Putting Your Money Where Your Mouth Is: Political Polarization, Consumption, and Investment

Nathan Fernig* Richard Sias† Austin Sobotka‡

September 13, 2026§

Abstract

We show that political alignment, the match between a county's partisan composition and the party of the sitting president, shapes realized household financial behavior. Using county-level data on discretionary consumption from the BEA and on equity investment constructed from IRS dividend income data, we find that counties increase both spending and equity investment when they become aligned with the party of the president. The effects are economically large relative to standard determinants of local economic activity and have intensified as Americans have become increasingly polarized. Our estimates are robust to instrumenting alignment with a Bartik-style instrument built from pre-sample vote shares, to border discontinuity comparisons of contiguous counties, and hold at the commuting zone and metropolitan statistical area levels. Our sample covers every U.S. county over more than two decades and six presidential terms, providing direct evidence that partisan economic expectations transmit into household financial decisions at magnitudes detectable in aggregate data.

JEL classifications: D72, D84, E21, E71, G41, G51, R11

Key words: Partisanship, Political Polarization, Economic Expectations, Motivated Beliefs, Household Consumption, Equity Investment, Household Finance

*Eller College of Management, University of Arizona. Email: nfernig@arizona.edu.

†Eller College of Management, University of Arizona. Email: sias@arizona.edu.

‡Eller College of Management, University of Arizona. Email: 205asobotka@arizona.edu.

§We thank participants from the Research in Behavioral Finance Conference, the Boulder Summer Conference, and the Chicago Quantitative Alliance Spring Conference for their thoughtful comments and feedback.

A previous version of this paper was circulated under the title "Political Polarization and Partisan Economic Cycles." First draft: December 6, 2025. Copyright © 2025 by the authors.

1 Introduction

A growing body of work provides compelling evidence that political affiliation and polarization strongly shape individuals' economic expectations: Democrats report more optimistic outlooks than Republicans when a Democrat holds the presidency, and vice versa, a pattern that has grown sharply over the past quarter century as political polarization has intensified. Despite this well-documented connection between partisanship and economic outlooks, direct evidence that these expectations translate into material economic choices remains limited: Studies that focus wholly on self-reported expectations or intentions cannot distinguish between partisan signaling and actual expectations, while studies that examine observed behavior find mixed results and tend to cover short time periods or narrow population subsets, making it unclear whether their findings reflect broad, generalizable economic responses.

In this paper, we investigate whether political affiliation and polarization shape realized household financial behavior in levels detectable in aggregate data by examining county-level discretionary consumption and investment decisions. Our sample includes all counties in the United States, spanning more than two decades and six presidential terms, making our study, as far as we are aware, the most comprehensive to date. Our results indicate that political affiliation strongly influences both discretionary consumption and equity investment: Republican counties increase consumption and investment significantly more than Democratic counties when a Republican holds the presidency, and vice versa. The effect size of political alignment is economically large relative to standard determinants of local economic activity and has increased over time as Americans have become increasingly polarized.

We measure a county's political alignment as the match between its partisan composition (taken from presidential vote shares) and the party of the sitting president, and we track two realized outcomes: discretionary consumption, measured from BEA county GDP in consumer-facing industries, and equity investment, measured from changes in IRS dividend income. In our baseline specification, we estimate that a one standard deviation increase in the share of partisans aligned with the party of the president is associated with an additional 0.47 percentage point increase in year-over-year discretionary consumption growth and an additional 0.47 percentage point increase in year-over-year growth in equity holdings, our measure of investment.[1] These effects are economically large: for consumption, the effect is comparable to that of a one standard deviation increase

[1]We measure investment as year-over-year growth in county dividend income net of the yearly cross-sectional mean, an approximate flow measure of county level equity purchases; we detail the construction of this measure in Section 3.1.

in population growth against an unconditional mean consumption growth of 1.84 percent, and for investment it is more than twice the effect of population growth. Consistent with broad evidence that Americans have become increasingly polarized (e.g., Boxell, Gentzkow, and Shapiro (2017), Iyengar, Lelkes, Levendusky, Malhotra, and Westwood (2019)) and with evidence that polarization amplifies differences in partisan perceptions (e.g., Angrisani, Sias, Sobotka, and Turtle (2025)), we find strong evidence that the consumption response has intensified over time with the level of national affective polarization, with a one standard deviation increase in affective polarization adding a further 0.52 percentage points to the consumption response of aligned counties, and some evidence that the investment response has increased as well. This time variation also helps reconcile the mixed evidence in prior work: the effect was genuinely smaller in earlier, less polarized periods, so studies drawing on those samples were less likely to detect it.

