginal propensities to consume and the share of employment in non-tradable sectors, while Beraja et al. (2019), Di Maggio et al. (2017), and Cloyne et al. (2020) highlight the roles of household balance sheets, mortgage structure, and homeownership. Household demographics also matter: Lakdawala and Moreland (2026) document stronger monetary transmission to the inflation experienced by younger households, while Leahy and Thapar (2022) show that U.S. states respond more strongly when middle-aged households constitute a larger share of the population.⁵ We identify price

⁴Not all studies find a strong relationship between price-adjustment frequency and monetary non-neutrality. For example, Özmen and Tuğan (2022) find relatively weak evidence across sectors, while Allayioti et al. (2024) find that the frequency of price changes does not explain differences in the sensitivity of core goods and services to monetary policy in the Euro area.

⁵These papers represent only a subset of a large literature on heterogeneous monetary policy transmission. Other local characteristics studied in this literature include income, industry composition, firm size,

stickiness as a distinct source of geographic heterogeneity in monetary transmission.⁶ Importantly, we show that price stickiness matters above and beyond these other channels: its relationship with local monetary policy transmission remains largely unchanged when we account for alternative local determinants, including intertemporal marginal propensities to consume, industry composition, firm size, housing characteristics, and demographics.

2 Data

Our primary data source is the NielsenIQ Retail Scanner data, obtained through the Kilts Center for Marketing at the University of Chicago Booth School of Business ([Kilts Center for Marketing, 2025](#)).⁷ For each store in the sample, the data report weekly unit sales and prices for each UPC, as well as the store’s location, from 2006 to 2019. The data cover approximately 30,000 to 50,000 stores per year from roughly 90 retail chains across a broad set of retail formats, including food and drug stores, mass merchandisers, warehouse clubs, and convenience stores.⁸ While the data provide broad geographic coverage, they do not include the full universe of U.S. retail outlets and exclude online retailers ([Kilts Center for Marketing, 2021](#)). An important advantage of the Retail Scanner data for our purposes is their fine geographic coverage, which allows us to construct time-varying measures of price population density, rurality, and climate.

⁶Using US metropolitan areas, [Herreno and Pedemonte \(2025\)](#) find that employment falls more after a contractionary monetary policy shock in the cities where CPI falls more. This is potentially inconsistent with nominal price rigidities amplifying monetary policy shocks. Our paper differs from theirs in at least three key ways. First, our sample is different: we use approximately 2,500 counties, while they use 28 metro areas. Second, our baseline time period lies completely after their baseline time period, and, thus, we capture the zero lower bound and the use of unconventional monetary policy. Third, we directly measure price stickiness using retail scanner data and interact our stickiness measure with the monetary shock.

⁷Researcher(s)’ own analyses calculated (or derived) based in part on data from Nielsen Consumer LLC and marketing databases provided through the NielsenIQ Datasets at the Kilts Center for Marketing Data Center at The University of Chicago Booth School of Business. The conclusions drawn from the NielsenIQ data are those of the researcher(s) and do not reflect the views of NielsenIQ. NielsenIQ is not responsible for, had no role in, and was not involved in analyzing and preparing the results reported herein.

⁸Coverage varies across retail sectors. For example, in 2017, the dataset included 5,691 food stores, representing approximately 17 percent of all U.S. food stores. Coverage in other sectors ranged from about 2 percent of convenience and liquor stores to roughly 52 percent of drug stores.

stickiness for 2,454 U.S. counties. To our knowledge, we are the first to exploit this geographic dimension of the data to measure local price stickiness and study its role in monetary policy transmission.

2.1 Measuring County-Level Price Stickiness

We construct monthly county-level price stickiness in three steps. First, we estimate a national measure of price stickiness for each UPC based on its frequency of price adjustment. Second, we calculate each UPC’s share of retail expenditure in each county-month. Third, we combine these measures to construct an expenditure-weighted average of product-level price stickiness for each county-month. Because each UPC is assigned the same price-stickiness measure in every county, variation in county-level price stickiness reflects differences in local expenditure shares rather than shocks or pricing behavior specific to a county.

2.1.1 Product-Level Price Stickiness

Consistent with the literature, we measure product-level price stickiness as the duration between price changes for a UPC, defined as the inverse of its frequency of price adjustment. To isolate product-level stickiness from local shocks or pricing behavior, we assign each UPC a single nationally-derived price-stickiness measure that is common across locations.

To estimate these product-level measures while maintaining computational feasibility, we select 2,000 stores observed in both 2006 and 2019 using a procedure designed to maximize UPC sales coverage. We begin our sample with the store with the highest total sales in 2006 and then iteratively add the store with the largest sales of UPCs not yet represented in the selected sample, continuing until 2,000 stores are selected. UPCs may appear in multiple selected stores. Although these stores account for roughly five percent of the stores in the Retail Scanner data, the UPCs they contain represent more than 75 percent of annual sales in every year of our baseline sample.

Using this store sample, we construct weekly UPC-store price series. We exclude UPC-store combinations with fewer than four years of observations to ensure sufficient price history, and we carry prices forward over gaps of at most two consecutive weeks to reduce the influence of short missing spells. We then construct two measures of price adjustment. Our baseline "raw" measure records *any* change in a UPC's weekly price relative to the previous week. As an alternative, we construct a "regular" measure that filters out temporary sales and promotional discounts, following [Nakamura and Steinsson \(2008\)](#).⁹

We then calculate the frequency of price adjustment for each UPC in our subsample as the share of store-weeks with a price change among all store-weeks with positive sales:

$$f_u = \frac{\sum_t \mathbf{1}(\Delta p_{ut} \neq 0)}{\sum_t \mathbf{1}(q_{ut} > 0)},$$

where Δp_{ut} denotes a price change for UPC u in store-week t , and q_{ut} denotes sales quantity. We construct both raw and regular versions of f_u and use the raw measure in our baseline specification; results are nearly unchanged when we instead use the regular-price measure.

2.1.2 County-Level Expenditure Shares

We next use the full universe of stores in the Retail Scanner data to construct county-month UPC expenditure shares. We calculate UPC dollar sales as units sold multiplied by price, assign each store to a county, and assign each retail week to the calendar month containing the majority of its days.

Then, for UPC u , county c , and month m , we define its share of covered retail expenditure as:

⁹Specifically, we use the most restrictive filter ("Sales Filter A"), as described in the supplementary materials of [Nakamura and Steinsson \(2008\)](#). Under this measure, a temporary price decline that reverts within three weeks without exceeding the prior regular price is not classified as a price change.

$$\omega_{ucm} = \frac{Sales_{ucm}}{\sum_{v \in U} Sales_{vcm}},$$

where $Sales_{ucm}$ denotes dollar sales of UPC u in county c and month m , and U is the set of UPCs for which a product-level frequency measure f_u is available. These shares capture the geographic and temporal variation in local retail expenditure shares.

