

Do Voters Punish Inflation or Pay Cuts? Inflation and Real Wages in U.S. Elections*

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Abstract: Between 2021 and 2024, the United States experienced one of the most severe inflation episodes in decades, coinciding with renewed debate over the role of economic conditions for electoral outcomes. This paper studies the political economy of inflation and real wages using U.S. county-level data on family budget costs, nominal income, and electoral results for President and Congress during this period. Exploiting within-state, cross-county variation in changes in local prices, it examines how inflation, real wage growth, and purchasing power relate to changes in vote shares of incumbents, vote margins, and turnout. Decreases in real wages (not inflation or prices per se) are strongly associated with lower support for the incumbent party. Widespread real wage contractions are associated with higher Republican vote shares for President in 2024. In addition, conditional on real wage growth, local inflation retains an independent association with partisan swings. Finally, presidential turnout responds to real wage changes, though the relationship reverses for congressional elections.

Keywords: Inflation, Economic Voting, Pocketbook Voting, Real Wages, Purchasing Power

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1 Introduction

Between 2021 and 2024, the United States experienced its most sustained episode of consumer price inflation in four decades (Bernanke & Blanchard, 2025). Figure 1 displays annual CPI inflation rates and cumulative price growth since 2020. After decades of moderate inflation averaging roughly two percent per year, consumer prices rose sharply beginning in 2021. Annual inflation peaked above eight percent in 2022 and remained elevated through 2023 before moderating. By 2024, the cumulative increase in the CPI-U since 2020 exceeded 20 percent—the largest sustained price-level shift in the experience of most voters under the age of 50.

Figure 1 also overlays cumulative real wage growth (average hourly earnings deflated by the CPI-U) since 2020. Nominal wages grew substantially over this period, but price increases outpaced earnings gains through 2022 and most of 2023. By the eve of the 2024 election, cumulative real wage growth remained well below cumulative inflation, implying that the average worker’s real wages had declined relative to 2020 levels. The gap between the two series—the erosion of real income—is the object that pocketbook voting theories predict should drive electoral responses (Fiorina, 1981; Lewis-Beck & Stegmaier, 2000).

The political consequences of the 2021-24 structural break in price dynamics have been widely debated. During the campaign ahead of the 2024 elections, public opinion research (Pew Research Center, 2024) listed the economy and inflation as the number one concern among President Trump (93%) and among Vice-President Harris (68%) supporters. Candidates also made inflation part of their platforms. For example, at an October 2024 campaign rally in Saginaw, Michigan, President Trump famously promised "*Starting on Day One of my new administration, we will end inflation, and we will make America affordable again.*"

This paper studies whether local inflation¹ is associated with changes in presidential election outcomes between 2020 and 2024, and whether such associations operate through realized real wage growth or through independent price salience. Because the presidency was held by Democrats during the 2021–2024 inflation episode, a canonical retrospective punishment story would predict that adverse economic conditions raise support for the opposition. Departures from that prediction are therefore informative about which notion of “economic conditions” voters respond to and how local price growth covaries with the broader set of factors shaping county political trends.

Our empirical design maps theoretical mechanisms from the economic voting literature into a small set of coefficient restrictions. We proceed in three steps. First, we estimate a conventional specification that enters price growth and wage growth separately. Second, we re-parameterize the same regression around the wage–price gap to isolate real wage growth. Third, we estimate a restricted “gap-only” model to assess whether real

¹We estimate local inflation at the county level by using the Economic Policy Institute (EPI)’s Family Budget Calculator available at <https://www.epi.org>.

wage growth alone is sufficient to summarize the empirical relationship, or whether prices retain incremental explanatory power.

Three main findings emerge. First, we show that real wage growth is positively correlated with support for the incumbent party in presidential voting. Second, local inflation—especially in transportation and healthcare—is a strong and robust correlate of partisan vote swings, even conditional on the wage–price gap. This “inflation beyond purchasing power” result suggests that the political economy of inflation extends beyond contemporaneous real wage growth: conditional on the wage–price gap, residual inflation acts as a proxy for local demand pressure and economic dynamism, which covaries with partisan realignment trends. Interestingly, such effects are only present for the presidential election, but not for Congress, suggesting that voters differentially attribute responsibility for the national policies to local representatives. Third, the wage–price gap is strongly related to turnout at both the county and congressional district levels, though with opposite signs: real wage growth is positively associated with presidential turnout but negatively associated with House turnout, indicating that the turnout channel operates differently across levels of government.

Overall, the paper shows how real wage growth, not inflation per se, may be key to validating an economic voting hypothesis (at least when considering the 2024 U.S. presidential elections). Once one controls for real wage dynamics, inflation indeed has predictive power, but it does not follow the logic of a retrospective electoral punishment of the incumbent party. We argue that, conditional on the wage–price gap, residual inflation proxies for local demand pressure: for a given level of real wage growth, higher inflation signals that both wages and prices grew faster, a marker of economic dynamism. Because the Republican coalition has shifted toward economically stagnant areas while economically thriving counties have trended Democratic (Rodden, 2019; Gethin, Martínez-Toledano, & Piketty, 2022), the negative inflation coefficient reflects both this compositional realignment and the demand-driven salience of favorable economic conditions in dynamic areas.

An apparent puzzle arises from recent survey evidence. Households perceive inflation as unambiguously negative and never associate it with favorable economic developments (Stantcheva, 2024; Binetti, Nuzzi, & Stantcheva, 2024), yet we estimate a negative association between local inflation and Republican gains after controlling for real wage growth. We address this tension in the Discussion. The short answer is that the places where prices rose fastest, conditional on real wages and important controls, tend to be places whose economies were running hot, whose labor markets were tight, and which had already been shifting toward the Democratic party for reasons largely independent of the 2021–2024 price episode. Our estimates therefore capture a compositional gradient across counties rather than a favorable voter response to rising prices.

The remainder of the paper is organized as follows. Section 2 reviews the related literature. Section 3 outlines the conceptual framework. Section 4 describes the data

sources and variable construction. Section 5 presents the empirical strategy. Section 6 reports the results. Section 7 discusses interpretation and limitations. Section 8 concludes.

2 Related Literature

This paper sits at the intersection of two broad research literatures: the study of economic voting in political economy/political science and the macroeconomics of heterogeneous inflation.

Economic voting. The idea that economic conditions predict electoral outcomes dates at least to [Key \(1966\)](#), who argued that voters retrospectively reward or punish incumbents based on economic performance. [Kramer \(1971\)](#) and [Fair \(1978\)](#) established that aggregate income growth and inflation predict presidential vote shares, [Hibbs \(1977\)](#) documented partisan differences in macroeconomic policy priorities, and [Tuftes \(1978\)](#) showed how electoral incentives shape fiscal policy. [Fiorina \(1981\)](#) formalized retrospective voting as rational behavior by boundedly informed citizens, while [Kinder & Kiewiet \(1981\)](#) distinguished “pocketbook” responses to personal finances from “sociotropic” responses to national economic conditions. A longstanding debate concerns the time horizon of these evaluations: [MacKuen, Erikson, & Stimson \(1992\)](#) present evidence that voters are forward-looking, updating expectations about future performance from current indicators, whereas [Healy & Lenz \(2014\)](#) show that voters respond primarily to election-year conditions rather than cumulative performance over the full term. Surveys of this literature ([Lewis-Beck & Stegmaier, 2000](#); [Healy & Malhotra, 2013](#)) document consistent evidence of economic voting across countries and elections, though the mechanisms remain debated: [Achen & Bartels \(2016\)](#) challenge the rationalist interpretation, arguing that voters are often myopic and may respond to conditions beyond government control, while more recent work shows that rational voting is empirically predictive when consistent estimators and robust empirical models are used ([Kendall, Nannicini, & Trebbi, 2015](#); [Ashworth, Bueno de Mesquita, & Friedenber, 2018](#); [Cruz, Labonne, & Trebbi, 2024](#); [Cruz, Keefer, Labonne, & Trebbi, 2024](#)). Most relevant for our purposes is the growing literature that exploits local economic shocks to study political outcomes, including the effects of trade exposure on voting ([Autor, Dorn, Hanson, & Majlesi, 2020](#)), the role of austerity-induced welfare losses in the Brexit referendum ([Fetzer, 2019](#)), and voter responses to local economic performance across levels of government ([de Benedictis-Kessner & Warshaw, 2020](#)).² We adopt a similar approach, using geographically granular economic variation at the county level to study the mechanisms linking inflation to voter behavior.

Inflation perceptions and salience. The economic voting literature typically treats inflation as a component of real income, but a distinct body of work suggests that prices carry political weight beyond their purchasing-power effect. [Shiller \(1997\)](#) documented

²See also [Baccini & Weymouth \(2021\)](#) on deindustrialization & white voter backlash as a driver of presidential voting shifts.

through survey evidence that households view inflation as eroding their standard of living and associate it with national economic mismanagement, even when their nominal incomes keep pace.³ [Stantcheva \(2024\)](#) updates this analysis with large-scale survey data, finding that respondents overwhelmingly perceive inflation as reducing their purchasing power and attribute it to government policy failures; these perceptions persist even among respondents whose incomes have risen in nominal terms. [Binetti et al. \(2024\)](#) provide a detailed account of how households understand inflation’s causes, consequences, and policy trade-offs: respondents perceive inflation as unambiguously harmful and identify the increased complexity of household decision-making as its most critical consequence. Notably, they document significant partisan differences, with Republicans more likely to attribute inflation to government policies and to foresee broader negative outcomes. [D’Acunto et al. \(2024\)](#) review the literature on household inflation expectations, documenting that experienced prices shape expectations more than official statistics and that these expectations influence consumption and financial decisions. [DiGiuseppe, Garriga, & Kern \(2025\)](#) show that inflation expectations are themselves partisan: supporters of the party in power report lower expected inflation, complicating the interpretation of survey-based measures. These findings motivate our distinction between the real wage growth channel and residual inflation salience.

Heterogeneous inflation. Whether inflation matters for voting depends in part on whether inflation rates vary enough across voters to generate differential political responses. Recent work demonstrates that they do. [Kaplan & Schulhofer-Wohl \(2017\)](#) use scanner data to estimate household-level inflation rates and find an annual interquartile range of 6.2–9.0 percentage points, driven mainly by variation in prices paid for similar goods rather than differences in consumption bundles.⁴ [Jaravel \(2019\)](#) and [Jaravel \(2021\)](#) show that this inflation inequality has important distributional consequences: lower-income households systematically face higher inflation due to product innovation patterns and relative demand shifts.⁵ [Cavallo & Kryvtsov \(2024\)](#) analyze within-store price variation during the post-pandemic surge and document “cheapflation,” a faster rise in prices of cheaper product varieties that disproportionately burdens budget-constrained consumers. [McMahon \(2024\)](#) develops experimental county-level regional price parities for the United States, providing new evidence on within-metropolitan price differentials.⁶ This literature motivates our use of geographically granular price data rather than aggregate CPI measures.

³See [Scheve \(2004\)](#) for evidence linking individual inflation aversion to preferences over macroeconomic policymaking, [Malmendier & Nagel \(2016\)](#) on how personal inflation experiences shape expectations persistently, and [D’Acunto, Malmendier, Ospina, & Weber \(2021\)](#) on the role of grocery price exposure in forming inflation expectations.

⁴For a discussion of the role of scanner data also see [Dubois, Griffith, & O’Connell \(2022\)](#).

⁵The distributional burden of inflation on low-income households has been documented in cross-country settings as well; see [Easterly & Fischer \(2001\)](#).

⁶See also [Handbury \(2021\)](#).

The 2024 election. The post-pandemic inflation episode has prompted several new studies directly relevant to our analysis. [Gordon \(2024\)](#) examines how electoral votes, presidential approval, and consumer sentiment respond to national economic indicators, finding that inflation is a significant predictor of electoral outcomes. [Baccini & Weymouth \(2025\)](#) study the 2022 congressional midterms using a survey experiment and find that individuals experiencing a higher personal inflation burden were more likely to support Republican candidates, consistent with standard retrospective accountability, but that the effect depends on blame attribution shaped by partisan messaging. In contrast, [Mutz & Mansfield \(2024\)](#) find no evidence that inflation affected 2022 congressional voting behavior, suggesting that the inflation–vote link may depend on the electoral context and the salience of presidential versus legislative accountability. Most closely related to our work, [Flavin \(2026\)](#) asks whether Trump performed better in counties where inflation was higher, using a county-level inflation measure constructed from EPI and public data sources.⁷ Using Google search volume data at the state level [Suwanprasert \(2025\)](#) reports similar results, while [Aytaç, McDowell, & Steinberg \(2025\)](#) finds a comparable adverse role of inflation for Democratic support using experimental variation via individual surveys of likely voters.

Contribution. Our paper contributes to this literature in three ways. First, we exploit the EPI Family Budget Calculator to measure category-specific inflation at the county level, providing finer geographic and expenditure-category variation than the CPI or alternative price measures computed at the commuting zone or state level. Relative to [Flavin \(2026\)](#), who documents a positive correlation between local inflation and Republican vote gains, our analysis shows that this association reverses sign once nominal wage growth is included, highlighting the importance of jointly modeling prices and wages. Second, we introduce a wage–price gap reparameterization that distinguishes the electoral comovement of real wage growth from residual inflation salience, a decomposition not previously implemented in the economic voting literature. Unlike studies that enter inflation as a single regressor ([Flavin, 2026](#); [Aytaç et al., 2025](#)), our gap framework yields a formal testable restriction linking regression coefficients to the retrospective pocketbook hypothesis. We reject this restriction: inflation predicts presidential vote swings even after controlling for real wage growth, but in a direction consistent with demand-driven salience rather than incumbent punishment. Third, we examine three distinct political margins (vote shares, margins, and turnout) and document that the real wage growth channel and the inflation-salience channel operate through different outcomes.

⁷The author reports a positive association between local inflation and Republican vote-share gains of 0.07 to 0.15 percentage points per standard deviation. However, the analysis does not separate the purchasing-power channel from nominal price salience, does not include wage data, and relies on specifications that are sensitive to controls and sample restrictions; Appendix F examines the robustness of these results.

3 Conceptual Framework

The motivating framework is the classic accountability view of elections (Key, 1966; Ferejohn, 1986): voters use economic conditions to discipline governments. In the “pocketbook” variant (Fiorina, 1981), the relevant state variable is households’ own material well-being; in the broader accountability tradition, voters may also respond to salient macroeconomic conditions that are interpreted as signals of competence, policy priorities, or future economic risk (Kinder & Kiewiet, 1981).

A central theoretical distinction is whether economic voting is primarily *retrospective* or *prospective* (Fiorina, 1981; MacKuen et al., 1992). In a purely retrospective pocketbook model, voters evaluate incumbents based on realized changes in their purchasing power, implying that inflation matters only insofar as it erodes real income. In a prospective model, voters use current economic conditions to forecast future performance: inflation can influence choices even when current real wages have kept pace, because it may signal future cost-of-living growth, heightened uncertainty, or weak policy competence. A second, related distinction is whether voters respond to *real* quantities (purchasing power) or to *nominally salient* prices. Even conditional on contemporaneous real income, inflation may matter if prices are more visible than wage growth, if households display nominal illusion, or if inflation carries utility costs beyond average real wage erosion (Shiller, 1997; Stantcheva, 2024), for example through redistribution via nominal contracts, the erosion of liquid savings, or planning and uncertainty costs.

These distinctions map into testable restrictions on regression coefficients when thinking about local inflation. To see this, let ΔP_i denote local price growth (inflation) in county i and let ΔW_i denote local nominal wage growth over the same period. The difference $G_i \equiv \Delta W_i - \Delta P_i$ measures the change in purchasing power: it is positive when wages outpace prices and negative when inflation erodes real income.⁸ Now consider a regression of an electoral outcome on both the purchasing-power gap G_i and price growth ΔP_i , along with controls. This produces two coefficients: one on the gap (call it δ_1) and one on inflation conditional on the gap (call it δ_2).

A retrospective pocketbook model generates testable predictions for both coefficients. The first concerns δ_1 . Under the null of no pocketbook voting, local real wage

⁸Strictly, real wage growth should be computed as $\tilde{G}_i = \left(\frac{1+\Delta W_i/100}{1+\Delta P_i/100} - 1 \right) \times 100$, which accounts for the compounding of percentage changes. The simple difference $G_i = \Delta W_i - \Delta P_i$ is a first-order approximation whose error term is $G_i - \tilde{G}_i \approx \Delta W_i \cdot \Delta P_i / (100 + \Delta P_i)$. In our sample, where wage growth and inflation are typically in the 15–30 percent range, the median approximation error is around 1 percentage point for total inflation. Despite this level shift, the cross-county *ranking* of real wage growth is virtually unchanged: the correlation between G_i and \tilde{G}_i is 0.998 for total inflation, and exceeds 0.98 for all expenditure categories except childcare (0.93), which exhibits the largest cross-county dispersion. Because our regressions use standardized variables and exploit relative variation across counties within states, the linear approximation has no material effect on estimated coefficients or inference.

dynamics are electorally irrelevant:

$$H_0^{(1)} : \text{No pocketbook voting: } \delta_1 = 0.$$

Rejecting $H_0^{(1)}$ would constitute evidence that voters respond to changes in local purchasing power, the core premise of the pocketbook tradition.

The second prediction concerns δ_2 . Under a pure pocketbook model, voters care exclusively about how much their real income has changed, so holding the gap G_i fixed, nominal price growth should carry no additional explanatory information:

$$H_0^{(2)} : \text{Pure real-income (pocketbook) channel: } \delta_2 = 0.$$

Rejecting $H_0^{(2)}$ is consistent with mechanisms in which inflation affects political behavior beyond its effect on contemporaneous real wages. Several channels could produce $\delta_2 \neq 0$: inflation salience, in which nominal price increases are more visible and psychologically costly than equivalent wage gains (Shiller, 1997; Stantcheva, 2024); nominal reference dependence, in which price increases loom larger than wage increases; or prospective updating, in which voters treat current inflation as a signal of future economic risk or policy incompetence of the incumbent (MacKuen et al., 1992).

The intensity of these channels may itself vary with local economic conditions. For a given wage–price gap, counties where inflation is higher are also counties where both wages and prices grew faster, a signature of stronger local demand pressure. In such environments, the pace of nominal change is higher: prices adjust more frequently, wages are renegotiated more often, and economic conditions are generally more *salient* to voters. If salience is demand-driven, δ_2 reflects the differential political response in counties where the local economy, not just prices, is more visible and psychologically present. The *sign* of δ_2 then depends on what aspect of the economic experience voters find most salient: if heightened visibility encompasses favorable conditions (tight labor markets, rising wages) alongside rising prices, the net political effect need not follow a standard incumbent-punishment prediction. We return to this interpretation in Section 7.

The framework also generates predictions about *which* electoral margins should respond to which channel. Purchasing power (the gap G_i) most directly maps onto material well-being, and is therefore a natural driver of engagement or withdrawal from the electoral process. Inflation conditional on the gap, by contrast, may be more relevant for *whom* to vote for than *whether* to vote, since it speaks to perceived competence or policy direction rather than to personal financial strain. Section 5 formalizes this framework and develops the estimating equations.

4 Data

Our analysis combines four main data sources: local cost-of-living estimates from the Economic Policy Institute, wage and employment data from the Bureau of Labor Statistics, election returns from the MIT Election Data and Science Lab, and socioeconomic controls from the Census Bureau, the USDA, and the Bureau of Economic Analysis. The primary unit of observation is the county; we also construct a congressional district (CD) dataset by spatially aggregating county covariates and merging district-level U.S. House election returns (Section 4.1). Summary statistics for the main regression variables are reported in Table 1; supplementary variables (levels and average-pay wage–price gaps) appear in Appendix Table B.3. The county estimation sample comprises 3,102 counties observed in both the 2020 and 2024 presidential elections. We describe each source and variable construction below. County-level maps illustrating the geographic distribution of each variable group are presented in Appendix Figures C.2–C.6.

Local prices Our primary measure of local price levels is the Economic Policy Institute (EPI) Family Budget Calculator (FBC). The FBC provides annual estimates of the cost of a “modest but adequate standard of living” for families at the county level. Budget estimates are disaggregated into seven expenditure categories: food, housing, child care, transportation, health care, other necessities, and taxes. Because the FBC estimates the cost of maintaining a defined living standard rather than pricing a fixed consumption basket, it is better described as a cost-of-living measure than a pure price index. Throughout the paper, we use “inflation” as shorthand for changes in these FBC cost estimates; we discuss the implications of this distinction, and why it is unlikely to bias our cross-sectional results, in the Limitations subsection of Section 7. We use the FBC data for 2021 and 2024 to construct within-county price changes over the period immediately preceding the 2024 election.⁹

For each expenditure category k and county i , we define local inflation as the percent change in the FBC cost estimate: $\Delta P_i^{(k)} = 100 \times \frac{P_{i,2024}^{(k)} - P_{i,2021}^{(k)}}{P_{i,2021}^{(k)}}$. The total budget measure aggregates all seven components, providing an overall cost-of-living change at the county level. A key advantage of the FBC relative to the Consumer Price Index (CPI) is its geographic granularity: whereas the CPI is defined for metropolitan statistical areas and has limited coverage of rural counties, the FBC provides estimates for every county in the contiguous United States. Figure 2(a) illustrates this limitation: the BLS collects CPI price quotes in a relatively small number of urban areas, leaving the vast majority of U.S. counties, particularly rural and exurban ones, without direct CPI coverage. This makes the CPI unsuitable for exploiting within-state cross-county variation of the kind we

⁹The FBC publishes data under year labels that correspond to its annual update cycle. We relabel them to align with the calendar years of the underlying price surveys: the 2022 release as 2021 and the 2025 release as 2024.

require. This is especially relevant given the sharp urban–rural divide in U.S. presidential and congressional voting.

Appendix B validates the EPI measure by comparing population-weighted EPI Family Budget inflation against CPI-U inflation for the 21 self-representing metropolitan areas where individual CPI indexes are published. When the EPI basket is restricted to the four categories with direct CPI counterparts (food, housing, transportation, and healthcare), the two measures are strongly correlated ($r = 0.74$; Appendix Figure B.1). The EPI additionally captures childcare and taxes, which together account for over a quarter of the family budget but have no CPI equivalent at the metropolitan level. These are cost pressures absent from the CPI but potentially central to household economic perceptions.

Each FBC component draws on distinct underlying data sources. Housing costs are based on HUD Fair Market Rents; food costs on the USDA Low-Cost Food Plan adjusted by Feeding America county-level meal-cost multipliers; child care costs on the National Database of Childcare Prices from the Department of Labor; transportation costs on the Center for Neighborhood Technology Housing and Transportation Affordability Index (which covers commuting-related costs; see Appendix Table B.2); health care costs on ACA marketplace premiums and out-of-pocket expenses; other necessities (clothing, personal care, and household supplies) on the BLS Consumer Expenditure Survey; and taxes on NBER TAXSIM simulations of federal and state liabilities. All budget estimates are computed for a reference family of two parents and two children, which we use throughout.¹⁰

Wages Wage data come from the Quarterly Census of Employment and Wages (QCEW), published by the Bureau of Labor Statistics. The QCEW provides county-level annual data on total covered wages and average annual pay for all industries and ownership types. We use total covered wages and average annual pay for 2021 and 2024 to construct two measures of nominal wage growth: $\Delta W_i^{\text{total}} = 100 \times \frac{W_{i,2024}^{\text{total}} - W_{i,2021}^{\text{total}}}{W_{i,2021}^{\text{total}}}$, $\Delta W_i^{\text{avg}} = 100 \times \frac{W_{i,2024}^{\text{avg}} - W_{i,2021}^{\text{avg}}}{W_{i,2021}^{\text{avg}}}$. Total covered wages capture changes in both compensation rates and the composition of employment, while average annual pay isolates changes in per-worker compensation. The main analysis uses total covered wages.¹¹

Electoral outcomes County-level presidential election returns for 2020 and 2024 are drawn from the MIT Election Data and Science Lab. From the raw candidate-level vote totals, $v_{i,t}^{\text{Rep}}$, $v_{i,t}^{\text{Dem}}$, we construct three outcome variables. First, the change in the two-party Republican vote share, defined as $\Delta s_i^{\text{Rep},2p} = 100 \left(\frac{v_{i,2024}^{\text{Rep}}}{v_{i,2024}^{\text{Rep}} + v_{i,2024}^{\text{Dem}}} - \frac{v_{i,2020}^{\text{Rep}}}{v_{i,2020}^{\text{Rep}} + v_{i,2020}^{\text{Dem}}} \right)$. Second, the change in the Republican vote margin, defined as $\Delta m_i = m_{i,2024} - m_{i,2020}$, where

¹⁰We focus on the two-parent, two-child (“2p2c”) family type, the default configuration in the FBC. Alternative family structures yield qualitatively similar patterns.

¹¹Appendix E presents our baseline results using average annual pay as an alternative.

$m_{i,t} = 100 \times (v_{i,t}^{\text{Rep}} - v_{i,t}^{\text{Dem}})$ is the Republican minus Democratic share of total votes in percentage points. Third, the change in turnout, defined as total votes cast divided by the county voting-age population (from Census Bureau age-sex estimates), expressed in percentage points and capped at 100 percent.