Three features of the response indicate that it reflects the aggregated behavior of individual partisans. First, the effect operates through the intensive margin: what matters is not whether a county's electorate tips into alignment with the president but how many of its residents are aligned. Once we allow the response to vary with the share of aligned voters, merely crossing the majority threshold has no effect on consumption (-0.11, $t = -1.21$), while the marginal effect of partisan intensity is large (0.86, $t = 8.41$), exactly as one would expect if county-level outcomes aggregate the decisions of individual partisans. Second, the response is not driven by one side of the partisan divide: both Republican and Democratic counties increase consumption and investment when they become aligned, and we cannot reject that the two respond equally to becoming aligned (difference of 0.17, $t = 0.76$ for consumption), so the effect reflects partisanship itself rather than the behavior of one party. Third, the response is not a transitory burst of post-election enthusiasm. The consumption effect is fully present in the first half of the presidential term (0.51, $t = 9.46$) and does not decay in the second half (the second-half increment is 0.06, $t = 0.86$), even though the president's party lost congressional seats at the midterm in every term of our sample but one, curtailing its capacity to legislate (Howell, Adler, Cameron, and Riemann (2000)). The pattern is consistent with partisans holding on to their optimism even as the president's capacity to deliver declines, and it implies that partisan behavior operates throughout the presidency rather than only around elections.

These estimates admit a causal interpretation. The variation that drives them comes from national election outcomes: no county can meaningfully affect who wins the presidency, and daily survey data show that partisan economic expectations do not adjust in anticipation of elections but update sharply once the outcome is realized, so a change in a county's alignment arrives as a shock

from outside its local economy.[2] The remaining concerns are that alignment is built in part from county vote shares, which could themselves respond to local economic conditions, and that partisan composition is correlated with regional shocks or industry mix. Section 5 addresses each possibility directly: instrumenting alignment with a Bartik-style instrument built from pre-sample vote shares and estimated by two-stage least squares (Bartik (1991)), comparing only neighboring counties that share a local economy, and controlling for each county's exposure to national industry performance, the alignment effect remains large and highly significant in every design, and randomly reassigned county alignment histories never produce coefficients approaching the magnitudes we estimate. Our results are also robust to alternative geographies, to an alternative consumption measure built from administrative employment records, and to controls for home prices and household leverage, as well as to excluding the financial crisis and pandemic years. Each design targets a different confound, and each returns the same answer: counties increase their consumption and investment when they become aligned with the sitting president.

Our findings contribute to two strands of research. The first asks whether partisan shifts in economic expectations translate into actual household spending. Although there is now overwhelming evidence that political affiliation and polarization impact economic perceptions, evidence that these expectations effect actual behavior remains mixed. The initial affirmative finding, that county taxable sales rise after elections in counties favoring the winning party (Gerber and Huber (2009)), was later shown to rest on a single state-year observation (McGrath (2017)), and spending measured from credit card transactions and automobile purchases shows no partisan response in windows around the 2008 and 2016 elections (Mian, Sufi, and Khoshkhou (2023)). The positive evidence that remains comes from self-reported consumption in the weeks following the 2016 and 2020 elections (Kamdar and Ray (2022)) and from automobile purchases around Australian elections (Gillitzer and Prasad (2018)). Though these studies provide important contributions to the question of whether politically-motivated expectations transmit into realized behavior, they cover relatively short time periods and focus on either self-reported behavior, subsets of the population, or both, and so cannot speak to whether differences in politically-motivated expectations transmit into broad, generalizable differences in behavior.[3] Relative to this work, we provide realized-spending evidence that covers every county in the country, spans six presidential terms over more than two decades, and follows behavior across entire terms rather than windows around elections, providing direct evidence that political affiliation and polarization have large, generalizable impacts on

[2]We document these expectation dynamics using daily polling data in Section IA-4 of the Internet Appendix.