2.1.3 County-Level Price Stickiness

Lastly, we combine the UPC-level price-adjustment frequencies with the county-month expenditure shares to construct county-level price stickiness. First, for county c in month m , we define:

$$F_{cm} = \sum_{u \in U} \omega_{ucm} f_u,$$

where ω_{ucm} is UPC u 's expenditure share in county c and month m , and f_u is its national price-adjustment frequency.

Then, so that higher values of our measure correspond to higher price stickiness, we define our price stickiness measure as the inverse of the county-month frequency:

$$PS_{cm} = F_{cm}^{-1}$$

Counties whose expenditure shares are concentrated in goods with infrequent price adjustment have higher values of PS_{cm} and therefore greater local price stickiness. The inverse of frequency can also be interpreted as a measure of duration between price changes.¹⁰ In our baseline specification, we use the 12-month rolling average of PS_{cm} to smooth transitory and seasonal variation in local expenditure shares. Because values of f_u are not specific to

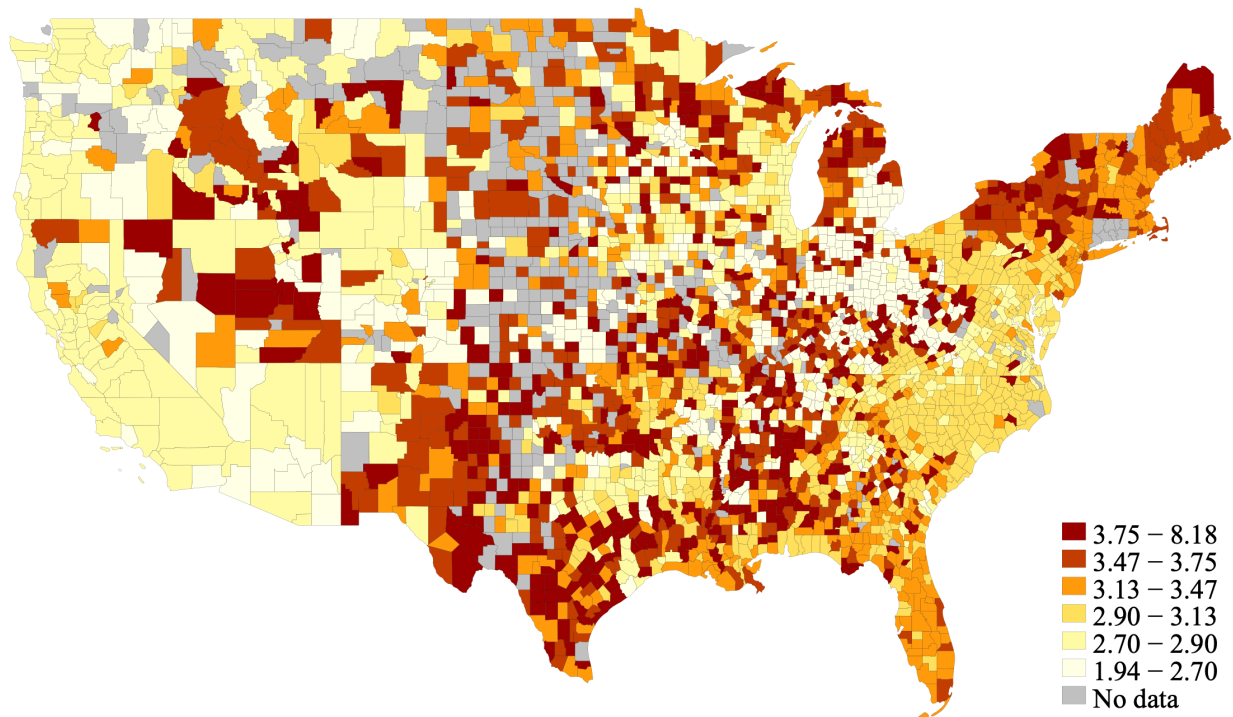
¹⁰Alternatively, we could define $d_u = f_u^{-1}$ or $d_u = \frac{-1}{\ln(1-f_u)}$, as in [Nakamura and Steinsson \(2008\)](#), at the UPC level before aggregating to the county level. These alternative measures are highly correlated with our baseline measure, with correlations close to 0.9, and do not meaningfully change our results.

a location, variation in county-level price stickiness reflects differences in local expenditure shares rather than local shocks or local pricing behavior.

2.2 Geographic Heterogeneity in Price Stickiness

Figure 1 displays our baseline county-level price-stickiness measure, averaged over the full sample period. Counties shown in gray have no Retail Scanner stores from which to construct local expenditure shares and are predominantly small, rural counties. The most flexible-price counties are concentrated in parts of the West and Midwest, while the stickiest-price counties are more prevalent in New England and along the Gulf Coast. At the same time, substantial variation in price-stickiness remains both within and across states.

Figure 1: Average County Price Stickiness, 2006–2019



Notes: The figure maps the county-level price-stickiness measure averaged over 2006–2019. The measure is the expenditure-weighted average duration between price changes, measured in weeks; higher values indicate greater price stickiness. Gray counties have no available measure.

2.3 County Characteristics by Price Stickiness

Table 1 reports county characteristics by quintile of average price stickiness. It shows that counties differ across the stickiness distribution. Relative to counties in the most flexible quintile, counties in the stickiest quintile tend to be smaller, older, and somewhat less affluent, with lower population, median household income, home values, and educational attainment. They also have slightly higher poverty rates and a lower manufacturing GDP share. In contrast, racial composition, unemployment, GDP per capita and several industry shares, are relatively similar across the most flexible and stickiest quintiles.

These differences are important for interpreting the empirical results because local price stickiness is correlated with county characteristics that may themselves affect monetary policy transmission. Accordingly, our baseline specifications include county fixed effects to account for time-invariant county heterogeneity. In Section 5.2, we further account for a range of alternative local determinants of monetary transmission and their interactions with monetary policy shocks, and show that our estimated differential response by price stickiness is effectively unchanged.