Additional covariates We construct a vector of county-level controls: the share of land area classified as urban (Census 2020), population density, median age, three educational attainment shares (high school graduate, some college or associate degree, and bachelor’s degree or higher, from the 2019–2023 American Community Survey), real GDP (BEA, in thousands of chained 2017 dollars), and demographic composition (Hispanic share and White share, from the 2020 Decennial Census DP1 file). These controls account for baseline differences across counties in urbanization, age structure, human capital, economic size, and demographic makeup that may independently correlate with both inflation exposure and electoral trends.

4.1 Congressional district data

To examine whether the county-level patterns extend to legislative elections, we construct a parallel congressional district (CD) dataset using U.S. House election outcomes. Building this dataset requires two steps: aggregating county-level covariates to district boundaries, and merging district-level House election returns.

Spatial aggregation. County and congressional district boundaries do not nest: a single county may straddle two or more districts, and vice versa. We therefore project county-level variables onto districts using area-weighted spatial intersections.¹² For the 117th and 118th Congresses, we intersect Census TIGER/Line county shapefiles with congressional district shapefiles, compute the fraction of each county’s area falling within each district, and use these fractions as aggregation weights. For the 116th Congress, we use the Missouri Census Data Center’s Geocorr crosswalk, which provides analogous population-based county-to-CD weights. Weights are normalized so that each county’s contributions sum to one, correcting for coastline mismatches and boundary gaps.

Variables are classified as either *extensive* (counts and totals that scale with area: population, employment, total wages, vote totals) or *intensive* (rates and averages that do not: inflation, average pay, demographic shares). Extensive variables are aggregated as area-weighted sums: $X_d = \sum_i w_{id} X_i$, where w_{id} is the fraction of county i ’s area in district d . Intensive variables are aggregated as population-weighted averages: $Z_d = \sum_i w_{id} \text{pop}_i Z_i / \sum_i w_{id} \text{pop}_i$, where the weight for each county–district fragment is the product of its area share and the county’s voting-age (18-plus) population. This ensures that intensive variables such as inflation rates reflect the population-weighted experience of district residents rather than the unweighted average across (potentially empty) land

¹²All spatial operations use an Albers Equal-Area projection (EPSG:5070) to ensure correct area calculations.

area.

House elections and turnout. District-level U.S. House vote shares from 1976 to 2024 are drawn from the MIT Election Data and Science Lab. We use House rather than presidential returns at the CD level because House races are the relevant electoral margin for congressional representation, and because aggregating county-level presidential returns to districts via spatial intersection would introduce additional measurement error. CD-level turnout is defined as total House votes cast divided by the voting-age population (18 and older) sourced directly from the Census Bureau’s American Community Survey for each redistricting cycle: ACS 2019 five-year estimates for the 116th Congress, ACS 2021 for the 117th, and ACS 2022 for the 118th.¹³

Challenger-party outcome. To provide a direct test of incumbent-punishment theories, we construct a challenger-party vote share variable. The incumbent party in each district is defined as the party that won the prior House cycle: the 2018 result determines incumbency for the 2020 election, and the 2022 result for the 2024 election. The challenger two-party vote share is then the non-incumbent party’s share of the two-party House vote in the current cycle. Districts redistricted between Congresses, where prior-cycle results are unavailable or mapped to different boundaries, are coded as missing, affecting approximately 27 of 435 districts. The outcome variable is the change in challenger two-party vote share between 2020 and 2024.

The final CD estimation sample pools districts across the three redistricting vintages (116th, 117th, and 118th Congresses), with each set of approximately 435 district boundaries matched to county-level covariates and to the 2020 and 2024 House election results via the spatial aggregation procedure described above. Pooling yields approximately 1,270–1,294 district-congress observations, depending on the outcome variable and data availability. The smaller effective sample relative to the county analysis (3,102 counties), combined with measurement error introduced by the spatial aggregation, is expected to attenuate coefficient estimates and widen confidence intervals.

4.2 Cross-county heterogeneity in inflation and wages

The summary statistics in Table 1 and Figure 2 reveal considerable variation in experienced inflation across counties. Total family budget cost growth between 2021 and 2024 averages approximately 24.5 percent, but ranges from near zero to over 57 percent across the sample. The dispersion is even more pronounced for individual components: childcare inflation ranges from -63 to 240 percent, and healthcare inflation from -19 to 115 percent. Transportation and food inflation exhibit smaller, but still substantial cross-

¹³We use district-specific Census ACS population estimates rather than county-aggregated population because the spatial intersection can produce unreliable population estimates for districts that split counties with small land areas, particularly in states such as Connecticut and Rhode Island that abolished county government after the 2020 Census. Districts in these states are excluded from the turnout analysis.

county differences (standard deviations of 6.9 and 8.5 percentage points, respectively).

Figure 2(b) maps the geographic distribution of total inflation over the 2021–2024 period. The map displays clear regional clustering (for instance, higher cost growth in parts of the Mountain West, the Northeast, and some Southern states) alongside substantial within-state heterogeneity. This heterogeneity is the source of identifying variation in our regressions: conditional on state fixed effects and controls, we exploit remaining within-state differences in price growth to estimate associations with electoral outcomes.

5 Empirical Strategy

Our empirical design maps the theoretical mechanisms of Section 3 into a small set of coefficient restrictions. Appendix A derives these specifications formally from a standard logit model of voter choice; here we state the estimating equations directly and discuss identification. We proceed in three steps.

5.1 Baseline specification

We begin with a specification that allows voters to respond independently to nominal wages and to nominal prices:

$$(1) \quad \Delta y_{i,2020-2024} = \beta_1 \Delta P_{i,2021-2024}^{(k)} + \beta_2 \Delta W_{i,2021-2024} + \mathbf{X}_i' \gamma + \mu_{s(i)} + \varepsilon_i,$$

where $\Delta y_{i,2020-2024}$ is the change in the electoral outcome in county i between 2020 and 2024; $\Delta P_{i,2021-2024}^{(k)}$ is the change in the family budget cost (FBC) index for category $k \in \{\text{total, food, transportation, housing, childcare, healthcare, other necessities, taxes}\}$, and nominal wage growth is measured using total covered wages from the Quarterly Census of Employment and Wages (QCEW) and represented by $\Delta W_{i,2021-2024}$. The vector \mathbf{X}_i includes controls for urbanization, population density, age structure, educational attainment, real GDP, and demographic composition. $\mu_{s(i)}$ are state fixed effects.¹⁴ All variables are standardized to mean zero and unit variance, so coefficients are directly comparable across price categories.¹⁵

¹⁴Because we work with a two-election change, (1) can be viewed as the first-differenced form of an equivalent two-period panel specification in levels. Let $t \in \{1, 2\}$. Consider

$$y_{it} = \alpha_i + \lambda_t + \eta_{s(i)} \cdot t + \beta_1 P_{i,t}^{(k)} + \beta_2 W_{i,t} + X_i' \gamma + u_{it},$$

where α_i are county fixed effects and $\eta_{s(i)} \cdot t$ allows each state to have its own average change between 1 and 2 (with two periods, this is equivalent to a state-specific linear trend). First-differencing the 2024 and 2020 equations eliminates α_i and λ_t and yields (1) with $\mu_{s(i)} = \eta_{s(i)}$ and $\varepsilon_i = u_{i,2024} - u_{i,2020}$. Likewise, including time-invariant county characteristics X_i in the differenced regression is equivalent to allowing outcomes to follow differential linear trends between 2020 and 2024 as a function of those baseline covariates; if controls are observed in both years, replacing X_i by ΔX_i instead controls directly for changes in those covariates.

¹⁵With standardized outcomes, coefficients can be converted into percentage-point changes by multiplying by the sample standard deviation of the relevant dependent variable.

Notice that in equation (1), β_1 captures how electoral outcome change Δy_i covaries with local price growth holding nominal wages fixed, and β_2 captures how outcomes covary with nominal wage growth holding prices fixed.

5.2 The wage–price gap

To sharpen interpretation in terms of real income, we re-parameterize the baseline around the wage–price gap. Define category-specific real wage growth as

$$(2) \quad G_i^{(k)} \equiv \Delta W_{i,2021-2024} - \Delta P_{i,2021-2024}^{(k)}.$$

Table 1 Panel D presents the descriptive statistics for these new variables. We then estimate:

$$(3) \quad \Delta y_{i,2020-2024} = \delta_1 G_i^{(k)} + \delta_2 \Delta P_{i,2021-2024}^{(k)} + \mathbf{X}'_i \gamma + \mu_{s(i)} + \varepsilon_i.$$

This is an algebraic rotation of (1), so it leaves fitted values and R^2 unchanged within each column. Its value is interpretive. The coefficient δ_1 measures how outcomes covary with real wage growth (the wage–price gap). The coefficient δ_2 measures whether nominal price growth has a residual association with electoral change *even conditional on* real wage growth.

5.3 Gap-only specification

Finally, we estimate a restricted model that includes only the wage–price gap:

$$(4) \quad \Delta y_{i,2020-2024} = \delta_1 G_i^{(k)} + \mathbf{X}'_i \gamma + \mu_{s(i)} + \varepsilon_i.$$

Unlike (3), this restriction is not equivalent to (1). It is a direct diagnostic for whether real wage growth alone captures the relevant covariation, or whether separating prices from the gap is empirically important.

5.4 Inference

We report heteroskedasticity-robust standard errors and state-cluster block-bootstrap standard errors. The bootstrap resamples states (100 replications), allowing for arbitrary within-state correlation in ε_i . Unless otherwise noted, statistical significance discussed in the text refers to the state-cluster bootstrap.

Because our data are measured at the county or congressional district level, the estimated coefficients describe associations between local economic aggregates and local electoral outcomes. They should not be interpreted as individual-level behavioral responses: the relationships we document could reflect within-county compositional shifts in the electorate (e.g., differential turnout across income groups) as well as changes in individual vote choice.

6 Results

Tables 2–4 report estimates for three outcomes: changes in two-party Republican vote share, changes in the Republican vote margin, and changes in turnout between 2020 and 2024. Each column corresponds to a different inflation measure (total inflation and seven specific expenditure components). The headline results come from the county-level wage–price gap reparameterization (Table 3); the baseline specification (Table 2) and the restricted gap-only model (Table 4) serve as diagnostic bookends. Because outcomes are standardized, the discussion emphasizes relative magnitudes and sign patterns; the main qualitative conclusions are invariant to this scaling.

6.1 Baseline results

Republican vote share and margin. Table 2 shows a clear empirical regularity: counties with larger increases in prices experienced *smaller* increases in Republican vote share and Republican margin (i.e., a relative shift away from Republicans). For total inflation (column (1)), a one-standard-deviation increase in price growth is associated with a 0.083 standard-deviation *decline* in the change in Republican two-party vote share and a 0.076 standard-deviation decline in the change in the Republican margin. The pattern is especially pronounced for transportation and healthcare prices, which deliver the largest negative point estimates among the budget components.

Nominal wage growth enters with a negative sign, and the point estimates are remarkably stable across price categories (roughly -0.03 to -0.04 standard deviations). Precision is weaker under state clustering, but the estimates are frequently marginally distinguishable from zero. Taken at face value, these baseline correlations indicate that (i) price growth is the dominant correlate of partisan vote swings in this setting but is negatively associated with Republican vote gains, and (ii) higher nominal wage growth is associated with lower Republican gains.

Turnout. For turnout (Panel C), the baseline results are more heterogeneous across price components. Total inflation is close to zero, while some budget categories are more predictive: housing price growth is positively associated with turnout change and childcare price growth is negatively associated with turnout change. Nominal wage growth is positively associated with turnout growth, although the estimate is imprecise under clustered inference.

6.2 Wage–price gap reparameterization

Table 3 reports the re-parameterized estimates from (3), which decomposes the economic environment into (i) the wage–price gap $G_i^{(k)}$ and (ii) nominal price growth $\Delta P_i^{(k)}$. This reparameterization offers a clearer reading of the evidence.

Republican vote share and margin. Across vote-share and margin outcomes, the coefficient on real wage growth is negative in every column. For the case of using total

inflation as the deflator of wages, the estimates are -0.052 for vote-share change and -0.048 for margin change. This sign is consistent with a basic retrospective accountability intuition applied to real income: counties where wages outpaced prices more strongly (higher real wage growth) tended to support the incumbent party more and shift less toward the opposition party. However, the gap coefficients are estimated with some imprecision under state clustering, and when the gap is entered alone (Table 4) it explains little of the variation in partisan swings.

Because all variables are standardized to zero mean and unit variance, coefficients describe the effect of moving one standard deviation from the mean county. Real wage growth is negative on average in our sample (Table 1, Panel D), meaning the typical county experienced a real wage *decline* during 2021–2024. With $\delta_1 < 0$, a county whose real wage loss is one standard deviation larger than the mean county’s is predicted to shift toward the Republican ticket by 0.052 standard deviations in vote-share change and 0.048 standard deviations in margin change (total-inflation column), or approximately 0.10 and 0.19 percentage points, respectively.¹⁶ Thus, δ_1 captures a gradient: among counties that all lost real purchasing power on average, those that lost *more* relative to the mean shifted more strongly toward the opposition party, consistent with retrospective “punish-the-incumbent” voting.

Turnout. For turnout, the gap coefficient is positive (around 0.10 in Table 3, or roughly 0.42 percentage points of turnout). This finding is consistent with greater participation growth where real wages improve. While this pattern is not always statistically sharp in unrestricted specifications, it becomes clearer in the gap-only model discussed below.

Evidence on pocketbook voting. The non-zero δ_1 estimates across outcomes provide initial evidence against $H_0^{(1)}$ ($\delta_1 = 0$) from Section 3: real wage dynamics are not electorally irrelevant. For partisan outcomes the evidence is suggestive but imprecise under state clustering, whereas for turnout the positive gap coefficient points more consistently toward a pocketbook channel. The gap-only specification below strengthens this conclusion.

6.2.1 Rejection of the pure pocketbook restriction

The key result for connecting the evidence to theory is the behavior of δ_2 . Conditional on the wage–price gap, the price-growth term remains negative and is often precisely estimated for vote share and margin. For total inflation, the residual price coefficient is -0.121 for vote-share change and -0.110 for margin change (approximately -0.23 and -0.44 percentage points); transportation and healthcare again stand out with large negative coefficients. This finding rejects $H_0^{(2)}$ ($\delta_2 = 0$) from Section 3: local inflation has an independent association with vote swings even when contemporaneous purchasing power is held fixed. Appendix D confirms that this result is not an artifact of the linear

¹⁶Multiplying by the sample standard deviations of the dependent variables: 1.89 pp for two-party vote-share change and 4.01 pp for margin change (Table 1, Panel A).

functional form. We return to interpretation in Section 7.

6.3 Gap-only specification

Table 4 estimates (4) and therefore isolates the predictive content of real wage growth alone.

Vote share and margin. When prices are omitted, the gap coefficients for vote-share and margin changes become small and are typically statistically indistinguishable from zero, including for total inflation. In combination with the strong residual price associations in Table 3, this pattern underscores that the empirically relevant object for partisan swings in this sample is not well summarized by the wage–price gap alone. Put differently, county-level variation in *inflation* appears more closely linked to changes in partisan vote outcomes than county-level variation in *real wage growth* as measured here.

Turnout. For turnout, the restricted model delivers a clearer and more consistent pattern: the gap coefficient is positive and statistically distinguishable from zero in multiple columns (including total, food, transportation, childcare, healthcare, and taxes). This result is consistent with a resource-based or mobilization interpretation of pocketbook voting: when real wages improve, voters are more likely to engage and politically participate (or less likely to disengage), even if partisan vote switching is governed by other channels. The statistically significant gap coefficients for turnout constitute the clearest rejection of $H_0^{(1)}$ ($\delta_1 = 0$) from Section 3: real wage dynamics are electorally relevant, operating primarily through the participation margin rather than through partisan vote switching.

6.4 Congressional district results

We replicate the county-level analysis at the congressional district level, using U.S. House election outcomes in place of presidential results. Table 5 reports summary statistics for the CD estimation sample. Tables 6–8 report, respectively, the baseline specification, the wage–price gap re-parameterization, and the restricted gap-only model for four House electoral outcomes: change in two-party Republican vote share, change in the Republican vote margin, change in turnout, and change in challenger-party vote share between 2020 and 2024. The challenger is defined as the party that did not win the district in the prior House cycle (2018 for 2020, 2022 for 2024).

Republican vote share and margin. The CD-level estimates for inflation and wages are substantially noisier than the county results. Point estimates on inflation are generally negative, consistent with the presidential-level results, but magnitudes are smaller and precision is markedly lower, particularly under state-clustered inference. Wage coefficients are also close to zero and typically statistically insignificant. This attenuation reflects the smaller effective sample ($N \approx 1,270$ district-congress observations versus 3,102 counties) and the aggregation of county-level covariates to district boundaries, which introduces additional measurement error.

Turnout. For turnout, the CD baseline results diverge sharply from the county-level pattern. Nominal wage growth enters with a *negative* sign and is precisely estimated: the coefficient ranges from -0.222 to -0.266 across price categories, significant at the five percent level under state-clustered inference in every column. This contrasts with the county-level presidential analysis, where nominal wage growth is positively associated with turnout. The sign reversal is discussed further in Section 6.4.1.

Challenger-party vote share. To test whether inflation predicts gains for the opposition party, the mechanism most directly tied to incumbent-punishment theories, we construct a challenger outcome: the two-party vote share of the party that did not hold the district in the prior cycle.¹⁷ The estimates reveal a mixed picture: while some inflation categories are associated with modestly higher challenger vote shares, the estimates are imprecise and do not consistently reach statistical significance. This null-to-weak finding suggests that local inflation shocks, as measured here, do not reliably translate into anti-incumbent vote switching in House races.

In sum, the CD-level results for partisan outcomes are broadly consistent with, but weaker than, the presidential-level findings. The attenuation likely reflects three factors: (i) aggregation noise from projecting county covariates onto congressional district boundaries; (ii) the smaller sample size; and (iii) the different electoral context of House races, where candidate-specific factors, incumbency advantages, and redistricting effects may confound the link between local economic conditions and electoral outcomes. For turnout, however, the CD results depart from the county pattern: nominal wage growth is negatively associated with House turnout change, a sign reversal relative to presidential turnout that we examine in the gap reparameterization below.

6.4.1 Wage–price gap re-parameterization at the congressional district level

Table 7 reports the re-parameterized specification that decomposes the economic environment into the wage–price gap $G_i^{(k)}$ and nominal price growth $\Delta P_i^{(k)}$, mirroring the county-level Table 3.

Republican vote share and margin. The gap coefficient δ_1 is close to zero and statistically insignificant across all price categories for both partisan outcomes. For total inflation, the estimates are -0.014 for vote-share change and -0.012 for margin change, an order of magnitude smaller than the corresponding county-level estimates (-0.052 and -0.048). The residual price coefficient δ_2 is negative for several categories—notably healthcare (-0.174) and taxes (-0.151)—but these estimates reach significance only under robust standard errors and lose precision under state clustering. The direction is consistent with the county-level pattern, but the evidence is insufficient to reject $H_0^{(2)}$ at the district level for partisan outcomes.

¹⁷Districts redistricted between the 116th and 118th Congresses may have missing incumbent-party status from the prior cycle, reducing the effective sample for this outcome.

Turnout. The turnout panel reveals a striking departure from the county-level results. The gap coefficient is *negative* and precisely estimated across all eight price categories: for total inflation, $\delta_1 = -0.272$ ($p < 0.05$ under state-clustered inference), and estimates range from -0.333 (childcare) to -0.244 (housing), all significant under both robust and clustered standard errors. This sign reversal—positive at the county level for presidential turnout (Table 3, Panel C), negative at the district level for House turnout—is the most notable difference between the presidential and congressional analyses.

Several factors may account for the divergence. First, the two analyses measure different electoral margins: county regressions capture presidential turnout, whereas the CD regressions capture House turnout. Although both are measured in presidential election years, House-specific turnout dynamics—driven by district competitiveness, incumbency advantage, and candidate salience—may respond differently to local economic conditions than presidential turnout. Second, the direction of the reversal is consistent with the paper’s broader finding that economically dynamic areas have trended Democratic: districts with stronger real wage growth tend to be safe Democratic seats where House races are less competitive, dampening district-level turnout mobilization for the congressional contest even as presidential turnout in these same areas may grow. Third, the aggregation from counties to districts mechanically smooths within-state variation and may introduce composition effects that shift the estimated relationship. The negative coefficient is therefore best interpreted as reflecting the distinct electoral context of House races rather than contradicting the county-level mobilization channel for presidential elections.

The residual price coefficient δ_2 is also negative and statistically significant for turnout in most columns—for instance, -0.258 for total inflation, -0.346 for childcare, and -0.295 for healthcare, all significant under state clustering. Combined with the negative δ_1 , the CD turnout results indicate that both dimensions of local economic dynamism—real wage growth and residual nominal activity—are associated with *lower* House turnout growth, the opposite of the presidential-turnout pattern. This divergence underscores that the mechanisms linking local economic conditions to participation may operate differently across levels of government.

Challenger-party vote share. The challenger-party panel provides a direct test of the incumbent-punishment mechanism. The gap coefficient is small and positive (ranging from 0.013 to 0.030) and statistically insignificant across all columns. The residual price coefficient is likewise close to zero, with the exception of transportation (0.150, significant under robust but not clustered inference). These null results offer no clear evidence that local inflation or real wage losses translate into anti-incumbent vote switching in House races.

Gap-only specification at the congressional district level. Table 8 estimates the restricted model that includes only the wage–price gap, omitting prices as a separate

regressor. For partisan outcomes (Panels A and B), the gap coefficient is small and generally insignificant, consistent with the county-level finding that real wage growth alone explains little variation in partisan swings. For turnout (Panel C), the gap coefficient remains negative—ranging from -0.041 to -0.166 —and is statistically distinguishable from zero under state clustering for food (-0.166), housing (-0.077), and other necessities (-0.110), confirming that the negative House-turnout association documented in Table 7 is not an artifact of including prices in the specification. The magnitudes are smaller than in the unrestricted model (roughly -0.04 to -0.17 versus -0.24 to -0.33), consistent with the omission of the negatively correlated price term. For the challenger-party outcome (Panel D), the gap coefficient is close to zero throughout, reinforcing the absence of an incumbent-punishment channel in House elections.

7 Discussion

7.1 Residual inflation as a demand-side signal

The national inflation shock of 2021–2024 was heavily covered in media and campaign messaging and plausibly contributed to aggregate Republican popular-vote gains. Our regressions do not estimate the effect of this national shock, which is absorbed by the intercept and state fixed effects. The identifying variation is instead *residual* within-state inflation, that is, local price growth that remains after partialing out state means and differential linear trends in urbanization, density, age structure, education, real GDP, and demographic composition.

Consider what it means for a county to have higher inflation conditional on the same wage–price gap. Because the gap $G_i = \Delta W_i - \Delta P_i$ is held fixed, higher ΔP_i implies commensurately higher ΔW_i : both prices and wages grew faster by the same amount. Standard Phillips curve reasoning links this pattern to stronger local demand pressure, with the local economy operating above its potential, tighter labor markets bidding up wages, and stronger spending pushing up prices.¹⁸ Residual inflation therefore proxies for local economic dynamism: counties where the nominal economy ran hotter, labor demand was tighter, and economic activity was accelerating. This demand-driven nominal dynamism is also a source of differential *salience*. In counties where both wages and prices are adjusting faster, households encounter more frequent price and wage changes, making the overall economic environment more visible and psychologically present. Demand conditions thus provide a micro-foundation for the salience channel identified in Section 3: what is differentially salient across counties is not inflation per se but the entire local

¹⁸In standard New Keynesian terminology, conditional on the real wage gap, higher residual inflation identifies counties with a larger positive output gap, where actual output exceeds potential, pushing up both wages and prices along the short-run Phillips curve. [Bernanke & Blanchard \(2025\)](#) decompose pandemic-era U.S. inflation into demand and supply components and document that demand-side pressures varied substantially across sectors and time, consistent with the idea that residual within-state inflation variation loads on local demand conditions.

economic environment shaped by demand pressure.

The political connection follows from the well-documented transformation of the U.S. partisan landscape. Over the past two decades, the Republican coalition has expanded among working-class voters in deindustrializing, economically stagnant areas, while economically dynamic metropolitan counties, characterized by tighter labor markets, knowledge-economy employment, and faster nominal growth, have trended Democratic (Autor et al., 2020; Baccini & Weymouth, 2021; Rodden, 2019; Gethin et al., 2022). The negative δ_2 captures this compositional correlation: residual inflation proxies for local economic vitality, which is associated with Democratic-trending political shifts through realignment forces that operate independently of contemporaneous purchasing power.

Two features of the data support this reading. First, the expenditure categories with the largest negative coefficients (transportation and healthcare) are those where local demand conditions most sharply differentiate dynamic from stagnant areas: commuting costs rise with labor market tightness and congestion, and healthcare costs reflect service-sector concentration in growing metropolitan economies. Second, the congressional district challenger-party outcome shows no consistent inflation association, consistent with partisan realignment manifesting most strongly at the presidential level, where national party brands carry more weight than individual candidate characteristics.¹⁹

This interpretation has both a compositional and a behavioral dimension. The compositional dimension reflects the covariance of residual inflation with demand-side economic conditions that correlate with partisan realignment. The behavioral dimension operates through demand-driven salience: in economically dynamic counties, faster nominal adjustment makes the overall economic experience more visible to voters, and the favorable nature of those conditions (tight labor markets, rising wages alongside rising prices) reinforces rather than undermines support for the incumbent. Our cross-sectional design cannot decompose the relative magnitudes of these two components. Absent exogenous variation in local prices, such as supply-side shocks that shift costs independently of local demand, we cannot conclusively separate the demand-signal channel from a direct price-response channel. Resolving this decomposition would require instrumental variation that isolates local price shocks from local demand, a design beyond the scope of the present analysis.