[3]In concurrent work, Yi (2025) finds in household purchase data that politically aligned households spend more than misaligned households, consistent with our county-level evidence.

household spending.

The second strand of research asks whether political identity shapes household investment. Political preferences predict who participates in the stock market at all: international evidence shows that left-leaning individuals are less likely to hold stocks even conditional on income and wealth (Kaustia and Torstila (2011)), and in the United States the partisan gap in participation widens and narrows with the party of the president, with lasting consequences for wealth accumulation (Ke (2024)). Among those who do participate, alignment predicts beliefs and allocations: investors who share the president's party are more optimistic about markets and hold more equity (Bonaparte, Kumar, and Page (2017)), Republican-leaning investors shifted retirement portfolios toward equity after the 2016 election (Meeuwis, Parker, Schoar, and Simester (2022)), and partisan gaps in investor beliefs are visible in real time among retail traders (Cookson, Engelberg, and Mullins (2020)) and widen with affective polarization (Angrisani, Sias, Sobotka, and Turtle (2025)).[4] This evidence comes from surveys, single elections, brokerage and retirement subsamples, or markets outside the United States; whether political alignment influences general differences in realized equity investment thus remains an open question. Our contribution is to show that these partisan patterns aggregate into realized investment flows. We construct a county-level measure of equity purchases from administrative tax records on dividend income and find that this flow rises with political alignment, in Republican and Democratic counties alike, and increasingly so as polarization has deepened.

Beyond these two literatures, our results carry implications for the growing body of research on the consequences of political polarization for finance (see Kempf and Tsoutsoura (2024) for a review). Our evidence shows that its reach extends to the two central margins of household financial behavior, the consumption-saving decision and the portfolio decision, and that the belief differences behind these responses are systematic rather than idiosyncratic: they are shared by large groups of households, they switch sign with the party of the president, and they intensify as polarization deepens. These findings speak to a central question in household finance: whether the beliefs households report translate into the financial decisions they make (Giglio, Maggiori, Stroebel, and Utkus (2021)). We find that they do, at the scale of the entire country, and that political identity has joined income, wealth, and demographics as a determinant of household financial behavior, one whose influence grows as the electorate polarizes.

The remainder of the paper proceeds as follows. Section 2 provides background on partisanship,

[4]The pattern extends to professionals: Democratic-leaning fund managers tilt their portfolios away from socially irresponsible stocks (Hong and Kostovetsky (2012)), and fund managers overweight firms whose executives share their political affiliation (Wintoki and Xi (2020)).

polarization, and economic expectations. Section 3 describes our data. Section 4 presents our main results on political alignment, consumption, and investment. Section 5 presents our identification and robustness tests, and Section 6 concludes.

2 Background

While partisanship provides a useful bifurcation, it potentially masks meaningful heterogeneity in the intensity of individuals' views. Measures of polarization offer a more fine-grained perspective by capturing not only party affiliation but also the strength of that affiliation. Importantly, variation in polarization can generate heterogeneous responses both in the cross section—between more and less polarized individuals—and over time, as the overall degree of polarization within groups rises or falls. Our empirical design incorporates both dimensions: a county's partisan vote share serves as a proxy for the cross-sectional component, since counties with a stronger partisan lean should contain more highly polarized partisans, while national affective polarization, measured from survey data, captures the time-series component.

In the political science literature, polarization is defined broadly as either ideological (i.e., the degree to which individuals disagree on issues) or affective (i.e., the degree to which individuals view "their" party favorably and another party negatively). Although there is debate as to whether political ideological polarization has increased over time between Republicans and Democrats (e.g., Fiorina, Abrams, and Pope (2008)), there is a broad consensus that political affective polarization has increased over time (e.g., Boxell, Gentzkow, and Shapiro (2017)). This rise in polarization has been shown to impact how individuals behave across a large range of topics including social, legal, and healthcare related interactions.[5] Moreover, polarization has been shown to lead to disagreement between Republicans and Democrats over objective historical facts such as whether unemployment fell under the Reagan administration (Bartels (2002)).