2.4 Monetary Policy Shocks

We measure monetary policy shocks using the high-frequency identified (HFI) shocks of [Bauer and Swanson \(2023\)](#), orthogonalized with respect to financial and macroeconomic variables. This shock is constructed from changes in the first four quarterly Eurodollar futures contracts (ED1–ED4) within a 30-minute window around FOMC announcements and is defined as the first principal component of these changes. We scale this measure so that a one-unit change corresponds to a 25bps increase in ED4. To match the monthly frequency of our county-level employment data, we aggregate the shocks by summing all shocks occurring within a given month.

For robustness, we show that our results hold using a monetary policy shock measure

Table 1: County Characteristics by Price Stickiness Quintile

	Q1	Q2	Q3	Q4	Q5	p-value
Price stickiness	2.553	2.801	3.018	3.368	3.702	0.000
Population	1.198	2.399	1.326	1.047	0.585	0.000
White share	88.025	84.882	78.542	86.959	87.608	0.661
Black share	8.118	9.507	18.067	8.158	8.912	0.372
Age 0–19 share	25.424	25.100	24.313	24.469	24.594	0.000
Age 20–64 share	56.795	57.144	56.772	55.803	55.625	0.000
Age 65+ share	17.781	17.756	18.915	19.728	19.781	0.000
Median household income	55.049	60.264	51.726	52.993	50.574	0.000
Poverty rate	14.653	13.550	16.690	15.051	15.652	0.009
Unemployment rate	5.230	4.997	5.948	5.191	5.447	0.167
Bachelor’s degree share	23.933	26.822	22.205	21.555	19.034	0.000
Median home value	156.951	211.679	155.427	149.380	132.485	0.000
GDP per capita	45.776	52.660	45.060	53.644	49.229	0.236
Construction share	4.811	5.158	4.811	4.839	4.697	0.537
Manufacturing share	16.852	14.535	15.319	12.946	14.741	0.010
Finance/real estate share	17.162	18.389	18.764	18.684	17.764	0.149
Government share	14.738	14.730	14.815	15.735	15.680	0.084

Notes: Entries report means by quintile of average county price stickiness, with Q1 denoting the least sticky and Q5 the stickiest counties. Population is measured in 100,000s. Median household income, median home value, and GDP per capita are measured in thousands of dollars. Share variables are reported in percentage points. The final column reports p-values from two-sample tests of equality of means between Q1 and Q5.

that accounts for the central bank information effect. The measure of [Miranda-Agrippino and Ricco \(2021\)](#), extended through the zero lower bound (ZLB) period by [Degasperi and Ricco \(2021\)](#), captures surprises in the three-month-ahead federal funds futures rate (FF4) that are orthogonalized with respect to Greenbook forecasts/revisions and past monetary surprises. We scale the shock to correspond to a 25bps increase in the one-year Treasury rate.

2.5 Labor Market Data

Our primary labor market outcome is monthly county-level total employment from the U.S. Bureau of Labor Statistics (BLS) Local Area Unemployment Statistics (LAUS) database ([U.S. BLS, 2026a](#)). For robustness, we show that our results are similar when we instead use monthly county-level employment from the BLS Quarterly Census of Employment and

Wages (QCEW) ([U.S. BLS, 2026b](#)).¹¹

2.6 Other Data

We supplement the main data with county-level demographic and economic characteristics. Population, age, and racial composition come from Surveillance, Epidemiology, and End Results (SEER) population estimates ([SEER, 2026](#)); income, poverty, education, and housing characteristics come from the 2019 American Community Survey (ACS) five-year estimates ([U.S. Census Bureau, 2019](#)); and GDP and industry shares come from Bureau of Economic Analysis (BEA) county GDP in current dollars data ([U.S. Bureau of Economic Analysis, 2026](#)). All variables are measured at the county-year level.

To account for alternative transmission channels, we use the following county-level data: intertemporal marginal propensities to consume from [Bellifemine et al. \(2023\)](#); the share of non-tradable employment¹² and average establishment size from the Census Bureau’s County Business Patterns (CBP) dataset ([U.S. Census Bureau, 2026](#)); and median household income, the share of households with high housing costs, the homeownership rate, population density, rural population share, and average temperature from the U.S. Congress Joint Economic Committee’s Social Capital Project data ([U.S. Congress Joint Economic Committee, 2018](#)).

¹¹LAUS employment is measured based on workers’ place of residence, whereas QCEW employment is measured based on the location of the employing establishment.

¹²We classify sectors as in [Mian and Sufi \(2014\)](#).

3 Empirical Strategy

We estimate the following lag-augmented panel local projection (Jordà, 2005; Montiel Olea and Plagborg-Møller, 2021) over the sample period January 2008 - December 2017:¹³

$$\Delta y_{i,t+h} = \alpha_{i,h} + \alpha_{t,h} + \sum_{g=1}^2 \alpha_{g,h} + (1 + D_{i,t}^g \zeta_h') (\beta_h \epsilon_t^m + \sum_{l=1}^3 (\beta_h^l \epsilon_{t-l}^m + \gamma_h^l \Delta y_{i,t-l})) + \epsilon_{i,t+h}, \quad (1)$$

where $\Delta y_{i,t+h} \equiv \ln(y_{i,t+h}) - \ln(y_{i,t-1})$ is the cumulative log-change in county i 's monthly total employment over horizon h , $\alpha_{i,h}$ is a county fixed effect, $\alpha_{t,h}$ is a monthly fixed effect, $\alpha_{g,h}$ is a fixed effect for each price stickiness group, $D_{i,t}^g$ is an indicator for county i 's price-stickiness group in month t , ζ_h' is a matrix of factors that scale the coefficient for each g -specific interaction between $D_{i,t}^g$ and the other variables, ϵ_t^m is the high-frequency monetary shock measure and $\epsilon_{i,t+h}$ is the error term.¹⁴ Standard errors are two-way clustered by county and month.

We divide counties into three groups based on their trailing 12-month average price stickiness as of month t : the bottom quintile (most flexible, $g = 1$), quintiles 2–4 ($g = 0$), and the top quintile (most sticky, $g = 2$). Quintiles 2–4 exhibit similar employment responses, so we combine them into a single group for a cleaner presentation of the impulse response functions. Because all non-fixed-effect terms are allowed to vary across groups, Equation 1 is equivalent to estimating a separate response for $g = 1$ and $g = 2$, relative to $g = 0$.

To handle outlier counties, we exclude counties with at least one month in the top one percent of the price-stickiness distribution and county-month observations for which the share of sales covered by UPCs with an available duration measure falls below the 10th

¹³Because we require UPC-store combinations to have at least four years of observations, product coverage is lower near the beginning and end of the Retail Scanner sample. UPCs that disappear early in the sample or enter late in the sample may not have sufficient price history to satisfy this requirement. In Section 4.3, we show results for the full January 2006–December 2019 period.