7.2 Reconciling with survey evidence

Recent survey evidence poses a challenge to any interpretation that relies on voters perceiving inflation favorably. Binetti et al. (2024) show that respondents view inflation as unambiguously harmful and identify the increased complexity of household decision-making as its most critical consequence. Stantcheva (2024) documents similar patterns:

¹⁹This presidential–congressional divergence echoes Mutz & Mansfield (2024), who find no effect of inflation on 2022 congressional voting, and is consistent with voters attributing macroeconomic conditions primarily to the executive branch.

households attribute inflation to government failure even when their own nominal incomes have kept pace. If voters process local price signals in isolation, the predicted direction is incumbent punishment, not the negative coefficient we estimate. This tension suggests that the compositional channel bears more of the explanatory weight than the behavioral one. The negative δ_2 may reflect not that voters in high-inflation counties respond favorably to prices, but rather that the counties where residual inflation is highest are, for structural reasons, the same counties that have been trending Democratic through forces largely independent of the 2021–2024 price episode. The inflation variable, conditional on the wage–price gap and controls, acts as a sufficient statistic for local demand conditions that correlate with realignment.

That said, the behavioral and compositional channels need not be mutually exclusive, and the survey evidence does not fully rule out the former. [Binetti et al. \(2024\)](#) also document that respondents’ preferred anti-inflation policies tend to involve progressive taxation, corporate regulation, and support for vulnerable households rather than monetary tightening or demand restraint. These policy preferences align more closely with the Democratic platform, raising the possibility that voters in high-inflation areas do not reward the incumbent for inflation per se but rather find the incumbent party’s proposed remedies more appealing. More broadly, the survey evidence captures stated perceptions about inflation as a national phenomenon, whereas the variation we exploit is residual and local. Whether voters form the same uniformly negative assessment of inflation when their own wages are also rising rapidly, their local labor market is tight, and businesses are expanding is an open question that individual-level survey data linked to local economic conditions could address.

Two additional channels may reinforce this mechanism. Counties with higher real wage growth experienced greater presidential turnout increases, one of the clearest findings in the paper. If this differential presidential turnout is compositionally favorable to Democrats, as prior evidence on the U.S. suggests, it could contribute to the negative conditional association between inflation and Republican vote shares. Several mechanisms may underlie the gap–turnout link: households facing real income losses may reduce political participation due to resource constraints or disengagement, economically dynamic areas may sustain more active mobilization infrastructure, and such areas may attract immigration of politically engaged residents. Our data document the gap–turnout relationship directly, but we cannot decompose turnout changes into their constituent mechanisms or verify that their magnitude is sufficient to account for the inflation–vote-share pattern.

The sign reversal at the congressional district level—where higher real wage growth is associated with *lower* House turnout growth—warrants brief comment. This pattern is consistent with a broader finding in the multi-level accountability literature that voter responses to local economic conditions differ across levels of government ([de Benedictis-Kessner & Warshaw, 2020](#)), and with the nationalization of American political behavior,

which has weakened the link between local conditions and engagement in sub-presidential elections (Hopkins, 2018). In our setting, the reversal may also reflect the CD estimation sample, which pools election-pair changes across multiple congressional cycles, potentially mixing presidential-year and midterm-year turnout dynamics. We view this divergence as a secondary result that underscores the level-of-government specificity of the turnout channel rather than as evidence against the presidential-level mobilization finding.

A related channel operates through inflation salience and prospective voting. Households perceive and respond to price increases more readily than to equivalent wage gains (Shiller, 1997; Stantcheva, 2024), implying that prices carry political weight beyond their effect on real income. Our interpretation integrates this salience asymmetry with the demand-side mechanism: in high-demand counties, salience is heightened not only for prices but for the entire economic experience, including wages adjusting, jobs plentiful, and businesses expanding. If voters integrate these signals rather than responding to price salience in isolation, then demand-driven salience can explain both the rejection of $H_0^{(2)}$ and the negative sign of δ_2 . This reading requires that voters form holistic economic assessments rather than responding to price salience alone, an assumption consistent with the pocketbook voting literature’s emphasis on real income but one we cannot directly test with aggregate data. Prospective voting (MacKuen et al., 1992) may reinforce this channel if voters in high-demand areas update favorably about future economic prospects.

In sum, these channels are not mutually exclusive. Our preferred interpretation is that $\delta_2 < 0$ reflects two reinforcing forces: a compositional channel, whereby residual inflation covaries with local economic dynamism and the ongoing partisan realignment along that dimension, and a behavioral channel, whereby demand-driven nominal dynamism heightens the salience of favorable local economic conditions, reinforcing incumbent support. Turnout composition may further amplify the association. Definitively decomposing these pathways would require either exogenous variation in local inflation paired with measures of individual perceptions and expectations, or individual-level panel data linking experienced inflation to vote choice and turnout decisions.

7.3 Limitations

The patterns we document in the previous sections should be interpreted with care. Both wages and prices are endogenous to local economic shocks that may themselves be politically relevant. Moreover, our wage measure is based on total covered wages, which can reflect changes in employment and industrial composition in addition to changes in pay rates faced by the median voter; measurement error in the gap would mechanically attenuate δ_1 and leave residual variation to be absorbed by δ_2 . Note, however, that the direction of this mismatch is predictable: the QCEW covers all workers, including singles and childless households whose wage trajectories may differ from those of families, while the FBC prices a family budget. To the extent that aggregate wages include workers

in higher-growth sectors or household types (e.g., single professionals in dynamic urban labor markets), our wage measure likely *overstates* nominal wage growth relative to the family-relevant benchmark, making the measured wage–price gap more conservative.

A separate concern is that the EPI Family Budget Calculator does not hold a fixed consumption basket across time. Instead, it estimates the cost of an “adequate” standard of living as defined *contemporaneously*: for instance, according to EPI FBC documentation, housing costs reflect the 40th percentile of current-year rents rather than a base-year rent inflated by a price index. Changes in FBC costs therefore conflate a price component (the cost of a fixed basket) with a standards component that captures shifts in what counts as adequate (e.g., if the housing stock upgrades over time, the 40th-percentile unit in 2024 may be larger or better-appointed than its 2021 counterpart). Formally, if FBC costs $C_{it}^{(k)}$ reflects both local prices $P_{it}^{(k)}$ and a reference-quality standard $Q_t^{(k)}$, then the percent change in it can be decomposed as $\% \Delta C_i^{(k)} \approx \% \Delta P_i^{(k)} + \% \Delta Q^{(k)} + \% \Delta P_i^{(k)} \times \% \Delta Q^{(k)}$, where the second and third terms represent quality updating and a price–quality interaction.

Several features of our design limit this concern. First, over a three-year window (2021–2024) the scope for quality upgrading is modest: the housing stock, dietary standards, and childcare norms do not transform meaningfully in three years, especially relative to the large price movements of the post-pandemic inflation episode. Second, because the reference standards $Q_t^{(k)}$ are set at national or broad-area levels (e.g., USDA food plans are national; HUD Fair Market Rents are metropolitan-wide), the standards component $\% \Delta Q^{(k)}$ is approximately common across counties within a state and is therefore absorbed by state fixed effects. The identifying variation in our regressions comes from residual within-state differences in $\% \Delta P_i^{(k)}$. Third, our CPI validation (Appendix B) confirms that cross-metropolitan variation in FBC inflation tracks the quality-adjusted CPI closely ($r = 0.74$ for the comparable basket), indicating that the cross-sectional signal is predominantly a price signal. Finally, to the extent that residual quality upgrading remains, it likely biases our estimates toward zero: counties where living standards improve most should, all else equal, experience more favorable voter sentiment, making the negative inflation coefficients we report conservative. More broadly, for the purpose of studying pocketbook voting, the relevant object may be the *total cost of maintaining a normal standard of living*, encompassing both price and standards components, rather than a pure price index, since voters respond to what they must actually spend, not to a hedonic-adjusted abstraction. At the congressional district level, the challenger-party definition relies on prior-cycle vote shares rather than a verified incumbency indicator, and House races are more sensitive to candidate quality and redistricting than presidential races, limiting the generalizability of CD null results. Addressing these limitations and moving toward causal identification of inflation and real wage growth shocks requires additional design elements beyond the scope of the present analysis.

8 Conclusion

This paper provides new evidence on the relationship between local economic conditions and electoral outcomes in the 2024 U.S. presidential election. Using county-level variation in cost-of-living changes from the EPI Family Budget Calculator and wage data from the QCEW, we document three main findings. First, local inflation is systematically associated with partisan vote swings, but the direction within states is the opposite of what a simple incumbent-punishment model would predict: higher local inflation correlates with a relative shift *away* from Republicans. We argue that this negative sign reflects both the compositional covariance of residual inflation with local economic dynamism and the demand-driven salience of favorable conditions in areas undergoing partisan realignment toward the Democratic party (Rodden, 2019; Gethin et al., 2022). Second, we reject the pure pocketbook restriction $H_0^{(2)}$: inflation retains an independent association with partisan vote swings even when contemporaneous real wage growth is held constant, suggesting that prices carry political weight beyond their effect on purchasing power. Third, the wage–price gap is more closely linked to turnout than to partisan choice: in presidential elections, real wage growth is positively associated with participation, while in House elections the association reverses, suggesting that the mechanisms linking material conditions to turnout depend on the electoral context and level of government.

These results underscore that inflation is not merely an economic aggregate but a politically consequential signal at the local level. The rejection of $H_0^{(2)}$ implies that matching wage growth to price growth may be necessary but not sufficient to neutralize the political costs of inflation: nominal price increases appear to carry independent weight in voters’ evaluations, consistent with the salience and reference-dependence mechanisms emphasized by Shiller (1997) and Stantcheva (2024). The distinction between turnout and vote-choice channels further suggests that mobilizing disengaged voters may require addressing real wage concerns, while persuading swing voters may hinge on inflation perceptions not fully captured by standard purchasing power metrics. The heterogeneous effects across expenditure categories, with transportation, housing, and healthcare emerging as particularly strong correlates, suggest that voters respond to the costs of specific, visible, and recurring necessities rather than to an abstract price level.

Our analysis has important limitations, including the endogeneity of both prices and wages to local shocks, the gap between county-level economic aggregates and individual household experiences, and the inability to definitively decompose the residual inflation–vote association into its compositional and behavioral salience components without additional exogenous variation. Addressing these limitations points to several directions for future work.

A first priority is the construction of systematic, geographically granular measures of local inflation suitable for economic voting research. The EPI Family Budget Calculator

provides useful cross-sectional variation, but it was not designed as a price index and its coverage is limited to the United States and to a single family type. Developing consistent local price measures, built from administrative records, scanner data, or web-scraped prices, that can be linked to electoral outcomes across countries and over time would substantially expand the empirical toolkit available to the economic voting literature. A second direction is to extend the analysis longitudinally within the United States, exploiting variation across multiple presidential and midterm cycles to assess whether the patterns documented here, particularly the rejection of the pocketbook restriction and the demand-side interpretation of residual inflation, are specific to the 2021–2024 inflation episode or reflect more durable features of the inflation-voting relationship. Third, the mechanisms we identify are not inherently specific to the United States. Many democracies experienced significant post-pandemic inflation, and the interaction between local price dynamics, real wage growth, and partisan realignment is likely to vary with institutional features such as electoral systems, the degree of fiscal and monetary policy centralization, and the salience of different expenditure categories across countries. Comparative analyses that replicate the wage–price gap decomposition in other national contexts would help distinguish which of our findings reflect general features of economic voting and which are particular to the U.S. partisan landscape. Finally, linking local inflation measures to individual-level survey or administrative data on vote choice and turnout would allow researchers to move beyond ecological inference, directly testing whether the compositional and behavioral channels we discuss operate at the level of individual voters.

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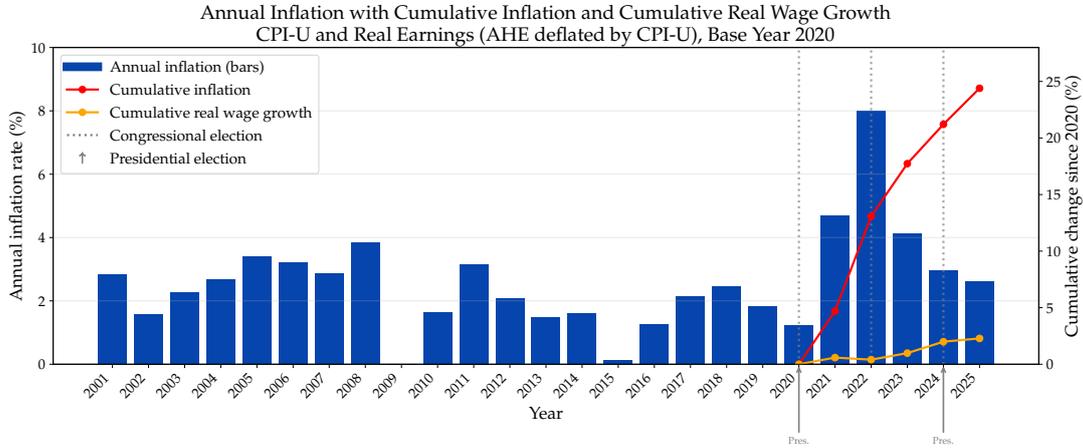
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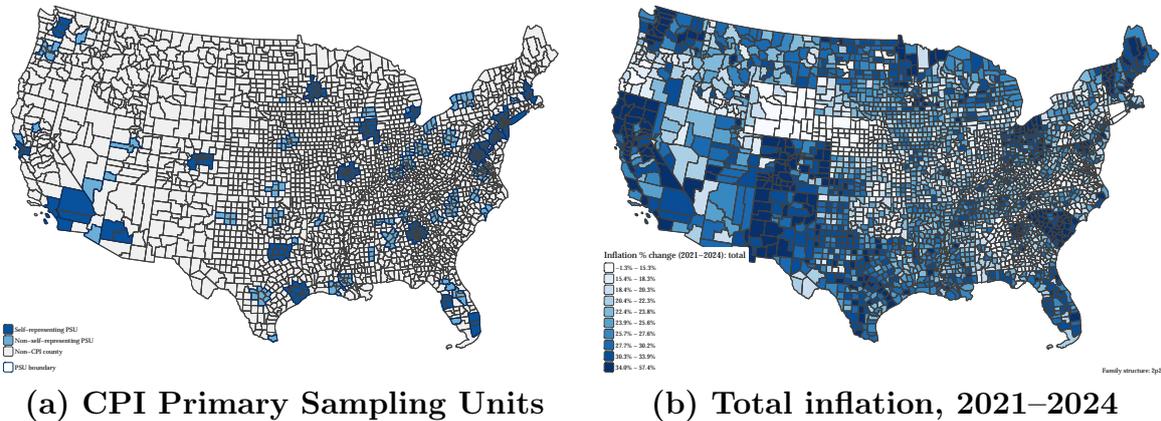
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Figure 1: U.S. inflation, cumulative inflation, and cumulative real wage growth



Notes: CPI-U and real earnings (average hourly earnings deflated by CPI-U), base year 2020. Bars show annual inflation; lines show cumulative inflation (red) and cumulative real wage growth (orange) since 2020. Vertical markers indicate congressional and presidential election years.

Figure 2: CPI price collection areas and local inflation across U.S. counties



Notes: Panel (a) shows the counties where the BLS collects price data for the Consumer Price Index. Dark blue shading indicates self-representing PSUs—metropolitan areas for which BLS publishes individual CPI indexes; medium blue indicates non-self-representing PSUs, which contribute to regional aggregates but do not receive individual indexes. Dark blue borders delineate PSU boundaries. Coverage is concentrated in metropolitan statistical areas, leaving most rural and small-metro counties (gray) without direct CPI observations. Source: Bureau of Labor Statistics, 2018 geographic revision. Panel (b) shows the percent change in total family budget costs (EPI FBC, two-parent two-child family) across all U.S. counties from 2021 to 2024, binned in deciles. Darker shading indicates larger price increases.

Table 1: Summary Statistics — County Level

Variable	Mean	SD	Min	P25	P50	P75	Max	N
<i>Panel A: Electoral Outcomes — Changes</i>								
Change in Republican vote share pct (alt) 2024 vs 2020	1.6957	1.8902	-15.0872	0.6610	1.5268	2.5298	38.8501	3102
Change in Democratic vote share pct (alt) 2024 vs 2020	-1.6957	1.8902	-38.8501	-2.5298	-1.5268	-0.6610	15.0872	3102
Change in turnout, 2020–2024 (pp)	-0.4560	4.1864	-69.5223	-2.6316	-0.5854	1.6430	37.7162	3102
Change in Rep. vote margin, 2020–2024 (pp)	3.4711	4.0098	-38.4808	1.4383	3.1429	5.0705	79.1250	3102
<i>Panel B: Inflation Changes 2021–2024</i>								
Inflation pct change food 2021 to 2024	26.5754	8.5289	-22.3522	20.7528	25.9392	31.8618	86.5742	3102
Inflation pct change transportation 2021 to 2024	31.3803	6.8877	12.1652	27.9450	31.2794	34.6347	169.5795	3102
Inflation pct change housing 2021 to 2024	25.6759	9.9363	-21.7257	19.4823	24.0979	31.3841	91.8256	3102
Inflation pct change childcare 2021 to 2024	20.6507	29.7937	-62.6153	1.3011	14.6909	34.8736	240.4922	3102
Inflation pct change healthcare 2021 to 2024	14.8055	14.4250	-19.0847	6.0259	14.0757	21.6904	114.8759	3102
Inflation pct change other necessities 2021 to 2024	16.9965	6.0509	-10.5886	13.4009	16.7196	20.2948	56.9637	3102
Inflation pct change taxes 2021 to 2024	47.5915	20.2203	-11.7566	33.9258	44.5839	58.2316	152.2113	3102
Inflation pct change total 2021 to 2024	24.4967	7.6450	-1.2658	19.4277	23.9201	28.9387	57.3774	3102
<i>Panel C: Nominal Wage Growth</i>								
Change in total wages, 2021–2024 (%)	20.7230	15.1709	-59.0444	14.8851	19.3577	24.8630	505.0274	3102
Change in average pay, 2021–2024 (%)	14.6614	6.4340	-41.8302	11.5791	14.3310	17.1263	99.4425	3102
<i>Panel D: Real Wage Growth (Total Wages – Inflation)</i>								
Real wage growth: total (Total Wages)	-3.7737	16.7239	-92.5923	-11.5965	-4.2827	3.1381	470.3192	3102
Real wage growth: food (Total Wages)	-5.8524	17.6239	-83.1710	-14.5751	-6.6608	1.5086	483.2020	3102
Real wage growth: transportation (Total Wages)	-10.6573	16.5764	-142.0470	-18.1019	-11.0992	-3.9240	445.3246	3102
Real wage growth: housing (Total Wages)	-4.9529	17.6931	-108.4038	-13.2976	-5.2269	1.9990	476.5824	3102
Real wage growth: childcare (Total Wages)	0.0723	32.9127	-231.2699	-15.4303	4.6042	19.9323	433.3046	3102
Real wage growth: healthcare (Total Wages)	5.9175	21.0701	-90.7345	-3.9848	5.6272	16.3468	507.4466	3102
Real wage growth: other necessities (Total Wages)	3.7265	16.1780	-75.2423	-3.4295	2.4317	9.2847	488.8281	3102
Real wage growth: taxes (Total Wages)	-26.8685	24.8572	-147.9322	-39.5723	-24.8640	-12.1055	425.4342	3102
<i>Panel E: Controls</i>								
Percent of 2020 land classified as urban	0.0681	0.1684	0.0000	0.0000	0.0081	0.0397	1.0000	3102
2020 population density of the county (per square mile)	277.5016	1877.3928	0.0957	16.8894	44.8025	118.0061	7.48e+04	3102
Median age for total population	41.7207	5.3221	20.8000	38.6000	41.5000	44.6000	68.1000	3102
Percent of adults who are high school graduates (or equivalent), 2019-23	33.8256	7.6361	6.1046	28.8682	33.9682	39.4630	55.2022	3102
Percent of adults completing some college or associate degree, 2019-23	30.8856	5.2579	9.3964	27.4295	30.8577	34.2037	73.0769	3102
Percent of adults with a bachelor's degree or higher, 2019-23	24.0140	10.1421	0.0000	16.9949	21.4127	28.5636	79.7257	3102
Real GDP (thousands of chained 2017 dollars) (2020)	6.50e+06	2.85e+07	1.23e+04	3.85e+05	9.91e+05	2.92e+06	7.29e+08	3102
Percent: HISPANIC OR LATINO: Total population: Hispanic or Latino (of any race)	9.8298	13.7521	0.3000	2.4000	4.6000	10.5000	97.7000	3102
Percent: RACE: Total population: One Race: White (%) (DP1 2020)	76.9954	17.6911	3.3000	66.2000	82.9000	91.4000	97.6000	3102

Notes: Observations are at the county level. All variables correspond to the county estimation sample ($N = 3,102$ counties).