Political affiliation and polarization have also been shown to have material impacts on individuals' expectations of the current and future states of the economy and stock market.[6] For example, Conover, Feldman, and Knight (1987) provide evidence that "...economic forecasts reflect more a combination of hopes and politicized guesses about the future than they do an awareness of cur-

[5] See, e.g., Iyengar, Sood, and Lelkes (2012); Hersh and Goldenberg (2016); Nicholson, Coe, Emory, and Song (2016); Huber and Malhotra (2017); Iyengar, Konitzer, and Tedin (2018); Chen and Rohla (2018); Cohen and Yang (2019); Milosh, Painter, Sonin, Van Dijcke, and Wright (2021)

[6] A partial list of studies that investigate economic expectations and partisanship include Conover, Feldman, and Knight (1987), Wlezien, Franklin, and Twiggs (1997), Bartels (2002), Evans and Andersen (2006), Gerber and Huber (2010), Gillitzer and Prasad (2018), Coibion, Gorodnichenko, and Weber (2020), Kamdar and Ray (2022), Mian, Sufi, and Khoshkhou (2023), and Angrisani, Sias, Sobotka, and Turtle (2025).

rent economic conditions and an understanding of how the economy functions." Consistent with this view, Bartels (2002) finds that Republicans held significantly more optimistic views about the economy under Bush relative to Democrats, Gerber and Huber (2010) find that Democrats (Republicans) became significantly more optimistic (pessimistic) about the economy following the 2006 midterm elections when the Democratic party regained control of both the house and the senate, and Huberman, Konitzer, Krupenkin, Rothschild, and Hill (2018) find that partisans become significantly more optimistic (pessimistic) about the economy following elections where the party of the president becomes aligned (unaligned) with their political views. Relatedly, Angrisani, Sias, Sobotka, and Turtle (2025) provide evidence that political affective polarization significantly amplifies differences in partisan expectations about the stock market and economy over a long sample period. Whether these expectations translate into realized behavior is the question of this paper.

3 Data

3.1 Consumption and Investment

We exploit a relatively new dataset (first released in 2019) to proxy for local consumption: The Bureau of Economic Analysis' (BEA) county-industry-level GDP estimate. Specifically, we proxy for local consumption by focusing on the sum of GDP from the retail trade industry (NAICS codes 44-45) and accommodations and food service industry (NAICS code 72). This follows work (e.g., Chodorow-Reich, Nenov, and Simsek (2021); Mian and Sufi (2014)) that uses Quarterly Census of Employment and Wages (QCEW) employment in these industries as a proxy for local consumption. As noted by Guren, McKay, Nakamura, and Steinsson (2021), using employment in the retail sector as a proxy for local consumption is a "...standard choice in the measurement literature" employed both by economists and the private sector (e.g., Moody's and the Survey of Buying Power). For instance, Chodorow-Reich, Nenov, and Simsek (2021) find that payroll growth in these industries (i.e., NAICS codes 44-45 and 72) closely tracks BEA consumption growth expenditures at the state level (e.g., see the authors' Figure 6). Similarly, Guren, McKay, Nakamura, and Steinsson (2021) demonstrate that aggregate changes in national retail employment are strongly correlated with personal consumption expenditures from the BEA's National Income and Product Accounts (NIPA) which is, "...typically viewed as the gold-standard measure of aggregate consumption."

We focus on the BEA (rather than QCEW) data because it provides a more complete estimate of local consumption (nonetheless, in robustness tests, we find nearly identical results when repeating our tests using the QCEW employment data). The BEA county-industry GDP estimate

consists of three components: wages to employees, income to proprietors, and income to other suppliers of capital. Employee compensation is constructed from employer wage reports, primarily the QCEW data which Bureau of Labor Statistics (BLS) compiles from employer unemployment insurance filings by county and six-digit NAICS industry on a quarterly basis. Supplements to wages are derived from BEA estimates of employer pension and insurance contributions and from state unemployment insurance contribution records. Proprietors' income is measured using IRS tax data on the net profits of sole proprietorships and partnerships. Income from other suppliers of capital is estimated by allocating state totals to counties based on Economic Census data. We also collect county level personal income and population data from the BEA's County and MSA Personal Income Summary.[7] The Internet Appendix provides greater detail regarding the BEA data construction process.