¹⁴If we replace the monthly fixed effect with lagged U.S. macroeconomic controls, including unemployment, industrial production growth, and CPI inflation, the results are similar and somewhat stronger. We consider Equation 1 to be a more robust specification.

percentile. Our results are robust to varying thresholds for these restrictions.

Our coefficients of interest trace the employment response to a monetary policy shock for each price-stickiness group over horizons $h = 0, \dots, 30$. Due to the monthly fixed effect that absorbs the monetary policy shock, our baseline responses are reported relative to the base group ($g = 0$). For the bottom and top quintiles ($g = 1, 2$), this response is given by $D_{i,t}^1 \zeta'_h \beta_h$ and $D_{i,t}^2 \zeta'_h \beta_h$, respectively. We are primarily interested in the difference between these two responses.

4 Results

In this section, we present our main result. Following a 25bps monetary policy shock, counties in the top quintile of price stickiness experience a peak change in employment 2.0 percentage points greater than counties in the bottom quintile. This is relative to an average peak employment response of 3.6%. We then show that this result is robust across a range of alternative specifications.

4.1 Baseline Monetary Policy Effect

First, we establish the average county-level employment response to our baseline monetary policy shock, the [Bauer and Swanson \(2023\)](#) shock normalized to a 25bps magnitude. To do so, we estimate Equation 1 without a monthly fixed effect and without allowing the response to vary by price stickiness. Figure 2 Panel (a) reports the cumulative employment response over the 2008-2017 period. Following a 25bps shock, employment falls by 3.6% by month 16. The response is negative and statistically significant at the 68% confidence level after the first two months and remains so through month 20; at the 90% confidence level, it is statistically significant for 9 of the first 18 months. Despite the sample largely overlapping with the ZLB period, we identify relatively large and statistically significant real effects of

monetary policy at the county level.

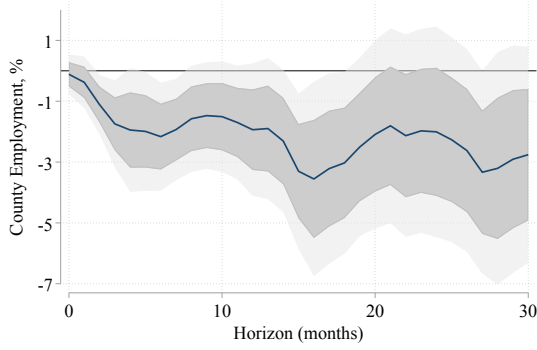
4.2 The Role of Price Stickiness in Monetary Transmission

Next, we test whether local price stickiness matters for monetary policy’s transmission to local employment. Figure 2 Panel (b) reports the cumulative employment responses for each price stickiness group estimated from Equation 1, still without monthly fixed effects. Counties in the most flexible quintile ($g = 1$) exhibit the smallest decline in employment at most horizons, while counties in the most sticky quintile ($g = 2$) exhibit the largest decline. The response of the middle three quintiles ($g = 0$) generally lies between these two groups.

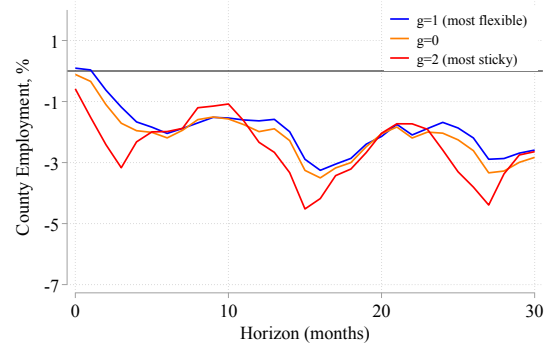
We then estimate Equation 1 with a monthly fixed effect that absorbs aggregate shocks common to all counties within a month, and focus on the most flexible and most sticky counties in Figure 2 Panels (c) - (e). With monthly fixed effects, the estimated responses of these groups are relative to the middle three quintiles ($g = 0$). Panels (c) and (d) show the responses of the most flexible and most sticky counties, respectively, with 68% and 90% confidence intervals. The most flexible counties exhibit a weaker employment response than the middle quintiles, while the most sticky counties exhibit a stronger response at all horizons.

Panel (e) reports the difference between the most sticky and most flexible quintiles. By month 16, employment in the most sticky counties has fallen 2.0pp more than in the most flexible counties, a difference that is significant at the 90% confidence level. In fact, the difference is statistically significant for 11 (13) of the first 16 months after impact at the 90% (68%) confidence level and remains statistically significant through month 29. Thus, the county-level employment response to a monetary policy shock varies substantially by local price stickiness. This finding aligns with conventional sticky-price New Keynesian models; however, this paper is the first to provide empirical evidence for a sticky-price mechanism working within local economies.

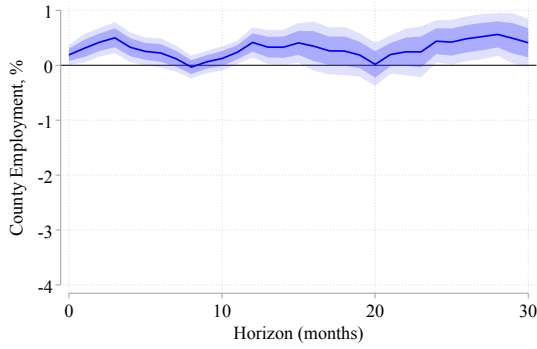
Figure 2: Employment Response to a Monetary Policy Shock