Table 2: Baseline Specification: Inflation, Wages, and Presidential Electoral Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>Panel A: Two-Party Republican Vote Share Change, 2020–2024</i>								
Change in Prices	−0.0832 (0.0243)*** [0.0293]***	0.0131 (0.0187) [0.0215]	−0.1522 (0.0541)*** [0.0532]***	−0.0225 (0.0164) [0.0177]	−0.0563 (0.0281)** [0.0373]	−0.0598 (0.0232)*** [0.0287]**	−0.0082 (0.0165) [0.0231]	−0.0518 (0.0259)** [0.0314]*
Change in Wages	−0.0373 (0.0150)** [0.0225]*	−0.0390 (0.0154)** [0.0231]*	−0.0298 (0.0142)** [0.0229]	−0.0385 (0.0155)** [0.0230]*	−0.0384 (0.0150)** [0.0229]*	−0.0402 (0.0156)** [0.0226]*	−0.0390 (0.0155)** [0.0230]*	−0.0387 (0.0151)** [0.0228]*
Observations	3,102	3,102	3,102	3,102	3,102	3,102	3,102	3,102
R^2	0.413	0.409	0.420	0.410	0.411	0.411	0.409	0.410
<i>Panel B: Republican Vote Margin Change, 2020–2024</i>								
Change in Prices	−0.0759 (0.0227)*** [0.0278]***	0.0191 (0.0180) [0.0223]	−0.1212 (0.0534)** [0.0551]**	−0.0240 (0.0159) [0.0173]	−0.0447 (0.0266)* [0.0333]	−0.0733 (0.0246)*** [0.0293]**	−0.0050 (0.0155) [0.0224]	−0.0521 (0.0243)** [0.0299]*
Change in Wages	−0.0345 (0.0130)*** [0.0179]*	−0.0359 (0.0131)*** [0.0183]**	−0.0287 (0.0137)** [0.0192]	−0.0355 (0.0132)*** [0.0182]*	−0.0355 (0.0130)*** [0.0183]*	−0.0375 (0.0133)*** [0.0178]**	−0.0360 (0.0132)*** [0.0181]**	−0.0357 (0.0130)*** [0.0181]**
Observations	3,102	3,102	3,102	3,102	3,102	3,102	3,102	3,102
R^2	0.370	0.367	0.374	0.367	0.368	0.369	0.367	0.368
<i>Panel C: Turnout Change, 2020–2024</i>								
Change in Prices	−0.0010 (0.0178) [0.0306]	−0.0039 (0.0154) [0.0172]	−0.0176 (0.0224) [0.0379]	0.0748 (0.0177)*** [0.0209]***	−0.0516 (0.0203)** [0.0256]**	0.0369 (0.0211)* [0.0333]	0.0521 (0.0165)*** [0.0198]***	−0.0287 (0.0210) [0.0331]
Change in Wages	0.0712 (0.0353)** [0.0446]	0.0712 (0.0353)** [0.0447]	0.0723 (0.0348)** [0.0440]	0.0692 (0.0350)** [0.0448]	0.0719 (0.0347)** [0.0445]	0.0719 (0.0352)** [0.0445]	0.0702 (0.0350)** [0.0442]	0.0715 (0.0350)** [0.0446]
Observations	3,102	3,102	3,102	3,102	3,102	3,102	3,102	3,102
R^2	0.402	0.402	0.402	0.406	0.403	0.403	0.404	0.402
Price measure	Total	Food	Transport	Housing	Childcare	Healthcare	Other	Taxes
Wage measure	Total Wages	Total Wages	Total Wages	Total Wages	Total Wages	Total Wages	Total Wages	Total Wages
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Observations are at the county level. Each column reports estimates of specification (1) for a different price category k . Wage measure is total covered wages (QCEW) throughout. All variables standardized to zero mean and unit variance; coefficients are in standard-deviation units. Robust standard errors in parentheses; cluster-bootstrapped standard errors (100 replications, clustered at the state level) in brackets. All specifications include state fixed effects and controls for urbanization share, population density, median age, educational attainment (three shares), real GDP, and demographic composition. Significance stars reported next to each standard error type. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 3: Wage–Price Gap Re-parameterization: Real Wage Growth, Prices, and Presidential Electoral Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>Panel A: Two-Party Republican Vote Share Change, 2020–2024</i>								
Real Wage Growth	−0.0517 (0.0208)** [0.0311]*	−0.0560 (0.0221)** [0.0331]*	−0.0419 (0.0199)** [0.0322]	−0.0530 (0.0214)** [0.0317]*	−0.0533 (0.0208)** [0.0318]*	−0.0571 (0.0222)** [0.0321]*	−0.0543 (0.0216)** [0.0320]*	−0.0538 (0.0210)** [0.0317]*
Change in Prices	−0.1205 (0.0275)*** [0.0386]***	−0.0259 (0.0241) [0.0322]	−0.1821 (0.0518)*** [0.0530]***	−0.0610 (0.0224)*** [0.0306]***	−0.0947 (0.0315)*** [0.0467]**	−0.1000 (0.0280)*** [0.0319]***	−0.0472 (0.0223)** [0.0344]	−0.0905 (0.0292)*** [0.0375]**
Observations	3,102	3,102	3,102	3,102	3,102	3,102	3,102	3,102
R^2	0.413	0.409	0.420	0.410	0.411	0.411	0.409	0.410
<i>Panel B: Republican Vote Margin Change, 2020–2024</i>								
Real Wage Growth	−0.0477 (0.0181)*** [0.0248]*	−0.0516 (0.0188)*** [0.0262]**	−0.0403 (0.0193)** [0.0270]	−0.0488 (0.0181)*** [0.0251]*	−0.0493 (0.0180)*** [0.0254]*	−0.0532 (0.0189)*** [0.0253]**	−0.0502 (0.0184)*** [0.0253]**	−0.0496 (0.0181)*** [0.0252]**
Change in Prices	−0.1104 (0.0248)*** [0.0325]***	−0.0168 (0.0210) [0.0309]	−0.1499 (0.0493)*** [0.0511]***	−0.0595 (0.0215)*** [0.0269]**	−0.0803 (0.0285)*** [0.0377]**	−0.1108 (0.0299)*** [0.0329]***	−0.0410 (0.0197)** [0.0310]	−0.0878 (0.0271)*** [0.0337]***
Observations	3,102	3,102	3,102	3,102	3,102	3,102	3,102	3,102
R^2	0.370	0.367	0.374	0.367	0.368	0.369	0.367	0.368
<i>Panel C: Turnout Change, 2020–2024</i>								
Real Wage Growth	0.0987 (0.0489)** [0.0618]	0.1022 (0.0507)** [0.0642]	0.1016 (0.0489)** [0.0618]	0.0953 (0.0482)** [0.0616]	0.0997 (0.0481)** [0.0617]	0.1020 (0.0499)** [0.0631]	0.0979 (0.0488)** [0.0616]	0.0993 (0.0486)** [0.0619]
Change in Prices	0.0703 (0.0406)* [0.0516]	0.0672 (0.0378)* [0.0482]	0.0547 (0.0428) [0.0647]	0.1440 (0.0393)*** [0.0413]***	0.0203 (0.0421) [0.0455]	0.1088 (0.0405)*** [0.0647]*	0.1224 (0.0381)*** [0.0415]***	0.0428 (0.0426) [0.0567]
Observations	3,102	3,102	3,102	3,102	3,102	3,102	3,102	3,102
R^2	0.402	0.402	0.402	0.406	0.403	0.403	0.404	0.402
Price measure	Total	Food	Transport	Housing	Childcare	Healthcare	Other	Taxes
Wage measure	Total Wages	Total Wages	Total Wages	Total Wages	Total Wages	Total Wages	Total Wages	Total Wages
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Observations are at the county level. Each column reports estimates of specification (3) for a different price category k . $G_i^{(k)} \equiv \Delta W_i - \Delta P_i^{(k)}$ is category-specific real wage growth. The gap and all other variables are standardized to zero mean and unit variance. This specification is algebraically equivalent to the baseline (Table 2); R^2 values are identical by construction. Wage measure is total covered wages (QCEW) throughout. Robust standard errors in parentheses; cluster-bootstrapped standard errors (100 replications, clustered at the state level) in brackets. All specifications include state fixed effects and the full control set. Significance stars reported next to each standard error type. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 4: Restricted Model: Real Wage Growth Only

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>Panel A: Two-Party Republican Vote Share Change, 2020–2024</i>								
Real Wage Growth	0.0034 (0.0264) [0.0303]	-0.0388 (0.0187)** [0.0236]	0.0402 (0.0449) [0.0495]	-0.0187 (0.0200) [0.0226]	-0.0151 (0.0244) [0.0264]	-0.0186 (0.0239) [0.0336]	-0.0249 (0.0191) [0.0247]	-0.0164 (0.0240) [0.0313]
Observations	3,102	3,102	3,102	3,102	3,102	3,102	3,102	3,102
R ²	0.408	0.409	0.409	0.408	0.408	0.408	0.408	0.408
<i>Panel B: Republican Vote Margin Change, 2020–2024</i>								
Real Wage Growth	0.0027 (0.0218) [0.0246]	-0.0404 (0.0169)** [0.0196]**	0.0272 (0.0404) [0.0443]	-0.0154 (0.0161) [0.0177]	-0.0169 (0.0203) [0.0212]	-0.0105 (0.0197) [0.0277]	-0.0247 (0.0160) [0.0201]	-0.0133 (0.0199) [0.0258]
Observations	3,102	3,102	3,102	3,102	3,102	3,102	3,102	3,102
R ²	0.366	0.367	0.366	0.366	0.366	0.366	0.366	0.366
<i>Panel C: Turnout Change, 2020–2024</i>								
Real Wage Growth	0.0666 (0.0268)** [0.0289]**	0.0575 (0.0234)** [0.0204]**	0.0769 (0.0292)** [0.0323]**	0.0144 (0.0177) [0.0203]	0.0915 (0.0324)** [0.0384]**	0.0601 (0.0294)** [0.0250]**	0.0217 (0.0186) [0.0195]	0.0816 (0.0307)** [0.0333]**
Observations	3,102	3,102	3,102	3,102	3,102	3,102	3,102	3,102
R ²	0.401	0.400	0.401	0.398	0.403	0.400	0.398	0.402
Price measure	Total	Food	Transport	Housing	Childcare	Healthcare	Other	Taxes
Wage measure	Total Wages	Total Wages	Total Wages	Total Wages	Total Wages	Total Wages	Total Wages	Total Wages
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Observations are at the county level. Each column reports estimates of the restricted specification (4), which includes the wage–price gap $G_i^{(k)} \equiv \Delta W_i - \Delta P_i^{(k)}$ but omits $\Delta P_i^{(k)}$ as a separate regressor. The gap is standardized to zero mean and unit variance. Wage measure is total covered wages (QCEW) throughout. Robust standard errors in parentheses; cluster-bootstrapped standard errors (100 replications, clustered at the state level) in brackets. All specifications include state fixed effects and the full control set. Significance stars reported next to each standard error type. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 5: Summary Statistics — Congressional District Level

Variable	Mean	SD	Min	P25	P50	P75	Max	N
<i>Panel A: Electoral Outcomes — Changes</i>								
Change in Republican vote share pct (alt) 2024 vs 2020 (pp)	2.7725	14.5111	-54.4782	-2.0228	1.6257	6.9168	76.7762	1270
Change in Democratic vote share pct (alt) 2024 vs 2020 (pp)	-2.7725	14.5111	-76.7762	-6.9168	-1.6257	2.0228	54.4782	1270
Change in turnout, 2020–2024 (pp)	-1.1979	8.9272	-58.6538	-4.6976	-0.7063	2.8496	100.0004	1257
Change in Rep. vote margin, 2020–2024 (pp)	5.2059	26.2494	-108.9554	-4.0025	3.6392	13.4145	153.5524	1270
Change in challenger two-party vote share, 2020–2024 (pp)	-0.3500	13.6021	-46.5021	-5.0370	-0.1295	4.3564	45.5218	1270
<i>Panel B: Inflation Changes 2021–2024</i>								
Inflation food 2021–2024 (%) (CD agg)	28.3555	5.6174	4.7114	24.5436	28.0402	31.5192	63.0183	1270
Inflation transportation 2021–2024 (%) (CD agg)	27.5435	9.1898	12.9496	23.1298	27.4612	29.8338	139.7267	1270
Inflation housing 2021–2024 (%) (CD agg)	29.1304	9.4612	-6.0848	22.9856	28.7582	34.7958	59.2740	1270
Inflation childcare 2021–2024 (%) (CD agg)	27.2970	32.0684	-28.6451	5.3153	22.4954	45.1684	179.0404	1270
Inflation healthcare 2021–2024 (%) (CD agg)	17.3690	10.5466	-12.1624	11.7117	17.5795	22.6908	89.8601	1270
Inflation other necessities 2021–2024 (%) (CD agg)	19.6187	5.9073	-6.8493	16.0424	19.6606	23.3562	37.6495	1270
Inflation taxes 2021–2024 (%) (CD agg)	52.1004	19.4494	10.9503	35.8228	52.4396	66.3782	115.9399	1270
Inflation total 2021–2024 (%) (CD agg)	27.0154	7.6776	9.4235	21.0616	27.0834	32.0112	54.8455	1270
<i>Panel C: Nominal Wage Growth</i>								
Change in total wages, 2021–2024 (%)	19.6231	6.0939	-99.9966	15.7724	19.5875	22.9625	39.6619	1270
Change in average pay, 2021–2024 (%)	12.0942	3.0315	-5.0221	10.0136	12.5690	14.5290	19.8954	1270
<i>Panel D: Real Wage Growth (Total Wages – Inflation)</i>								
Real wage growth: total (Total Wages)	-7.3924	9.6474	-123.6485	-13.0591	-6.5442	-1.1732	17.0532	1270
Real wage growth: food (Total Wages)	-8.7325	8.4608	-130.7533	-13.4156	-7.7972	-3.6932	13.0914	1270
Real wage growth: transportation (Total Wages)	-7.9204	11.4246	-128.2221	-11.7332	-7.1896	-2.6737	11.5791	1270
Real wage growth: housing (Total Wages)	-9.5073	9.1512	-122.5997	-14.5879	-8.6946	-4.3744	20.4806	1270
Real wage growth: childcare (Total Wages)	-7.6739	33.4083	-168.0768	-22.5977	-1.1342	15.0679	49.7485	1270
Real wage growth: healthcare (Total Wages)	2.2541	11.8431	-112.2134	-4.7921	2.2039	9.0528	37.7086	1270
Real wage growth: other necessities (Total Wages)	0.0044	6.9654	-117.1529	-3.5652	0.1121	3.6635	25.2430	1270
Real wage growth: taxes (Total Wages)	-32.4773	19.7202	-142.2719	-46.2731	-31.9774	-17.0580	8.9568	1270
<i>Panel E: Controls</i>								
Percent of 2020 land classified as urban (CD agg)	0.3360	0.2844	0.0043	0.1070	0.2364	0.5157	1.0000	1270
2020 population density of the county (per square mile) (CD agg)	2378.3489	7143.2386	12.7143	287.0554	704.5940	1787.8507	7.48e+04	1270
Median age for total population (CD agg)	38.7762	3.0334	28.5059	37.0853	38.4216	40.3455	55.9452	1270
Percent of adults who are high school graduates (or equivalent), 2019–23 (CD agg)	26.1448	5.8529	9.9035	22.1227	26.1720	29.8108	43.6617	1270
Percent of adults completing some college or associate degree, 2019–23 (CD agg)	28.1863	4.2884	13.3611	25.5262	28.5886	31.2454	38.3545	1270
Percent of adults with a bachelor's degree or higher, 2019–23 (CD agg)	34.9019	9.1020	15.6038	28.1380	33.9776	41.0101	69.2567	1270
Real GDP (thousands of chained 2017 dollars) (2020) (CD agg)	4.65e+07	3.39e+07	968.3064	3.08e+07	3.90e+07	5.19e+07	3.38e+08	1270
Percent: HISPANIC OR LATINO: Total population: Hispanic or Latino (of any race)	18.8180	15.9063	1.2685	6.6860	12.9521	26.6364	89.0231	1270
Percent: RACE: Total population: One Race: White (%) (DP1 2020) (CD agg)	61.3989	17.4949	14.1000	47.0998	61.8209	76.0666	94.3410	1270

Notes: Observations are at the congressional district level. All variables correspond to the CD estimation sample ($N \approx 435$ districts).

Table 6: Congressional District: Inflation, Wages, and House Electoral Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>Panel A: Two-Party Republican Vote Share Change, 2020–2024</i>								
Change in Prices	−0.0697 (0.0611) [0.0923]	−0.0068 (0.0398) [0.0670]	0.0142 (0.0566) [0.1592]	0.0127 (0.0578) [0.0607]	0.0290 (0.0639) [0.1245]	−0.1699 (0.0669)** [0.1472]	0.0136 (0.0542) [0.0512]	−0.1383 (0.0705)* [0.0977]
Change in Wages	−0.0117 (0.0420) [0.0754]	−0.0062 (0.0425) [0.0778]	−0.0053 (0.0425) [0.0797]	−0.0094 (0.0434) [0.0800]	0.0012 (0.0427) [0.0749]	−0.0039 (0.0425) [0.0775]	−0.0093 (0.0427) [0.0794]	−0.0123 (0.0417) [0.0741]
Observations	1,270	1,270	1,270	1,270	1,270	1,270	1,270	1,270
R^2	0.108	0.108	0.108	0.108	0.108	0.115	0.108	0.111
<i>Panel B: Republican Vote Margin Change, 2020–2024</i>								
Change in Prices	−0.0643 (0.0621) [0.0811]	−0.0073 (0.0404) [0.0749]	0.0338 (0.0546) [0.1477]	0.0174 (0.0607) [0.0639]	0.0278 (0.0675) [0.1121]	−0.1790 (0.0708)** [0.1407]	0.0200 (0.0572) [0.0563]	−0.1401 (0.0723)* [0.0946]
Change in Wages	−0.0103 (0.0417) [0.0768]	−0.0053 (0.0426) [0.0811]	−0.0035 (0.0424) [0.0796]	−0.0096 (0.0437) [0.0838]	0.0018 (0.0419) [0.0757]	−0.0025 (0.0423) [0.0752]	−0.0098 (0.0428) [0.0840]	−0.0114 (0.0416) [0.0732]
Observations	1,294	1,294	1,294	1,294	1,294	1,294	1,294	1,294
R^2	0.072	0.071	0.071	0.071	0.071	0.079	0.071	0.075
<i>Panel C: Turnout Change, 2020–2024</i>								
Change in Prices	−0.0287 (0.0523) [0.0600]	0.0956 (0.0326)*** [0.0605]	0.0411 (0.0424) [0.1288]	0.0448 (0.0391) [0.0415]	−0.0847 (0.0596) [0.0694]	−0.0656 (0.0373)* [0.0519]	0.0797 (0.0371)** [0.0357]**	−0.0013 (0.0567) [0.0659]
Change in Wages	−0.2291 (0.0800)*** [0.1006]**	−0.2238 (0.0796)*** [0.1044]**	−0.2226 (0.0804)*** [0.1035]**	−0.2491 (0.0840)*** [0.1116]**	−0.2609 (0.0863)*** [0.1032]**	−0.2293 (0.0796)*** [0.1027]**	−0.2655 (0.0828)*** [0.1115]**	−0.2263 (0.0798)*** [0.1011]**
Observations	1,257	1,257	1,257	1,257	1,257	1,257	1,257	1,257
R^2	0.236	0.239	0.236	0.237	0.237	0.237	0.238	0.236
<i>Panel D: Challenger Two-Party Vote Share Change, 2020–2024</i>								
Change in Prices	−0.0147 (0.0596) [0.1026]	0.0226 (0.0410) [0.0854]	0.1322 (0.0563)** [0.1711]	−0.0687 (0.0433) [0.0685]	0.0148 (0.0617) [0.1467]	0.0116 (0.0488) [0.0919]	−0.0538 (0.0417) [0.0587]	0.0256 (0.0649) [0.1176]
Change in Wages	0.0112 (0.0401) [0.1151]	0.0135 (0.0396) [0.1135]	0.0177 (0.0404) [0.1121]	0.0310 (0.0411) [0.1204]	0.0160 (0.0405) [0.1225]	0.0123 (0.0396) [0.1133]	0.0258 (0.0404) [0.1171]	0.0136 (0.0398) [0.1141]
Observations	1,270	1,270	1,270	1,270	1,270	1,270	1,270	1,270
R^2	0.148	0.148	0.151	0.149	0.148	0.148	0.149	0.148
Price measure	Total	Food	Transport	Housing	Childcare	Healthcare	Other	Taxes
Wage measure	Total Wages							
State FE	Yes							
Controls	Yes							

Notes: Observations are at the congressional district level. Each column reports estimates of specification (1) for a different price category k , using U.S. House election outcomes. Wage measure is total covered wages (QCEW) throughout. All variables standardized to zero mean and unit variance. Robust standard errors in parentheses; cluster-bootstrapped standard errors (100 replications, clustered at the state level) in brackets. All specifications include state fixed effects and the full control set. The challenger outcome is defined as the two-party vote share of the party that did not win the prior House cycle (2018 for 2020, 2022 for 2024). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 7: Congressional District: Wage–Price Gap Re-parameterization (Most Demanding)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>Panel A: Two-Party Republican Vote Share Change, 2020–2024</i>								
Real Wage Growth	−0.0140 (0.0500) [0.0897]	−0.0077 (0.0524) [0.0959]	−0.0067 (0.0532) [0.0998]	−0.0092 (0.0425) [0.0785]	0.0015 (0.0544) [0.0956]	−0.0046 (0.0499) [0.0909]	−0.0094 (0.0432) [0.0804]	−0.0141 (0.0477) [0.0847]
Change in Prices	−0.0815 (0.0759) [0.1139]	−0.0130 (0.0627) [0.1153]	0.0089 (0.0751) [0.1767]	0.0034 (0.0633) [0.1036]	0.0302 (0.0840) [0.1486]	−0.1738 (0.0764)** [0.1574]	0.0043 (0.0628) [0.1010]	−0.1506 (0.0824)* [0.1172]
Observations	1,270	1,270	1,270	1,270	1,270	1,270	1,270	1,270
R^2	0.108	0.108	0.108	0.108	0.108	0.115	0.108	0.111
<i>Panel B: Republican Vote Margin Change, 2020–2024</i>								
Real Wage Growth	−0.0122 (0.0496) [0.0913]	−0.0065 (0.0526) [0.1000]	−0.0044 (0.0531) [0.0997]	−0.0094 (0.0429) [0.0821]	0.0023 (0.0535) [0.0967]	−0.0029 (0.0496) [0.0882]	−0.0100 (0.0433) [0.0851]	−0.0131 (0.0476) [0.0837]
Change in Prices	−0.0745 (0.0742) [0.0929]	−0.0125 (0.0641) [0.1288]	0.0303 (0.0713) [0.1628]	0.0078 (0.0646) [0.1039]	0.0297 (0.0852) [0.1248]	−0.1815 (0.0795)** [0.1504]	0.0102 (0.0645) [0.1085]	−0.1516 (0.0820)* [0.1105]
Observations	1,294	1,294	1,294	1,294	1,294	1,294	1,294	1,294
R^2	0.072	0.071	0.071	0.071	0.071	0.079	0.071	0.075
<i>Panel C: Turnout Change, 2020–2024</i>								
Real Wage Growth	−0.2724 (0.0951)*** [0.1196]**	−0.2760 (0.0982)*** [0.1288]**	−0.2789 (0.1008)*** [0.1297]**	−0.2442 (0.0823)*** [0.1094]**	−0.3330 (0.1102)*** [0.1317]**	−0.2690 (0.0934)*** [0.1205]**	−0.2687 (0.0838)*** [0.1129]**	−0.2587 (0.0912)*** [0.1156]**
Change in Prices	−0.2578 (0.0983)*** [0.1058]**	−0.1282 (0.0907) [0.1367]	−0.1815 (0.0984)* [0.1364]	−0.2042 (0.0801)** [0.1010]**	−0.3457 (0.1234)*** [0.1254]***	−0.2949 (0.0836)*** [0.1252]**	−0.1857 (0.0804)** [0.1086]*	−0.2276 (0.0979)** [0.1043]**
Observations	1,257	1,257	1,257	1,257	1,257	1,257	1,257	1,257
R^2	0.236	0.239	0.236	0.237	0.237	0.237	0.238	0.236
<i>Panel D: Challenger Two-Party Vote Share Change, 2020–2024</i>								
Real Wage Growth	0.0133 (0.0477) [0.1368]	0.0166 (0.0488) [0.1400]	0.0221 (0.0507) [0.1405]	0.0304 (0.0403) [0.1181]	0.0204 (0.0517) [0.1564]	0.0144 (0.0464) [0.1329]	0.0261 (0.0409) [0.1185]	0.0155 (0.0455) [0.1305]
Change in Prices	−0.0035 (0.0772) [0.1747]	0.0361 (0.0585) [0.1548]	0.1499 (0.0744)** [0.2047]	−0.0377 (0.0576) [0.1261]	0.0308 (0.0814) [0.2202]	0.0239 (0.0626) [0.1400]	−0.0280 (0.0568) [0.1283]	0.0391 (0.0805) [0.1762]
Observations	1,270	1,270	1,270	1,270	1,270	1,270	1,270	1,270
R^2	0.148	0.148	0.151	0.149	0.148	0.148	0.149	0.148
Price measure	Total	Food	Transport	Housing	Childcare	Healthcare	Other	Taxes
Wage measure	Total Wages	Total Wages	Total Wages	Total Wages				
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Observations are at the congressional district level. Each column reports estimates of specification (3) for a different price category k , using U.S. House election outcomes. $G_i^{(k)} \equiv \Delta W_i - \Delta P_i^{(k)}$ is category-specific real wage growth. Wage measure is total covered wages (QCEW) throughout. All variables standardized. Robust standard errors in parentheses; cluster-bootstrapped standard errors (100 replications, clustered at the state level) in brackets. All specifications include state fixed effects and the full control set. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 8: Congressional District: Restricted Model, Real Wage Growth Only

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>Panel A: Two-Party Republican Vote Share Change, 2020–2024</i>								
Real Wage Growth	0.0298 (0.0420) [0.0640]	0.0012 (0.0329) [0.0502]	−0.0114 (0.0402) [0.0872]	−0.0113 (0.0429) [0.0412]	−0.0171 (0.0435) [0.0687]	0.1000 (0.0525)* [0.0987]	−0.0123 (0.0413) [0.0364]	0.0595 (0.0459) [0.0657]
Observations	1,270	1,270	1,270	1,270	1,270	1,270	1,270	1,270
R ²	0.108	0.108	0.108	0.108	0.108	0.111	0.108	0.109
<i>Panel B: Republican Vote Margin Change, 2020–2024</i>								
Real Wage Growth	0.0278 (0.0439) [0.0690]	0.0021 (0.0326) [0.0488]	−0.0206 (0.0410) [0.0812]	−0.0143 (0.0451) [0.0447]	−0.0159 (0.0460) [0.0766]	0.1070 (0.0553)* [0.1000]	−0.0167 (0.0435) [0.0367]	0.0611 (0.0482) [0.0670]
Observations	1,294	1,294	1,294	1,294	1,294	1,294	1,294	1,294
R ²	0.071	0.071	0.071	0.071	0.071	0.075	0.071	0.072
<i>Panel C: Turnout Change, 2020–2024</i>								
Real Wage Growth	−0.0899 (0.0518)* [0.0698]	−0.1656 (0.0376)*** [0.0506]***	−0.1472 (0.0467)*** [0.0896]	−0.0765 (0.0387)** [0.0442]*	−0.0670 (0.0490) [0.0882]	−0.0407 (0.0455) [0.0442]	−0.1104 (0.0378)*** [0.0405]***	−0.1080 (0.0539)** [0.0740]
Observations	1,257	1,257	1,257	1,257	1,257	1,257	1,257	1,257
R ²	0.232	0.237	0.234	0.232	0.231	0.230	0.234	0.232
<i>Panel D: Challenger Two-Party Vote Share Change, 2020–2024</i>								
Real Wage Growth	0.0152 (0.0377) [0.0689]	−0.0078 (0.0343) [0.0667]	−0.0579 (0.0393) [0.0886]	0.0543 (0.0333) [0.0567]	0.0015 (0.0408) [0.0881]	0.0000 (0.0370) [0.0815]	0.0447 (0.0325) [0.0492]	−0.0036 (0.0380) [0.0758]
Observations	1,270	1,270	1,270	1,270	1,270	1,270	1,270	1,270
R ²	0.148	0.148	0.149	0.149	0.148	0.148	0.149	0.148
Price measure	Total	Food	Transport	Housing	Childcare	Healthcare	Other	Taxes
Wage measure	Total Wages	Total Wages	Total Wages	Total Wages	Total Wages	Total Wages	Total Wages	Total Wages
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Observations are at the congressional district level. Each column reports estimates of specification (4), which includes the wage–price gap but omits prices as a separate regressor. U.S. House election outcomes. Wage measure is total covered wages (QCEW) throughout. All variables standardized. Robust standard errors in parentheses; cluster-bootstrapped standard errors (100 replications, clustered at the state level) in brackets. All specifications include state fixed effects and the full control set. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Online Appendix - Not for publication

Figures in the Appendix

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A Micro-Foundation: From Voter Choice to Estimating Equations

This appendix derives the paper’s estimating equations from a standard discrete-choice model of voting. The main text presents specifications (1)–(4) directly and discusses identification informally; here we show that these equations are the natural reduced-form implications of a logit model of two-party voter choice observed over two elections.