We use county-level data from the IRS on dividend income as a proxy for equity holdings.[8] Dividend income reported on tax returns is the standard basis for locating equity ownership: studies of the wealth distribution infer households' equity wealth primarily from tax-reported dividends (Saez and Zucman (2016); Smith, Zidar, and Zwick (2023)), and Chodorow-Reich, Nenov, and Simsek (2021) measure county-level stock market wealth by capitalizing dividend income from the same IRS county data we use. To convert holdings into a flow, we compute county-level year-over-year growth in dividend income and subtract the yearly cross-sectional mean.[9] If Republican and Democratic counties hold the market on average, the common component of dividend growth—the market's payout growth—is removed by the demeaning, and the remaining variation reflects changes in the quantity of equity a county holds: positive values indicate investment while negative values indicate disinvestment. The assumption that county portfolios are similar on average is testable: as Table I reports, average demeaned dividend growth is essentially zero for Republican and Democratic counties separately, exactly what similar average portfolios predict, whereas a

[7]Personal income is constructed from administrative records including IRS tax data for wages and proprietors' income and federal program records for transfers. Personal income is allocated by place of residence via a residence adjustment for intercounty commuting and is distinct from county level GDP, which is measured by place of production and includes corporate profits, depreciation and no transfers. As such, personal income captures local household purchasing power. The BEA's county level population data is based on U.S. Census data.

[8]Additional details regarding the IRS data are available in the Internet Appendix. Dividends earned within tax-deferred retirement accounts are not reported as dividend income on individual tax returns, so our measure reflects equity held in taxable accounts. To the extent that partisan investment responses occur inside retirement accounts (e.g., Meeuwis, Parker, Schoar, and Simester (2022) document partisan rebalancing in retirement portfolios), our measure misses them, biasing against finding an effect.

[9]We use growth rather than capitalized levels, and do not project county dividend income onto aggregate equity holdings from the Federal Reserve's Financial Accounts, because any persistent county-level tilt toward high- or low-yield stocks enters the level of dividend income multiplicatively and would bias capitalized levels, but cancels in year-over-year growth. Projected levels would also impound aggregate valuation changes, which reflect prices rather than purchases.

persistent difference in portfolio style would push the two groups' means apart.[10,11]

An alternative interpretation of our investment measure is mechanical: politically aligned households might hold portfolios that perform better under aligned administrations, raising their dividend income without any change in investment behavior. Two well-established facts limit this channel. First, variation in stock prices overwhelmingly reflects movements in discount rates rather than news about future cash flows (Campbell and Shiller (1988); Cochrane (2008)), so even if aligned counties' holdings enjoyed superior price performance, that performance would carry little information about the growth of the dividends those holdings pay. Second, and relatedly, corporate payouts are heavily smoothed: managers adjust dividends slowly toward long-run targets and avoid cuts (Lintner (1956); Brav, Graham, Harvey, and Michaely (2005)), further insulating dividend income growth from short-run differences in portfolio performance. While we cannot strictly rule out the possibility that portfolio composition may contribute to the observed patterns, the evidence is more consistent with changes in household investment behavior than with mechanical changes in dividend payouts.

Our main variables of interest are year-over-year county-level consumption growth and investment growth:

$$\text{Consumption Growth}_{c,t} = \frac{\text{Consumption}_{c,t}}{\text{Consumption}_{c,t-1}} - 1 \tag{1}$$

$$\text{Investment Growth}_{c,t} = \text{Dividend Growth}_{c,t} - \overline{\text{Dividend Growth}}_t \tag{2}$$

where $\text{Consumption}_{c,t}$ is the sum of BEA GDP (in constant 2023 dollars) in the retail trade and accommodation and food services industries for county c in year t, $\text{Dividend Growth}_{c,t} = \text{Dividend Income}_{c,t} \,/\, \text{Dividend Income}_{c,t-1} - 1$ is the year-over-year growth in county c's IRS dividend income, and $\overline{\text{Dividend Growth}}_t$ is the cross-sectional mean of $\text{Dividend Growth}_{c,t}$ across counties in year t. We do not deflate dividend income because inflation is common to all counties within a year and is therefore removed when we subtract the yearly cross-sectional mean. We define analogous values for income and population growth and winsorize all growth variables at the 5% and 95% levels of the estimation sample.[12]

[10] We compute the yearly cross-sectional mean on our estimation sample, so average investment growth is zero by construction. That pins down the average across all counties but places no restriction on how far the Republican and Democratic means sit from each other, which is what makes their near-coincidence informative.