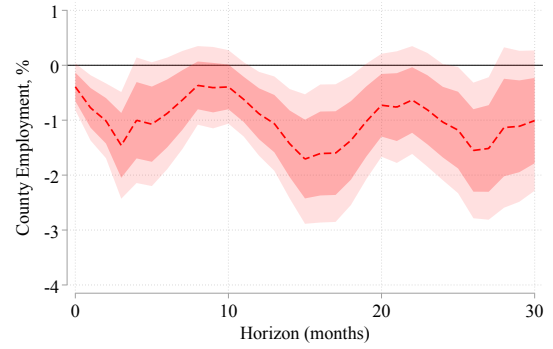
(a) Average Effect of MP Shock



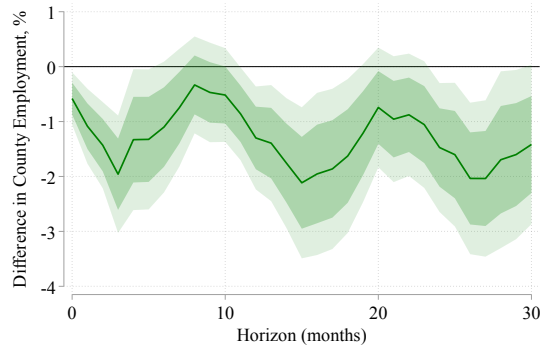
(b) Effect by Quintiles



(c) Quintile 1: Most Flexible



(d) Quintile 5: Most Sticky



(e) Quintile 5 Minus Quintile 1

Notes: The figure reports employment responses to a 25bps monetary policy shock estimated using variations of the local-projection specification in Equation 1. Panel (a) shows the average response (without monthly fixed effects or quintile groupings), and panel (b) shows responses (without monthly fixed effects) for the bottom quintile ($g=1$), middle three quintiles ($g=0$), and top quintile ($g=2$) of price stickiness. Panels (c) and (d) show responses (with monthly fixed effects) expressed relative to the middle three quintiles; panel (e) reports the difference between the most sticky and most flexible quintiles. A positive value of the monetary policy shock is contractionary. Shaded areas denote 68 and 90 percent confidence intervals.

4.3 Robustness Tests

Figure 3 reports six robustness checks for our baseline difference in employment responses between the most sticky and most flexible counties, presented in Panel (e) in Figure 2.¹⁵ First, in panel (a), we replace the monthly fixed effects in Equation 1 with state-by-month fixed effects, which absorb time-varying shocks common to counties within a state. The differential response remains negative and statistically significant at month 16 and at most horizons.

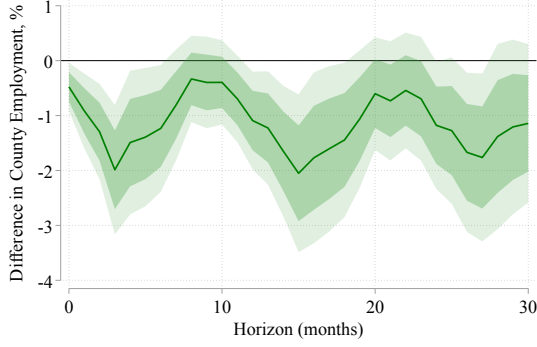
Panels (b) and (c) consider alternative constructions of price stickiness. In panel (b), we assign counties to quintiles using contemporaneous monthly price stickiness rather than the 12-month rolling average. The results are virtually unchanged. In panel (c), we use the regular-price measure that filters out temporary sales and promotional discounts following Nakamura and Steinsson (2008). The results are again nearly identical to our baseline estimates.

Panel (d) uses the alternative monetary policy shock of Miranda-Agrippino and Ricco (2021), extended through the ZLB period by Degasperis and Ricco (2021), which accounts for the central bank information effect. The point estimates remain very similar, although they are less precise, and the differential remains significant at the 68% confidence level at month 16. This decreased precision is consistent with the Miranda-Agrippino and Ricco (2021) shock being measured with a shorter-term maturity contract (FF4) than our baseline shock (ED1-ED4), resulting in less variation during the ZLB period.

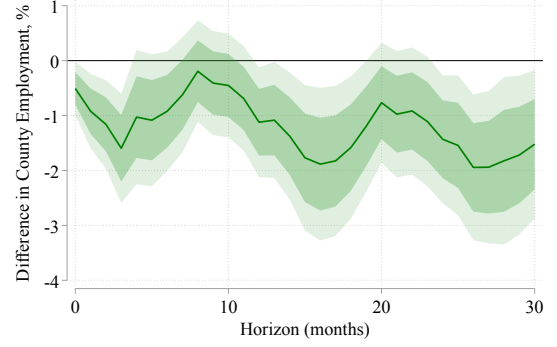
Panel (e) uses QCEW employment, which is measured by place of work rather than place of residence, as in our baseline LAUS measure, and yields qualitatively similar results. Panel (f) extends the sample from 2008–2017 to 2006–2019. Our baseline excludes the additional years because the four-year UPC requirement lowers sales coverage near the beginning and end of the scanner sample; moreover, longer-horizon responses beginning in 2018–2019 extend

¹⁵All results are robust to Winsorizing outliers. Winsorized results are not shown, as the differences are visually negligible.

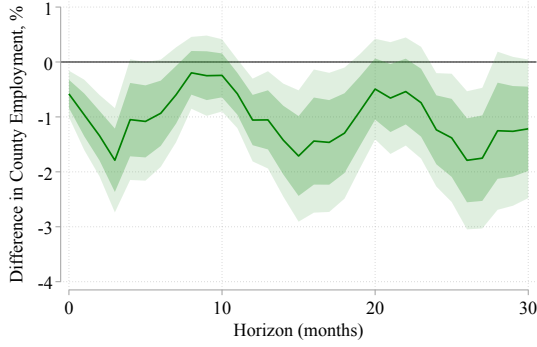
Figure 3: Difference in Employment Response by Price Stickiness
Robustness Tests



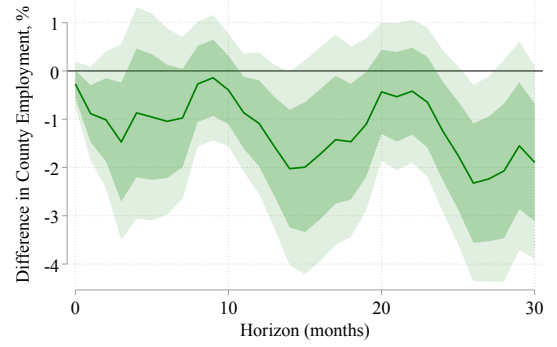
(a) State x Month Fixed Effect



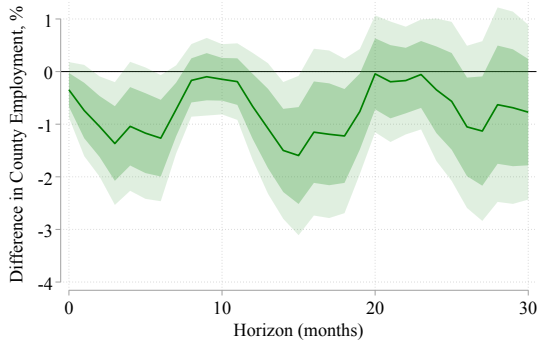
(b) Contemporaneous Stickiness



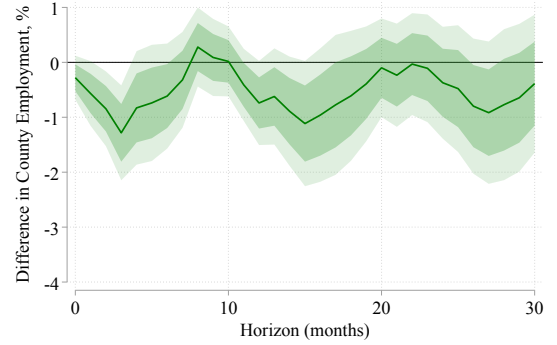
(c) Regular Price Changes Only



(d) Controlling for Central Bank Info.