Individual voter choice

Consider voter i in county c during election t , who chooses between two parties $j \in \{D, R\}$. The voter’s utility from party j is

$$(5) \quad U_{ijct} = V_{jct} + \varepsilon_{ijct},$$

where V_{jct} is a systematic component common to all voters in county c and ε_{ijct} is an idiosyncratic preference shock. We decompose the systematic component as

$$(6) \quad V_{jct} = \alpha_{jc} + \delta_{jt} + \mathbf{X}'_{ct}\boldsymbol{\beta}_j,$$

where α_{jc} is a county–party fixed effect capturing the long-run partisan lean of county c toward party j (ideology, demographics, political culture); δ_{jt} is a national election–party shock (candidate effects, national mood, party platform); and $\mathbf{X}'_{ct}\boldsymbol{\beta}_j$ captures how local economic conditions affect support for party j .²⁰

Aggregation to county-level vote shares

Under the logit assumption, the probability that voter i chooses party j takes the standard form:

$$\Pr(j \mid c, t) = \frac{\exp(V_{jct})}{\exp(V_{Dct}) + \exp(V_{Rct})}.$$

Invoking representativeness, the two-party vote share of party j in county c at election t is $s_{jct} = \Pr(j \mid c, t)$. Taking the log-odds of Republican relative to Democratic vote share and substituting (6):

$$(7) \quad \begin{aligned} \log\left(\frac{s_{Rct}}{s_{Dct}}\right) &= (V_{Rct} - V_{Dct}) \\ &= \underbrace{(\alpha_{Rc} - \alpha_{Dc})}_{\equiv \alpha_c} + \underbrace{(\delta_{Rt} - \delta_{Dt})}_{\equiv \delta_t} + \mathbf{X}'_{ct} \underbrace{(\boldsymbol{\beta}_R - \boldsymbol{\beta}_D)}_{\equiv \boldsymbol{\beta}} + \varepsilon_{ct}, \end{aligned}$$

²⁰The model rests on four standard assumptions: (i) *additive separability* of systematic utility in county–party effects, national shocks, and local economic conditions; (ii) *logit errors*—the idiosyncratic shocks ε_{ijct} are i.i.d. Type-I extreme value (Gumbel) across voters, parties, and elections; (iii) *representativeness*—aggregate choice probabilities equal observed two-party vote shares; and (iv) *common coefficients*—the economic response parameters $\boldsymbol{\beta}_j$ are common across counties in the baseline specification.

where ε_{ct} is a county-level unobservable that satisfies the conditional exogeneity restriction $E[\varepsilon_{ct} \mid \alpha_c, \delta_t, \mathbf{X}_{ct}] = 0$. The relative parameter $\beta \equiv \beta_R - \beta_D$ captures the *differential* effect of economic conditions on Republican versus Democratic support: a positive coefficient means the economic variable favors Republicans on net.

First-differencing with two elections

We observe each county in exactly two presidential elections, $t \in \{2020, 2024\}$. Applying equation (7) to both years and taking the difference within county c :

$$(8) \quad \begin{aligned} \Delta \log \left(\frac{s_R}{s_D} \right)_c &\equiv \log \left(\frac{s_{Rc,2024}}{s_{Dc,2024}} \right) - \log \left(\frac{s_{Rc,2020}}{s_{Dc,2020}} \right) \\ &= \underbrace{(\delta_{2024} - \delta_{2020})}_{\kappa} + \Delta \mathbf{X}'_c \beta + \Delta \varepsilon_c. \end{aligned}$$

The county fixed effects α_c are eliminated exactly. The national swing between elections collapses to a constant κ , which is absorbed by the regression intercept. Identification comes entirely from cross-county heterogeneity in the changes $\Delta \mathbf{X}_c$, that is, from differences across counties in how local economic conditions evolved between 2020 and 2024. Any time-invariant county characteristic (baseline ideology, geography, demographics) is differenced out.

State-specific trends. Equation (7) assumes a single national swing δ_t common to all counties. In practice, electoral dynamics vary across states due to local political environments, down-ballot races, and turnout patterns. We therefore replace δ_t with state-specific swings $\delta_{s(c),t}$ in the level model. Upon first-differencing, these yield state fixed effects $\mu_{s(c)} \equiv \delta_{s(c),2024} - \delta_{s(c),2020}$, restricting identification to within-state variation in economic conditions.

Differential trends by baseline characteristics. Equation (8) includes only the *changes* $\Delta \mathbf{X}_c$ in time-varying covariates; any time-invariant county characteristic is absorbed by α_c and differenced out. However, we may want the electoral shift to vary systematically with observable baseline county characteristics. To do so, we augment the level model (7) with a term $\mathbf{Z}'_c \gamma \cdot t$, where \mathbf{Z}_c collects time-invariant (or slowly varying) characteristics such as urbanization share, population density, age structure, educational attainment, real GDP, and demographic composition. With two periods, first-differencing yields $\mathbf{Z}'_c \gamma$: baseline county characteristics enter the differenced equation *in levels*. The coefficient vector γ captures *differential linear trends*—that is, systematic differences in the 2020-to-2024 electoral shift as a linear function of baseline county type. Crucially, including \mathbf{Z}_c does not “control for” urbanization per se (which is time-invariant and already absorbed by α_c); rather, it allows the electoral *change* to differ across more urban versus more rural, more educated versus less educated counties, and so on.

Combining both extensions, the enriched first-differenced equation becomes:

$$(9) \quad \Delta \log \left(\frac{s_R}{s_D} \right)_c = \mu_{s(c)} + \Delta \mathbf{X}'_c \boldsymbol{\beta} + \mathbf{Z}'_c \boldsymbol{\gamma} + \Delta \varepsilon_c.$$

State fixed effects absorb state-level average swings; the level controls \mathbf{Z}_c absorb differential linear trends across county types. Identification of $\boldsymbol{\beta}$ therefore relies on within-state, within-county-type variation in economic changes, that is, the residual variation in $\Delta \mathbf{X}_c$ after partialing out state means and linear functions of baseline characteristics.

From log-odds to linear vote-share changes

Equation (8) implies that the natural dependent variable is the change in log-odds of the Republican two-party vote share. In practice, we use the linear change in vote share Δs_c^R and the change in the Republican margin Δm_c as our primary outcome variables. This linear approximation is standard in the applied political economy literature (Autor et al., 2020; Charles, Hurst, & Schwartz, 2019; Dippel, Gold, Hebllich, & Pinto, 2022, see, e.g.) and is justified as follows.

Let $s \equiv s_R/(s_R + s_D)$ denote the Republican two-party share. The logistic transformation $\ell(s) = \log(s/(1-s))$ is approximately linear in s for values away from 0 and 1. A first-order Taylor expansion around the sample mean \bar{s} gives

$$\Delta \log \left(\frac{s_R}{s_D} \right) \approx \frac{1}{\bar{s}(1-\bar{s})} \cdot \Delta s^R.$$

The coefficient $[\bar{s}(1-\bar{s})]^{-1}$ is a positive constant (approximately 4 when $\bar{s} \approx 0.5$, as in our sample). This is a scale factor that rescales coefficients but preserves their signs and relative magnitudes across specifications.

However, this approximation is exact only when two-party vote shares are far from 0 and 1 *and* when $\bar{s}(1-\bar{s})$ does not vary systematically across counties. In practice, counties with extreme partisan leans (very high or very low Republican share) have $\bar{s}(1-\bar{s})$ well below 0.25, which amplifies the log-odds change relative to the linear change.

Table A.1 verifies this empirically for total inflation using the paper's three specifications (baseline, gap-plus-price, and gap-only) under the most demanding controls. The two dependent variables, the standardized linear change Δs^R and the standardized log-odds change $\Delta \log(s_R/s_D)$, have a cross-county correlation of 0.93; three counties with 0% or 100% Republican two-party share are excluded from the log-odds specification. The qualitative conclusions are identical across both dependent variables: inflation is negative and significant in the baseline and gap-plus-price specifications; real wage growth is negative and significant in the gap-plus-price specification; and the gap alone is statistically insignificant when entered without prices. In the gap-plus-price specification, the log-odds coefficients are in fact larger in absolute value and more precisely estimated than their

linear counterparts (e.g., the real-wage-growth coefficient is -0.136 with $t = -2.63$ under log-odds versus -0.052 with $t = -2.49$ under the linear DV), reflecting the logit model’s natural weighting of vote-share variation.

The vote margin $\Delta m_c = \Delta(v^R - v^D)$ is an affine transformation of Δs^R and inherits the same properties. Turnout, the third outcome, is not derived from the two-party logit but is motivated by the same accountability framework: voters who experience adverse (or favorable) economic conditions may increase (or decrease) their propensity to participate, a channel that maps into a reduced-form specification with the same right-hand-side structure.

Table A.1: Linear Vote-Share Change vs. Log-Odds Change as Dependent Variable (Total Inflation)

	Baseline		Gap + Price		Gap Only	
	(1) Δs^R	(2) $\Delta \log \frac{s_R}{s_D}$	(3) Δs^R	(4) $\Delta \log \frac{s_R}{s_D}$	(5) Δs^R	(6) $\Delta \log \frac{s_R}{s_D}$
Change in Prices	-0.083^{***} (0.024)	-0.045^{**} (0.022)	-0.121^{***} (0.027)	-0.143^{***} (0.044)		
Change in Wages	-0.037^{**} (0.015)	-0.098^{***} (0.037)				
Real Wage Growth			-0.052^{**} (0.021)	-0.136^{***} (0.052)	0.003 (0.026)	-0.071 (0.049)
R^2	0.413	0.430	0.413	0.430	0.408	0.423
Observations	3,102	3,099	3,102	3,099	3,102	3,099
Controls	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Observations are at the county level. All regressions use total EPI Family Budget inflation (2021–2024) and the most demanding specification (full controls and state fixed effects). Odd columns use the standardized change in two-party Republican vote share (Δs^R) as the dependent variable; even columns use the standardized change in log-odds ($\Delta \log(s_R/s_D)$). Columns (1)–(2) correspond to the baseline specification (equation 1); (3)–(4) to the gap-plus-price reparameterization (equation 3); (5)–(6) to the restricted gap-only model (equation 4). The two dependent variables have a cross-county correlation of 0.93; 3 counties with 0% or 100% Republican two-party share are dropped in the log-odds specification. All variables standardized. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Deriving the paper’s estimating equations

We now apply equation (9). The time-varying economic conditions \mathbf{X}_{ct} comprise local inflation $P_c^{(k)}$ for expenditure category k and nominal wage growth W_c , so $\Delta \mathbf{X}_c$ includes $\Delta P_c^{(k)}$ and ΔW_c . The baseline characteristics \mathbf{Z}_c and state fixed effects $\mu_{s(c)}$ are as defined above. The estimating equation is therefore:

$$(1) \quad \Delta y_c = \beta_1 \Delta P_c^{(k)} + \beta_2 \Delta W_c + \mathbf{Z}'_c \boldsymbol{\gamma} + \mu_{s(c)} + \varepsilon_c,$$

which is the baseline specification in the main text. Here β_1 captures how Republican electoral gains covary with local price growth holding nominal wages fixed, and β_2 captures the association with nominal wage growth holding prices fixed.

Defining the category-specific wage–price gap $G_c^{(k)} \equiv \Delta W_c - \Delta P_c^{(k)}$ and substituting $\Delta W_c = G_c^{(k)} + \Delta P_c^{(k)}$ into the baseline yields:

$$(3) \quad \Delta y_c = \delta_1 G_c^{(k)} + \delta_2 \Delta P_c^{(k)} + \mathbf{Z}'_c \boldsymbol{\gamma} + \mu_{s(c)} + \varepsilon_c,$$

where $\delta_1 = \beta_2$ and $\delta_2 = \beta_1 + \beta_2$. This is an algebraic rotation: it leaves fitted values and R^2 unchanged. Its value is interpretive. The coefficient δ_1 measures how outcomes covary with real wage growth (the purchasing-power gap), while δ_2 captures the residual association of nominal price growth *conditional on* the gap. The pocketbook voting restriction $H_0^{(1)}$ ($\delta_1 = 0$) tests whether real wage growth is electorally relevant; the pure pocketbook restriction $H_0^{(2)}$ ($\delta_2 = 0$) holds if and only if $\beta_1 = -\beta_2$, meaning voters respond to inflation only through its effect on real wages, with no residual price salience. Finally, the gap-only specification

$$(4) \quad \Delta y_c = \delta_1 G_c^{(k)} + \mathbf{Z}'_c \boldsymbol{\gamma} + \mu_{s(c)} + \varepsilon_c$$

imposes this restriction and tests whether real wage growth alone captures the relevant covariation.

B Validation: EPI Family Budget Inflation vs. CPI-U

A natural concern with any non-standard price measure is whether it tracks established price indexes. We address this by comparing our county-level EPI Family Budget inflation against official CPI-U inflation from the Bureau of Labor Statistics. The CPI is constructed from price quotes collected in 75 Primary Sampling Units (PSUs), of which 23 are “self-representing”—metropolitan areas large enough that BLS publishes individual CPI indexes. Figure 2(a) maps both types of PSUs. We restrict attention to the 21 self-representing PSUs in the continental United States (excluding Anchorage and Honolulu), computing CPI inflation as the percentage change in the annual average CPI-U index between 2021 and 2024 (not seasonally adjusted). We aggregate county-level EPI Family Budget inflation to the PSU level using 2020 Census county population as weights.

The EPI Family Budget Calculator includes seven expenditure categories: food, housing, transportation, healthcare, childcare, taxes, and other necessities. Four of these—food, housing, transportation, and healthcare—have direct counterparts in the CPI. The remaining three—childcare, taxes, and other necessities—have no metropolitan-level CPI equivalent but together account for over a third of the family budget. Table B.2 reports the mean and median annual expenditure and budget share for each category.

Table B.2: EPI Family Budget Composition, 2021

Category	Mean (\$)	Median (\$)	Share (%)	CPI counterpart
Food	9	9	11.8	✓
Housing	10	9	13.0	✓
Transportation	1	1	1.6	✓
Healthcare	16	16	20.6	✓
Childcare	13	12	16.4	
Taxes	8	8	10.3	
Other necessities	7	7	9.0	
Total	78	76	100.0	

Notes: Observations are at the county level. Annual expenditures for a two-parent, two-child family from the EPI Family Budget Calculator (2021 values). Mean and median computed across all counties in the regression sample. The “CPI counterpart” column indicates categories with a direct equivalent in the CPI-U index at the metropolitan level.

To validate the EPI measure, we construct a restricted basket containing only the four CPI-comparable categories and compare its inflation against CPI-U All Items. Figure B.1 plots the result. The two measures are strongly correlated ($r = 0.74$), confirming that the EPI captures the same broad cross-sectional pattern of local price growth as the CPI when the basket composition is comparable. Metropolitan areas where CPI inflation was highest (Miami, Tampa, Phoenix) also exhibit the highest EPI inflation in CPI-comparable categories, and vice versa.

This validation exercise highlights a key advantage of the EPI measure over the

CPI for studying pocketbook voting. The EPI not only tracks CPI-comparable price variation well, but also captures childcare and tax burdens—expenditure categories absent from the CPI that exhibit large geographic variation and are highly salient for household economic perceptions. The identifying variation in our regressions therefore reflects local differences in the full cost of raising a family, including cost pressures that the CPI does not measure.

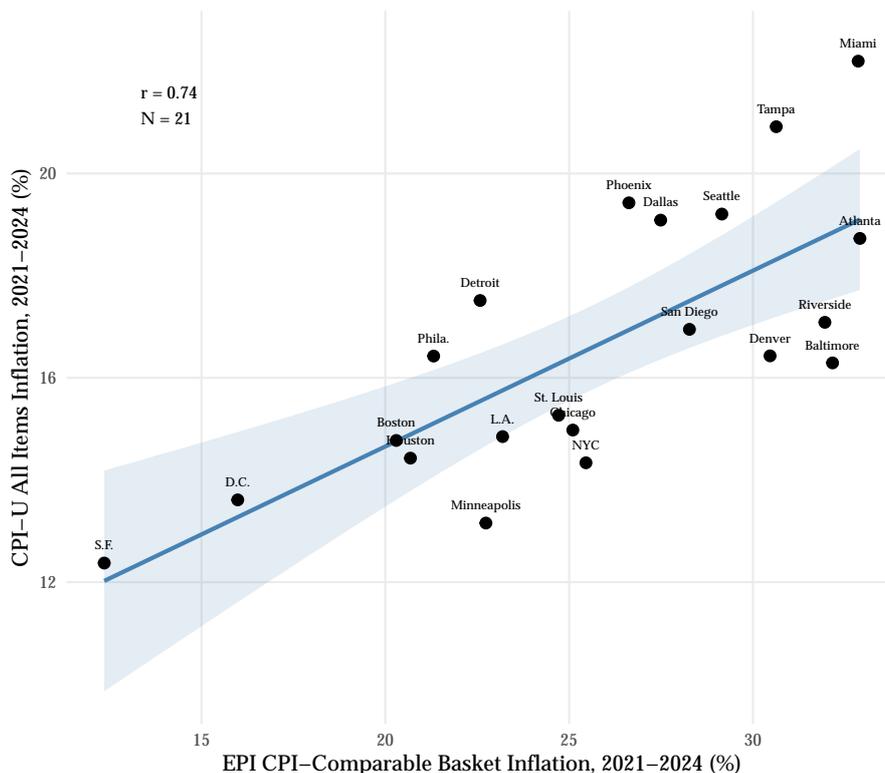


Figure B.1: EPI CPI-Comparable Basket Inflation vs. CPI-U All Items Inflation, 2021–2024

Notes: Each point represents one of 21 self-representing CPI Primary Sampling Units in the continental United States. The horizontal axis plots population-weighted average EPI Family Budget inflation computed using only the four expenditure categories with direct CPI counterparts: food, housing, transportation, and healthcare (excluding taxes, childcare, and other necessities). The vertical axis plots official CPI-U All Items inflation (annual average, not seasonally adjusted). The solid line is the OLS fit. CPI data retrieved from the Bureau of Labor Statistics public API.

Table B.3: Summary Statistics — County Level, Supplementary Variables

Variable	Mean	SD	Min	P25	P50	P75	Max	N
<i>Panel A: Electoral Outcomes — Levels</i>								
Two-party Rep. vote share 2020 (%)	66.1992	16.2658	5.5330	57.0047	69.5876	78.7896	96.8864	3102
Two-party Rep. vote share 2024 (%)	67.9132	15.8099	6.6853	59.0949	71.3618	80.2003	100.0000	3100
Turnout 2020 (%)	63.7016	9.8037	3.3900	57.3929	63.8743	70.3684	100.0000	3102
Turnout 2024 (%)	63.2457	11.0023	0.0000	56.2957	63.3827	70.7273	100.0000	3102
Rep. vote margin 2020 (pp)	31.8912	32.0096	-86.7524	13.7477	38.5096	56.6332	93.0909	3102
Rep. vote margin 2024 (pp)	35.3622	31.1832	-83.1600	17.9302	42.1083	59.6479	98.6502	3102
<i>Panel B: Family Budget Costs — Levels</i>								
FBC food budget 2021 (thousands)	0.0092	0.0010	0.0064	0.0086	0.0091	0.0097	0.0209	3102
FBC food budget 2024 (thousands)	0.0117	0.0012	0.0089	0.0109	0.0115	0.0122	0.0257	3102
FBC housing budget 2021 (thousands)	0.0102	0.0031	0.0058	0.0086	0.0090	0.0106	0.0480	3102
FBC housing budget 2024 (thousands)	0.0128	0.0040	0.0064	0.0105	0.0111	0.0134	0.0486	3102
FBC childcare budget 2021 (thousands)	0.0128	0.0041	0.0040	0.0102	0.0122	0.0151	0.0420	3102
FBC childcare budget 2024 (thousands)	0.0151	0.0052	0.0046	0.0118	0.0142	0.0175	0.0529	3102
FBC transportation budget 2021 (thousands)	0.0012	0.0001	0.0002	0.0012	0.0012	0.0013	0.0016	3102
FBC transportation budget 2024 (thousands)	0.0016	0.0001	0.0005	0.0015	0.0016	0.0017	0.0022	3102
FBC healthcare budget 2021 (thousands)	0.0161	0.0027	0.0104	0.0143	0.0158	0.0173	0.0280	3102
FBC healthcare budget 2024 (thousands)	0.0183	0.0032	0.0121	0.0163	0.0177	0.0197	0.0308	3102
FBC other necessities budget 2021 (thousands)	0.0070	0.0014	0.0051	0.0063	0.0066	0.0073	0.0217	3102
FBC other necessities budget 2024 (thousands)	0.0082	0.0016	0.0056	0.0073	0.0077	0.0085	0.0215	3102
FBC taxes budget 2021 (thousands)	0.0080	0.0027	0.0032	0.0063	0.0076	0.0092	0.0339	3102
FBC taxes budget 2024 (thousands)	0.0116	0.0037	0.0062	0.0094	0.0109	0.0126	0.0476	3102
FBC total budget 2021 (thousands)	0.0781	0.0108	0.0597	0.0712	0.0761	0.0826	0.1729	3102
FBC total budget 2024 (thousands)	0.0972	0.0142	0.0751	0.0886	0.0941	0.1021	0.2227	3102
<i>Panel C: Wage Levels</i>								
Annual Total Wages, Total covered, all industries, 2021	2.95e+09	1.38e+10	2.58e+06	1.27e+08	3.65e+08	1.18e+09	3.50e+11	3102
Annual Total Wages, Total covered, all industries, 2024	3.51e+09	1.62e+10	2.26e+06	1.54e+08	4.37e+08	1.42e+09	4.07e+11	3102
Annual Average Pay, Total covered, all industries, 2021	4.71e+04	1.11e+04	2.67e+04	4.07e+04	4.50e+04	5.07e+04	1.85e+05	3102
Annual Average Pay, Total covered, all industries, 2024	5.39e+04	1.20e+04	3.19e+04	4.67e+04	5.14e+04	5.79e+04	1.97e+05	3102
<i>Panel D: Real Wage Growth (Average Pay – Inflation)</i>								
Real wage growth: total (Avg Pay)	-9.8354	10.1513	-77.5309	-15.4514	-9.6027	-3.6100	80.9735	3102
Real wage growth: food (Avg Pay)	-11.9140	10.8512	-71.5596	-18.5245	-11.7965	-5.4017	63.0954	3102
Real wage growth: transportation (Avg Pay)	-16.7189	9.1303	-149.7011	-21.2437	-16.5210	-12.0456	69.5834	3102
Real wage growth: housing (Avg Pay)	-11.0145	11.8773	-78.7609	-17.7476	-10.5013	-4.1554	84.1568	3102
Real wage growth: childcare (Avg Pay)	-5.9893	30.5612	-231.5346	-20.5612	-0.2569	13.5161	104.9722	3102
Real wage growth: healthcare (Avg Pay)	-0.1442	16.1322	-102.9763	-8.5698	0.4594	9.5742	85.8988	3102
Real wage growth: other necessities (Avg Pay)	-2.3352	8.9276	-57.6257	-7.3355	-2.6723	2.3682	82.9040	3102
Real wage growth: taxes (Avg Pay)	-32.9301	21.3302	-134.7422	-44.2879	-30.4289	-18.8346	69.2668	3102

Notes: Observations are at the county level. All variables correspond to the county estimation sample ($N = 3,102$ counties).

C Geographic Distribution Maps

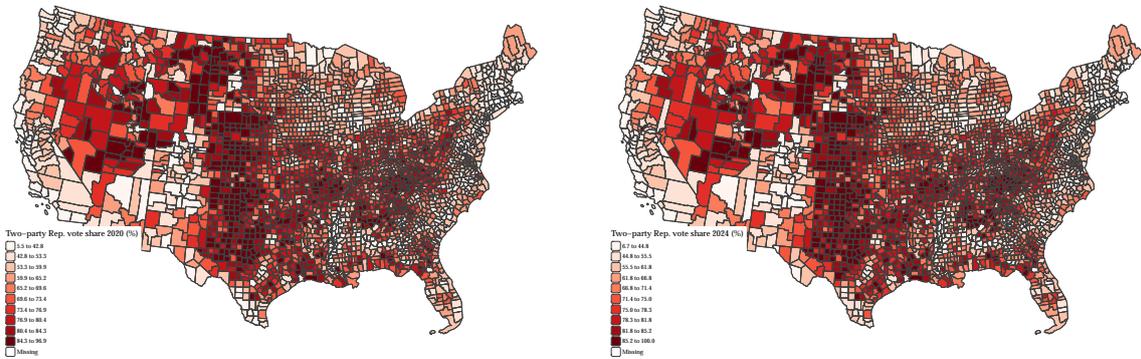
This appendix presents county-level maps illustrating the geographic distribution of electoral outcomes (levels and changes), expenditure-category inflation, wage–price gaps, and wage and inflation growth. These figures complement the summary statistics reported in Tables 1–B.3.