[11] All regressions also include county and year fixed effects. Year fixed effects absorb the common component of dividend growth, reinforcing the cross-sectional demeaning, and county fixed effects absorb any persistent county-specific tilt in dividend growth. A stable difference in portfolio composition therefore cannot drive our estimates; only differences that vary within county over time with alignment could.

[12] We winsorize Dividend Growth before subtracting the cross-sectional mean. That mean stands in for the market's payout growth, and it is subtracted from every county, so a single extreme county-year shifts every other county's investment growth in that year. Raw county dividend growth reaches 5,628% in one county-year, and leaving such

Following previous work that uses county level data (e.g., Mian, Sufi, and Khoshkhou (2023); McGrath (2017); Gerber and Huber (2009)), we compute local partisanship as each county's two-party vote share in the most recent presidential election:[13]

$$\text{Democratic Vote Share}_{c,t} = \frac{\text{Democratic Votes}_{c,t}}{\text{Democratic Votes}_{c,t} + \text{Republican Votes}_{c,t}} \tag{3}$$

which yields a county-level measure of political affiliation for each election cycle (i.e., the measure is constant across each presidential term).[14,15] We treat Democratic Share as a proxy for cross sectional polarization—assuming that individual level polarization is approximately normally distributed across partisans, the proportion of Democratic (Republican) voters in a county should capture the relative degree of political polarization between counties, since counties with a large partisan lean are more likely to contain larger numbers of highly polarized partisans.

Table I reports summary statistics for each of our main variables. Our sample includes 66,069 county-year observations running from 2001 to 2023, and includes 3,058 counties covering all 50 states plus the District of Columbia. This is our estimation sample, which requires non-missing consumption growth, dividend growth, and controls. We use it throughout the paper so that the consumption and investment columns of each table are estimated on the same set of county-years and are directly comparable.[16]

[Insert Table I about here.]

The average county in our sample has a Democratic vote share of 38%, 103k residents, per capita income of $50k, annual discretionary consumption of $663M, and annual dividend income

values in moves the yearly mean by as much as 10.9 percentage points.

[13]The data are from MIT Election Data and Science Lab. Because our local consumption data are annual, vote shares are based on the most recent presidential election prior to that year. For instance, a county's 2008 (2009) vote share is based on the November 2004 (2008) election.

[14]We discard independent and write-in ballots.

[15]Since vote counts are taken pre-inauguration, we shift our political affiliation measure to the inauguration year. For example, the affiliation measure for Obama's first term uses vote data from the end of 2008 but we do not apply the measure until 2009 (i.e., we use the affiliation measure from Bush's second term for all of 2008). To the extent that individuals alter their consumption very soon after the election outcomes (e.g., Kamdar and Ray (2022)), this lag would attenuate our results.

[16]Over our sample, the U.S. had a total of 3,143 counties and county equivalents (U.S. Census Bureau). The modest shortfall in coverage (85 counties) has three sources. Twenty-five never appear in the BEA county series, reflecting boundary changes, consolidations, and changes in county-equivalent definitions over our sample. Fifty-five appear in the BEA data but have no reported presidential vote share, for example 31 of them are Alaskan boroughs and census areas, since Alaska reports presidential results by state house district rather than by borough. The remainder have both but never a usable year, reflecting disclosure protection in the underlying administrative data, which works differently in each source: the BEA suppresses county-industry GDP wherever publishing it would disclose information about an individual business, while the IRS excludes any income item reported on fewer than 20 returns in a county, and these suppressed cells appear as zeros rather than as missing values, which we then code as missing.