(e) QCEW Employment



(f) Extended Sample: 2006-2019

Notes: Each panel reports the difference in employment responses between the most sticky and most flexible quintiles following a 25bps monetary policy shock under the indicated robustness check, explained in Section 4.3. Negative values indicate lower employment responses in sticky-price relative to flexible-price counties. A positive value of the monetary policy shock is contractionary. Shaded areas denote 68 and 90 percent confidence intervals.

into the COVID recession period. The extended-sample estimates are attenuated but remain qualitatively consistent with our baseline and statistically significant at the 68% confidence level at month 16.

In Appendix Figure [A.1](#), we replace the price-stickiness groups with a continuous measure of price stickiness interacted with the monetary policy shock. The interaction coefficient is negative throughout the horizon and statistically significant at similar horizons as our categorical measure, consistent with stronger employment responses in counties with greater price stickiness. This provides complementary evidence that greater local price stickiness is systematically associated with stronger employment responses, rather than reflecting the chosen quintile cutoffs.

Finally, in Appendix Figure [A.2](#), we report a placebo-timing test in which we randomly reassign the timing of the monetary policy shocks and re-estimate the local projections 1,000 times. This exercise tests whether the estimated differential response could arise from arbitrary shock timing rather than the observed monetary policy shocks. At horizons where the baseline differential in Panel (e) of Figure [2](#) is statistically significant, the estimates also lie outside the 5th–95th percentile range of the placebo distribution. This provides additional evidence that the differential response between sticky- and flexible-price counties is not driven by chance timing.

5 Tests of the Sticky Price Mechanism

In this section, we present further evidence that the differential employment response is consistent with a local price-stickiness mechanism. First, we show that, consistent with prior theoretical and empirical work, local price stickiness matters substantially more for contractionary shocks than for expansionary shocks. Second, we account for alternative local transmission channels and show that our results are largely unchanged. Together, these findings strongly suggest that our price stickiness results are not driven by an omitted

transmission mechanism.

5.1 Asymmetric Effects of Monetary Policy

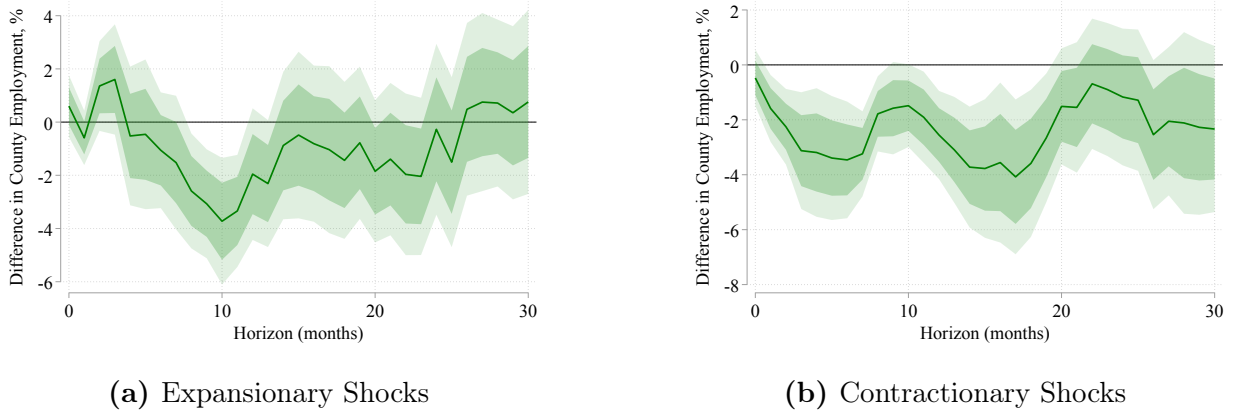
Prior work documents asymmetric real effects of monetary policy. [Cover \(1992\)](#) finds that negative money-supply shocks have large and statistically significant effects on output, while positive shocks do not, and [De Long and Summers \(1988\)](#) find similar evidence using earlier data. More recently, [Debortoli et al. \(2020\)](#) find that contractionary monetary shocks have substantially larger effects on output but relatively small effects on prices, while expansionary shocks have larger effects on prices but only small effects on output.

Sticky-price models can generate this asymmetry. In the menu-cost model of [Ball and Mankiw \(1992\)](#), positive trend inflation reduces firms’ incentives to cut nominal prices following a negative demand shock because inflation itself lowers their relative prices. Following a positive demand shock, by contrast, inflation moves relative prices in the opposite direction from firms’ desired adjustment, increasing the incentive to raise nominal prices. Prices therefore adjust less readily to negative demand shocks, generating larger effects on real activity.¹⁶

To examine this asymmetry, we estimate Equation 1 separately for expansionary and contractionary shocks. Consistent with this literature, Figure 4 shows that our differential employment response is mostly associated with contractionary monetary policy shocks. Panel (a) shows a statistically insignificant difference at most horizons between sticky- and flexible-price counties following a 25bps expansionary shock. In contrast, panel (b) shows that following a 25bps contractionary shock, the difference is statistically significant at the 90% confidence level for 17 of the first 19 months and the magnitude of the difference is persistent. Overall, this asymmetry is consistent with sticky-price models and provides further evidence that local price stickiness contributes to differences in monetary policy transmission.

¹⁶[Tsiddon \(1993\)](#) obtains a similar result under a different model setup.

Figure 4: Difference in Employment Response by Price Stickiness
Expansionary vs. Contractionary Shocks



Notes: The figure reports the difference in employment responses following a 25bps monetary policy shock between the most sticky and most flexible quintiles separately for expansionary (a) and contractionary (b) monetary policy shocks. Shaded areas denote 68 and 90 percent confidence intervals.

5.2 Accounting for Alternative Transmission Channels

Finally, we examine whether our estimated differential response is explained by alternative local transmission channels. Several county characteristics may be correlated with price stickiness and independently affect local monetary policy transmission, potentially confounding our estimates. We therefore re-estimate our baseline specification while accounting for alternative transmission channels one at a time. We do so in two ways. First, we include each alternative channel as a continuous measure and interact it with the monetary policy shock and its lags. Second, we divide each alternative measure into the bottom 20%, middle 60%, and top 20% of its distribution and interact these groups with the monetary policy shock and its lags, mirroring our treatment of price stickiness.