Table B.4: Summary Statistics — Congressional District Level, Supplementary Variables

Variable	Mean	SD	Min	P25	P50	P75	Max	N
<i>Panel A: Electoral Outcomes — Levels</i>								
Two-party Rep. vote share 2020 (%)	48.1657	20.8874	0.0000	34.6059	48.9706	62.5445	100.0000	1270
Two-party Rep. vote share 2024 (%)	50.9382	21.9529	0.0000	36.5745	50.6321	65.0498	100.0000	1270
Turnout 2020 (%)	63.1287	17.6396	-0.0004	52.8163	61.4231	71.2092	100.0000	1257
Turnout 2024 (%)	61.9308	18.3920	0.0001	50.8241	60.6204	71.1548	100.0000	1257
Rep. vote margin 2020 (pp)	-3.3874	39.1407	-100.0000	-29.8500	-2.0385	24.7131	100.0000	1270
Rep. vote margin 2024 (pp)	1.8185	40.3669	-100.0000	-26.8510	1.2643	29.7037	100.0000	1270
<i>Panel B: Family Budget Costs — Levels</i>								
FBC food budget 2021 (CD agg) (thousands)	0.0098	0.0012	0.0071	0.0091	0.0096	0.0104	0.0186	1270
FBC food budget 2024 (CD agg) (thousands)	0.0126	0.0012	0.0101	0.0117	0.0125	0.0133	0.0194	1270
FBC housing budget 2021 (CD agg) (thousands)	0.0151	0.0058	0.0081	0.0106	0.0135	0.0174	0.0458	1270
FBC housing budget 2024 (CD agg) (thousands)	0.0193	0.0069	0.0103	0.0137	0.0175	0.0234	0.0430	1270
FBC childcare budget 2021 (CD agg) (thousands)	0.0167	0.0057	0.0057	0.0131	0.0158	0.0188	0.0393	1270
FBC childcare budget 2024 (CD agg) (thousands)	0.0208	0.0072	0.0086	0.0157	0.0189	0.0250	0.0529	1270
FBC transportation budget 2021 (CD agg) (thousands)	0.0012	0.0001	0.0002	0.0011	0.0012	0.0012	0.0014	1270
FBC transportation budget 2024 (CD agg) (thousands)	0.0015	0.0002	0.0005	0.0014	0.0015	0.0016	0.0017	1270
FBC healthcare budget 2021 (CD agg) (thousands)	0.0147	0.0021	0.0104	0.0132	0.0144	0.0159	0.0258	1270
FBC healthcare budget 2024 (CD agg) (thousands)	0.0172	0.0027	0.0121	0.0151	0.0168	0.0183	0.0293	1270
FBC other necessities budget 2021 (CD agg) (thousands)	0.0090	0.0024	0.0059	0.0072	0.0084	0.0101	0.0211	1270
FBC other necessities budget 2024 (CD agg) (thousands)	0.0107	0.0026	0.0071	0.0087	0.0101	0.0124	0.0197	1270
FBC taxes budget 2021 (CD agg) (thousands)	0.0105	0.0047	0.0036	0.0074	0.0092	0.0122	0.0320	1270
FBC taxes budget 2024 (CD agg) (thousands)	0.0156	0.0063	0.0066	0.0112	0.0135	0.0188	0.0461	1270
<i>Panel C: Wage Levels</i>								
Annual Total Wages, Total covered, all industries, 2021 (CD agg)	2.11e+10	1.62e+10	4.62e+05	1.31e+10	1.75e+10	2.37e+10	1.59e+11	1270
Annual Total Wages, Total covered, all industries, 2024 (CD agg)	2.51e+10	1.87e+10	5.51e+05	1.59e+10	2.09e+10	2.81e+10	1.93e+11	1270
Annual Average Pay, Total covered, all industries, 2021 (CD agg)	6.27e+04	1.88e+04	3.95e+04	5.09e+04	5.84e+04	6.96e+04	1.85e+05	1270
Annual Average Pay, Total covered, all industries, 2024 (CD agg)	6.99e+04	1.92e+04	4.46e+04	5.78e+04	6.61e+04	7.73e+04	1.97e+05	1270
<i>Panel D: Real Wage Growth (Average Pay – Inflation)</i>								
Real wage growth: total (Avg Pay)	-14.9213	8.1958	-47.2128	-20.2612	-14.2713	-8.6854	3.4041	1270
Real wage growth: food (Avg Pay)	-16.2614	5.8025	-49.5415	-19.6126	-15.4161	-12.2965	0.0485	1270
Real wage growth: transportation (Avg Pay)	-15.4493	10.3629	-135.8562	-17.7780	-14.4625	-11.4876	2.6414	1270
Real wage growth: housing (Avg Pay)	-17.0362	9.0327	-47.0286	-23.0633	-15.9505	-10.9339	10.9392	1270
Real wage growth: childcare (Avg Pay)	-15.2028	32.6458	-172.9601	-30.6757	-9.7537	7.9650	40.1969	1270
Real wage growth: healthcare (Avg Pay)	-5.2749	11.3701	-74.2341	-12.1787	-5.2847	1.5471	25.6893	1270
Real wage growth: other necessities (Avg Pay)	-7.5245	5.5959	-25.8555	-11.1614	-7.0062	-4.2915	12.0886	1270
Real wage growth: taxes (Avg Pay)	-40.0062	19.3726	-108.3072	-54.8937	-40.5666	-23.5581	-4.1502	1270

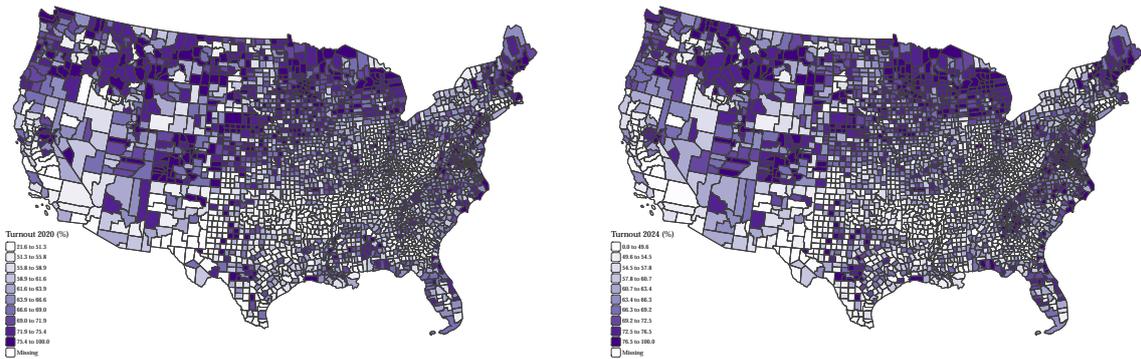
Notes: Observations are at the congressional district level. All variables correspond to the CD estimation sample ($N \approx 435$ districts).

Figure C.2: County-level electoral outcomes — levels



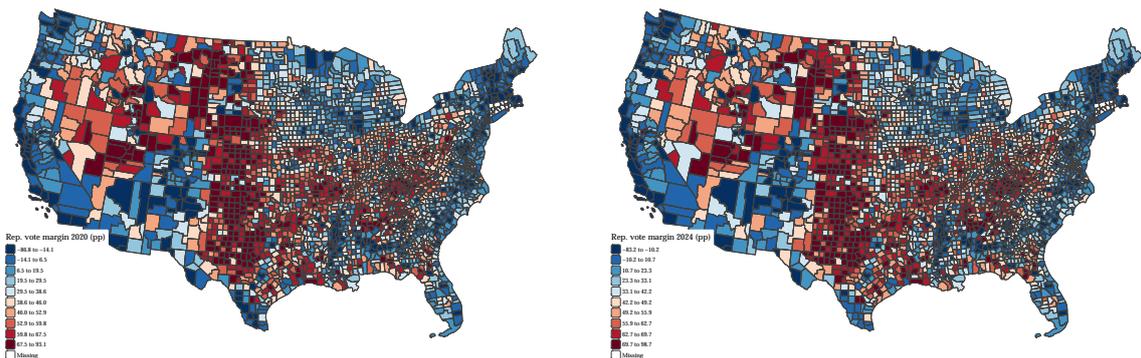
(a) Two-party Rep. vote share, 2020

(b) Two-party Rep. vote share, 2024



(c) Turnout, 2020

(d) Turnout, 2024

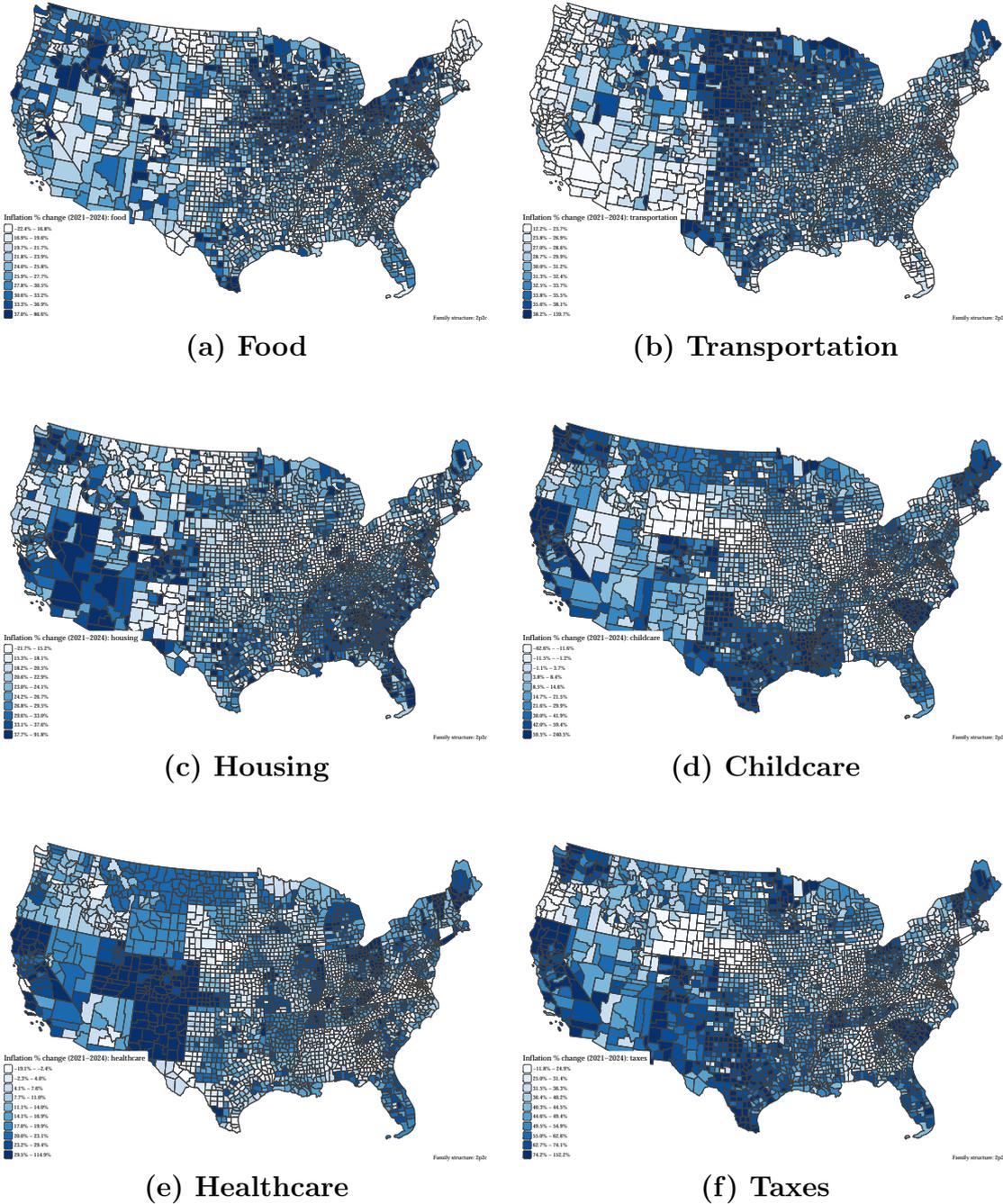


(e) Rep. vote margin, 2020

(f) Rep. vote margin, 2024

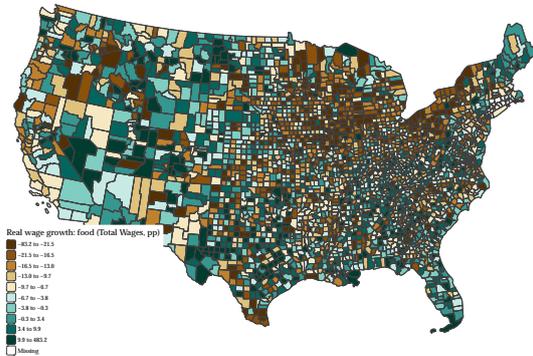
Notes: Each map shows the county-level distribution of the indicated electoral outcome variable, corresponding to Panel A of Table B.3. Counties are shaded by decile. CONUS counties only (Alaska, Hawaii, and territories excluded). Albers Equal-Area projection (EPSG:5070).

Figure C.3: County-level inflation, 2021–2024, by expenditure category

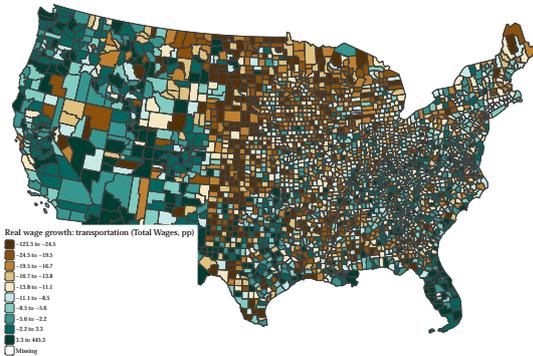


Notes: Each map shows the percent change in the EPI Family Budget Calculator cost estimate (two-parent, two-child family) between 2021 and 2024 for the indicated expenditure category, corresponding to Panel B of Table 1. Counties are shaded by decile. CONUS counties only. Albers Equal-Area projection (EPSG:5070).

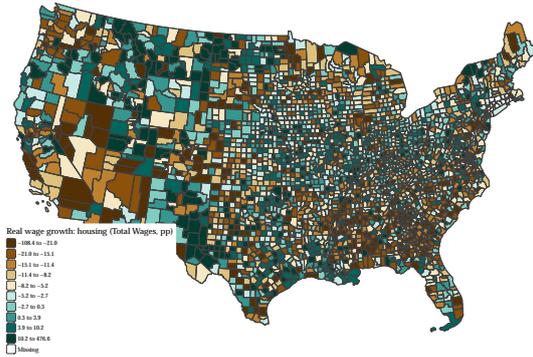
Figure C.4: County-level real wage growth (total wages), by expenditure category



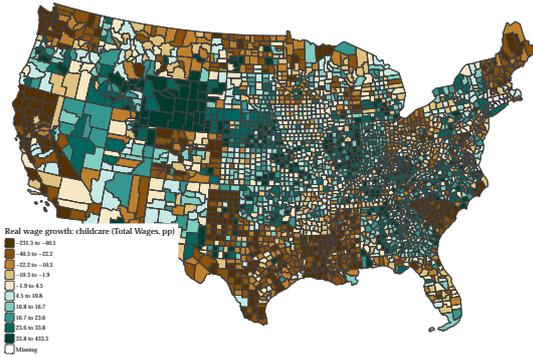
(a) Food



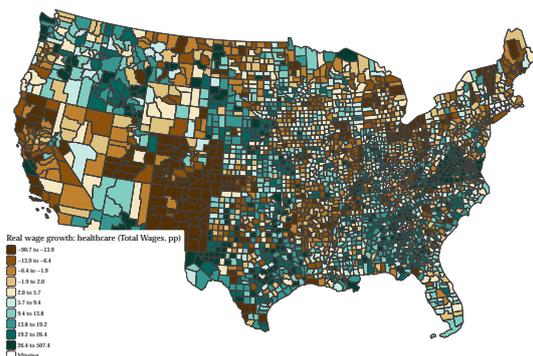
(b) Transportation



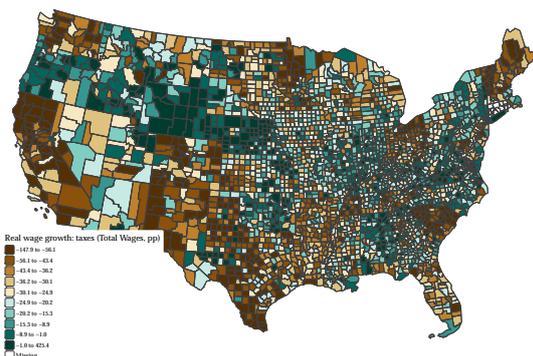
(c) Housing



(d) Childcare



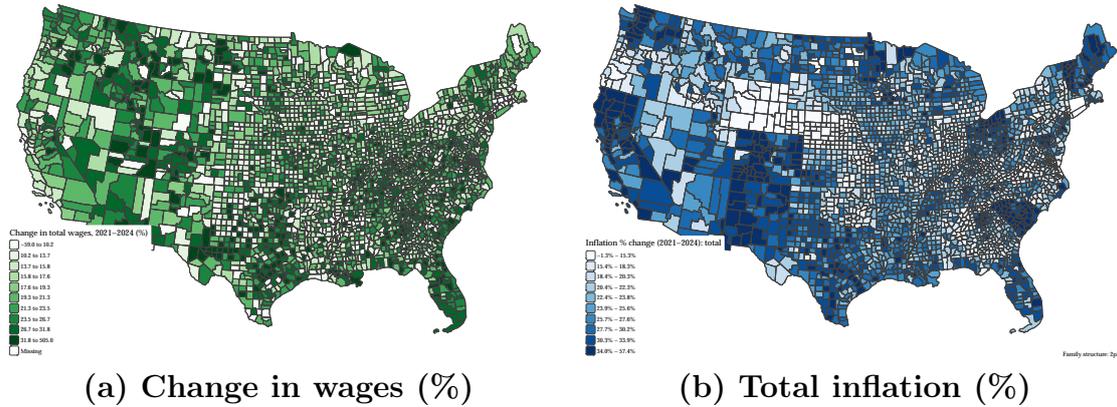
(e) Healthcare



(f) Taxes

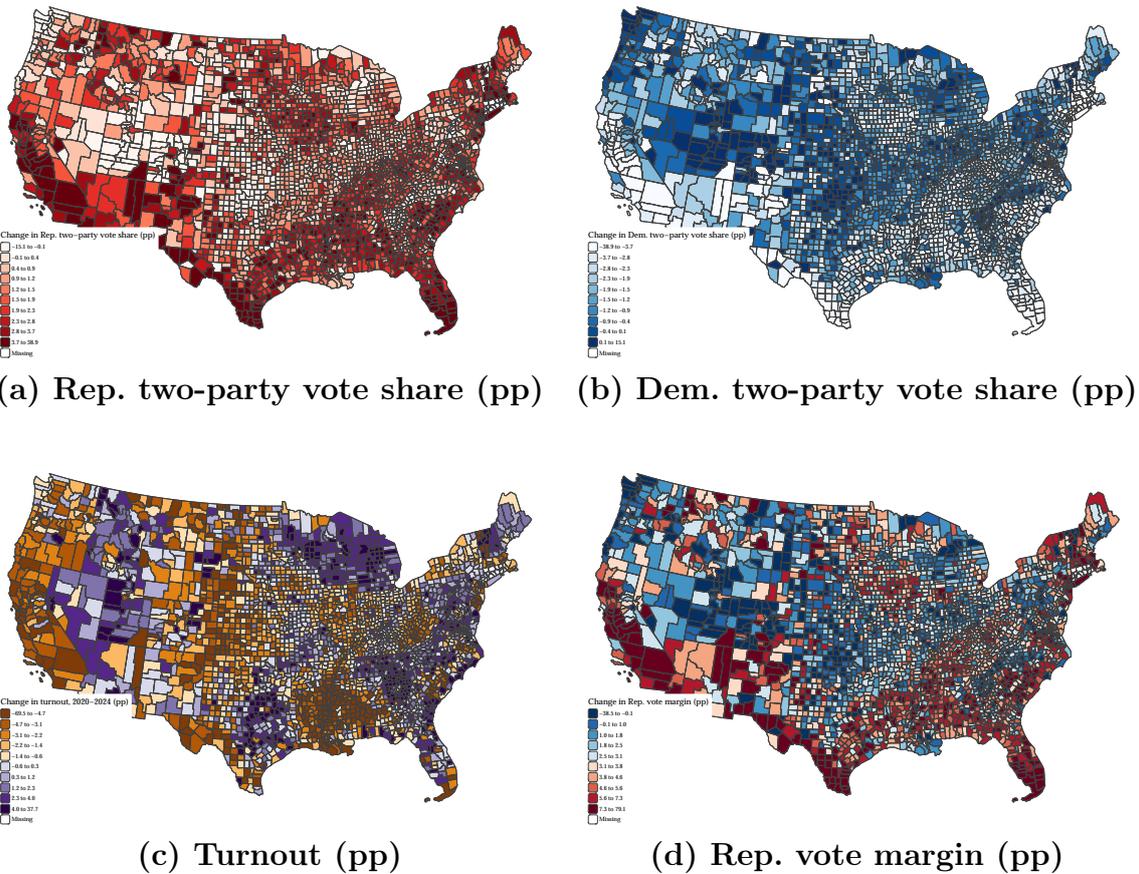
Notes: Each map shows the wage-price gap $G_i^{(k)} = \Delta W_i^{\text{total}} - \Delta P_i^{(k)}$ for the indicated expenditure category, corresponding to Panel D of Table 1. Total wages are from the QCEW. Counties are shaded by decile on a diverging scale. CONUS counties only. Albers Equal-Area projection (EPSG:5070).

Figure C.5: Total wage growth and total inflation, 2021–2024



Notes: Panel (a) shows the percent change in total covered wages (QCEW) between 2021 and 2024, corresponding to Panel C of Table 1. Panel (b) shows the percent change in total family budget costs (EPI FBC, two-parent two-child family) over the same period (same as Figure 2(b)). Counties are shaded by decile. CONUS counties only. Albers Equal-Area projection (EPSG:5070).

Figure C.6: County-level electoral outcomes — changes, 2020–2024



Notes: Each map shows the county-level change in the indicated electoral outcome between the 2020 and 2024 presidential elections, corresponding to Panel A of Table 1. Counties are shaded by decile on a diverging scale. CONUS counties only (Alaska, Hawaii, and territories excluded). Albers Equal-Area projection (EPSG:5070).

D Testing for Non-Linearities in the Inflation–Voting Relationship

A central finding of the main analysis is that local inflation retains a negative association with Republican vote gains even after conditioning on the wage–price gap (the coefficient δ_2 in equation 3). This negative residual price effect, documented in Table 3 and discussed in Section 6, is difficult to reconcile with a strict retrospective pocketbook model in which voters respond only to changes in real wages.

One possible explanation is that the linear specification imposes too restrictive a functional form. If the political response to inflation differs at different levels of price growth (for example, moderate inflation in economically dynamic areas versus extreme inflation that triggers punishment voting), the linear coefficient averages over these heterogeneous effects. Composition effects could also matter: if the negative linear slope is driven by the dominance of low-inflation rural counties where Republicans gained the most, the relationship might flatten or reverse at higher inflation levels.

This appendix tests whether allowing for flexible functional forms alters the interpretation. We employ three complementary approaches: residualized binscatter plots, natural cubic spline regressions with formal F -tests for linearity, and heatmaps of the joint inflation–wage distribution. All specifications condition on state fixed effects and the full set of county-level controls used in the main analysis. The estimation sample comprises 3,102 counties.

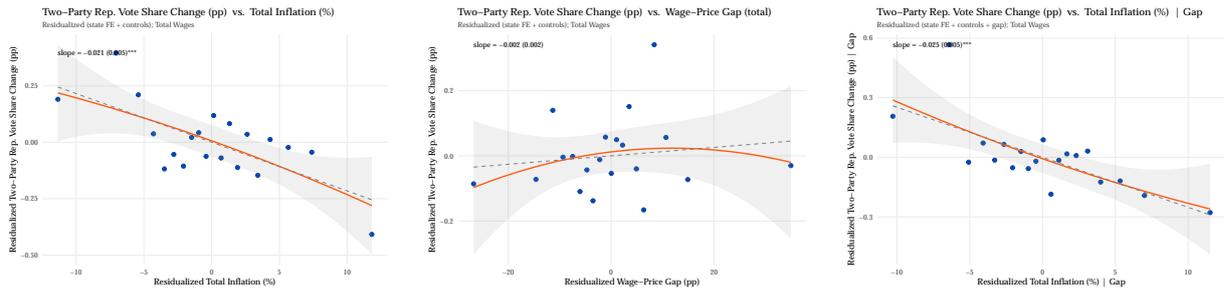
D.1 Residualized binscatter evidence

We begin with residualized binscatter plots, which provide a non-parametric visualization of the conditional expectation of each electoral outcome given inflation or the wage–price gap, after partialing out state fixed effects and controls. Each plot groups the residualized explanatory variable into 20 quantile bins, computes the mean of the residualized outcome within each bin, and overlays both a linear fit (dashed gray) and a quadratic fit with confidence band (orange). The linear slope and its standard error are reported on each panel.

For each outcome, three variants are shown:

1. **Baseline inflation.** Outcome versus inflation, both residualized with respect to state fixed effects and the full controls. This visualizes the relationship underlying β_1 in the baseline specification (1).
2. **Wage–price gap.** Outcome versus $G_i^{(k)}$, using the same controls. This visualizes the real wage growth channel (δ_1).
3. **Inflation conditional on the gap (δ_2).** Outcome versus inflation, residualized with respect to controls *and* the wage–price gap. This is the key diagnostic: it shows whether the residual inflation–voting relationship exhibits non-linear features that could alter the interpretation of the negative δ_2 .