of $92M (all monetary values are deflated to 2023 dollars). Average year-over-year consumption growth is 1.84% with a standard deviation of 6.93%. Average investment growth is zero with a standard deviation of 14.21%. The final two columns report means for Republican (Democratic Share < 0.50) and Democratic (Democratic Share > 0.50) counties separately. On average, Republican counties are more polarized in terms of vote share, are significantly smaller in terms of population, have slightly lower per capita income, have significantly lower total consumption, and have significantly lower total dividend income. Republican counties have greater population, income, and consumption growth than Democratic counties. As discussed above, average investment growth is essentially zero for the full sample, and also does not differ between Republican and Democrat counties, consistent with both types of counties holding similar portfolios on average and supporting the interpretation of demeaned dividend growth as an investment flow.[17]

3.2 Affective polarization

The American National Election Survey (ANES) asks, among many other questions, political "feeling thermometer" questions that have been used extensively in the political science literature to assess affective polarization between parties. These thermometer questions ask respondents to rank their feelings towards an issue, individual, or group on a scale of 0-100, where low values indicate unfavorable feelings, values near 50 indicate indifference, and high values indicate warmer or more favorable feelings. For example,

> Therm Dem: *On a scale of 0 (very cold/unfavorable) to 100 (very warm/favorable), how do you feel about the Democratic party?*
>
> Therm Rep: *On a scale of 0 (very cold/unfavorable) to 100 (very warm/favorable), how do you feel about the Republican party?*

Although the exact wording of these questions has varied slightly across survey waves, they consistently measure affective feelings toward each party.[18]

ANES also collects data on respondents' party identification. Specifically, participants report the party with which they most closely affiliate using either a three-point scale (Democrat, Inde-

[17] Regressing investment growth on an indicator for Republican counties over our estimation sample gives a difference of -0.08 percentage points ($t = -0.85$). Standard errors are clustered at the county level.

[18] For example, pre-1978, the relevant questions asked how respondent's felt towards "Democrats" and "Republicans" (rather than "the Democratic party" and "the Republican party"). While both versions of these questions are asked in the 1980 and 1982 surveys, Iyengar, Sood, and Lelkes (2012) find that these two measures are highly correlated, suggesting that "...people perhaps extend their dislike of the parties to dislike of people within the parties." As such, we simply use the contemporary set of questions (party specific) from 1980 onward.

pendent, Republican) or a seven-point scale (e.g., Strong Democrat, Weak Democrat, etc.). ANES also reports a composite party affiliation measure constructed by collapsing seven-point scale responses to the three-point scale when available and otherwise using the respondent's three-point scale response. We use this composite measure to determine an individual's political affiliation.

We first compute our measure of affective polarization at the individual level by taking the absolute difference between the individual's "in-party" and "out-party" warmth,

$$\text{Affective Polarization}_{i,t} = \begin{cases} \mid \text{Therm Dem}_{i,t} - \text{Therm Rep}_{i,t} \mid & \text{if } i \text{ is a Democrat} \\ \mid \text{Therm Rep}_{i,t} - \text{Therm Dem}_{i,t} \mid & \text{if } i \text{ is a Republican} \end{cases} \tag{4}$$

which yields a measure in $[0, 100]$, with 0 indicating no polarization and 100 indicating the maximum possible polarization.[19] We then estimate the level of national affective polarization by averaging this measure at the survey level,

$$\text{Affective Polarization}_t = \frac{1}{N} \sum_{i=1}^{N} \text{Affective Polarization}_{i,t} \tag{5}$$

Prior to 2000, ANES polls were conducted every two years. From 2000 to present, they have been conducted every four years. For the purpose of subsequent analysis, we linearly interpolate between polls to construct a continuous yearly measure of Affective Polarization. Figure 1 plots Affective Polarization from 1964 to 2024. Consistent with the political science literature (e.g., Iyengar, Sood, and Lelkes (2012); Iyengar, Lelkes, Levendusky, Malhotra, and Westwood (2019)), we document a systematic increase in Affective Polarization from the early 1980s to present and find that this increase has been driven entirely by out-party hate—that is, the division has been driven by individuals disliking the opposition party more, not by them liking their own party more.

[Insert Figure 1 about here.]

4 Empirical Results

4.1 Partisanship and Local Economic Activity

We begin by plotting differences in average growth in discretionary consumption and in equity holdings between Republican and Democratic counties over each of the six presidential terms in

[19] As is standard in the political science literature, we exclude Independents as it is not clear what the appropriate in- and out-parties are.

our sample.