We consider the following county-level characteristics identified in the literature as potential determinants of local monetary transmission: intertemporal marginal propensities to consume, the share of non-tradable employment, average establishment size, median household income, the share of households with high housing costs, the homeownership rate,

population density, rural population share, and average temperature.¹⁷ In Appendix Figure A.3, we show that accounting for these channels using continuous measures has little effect on the estimated differential response between sticky- and flexible-price counties. The results are similarly unchanged when we instead group each alternative channel as detailed above (not shown). Together, these results suggest that local price stickiness plays a distinct role in the transmission of monetary policy.

6 Conclusion

In this paper, we construct a new time-varying measure of nominal price stickiness for U.S. counties using retail scanner data and show that local price stickiness is strongly associated with the real effects of monetary policy. Following monetary shocks, employment responds substantially more in counties whose expenditure shares are more heavily weighted toward products that adjust prices infrequently. This differential response is robust across alternative specifications and local transmission channels and is concentrated among contractionary shocks, consistent with sticky-price models.

This paper provides the first direct evidence that local nominal price stickiness shapes how monetary policy affects local employment. Our findings extend existing evidence on nominal rigidities to local economies and identify price stickiness as a distinct source of geographic heterogeneity in monetary transmission. More broadly, they support the central New Keynesian prediction that greater price stickiness amplifies the real effects of monetary policy and imply that a common national monetary policy shock can generate uneven local labor-market effects based on differences in price stickiness.

¹⁷See Section 2.6 for details on the data sources.

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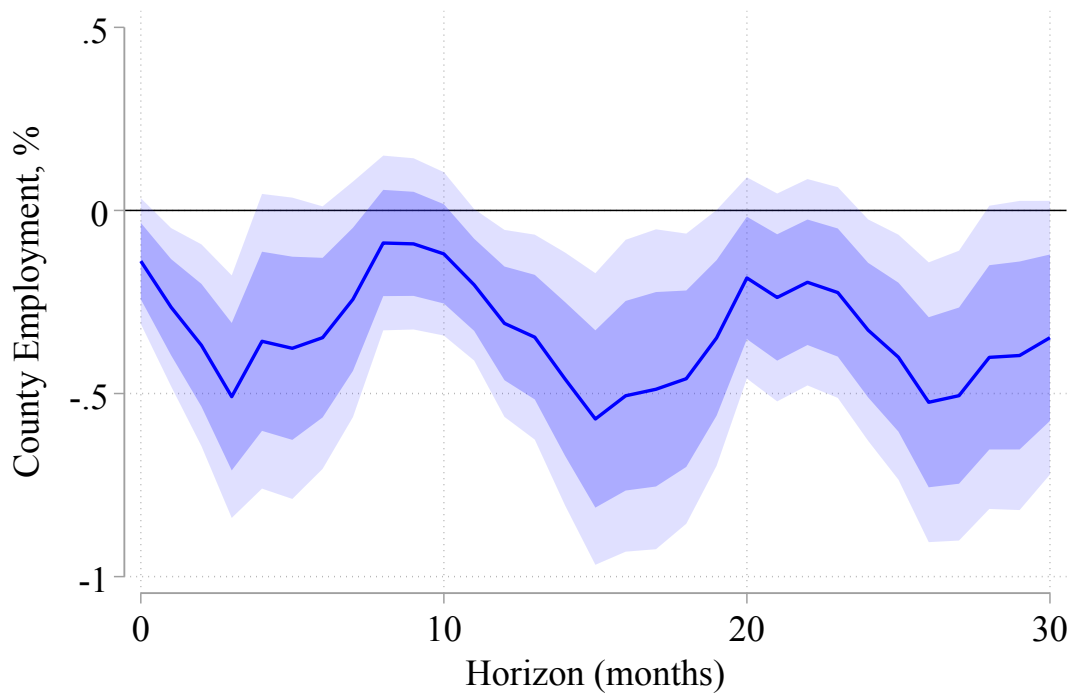
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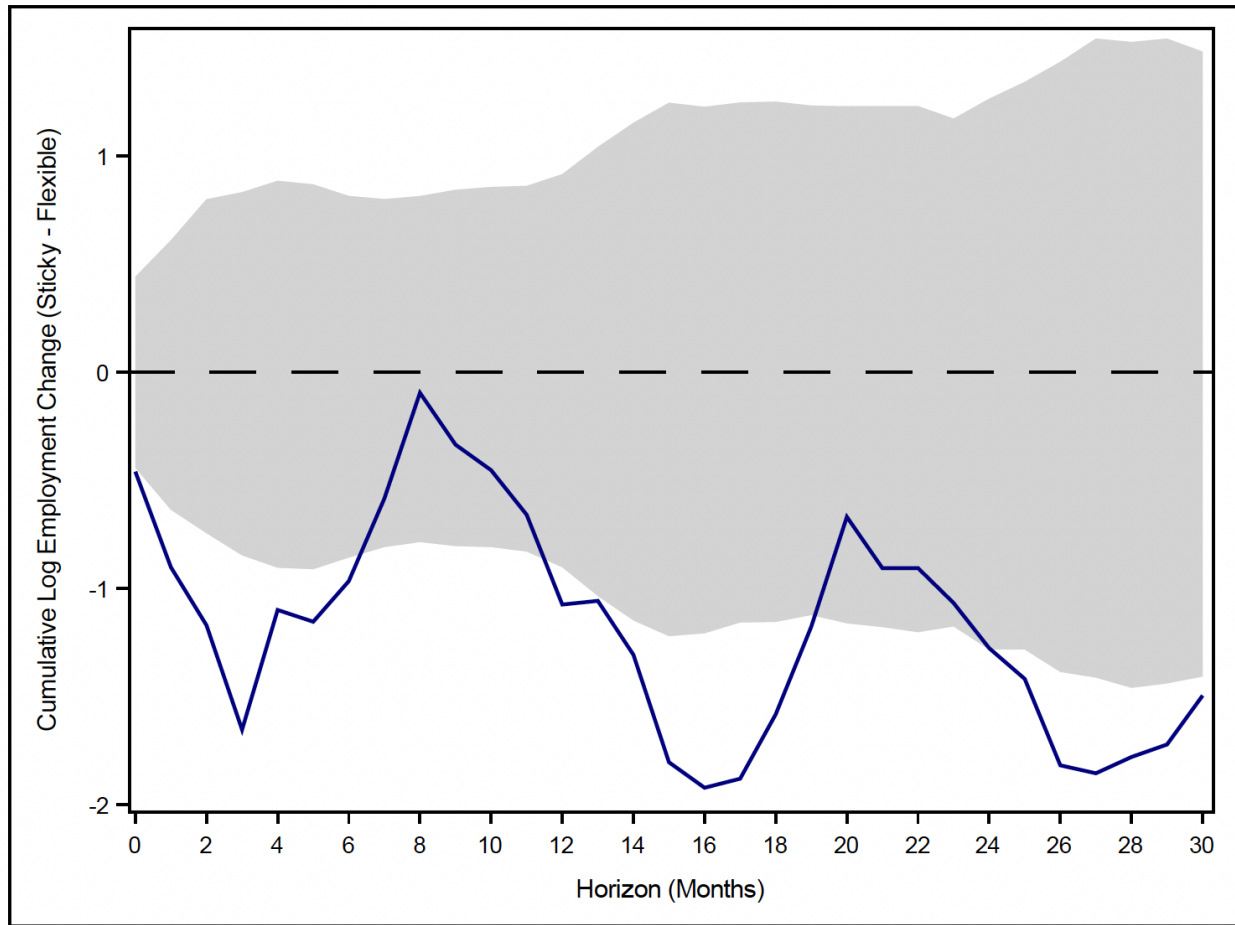
A Supplemental Appendix