Figure D.7: Vote share change vs. total inflation



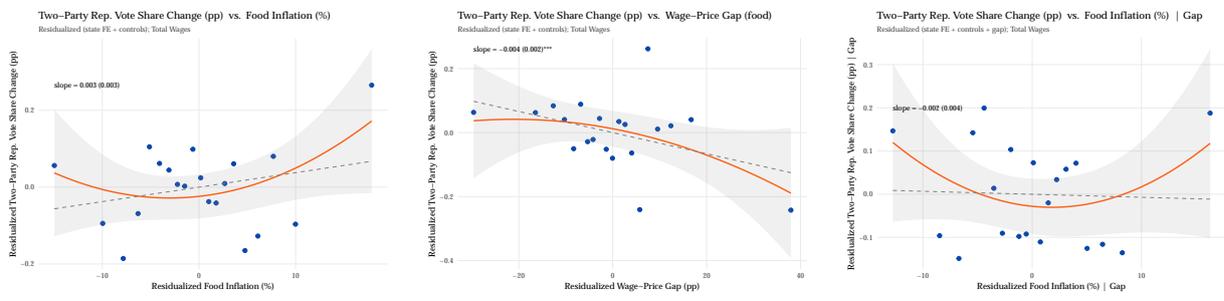
(a) Baseline

(b) Gap

(c) Inflation | Gap

Notes: 20 quantile bins. Gray dashed line: linear fit. Orange curve: quadratic fit with 95% confidence band. All variables residualized against state fixed effects and the full control set. Panel (c) additionally partials out the wage-price gap.

Figure D.8: Vote share change vs. food inflation



(a) Baseline

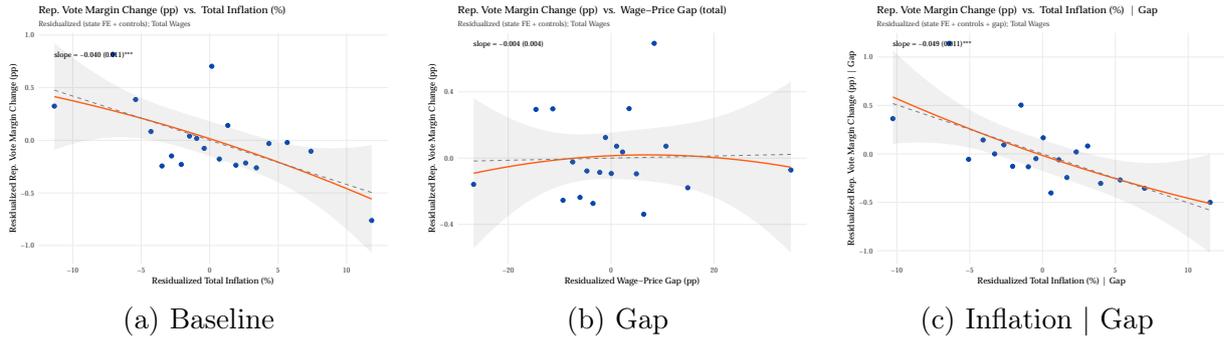
(b) Gap

(c) Inflation | Gap

Notes: Same as Figure D.7 but using food inflation.

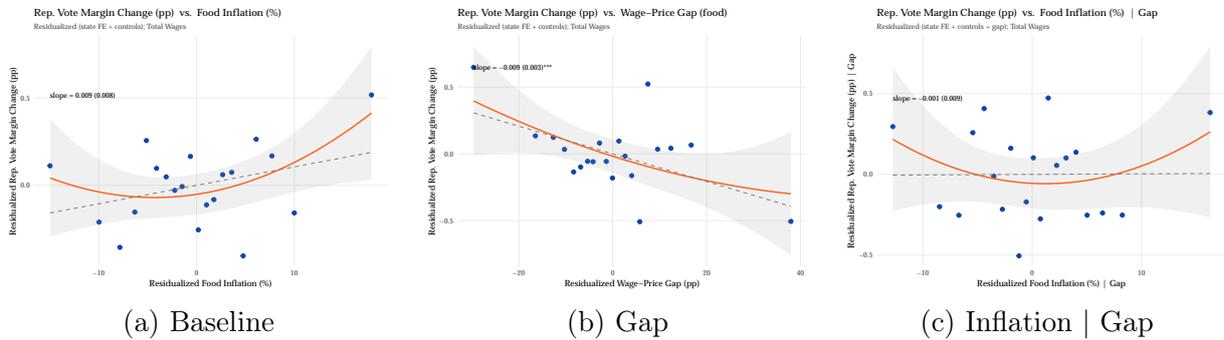
Across all outcomes and specifications, the binscatter plots show that the conditional expectation of electoral outcomes given inflation is largely monotone and approximately linear. The quadratic fits track the linear fits closely, without systematic curvature that would overturn the linear interpretation. This is particularly true for vote share and vote margin, where the bin means align tightly with the linear prediction. For turnout, there is slightly more dispersion in the tails, but no clear non-monotonic pattern. The δ_2 panels, which display inflation after partialing out the wage-price gap, show the same approximately linear, negative relationship as the baseline panels, indicating that the negative association documented in the main tables reflects a consistent downward slope across the inflation distribution rather than an artifact of averaging over a non-linear conditional expectation.

Figure D.9: Vote margin change vs. total inflation



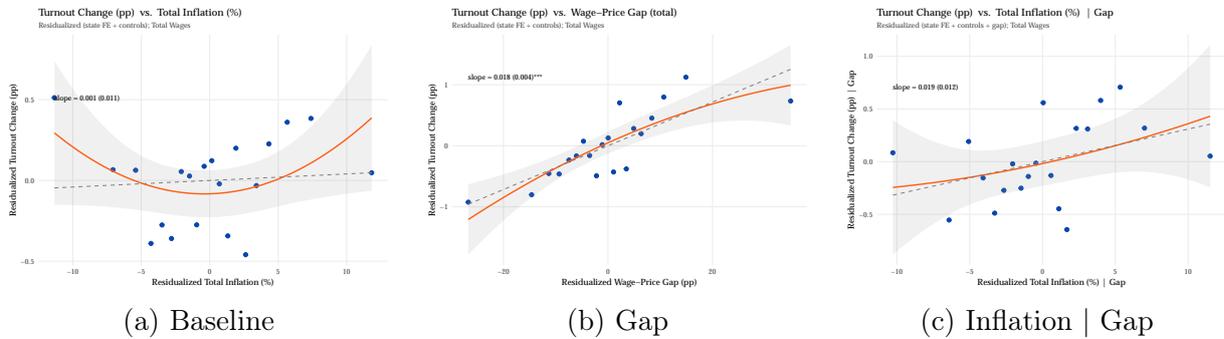
Notes: Same as Figure D.7 but for Republican vote margin change.

Figure D.10: Vote margin change vs. food inflation



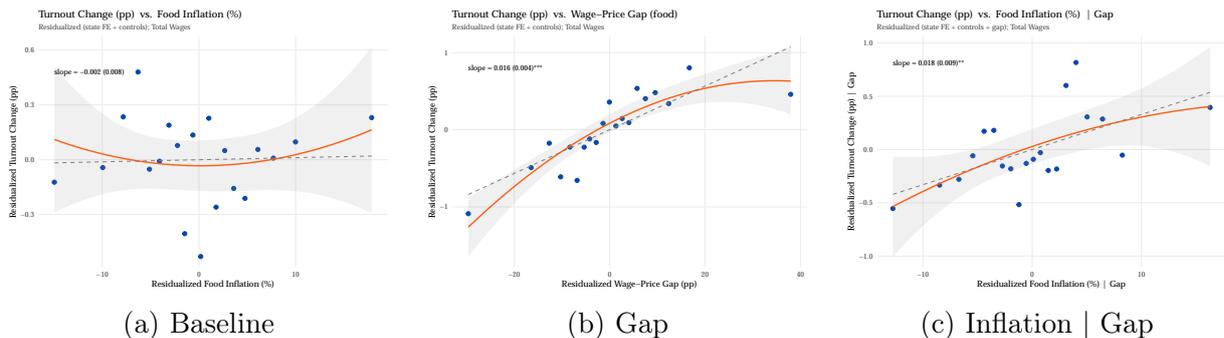
Notes: Same as Figure D.7 but for vote margin change and food inflation.

Figure D.11: Turnout change vs. total inflation



Notes: Same as Figure D.7 but for turnout change.

Figure D.12: Turnout change vs. food inflation



Notes: Same as Figure D.7 but for turnout change and food inflation.

D.2 Spline regressions and formal tests of linearity

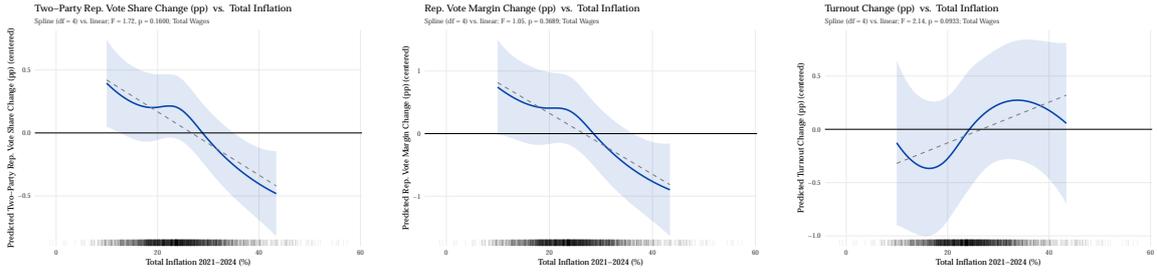
To complement the visual evidence, we estimate a natural cubic spline specification that allows the inflation–voting relationship to take a flexible, smooth non-linear form. Specifically, we replace the linear inflation term in specification (3) with a natural cubic spline basis with four degrees of freedom:

$$\Delta y_i = f_{\text{spline}}(\Delta P_i^{\text{total}}) + \delta_1 G_i^{\text{total}} + \mathbf{X}'_i \gamma + \mu_{s(i)} + \varepsilon_i,$$

where $f_{\text{spline}}(\cdot)$ is estimated from the data. The linear specification is nested within the spline model, so we use an F -test of the null hypothesis that the non-linear basis terms are jointly zero.

Figure D.13 displays the estimated spline curves (with 95% pointwise confidence intervals) alongside the linear fit for each outcome. A rug plot along the horizontal axis shows the density of the inflation distribution.

Figure D.13: Spline marginal effects: electoral outcomes vs. total inflation



(a) Vote Share Change

(b) Vote Margin Change

(c) Turnout Change

Notes: Blue line: natural cubic spline ($df = 4$) with 95% pointwise confidence interval. Gray dashed line: linear fit. Both specifications include the wage–price gap, state fixed effects, and the full control set. Rug plot shows the marginal distribution of total inflation. Predictions are centered at zero for comparability.

Table D.5 reports the F -statistics and p -values.

Table D.5: F -tests of linearity: spline vs. linear specification

Outcome	F -statistic	p -value	R^2 (linear)	R^2 (spline)
Two-Party Rep. Vote Share Change	1.724	0.160	0.4126	0.4136
Rep. Vote Margin Change	1.051	0.369	0.3697	0.3703
Turnout Change	2.139	0.093	0.4021	0.4033

Notes: F -test of the null that a natural cubic spline ($df = 4$) in total inflation reduces to a linear term. All specifications include the wage–price gap, state fixed effects, and the full control set. $N = 3,102$ counties.

The tests provide no evidence against linearity for the partisan vote outcomes: $p = 0.160$ for vote share and $p = 0.369$ for vote margin. For turnout there is marginal evidence of non-linearity ($p \approx 0.09$), consistent with slightly greater dispersion in the

binscatter tails for that outcome. Visually, the spline curves in Figure D.13 are nearly indistinguishable from the linear fit across the bulk of the inflation distribution. Any deviations are modest, concentrated in the sparse tails (as indicated by the rug plot), and fall within the confidence bands. The R^2 improvement from the spline is negligible (at most 0.001).

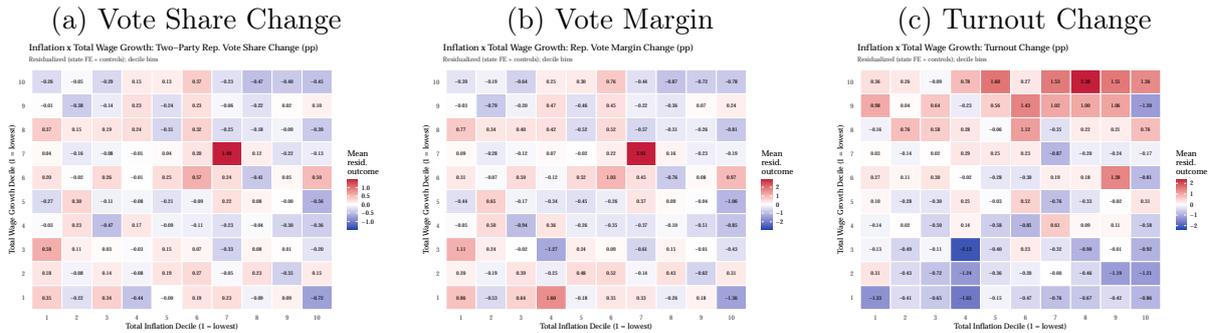
The formal tests therefore do not reject linearity at conventional significance levels for the partisan vote outcomes that are central to the negative δ_2 . For turnout, the marginal evidence does not alter the sign or substantive interpretation of the relationship.

D.3 Joint distribution of inflation and wage growth

The final exercise examines whether the inflation–voting relationship depends on the *joint* distribution of inflation and wage growth. If non-linear interactions between prices and wages drove the negative δ_2 , we would expect to see threshold effects or sign reversals in specific regions of the two-dimensional distribution, for example, Republican gains concentrated in “high inflation, low wage growth” cells.

We construct a 10×10 grid by binning counties into deciles of total inflation and total wage growth. Within each cell, we compute the mean of the residualized outcome (partialing out state fixed effects and controls). Cells are colored on a diverging scale centered at zero: blue indicates a relative shift away from Republicans, red indicates a shift toward Republicans.

Figure D.14: Inflation \times wage growth heatmaps: residualized electoral outcomes



Notes: Each cell shows the mean residualized outcome (partialing out state fixed effects and the full control set) within the intersection of a total inflation decile (horizontal axis) and a total wage growth decile (vertical axis). Blue: relative shift away from Republicans. Red: relative shift toward Republicans. $N = 3,102$ counties.

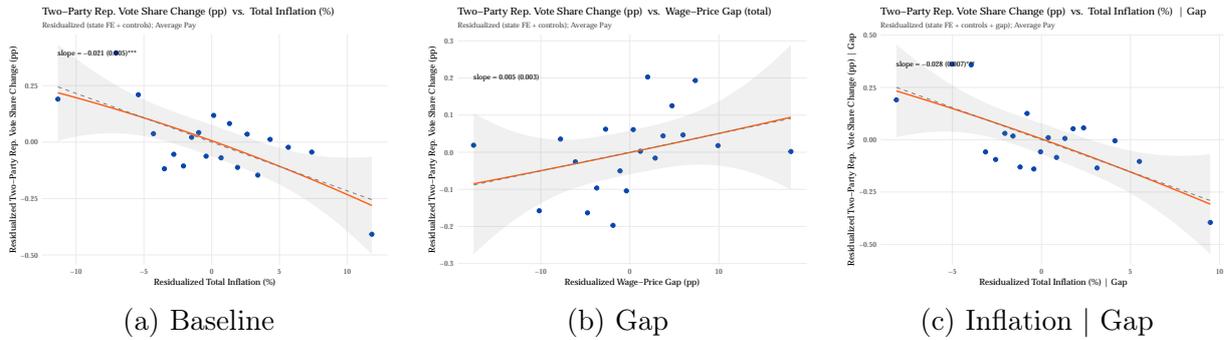
The heatmaps do not reveal sharp thresholds or strong interaction patterns. For vote share and vote margin (panels a and b), the color gradients are generally smooth along both axes, consistent with the additive, approximately linear effects estimated in Tables 3 and 4. There is no sign reversal in the “high inflation, low wage growth” corner that a non-linear punishment-voting story would predict. For turnout (panel c), the

gradient along the wage axis is more pronounced—consistent with the stronger real wage growth channel for participation documented in Table 4—but the pattern remains smooth and does not suggest discrete thresholds or strong inflation–wage interactions.

D.4 Non-linearity checks with average annual pay

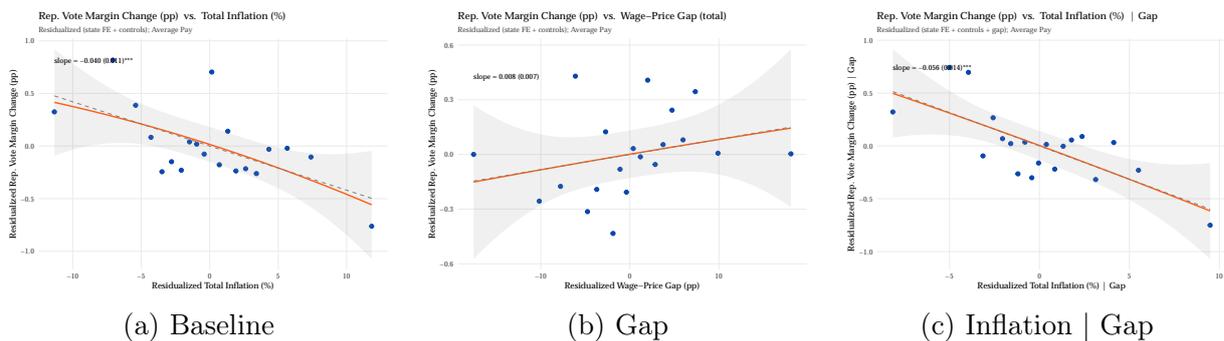
Appendix E documents that the turnout–real-wage-growth association vanishes when wages are measured by average annual pay rather than total covered wages. A natural question is whether this null result is an artifact of the linear functional form. We replicate all three non-linearity exercises—binscatter plots, spline regressions, and heatmaps—using the average-pay wage–price gap in place of the total-wages gap. Because the baseline binscatter (outcome versus inflation, partialing out state fixed effects and controls only) does not involve the wage measure, those panels are mechanically identical to the total-wages results above; the gap and δ_2 panels differ.

Figure D.15: Vote share change vs. total inflation (average pay)



(a) Baseline (b) Gap (c) Inflation | Gap
 Notes: Same as Figure D.7 but using average annual pay (QCEW) for the wage–price gap. Panel (a) is mechanically identical to the total-wages version.

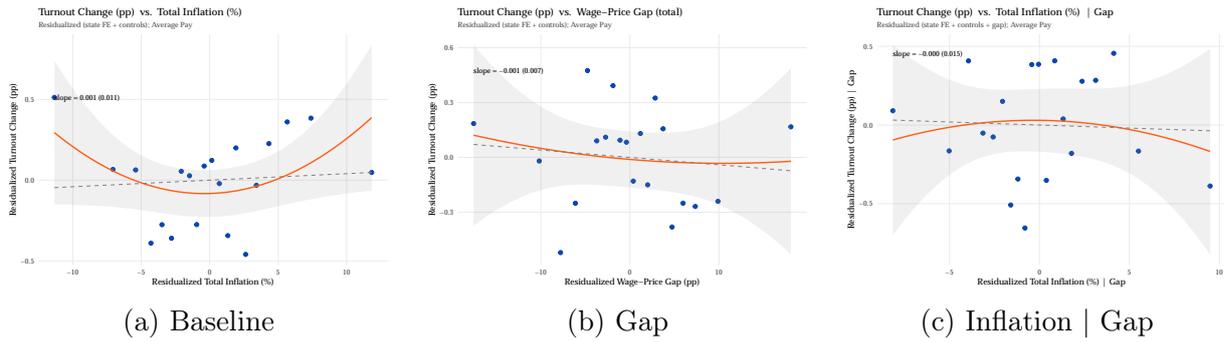
Figure D.16: Vote margin change vs. total inflation (average pay)



(a) Baseline (b) Gap (c) Inflation | Gap
 Notes: Same as Figure D.9 but using average annual pay (QCEW) for the wage–price gap.

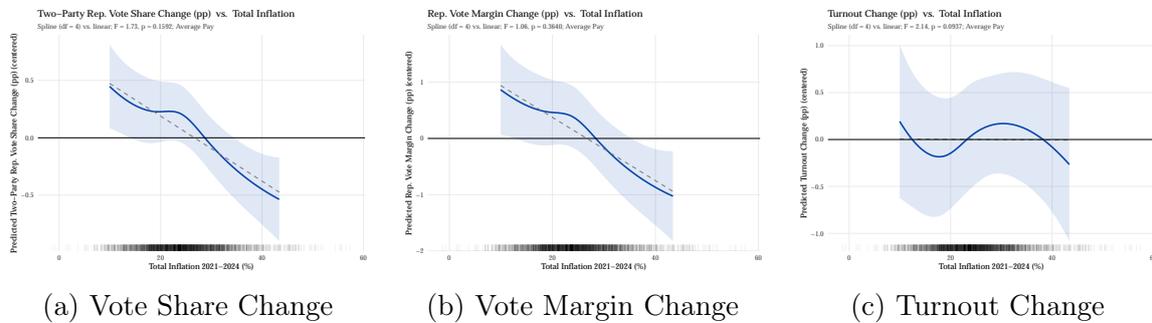
The results are virtually identical to the total-wages case. For partisan outcomes, the F -tests do not reject linearity ($p = 0.159$ and $p = 0.364$), binscatter plots show monotone conditional expectations, and heatmaps display smooth gradients. For turnout, the gap panel of the binscatter (Figure D.17b) confirms that the near-zero linear association documented in Table E.8 is not masking a non-linear relationship: the bin means scatter

Figure D.17: Turnout change vs. total inflation (average pay)



(a) Baseline (b) Gap (c) Inflation | Gap
 Notes: Same as Figure D.11 but using average annual pay (QCEW) for the wage–price gap. The gap panel (b) shows a near-zero slope, confirming the null turnout–real-wage–growth association documented in Table E.8.

Figure D.18: Spline marginal effects: electoral outcomes vs. total inflation (average pay)



(a) Vote Share Change (b) Vote Margin Change (c) Turnout Change
 Notes: Same as Figure D.13 but using average annual pay (QCEW) for the wage–price gap.

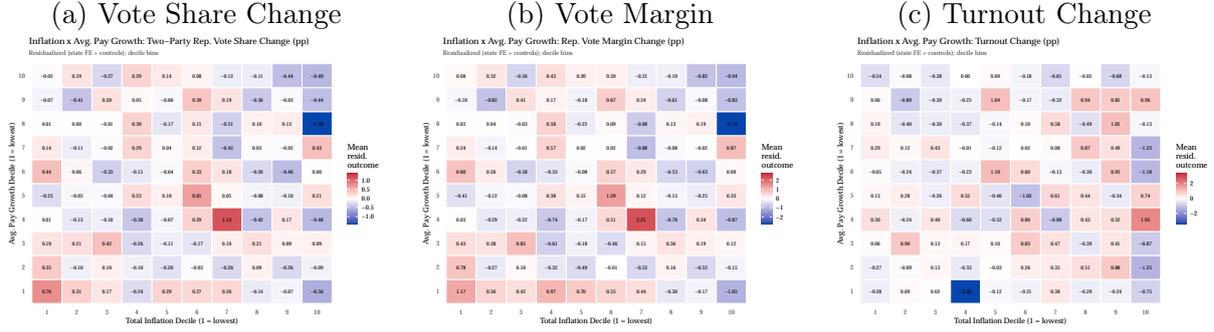
around zero with no discernible slope or curvature. The spline F -test for turnout is marginally significant ($p = 0.094$), as in the total-wages case, but the spline curve remains nearly flat, and the R^2 improvement is negligible (0.001). The heatmap (Figure D.19c) shows no gradient along the average-pay axis for turnout, in contrast to the clear wage gradient visible in the total-wages heatmap (Figure D.14c). This confirms that the null turnout result with average pay is robust to flexible functional forms: it reflects a genuine absence of association between per-worker real wage growth and turnout, not a specification artifact.

Table D.6: F -tests of linearity: spline vs. linear specification (average pay)

Outcome	F -statistic	p -value	R^2 (linear)	R^2 (spline)
Two-Party Rep. Vote Share Change	1.727	0.159	0.4119	0.4129
Rep. Vote Margin Change	1.062	0.364	0.3692	0.3698
Turnout Change	2.136	0.094	0.3974	0.3986

Notes: Same as Table D.5 but using average annual pay (QCEW) for the wage–price gap. $N = 3,102$ counties.

Figure D.19: Inflation \times average pay growth heatmaps: residualized electoral outcomes



Notes: Same as Figure D.14 but using average annual pay growth (QCEW) on the vertical axis instead of total wage growth. $N = 3,102$ counties.

Across all exercises and both wage measures, there is little evidence that non-linearities explain the negative coefficient on inflation documented in the main results. Residualized binscatter plots show monotone, approximately linear conditional expectations across the full distribution, including in the specification that conditions on the wage–price gap. Spline regressions do not reject linearity at conventional significance levels for the partisan vote outcomes ($p = 0.160$ and $p = 0.369$ with total wages; $p = 0.159$ and $p = 0.364$ with average pay). Heatmaps of the joint inflation–wage distribution display smooth gradients without threshold effects or interaction patterns. The negative δ_2 , the residual association between local inflation and Republican vote gains conditional on real wage growth, is therefore not an artifact of the linear functional form. The negative relationship is robust to flexible specifications, ruling out hidden reversals, threshold effects, or non-monotonic patterns that could reconcile the result with a standard incumbent-punishment prediction. The non-linearity tests also confirm that the null turnout–real-wage-growth association under average annual pay (Appendix E) is not a specification artifact.

E Robustness: Average Annual Pay

Tables E.7–E.9 replicate the three main regression tables using average annual pay (QCEW) instead of total covered wages as the wage measure. Average annual pay isolates changes in per-worker compensation, whereas total covered wages also reflect changes in employment levels and industrial composition.

For partisan outcomes (vote share and vote margin), the results are qualitatively robust to this alternative wage measure. Inflation coefficients are nearly identical in magnitude and significance across the two measures; the wage–price gap remains negatively associated with Republican gains in the re-parameterized specification; and prices retain a strong negative residual association conditional on the gap ($\delta_2 \neq 0$). In the gap-only model, real wage growth alone continues to explain little of the variation in partisan vote swings.

For turnout, however, the results diverge. The positive association between the wage–price gap and turnout growth documented in the main tables (Tables 3–4) is absent when wages are measured by average annual pay: the gap coefficient is close to zero and statistically insignificant across most price categories. This suggests that the turnout–real-wage-growth channel operates primarily through aggregate labor market strength, reflecting both higher per-worker compensation and employment growth, rather than through per-worker pay gains alone. Appendix D.4 confirms that this null result is not an artifact of the linear functional form: binscatter plots, spline regressions, and heatmaps using average pay show no hidden non-linear turnout–gap relationship.

Table E.7: Baseline Specification: Inflation, Wages, and Presidential Electoral Outcomes (Average Annual Pay)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>Panel A: Two-Party Republican Vote Share Change, 2020–2024</i>								
Change in Prices	−0.0845 (0.0242)*** [0.0295]***	0.0137 (0.0187) [0.0217]	−0.1542 (0.0535)*** [0.0527]***	−0.0234 (0.0164) [0.0178]	−0.0573 (0.0281)** [0.0379]	−0.0603 (0.0233)*** [0.0293]**	−0.0085 (0.0165) [0.0232]	−0.0526 (0.0258)** [0.0311]*
Change in Wages	−0.0255 (0.0143)* [0.0171]	−0.0261 (0.0145)* [0.0169]	−0.0190 (0.0136) [0.0167]	−0.0257 (0.0145)* [0.0169]	−0.0259 (0.0144)* [0.0172]	−0.0280 (0.0146)* [0.0167]*	−0.0258 (0.0145)* [0.0170]	−0.0260 (0.0144)* [0.0171]
Observations	3,102	3,102	3,102	3,102	3,102	3,102	3,102	3,102
R ²	0.412	0.409	0.420	0.409	0.410	0.410	0.409	0.410
<i>Panel B: Republican Vote Margin Change, 2020–2024</i>								
Change in Prices	−0.0771 (0.0226)*** [0.0279]***	0.0196 (0.0180) [0.0224]	−0.1229 (0.0528)** [0.0546]**	−0.0248 (0.0159) [0.0173]	−0.0457 (0.0267)* [0.0339]	−0.0740 (0.0248)*** [0.0296]**	−0.0052 (0.0155) [0.0225]	−0.0528 (0.0242)** [0.0296]*
Change in Wages	−0.0256 (0.0146)* [0.0132]*	−0.0262 (0.0147)* [0.0130]**	−0.0205 (0.0147) [0.0130]	−0.0257 (0.0147)* [0.0131]*	−0.0260 (0.0147)* [0.0132]**	−0.0286 (0.0149)* [0.0129]**	−0.0260 (0.0147)* [0.0132]**	−0.0261 (0.0147)* [0.0132]**
Observations	3,102	3,102	3,102	3,102	3,102	3,102	3,102	3,102
R ²	0.369	0.367	0.373	0.367	0.367	0.368	0.366	0.367
<i>Panel C: Turnout Change, 2020–2024</i>								
Change in Prices	0.0021 (0.0179) [0.0320]	−0.0047 (0.0155) [0.0175]	−0.0087 (0.0220) [0.0372]	0.0775 (0.0178)*** [0.0212]***	−0.0494 (0.0201)** [0.0268]*	0.0331 (0.0211) [0.0345]	0.0538 (0.0167)*** [0.0199]***	−0.0272 (0.0210) [0.0338]
Change in Wages	−0.0024 (0.0193) [0.0148]	−0.0024 (0.0193) [0.0148]	−0.0020 (0.0195) [0.0153]	−0.0036 (0.0191) [0.0149]	−0.0023 (0.0192) [0.0148]	−0.0013 (0.0192) [0.0147]	−0.0034 (0.0192) [0.0147]	−0.0024 (0.0192) [0.0148]
Observations	3,102	3,102	3,102	3,102	3,102	3,102	3,102	3,102
R ²	0.397	0.397	0.397	0.401	0.398	0.398	0.400	0.398
Price measure	Total	Food	Transport	Housing	Childcare	Healthcare	Other	Taxes
Wage measure	Average Pay	Average Pay	Average Pay	Average Pay	Average Pay	Average Pay	Average Pay	Average Pay
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Observations are at the county level. Each column reports estimates of specification (1) for a different price category k . Wage measure is average annual pay (QCEW) throughout. All variables standardized to zero mean and unit variance; coefficients are in standard-deviation units. Robust standard errors in parentheses; cluster-bootstrapped standard errors (100 replications, clustered at the state level) in brackets. All specifications include state fixed effects and controls for urbanization share, population density, median age, educational attainment (three shares), real GDP, and demographic composition. Significance stars reported next to each standard error type. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

F A comment on Flavin (2026) results

Flavin (2026) examines the relationship between county-level inflation and electoral outcomes in the 2024 U.S. presidential election. Using EPI Family Budget Calculator data, he measures inflation over 2023–2024 across eight expenditure categories (total, housing, food, transportation, healthcare, other necessities, childcare, and taxes) and regresses the change in Republican two-party vote share on each standardized inflation category in turn, controlling for the county unemployment rate in 2023, bachelor’s degree share, median age, evangelical congregation rate, percent white, and log population. He reports a positive and statistically significant association for several categories, notably total, childcare, and tax inflation, and concludes that rising prices contributed to Republican vote gains.

This appendix reassesses the robustness of that finding. Several aspects of Flavin’s empirical specification differ from ours and may account for the divergence in results.

Table E.8: Wage–Price Gap Re-parameterization: Real Wage Growth, Prices, and Presidential Electoral Outcomes (Average Annual Pay)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>Panel A: Two-Party Republican Vote Share Change, 2020–2024</i>								
Real Wage Growth	−0.0366 (0.0205)* [0.0245]	−0.0375 (0.0208)* [0.0243]	−0.0260 (0.0187) [0.0229]	−0.0364 (0.0206)* [0.0240]	−0.0369 (0.0204)* [0.0244]	−0.0407 (0.0211)* [0.0242]*	−0.0369 (0.0207)* [0.0243]	−0.0371 (0.0206)* [0.0244]
Change in Prices	−0.1100 (0.0285)*** [0.0352]***	−0.0124 (0.0230) [0.0259]	−0.1732 (0.0544)*** [0.0572]***	−0.0491 (0.0215)** [0.0251]*	−0.0832 (0.0314)*** [0.0426]*	−0.0883 (0.0283)*** [0.0305]***	−0.0344 (0.0214) [0.0284]	−0.0786 (0.0300)*** [0.0341]**
Observations	3,102	3,102	3,102	3,102	3,102	3,102	3,102	3,102
R^2	0.412	0.409	0.420	0.409	0.410	0.410	0.409	0.410
<i>Panel B: Republican Vote Margin Change, 2020–2024</i>								
Real Wage Growth	−0.0368 (0.0210)* [0.0190]*	−0.0377 (0.0211)* [0.0187]**	−0.0281 (0.0201) [0.0178]	−0.0365 (0.0209)* [0.0187]*	−0.0370 (0.0209)* [0.0189]**	−0.0415 (0.0216)* [0.0187]**	−0.0372 (0.0211)* [0.0188]**	−0.0373 (0.0210)* [0.0189]**
Change in Prices	−0.1027 (0.0266)*** [0.0299]***	−0.0066 (0.0215) [0.0250]	−0.1434 (0.0517)*** [0.0562]**	−0.0505 (0.0226)** [0.0223]**	−0.0717 (0.0293)** [0.0355]**	−0.1026 (0.0315)*** [0.0302]***	−0.0313 (0.0206) [0.0256]	−0.0789 (0.0288)*** [0.0298]***
Observations	3,102	3,102	3,102	3,102	3,102	3,102	3,102	3,102
R^2	0.369	0.367	0.373	0.367	0.367	0.368	0.366	0.367
<i>Panel C: Turnout Change, 2020–2024</i>								
Real Wage Growth	−0.0035 (0.0277) [0.0213]	−0.0034 (0.0277) [0.0213]	−0.0027 (0.0267) [0.0209]	−0.0050 (0.0271) [0.0211]	−0.0033 (0.0273) [0.0211]	−0.0019 (0.0278) [0.0213]	−0.0049 (0.0274) [0.0210]	−0.0034 (0.0274) [0.0212]
Change in Prices	−0.0003 (0.0243) [0.0329]	−0.0070 (0.0233) [0.0238]	−0.0107 (0.0247) [0.0348]	0.0739 (0.0273)*** [0.0249]***	−0.0517 (0.0274)* [0.0283]*	0.0318 (0.0278) [0.0382]	0.0504 (0.0252)** [0.0242]**	−0.0296 (0.0264) [0.0336]
Observations	3,102	3,102	3,102	3,102	3,102	3,102	3,102	3,102
R^2	0.397	0.397	0.397	0.401	0.398	0.398	0.400	0.398
Price measure	Total	Food	Transport	Housing	Childcare	Healthcare	Other	Taxes
Wage measure	Average Pay	Average Pay	Average Pay	Average Pay	Average Pay	Average Pay	Average Pay	Average Pay
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Observations are at the county level. Each column reports estimates of specification (3) for a different price category k . $G_i^{(k)} \equiv \Delta W_i - \Delta P_i^{(k)}$ is category-specific real wage growth. The gap and all other variables are standardized to zero mean and unit variance. This specification is algebraically equivalent to the baseline (Table E.7); R^2 values are identical by construction. Wage measure is average annual pay (QCEW) throughout. Robust standard errors in parentheses; cluster-bootstrapped standard errors (100 replications, clustered at the state level) in brackets. All specifications include state fixed effects and the full control set. Significance stars reported next to each standard error type. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

First, his specification omits wage data and therefore cannot distinguish between the real wage growth channel and nominal price salience, the central decomposition underlying our wage–price gap reparameterization. Second, his baseline model does not include state fixed effects, leaving the inflation coefficient identified in part from cross-state variation that is correlated with long-standing partisan alignments. Third, and most importantly, as we demonstrate below, the reported statistical inference is not robust: once heteroskedasticity-consistent standard errors or clustered error structures are employed, the estimated effects are no longer statistically significant.

We organize the analysis around three main findings. First, we replicate Flavin’s specification exactly and show that the reported significance does not survive standard inference corrections. Second, we demonstrate that the positive sign of the inflation coefficient is an artifact of between-state confounding, which reverses once state fixed

**Table E.9: Restricted Model: Real Wage Growth Only, Prices Omitted
(Average Annual Pay)**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>Panel A: Two-Party Republican Vote Share Change, 2020–2024</i>								
Real Wage Growth	0.0174 (0.0179) [0.0236]	-0.0291 (0.0172)* [0.0213]	0.0532 (0.0333) [0.0356]	-0.0073 (0.0157) [0.0182]	-0.0011 (0.0189) [0.0232]	-0.0044 (0.0173) [0.0253]	-0.0145 (0.0161) [0.0219]	-0.0026 (0.0179) [0.0248]
Observations	3,102	3,102	3,102	3,102	3,102	3,102	3,102	3,102
R^2	0.408	0.409	0.410	0.408	0.408	0.408	0.408	0.408
<i>Panel B: Republican Vote Margin Change, 2020–2024</i>								
Real Wage Growth	0.0136 (0.0180) [0.0211]	-0.0332 (0.0177)* [0.0194]*	0.0375 (0.0338) [0.0339]	-0.0065 (0.0147) [0.0152]	-0.0062 (0.0192) [0.0202]	0.0006 (0.0165) [0.0218]	-0.0168 (0.0158) [0.0195]	-0.0027 (0.0176) [0.0221]
Observations	3,102	3,102	3,102	3,102	3,102	3,102	3,102	3,102
R^2	0.366	0.367	0.367	0.366	0.366	0.366	0.366	0.366
<i>Panel C: Turnout Change, 2020–2024</i>								
Real Wage Growth	-0.0033 (0.0214) [0.0229]	0.0014 (0.0189) [0.0154]	0.0021 (0.0228) [0.0251]	-0.0489 (0.0186)*** [0.0170]***	0.0190 (0.0209) [0.0199]	-0.0149 (0.0223) [0.0214]	-0.0377 (0.0188)** [0.0164]**	0.0096 (0.0222) [0.0234]
Observations	3,102	3,102	3,102	3,102	3,102	3,102	3,102	3,102
R^2	0.397	0.397	0.397	0.399	0.398	0.398	0.399	0.397
Price measure	Total	Food	Transport	Housing	Childcare	Healthcare	Other	Taxes
Wage measure	Average Pay	Average Pay	Average Pay	Average Pay	Average Pay	Average Pay	Average Pay	Average Pay
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Observations are at the county level. Each column reports estimates of the restricted specification (4), which includes the wage–price gap $G_i^{(k)} \equiv \Delta W_i - \Delta P_i^{(k)}$ but omits $\Delta P_i^{(k)}$ as a separate regressor. The gap is standardized to zero mean and unit variance. Wage measure is average annual pay (QCEW) throughout. Robust standard errors in parentheses; cluster-bootstrapped standard errors (100 replications, clustered at the state level) in brackets. All specifications include state fixed effects and the full control set. Significance stars reported next to each standard error type. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

effects are included. Third, we show that omitting wage controls prevents distinguishing nominal price salience from real income effects, and that the inflation coefficient is negative regardless of which demographic control set is employed once wages are included. We conclude with a discussion of additional specification concerns.

Replication and statistical inference

We load Flavin’s replication data and code and reproduce his main results (corresponding to Figure 1 in his paper). His specification EPI regresses the change in Republican two-party vote share on each of eight standardized EPI Family Budget Calculator inflation categories (one at a time), controlling for the county unemployment rate in 2023, bachelor’s degree share, median age, evangelical congregation rate, percent white, and log population. Table F.10, Panel A confirms his reported coefficients and significance levels under conventional OLS standard errors: total inflation (0.068, $p < 0.01$), childcare (0.148, $p < 0.01$), and taxes (0.093, $p < 0.01$) are positively associated with gains in Trump’s vote share, while healthcare inflation (-0.106 , $p < 0.01$) carries a negative sign. The remaining four categories (housing, food, transportation, and other necessities) are not statistically significant even under OLS standard errors. We replicate these results exactly.

The key issue is inference. Panel A also reports heteroskedasticity-robust and

state-clustered standard errors for the same point estimates, that is, no changes to the model, only to the standard errors. EPI inflation draws on underlying data sources (HUD Fair Market Rents, USDA food plans, ACA premiums) that share common metro-area or regional components, inducing spatial correlation among counties within the same state. Clustering at the state level is therefore the appropriate error structure; failing to do so understates sampling variability.

With heteroskedasticity-robust standard errors, the changes are modest: standard errors increase by 5–15% and significance levels are largely preserved. With state-clustered standard errors, however, the picture changes dramatically. Standard errors increase by a factor of two to four; for example, the total inflation SE rises from 0.025 (OLS) to 0.086 (state-clustered), a 3.4-fold increase. Under state-clustered inference, only the childcare category retains statistical significance at the 5% level; the remaining seven categories, including total inflation, become statistically insignificant.

The role of state fixed effects

Flavin’s baseline specification does not include state fixed effects. Without them, the inflation coefficient is identified from both within-state and between-state variation in prices. Between-state variation is problematic: cross-state differences in inflation regimes (persistently high-cost coastal states versus lower-cost interior states) are correlated with long-standing political alignments and secular partisan realignment trends. If higher-inflation states also experienced larger Republican gains for reasons unrelated to prices (differential mobilization, candidate-specific effects, compositional demographic change), then the between-state component of the inflation coefficient is confounded. State fixed effects restrict identification to within-state variation: whether, among counties in the *same state*, those facing larger local price increases shifted differentially toward one party.

Panel B of Table F.10 reports results from Flavin’s specification augmented only with state fixed effects. The consequences are stark. Coefficients reverse sign for most categories: total inflation becomes -0.055 ($p < 0.10$), housing -0.057 ($p < 0.05$), and other necessities -0.071 ($p < 0.05$), all negative, directly contradicting the positive association reported in the original. Under cluster-bootstrapped standard errors (100 replications at the state level), none of the eight categories is statistically significant at conventional levels.

The sign reversal is itself diagnostic. It indicates that the positive OLS estimate was driven primarily by between-state covariation, that is, counties in high-inflation states that also happened to swing Republican, rather than by within-state differences in local price growth. Once this cross-state confound is absorbed, the within-state association between inflation and Republican gains turns negative, consistent with the results we report in the main text.

Omitted wage controls

Flavin’s specification does not include any measure of nominal wage growth. This omission is consequential because local inflation and wage growth are positively correlated: counties with larger price increases tend also to experience larger nominal wage gains, partly through labor market adjustment and partly because both respond to common demand shocks. Without conditioning on wages, the inflation coefficient confounds two conceptually distinct channels: (i) a *nominal price salience* channel, whereby voters punish incumbents for rising prices regardless of purchasing power, and (ii) a *real income* channel, whereby voters respond to changes in living standards. Our baseline specification (1) separates these channels by including wage growth alongside inflation, and the wage–price gap reparameterization (3) provides a direct test of whether prices retain explanatory power *beyond* their effect on real wages.

To demonstrate that it is the inclusion of wages and state fixed effects, rather than differences in the demographic control set, that accounts for the sign divergence, we conduct a cross-specification exercise. We re-estimate our baseline on our data using Flavin’s six control variables (unemployment rate, bachelor’s degree share, median age, evangelical congregation rate, percent white, and log population) in place of our own controls, while retaining wages and state fixed effects.

Table F.11 reports the results. The inflation coefficient remains negative across all eight price categories: for total inflation the estimate is -0.066 ($p < 0.01$ with robust SEs); for other necessities -0.047 ($p < 0.05$); and for childcare -0.032 ($p < 0.05$). These estimates closely mirror our main results reported in Tables 2 and E.7. The choice of demographic controls does not drive the sign reversal; wages and state fixed effects do.

Additional specification concerns

Two of Flavin’s controls raise endogeneity concerns. The county unemployment rate in 2023 is a “bad control” in the sense of Angrist & Pischke (2009): if inflation affects local labor market conditions (through real wage adjustments, changes in labor force participation, or firm entry and exit) then conditioning on unemployment absorbs part of the causal pathway from prices to electoral behavior, attenuating the estimated total effect. The evangelical congregation rate is a strong predictor of Republican vote-share *levels* but is nearly time-invariant; in a regression on vote-share *changes*, it functions more like a control for baseline partisanship than a demographic confounder, risking over-control that absorbs outcome variation attributable to economic conditions.²¹

Beyond the control set, two measurement choices are worth noting. Flavin measures inflation over 2023–2024 only, a single year of price changes, whereas we use cumulative 2021–2024 inflation spanning the full presidential term. A one-year window captures

²¹In a panel first-differencing framework, a time-invariant regressor would be eliminated entirely. Its appearance in the cross-sectional regression is an artifact of the single-cross-section design.

transitory price shocks rather than the persistent cost-of-living trajectory that voters plausibly attribute to incumbent economic management (Fiorina, 1981). Additionally, Flavin’s election returns are sourced from a FOX News GitHub repository rather than from official state-certified counts; county-level figures from election-night tallies can diverge from certified results due to provisional ballots and late tabulation corrections, introducing measurement error in the dependent variable.

In sum, the positive association between county-level inflation and Republican vote gains reported in Flavin (2026) does not survive standard inference corrections or the inclusion of state fixed effects and wage controls. The reported statistical significance relies on conventional OLS standard errors that ignore the spatial clustering inherent in MSA-level inflation measurement; once state-clustered standard errors are employed, most estimates become insignificant. The positive sign is driven by between-state covariation, and adding state fixed effects reverses it. Finally, omitting wages prevents distinguishing price salience from real income effects; when wages are included, the inflation coefficient is negative regardless of which control set is used.

Table F.10: Replication of Flavin (2026): Inference Sensitivity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Total	Housing	Food	Transport.	Healthcare	Other Nec.	Childcare	Taxes
<i>Panel A: Flavin (2026) specification (no state fixed effects)</i>								
Inflation (std)	0.0682***	-0.0196	0.0338	0.0462*	-0.1060***	-0.0348	0.1482***	0.0925***
OLS	(0.0254)	(0.0267)	(0.0253)	(0.0279)	(0.0255)	(0.0273)	(0.0255)	(0.0254)
Robust	(0.0266)	(0.0271)	(0.0240)	(0.0263)	(0.0287)	(0.0286)	(0.0269)	(0.0276)
State-clustered	[0.0860]	[0.0573]	[0.0250]	[0.1041]	[0.0967]	[0.0612]	[0.0719]	[0.0873]
Observations	3,103	3,103	3,103	3,103	3,103	3,103	3,103	3,103
R^2	0.292	0.290	0.290	0.291	0.294	0.290	0.298	0.293
<i>Panel B: Adding state fixed effects</i>								
Inflation (std)	-0.0549*	-0.0574**	0.0394*	1.6133*	-0.0473	-0.0705**	0.0119	-0.0543*
Robust	(0.0301)	(0.0282)	(0.0214)	(0.8299)	(0.0298)	(0.0292)	(0.0343)	(0.0324)
Cluster-bootstrap	[0.0375]	[0.0261]	[0.0224]	[0.8549]	[0.0470]	[0.0326]	[0.0380]	[0.0393]
Observations	3,103	3,103	3,103	3,103	3,103	3,103	3,103	3,103
R^2	0.477	0.477	0.477	0.477	0.476	0.477	0.476	0.477
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Price measure	Total	Housing	Food	Transport.	Healthcare	Other Nec.	Childcare	Taxes

Notes: Observations are at the county level. Each column uses a different EPI Family Budget Calculator inflation measure (2023–2024, standardized). Panel A replicates Flavin’s (2026) specification: OLS with controls for unemployment rate, bachelor’s degree share, median age, evangelical congregation rate, percent white, and log population. The first SE row reports conventional OLS standard errors (as in the original); the second row reports heteroskedasticity-robust standard errors; the third row reports standard errors clustered at the state level. Panel B adds state fixed effects; robust standard errors in parentheses, cluster-bootstrapped standard errors (100 replications, clustered at the state level) in brackets. Stars on the coefficient correspond to the first SE row in each panel. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

**Table F.11: Cross-Specification Robustness: Our Outcome with Flavin (2026)
Controls**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Total	Housing	Food	Transport.	Healthcare	Other Nec.	Childcare	Taxes
Change in Prices	-0.0656*** (0.0237) [0.0353]	-0.0163 (0.0155) [0.0236]	0.0106 (0.0225) [0.0324]	-0.0304* (0.0168) [0.0189]	-0.0422 (0.0299) [0.0346]	-0.0470** (0.0236) [0.0304]	-0.0319** (0.0160) [0.0249]	-0.0318 (0.0257) [0.0394]
Change in Wages	-0.0274* (0.0165) [0.0152]	-0.0269 (0.0165) [0.0150]	-0.0273* (0.0165) [0.0147]	-0.0266 (0.0164) [0.0147]	-0.0275* (0.0165) [0.0150]	-0.0289* (0.0165) [0.0147]	-0.0262 (0.0165) [0.0150]	-0.0274* (0.0165) [0.0151]
Observations	3,098	3,098	3,098	3,098	3,098	3,098	3,098	3,098
R^2	0.404	0.403	0.402	0.403	0.403	0.403	0.403	0.403
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Flavin (2026)	Flavin (2026)	Flavin (2026)	Flavin (2026)	Flavin (2026)	Flavin (2026)	Flavin (2026)	Flavin (2026)
Wages	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Price measure	Total	Housing	Food	Transport.	Healthcare	Other Nec.	Childcare	Taxes

Notes: Observations are at the county level. Dependent variable is the change in two-party Republican presidential vote share, 2020–2024 (our measure). Controls are Flavin’s (2026) set: unemployment rate (2023), bachelor’s degree share, median age, evangelical congregation rate, percent white, and log population. Wage measure is change in average annual pay (QCEW), 2021–2024. Inflation is EPI Family Budget Calculator, 2021–2024. All variables standardized to zero mean and unit variance. Robust standard errors in parentheses; cluster-bootstrapped standard errors (100 replications, clustered at the state level) in brackets. All specifications include state fixed effects. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.