Figure A.1: Continuous Measure of Price Stickiness



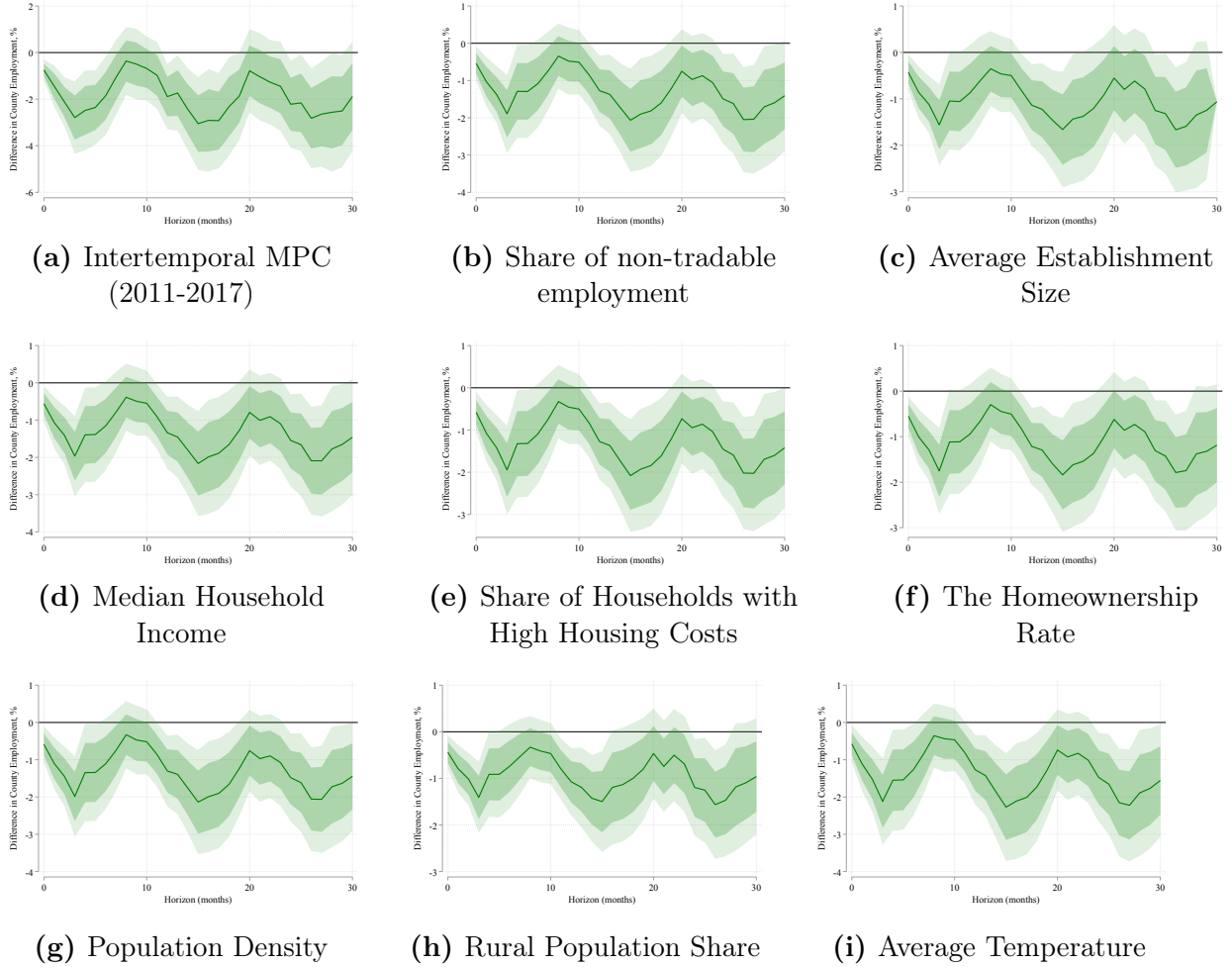
Notes: The figure reports the impact of increasing our price stickiness measure by one standard deviation on the employment response following a 25bps monetary policy shock. Negative values indicate stronger employment declines in counties with greater price stickiness. A positive value of the monetary policy shock is contractionary. Shaded areas denote 68 and 90 percent confidence intervals.

Figure A.2: Actual Sticky–Flexible vs. Placebo



Notes: The solid line reports our baseline estimate of the difference in employment responses between the most sticky and most flexible quintiles following a 25bps monetary policy shock. The shaded area shows the 5th–95th percentile range from 1,000 placebo estimates obtained by randomly reassigning the timing of the monetary policy shocks. A positive value of the monetary policy shock is contractionary.

Figure A.3: Difference in Employment Response by Price Stickiness,
Including Alternative Channels (Continuous Version)



Notes: Each panel reports the difference in employment response following a 25bps monetary policy shock between the most sticky and the most flexible quintiles when controlling for the indicated alternative transmission channel, explained in 5.2. The alternative channel enters continuously. Negative values indicate stronger employment responses in sticky-price relative to flexible-price counties. A positive value of the monetary policy shock is contractionary. Shaded areas denote 68 and 90 percent confidence intervals.