[Insert Figure 2 about here.]

Figure 2 reveals that election outcomes are associated with large differences in economic activity between Republican and Democratic counties. Panel A shows that counties differentially adjust their consumption following elections—when a Republican president is elected, Republican counties increase their consumption more than Democratic counties, and vice versa. For example, when Bush is reelected in 2005, the difference shifts upwards, indicating that Republican counties increased consumption more than Democratic counties. When Obama is first elected in 2009, the difference shifts downwards, indicating that Democratic counties increased consumption more than Republican counties. Aside from Obama's first term, aligned counties also have greater overall consumption growth than unaligned counties. That is, the difference is positive over both of Bush's terms and Trump's term, indicating that Republican counties had greater overall consumption growth than Democratic counties, while the difference is negative in Obama's second term and Biden's term, indicating that Democratic counties had greater overall consumption growth than Republican counties. Notably, the magnitude of these shifts has grown substantially over time—the shift from Obama to Trump is much larger than the shift from Bush to Obama, and the shift from Trump to Biden is larger still. This increase suggests that the rise in affective polarization documented in Figure 1 has potentially amplified partisan responses to the political environment.

Panel B plots the analogous differences for investment growth. Similar to Panel A, investment growth tends to adjust positively with political alignment: aside from Obama's reelection and Trump's election, investment growth shifts upwards when a Republican president is elected and downwards when a Democrat is elected. In four of the six presidencies, overall investment growth is aligned with the party of the president: differences are positive under both of Bush's terms and negative under Obama's first term and Biden's. The two exceptions, Obama's second term and Trump's, are both within 0.1 percentage points of zero.

We next test whether mean economic activity differs, in a statistical sense, between Republican and Democratic counties under Republican and Democratic presidencies. We classify county c as Democratic (Dem County$_{c,t} = 1$) when Democratic Share$_{c,t} > 0.50$ and as Republican (Rep County$_{c,t} = 1$) when Democratic Share$_{c,t} < 0.50$. Table II reports unconditional mean consumption and investment growth for the four county-president combinations, along with differences in these means

across presidencies and across county types.[20,21]

[Insert Table II about here.]

Table II confirms that mean economic activity differs significantly between Republican and Democratic counties across presidencies. Column (1) shows that, under Republican presidents, Republican counties have mean consumption growth of 1.05% while Democratic counties have mean consumption growth of 0.20%, with the difference being highly statistically significant (0.84, $t = 9.19$). In contrast, column (2) finds no significant difference in mean consumption growth between Republican and Democratic counties under Democratic presidents. Column (3) shows that both Republican and Democratic counties have significantly higher consumption growth under Democratic presidents but that Democrats respond more strongly—that is, Democratic counties increase their consumption under Democratic presidents more than Republican counties (and decrease their consumption under Republican presidents more than Republican counties do). Columns (4)–(6) present the same analysis for investment growth. Column (4) shows that Republican counties invest significantly more than Democratic counties under Republican presidents while column (5) shows that Democratic counties invest significantly more than Republican counties under Democratic presidents. Not only do aligned counties invest more than unaligned counties, unaligned counties actually disinvest on average: Under Republican presidents, Republican counties increase their equity holdings by 0.15 percentage points on average while Democratic counties decrease their holdings by 0.69 percentage points. Under Democratic presidents, Republican counties decrease their equity holdings 0.20 percentage points while Democratic counties increase theirs by 0.67 percentage points. Column (6) shows that Democratic counties once again respond more strongly than Republican counties.

The results in Table II show that the level of consumption and investment growth is larger for currently aligned counties than currently unaligned counties and suggests that counties respond to changes in political alignment. To formally test whether county partisan composition and the party of the sitting president affects local economic activity, we first define a county-level measure of political alignment

[20]The four combinations are Republican and Democratic counties under Republican and Democratic presidents. For each county type, columns (3) and (6) report the difference in mean growth between Republican and Democratic presidencies, and the bottom row reports the difference in mean growth between Republican and Democratic counties under each type of president.

[21]t-statistics for the means and their differences are based on standard errors clustered at the county level.

$$\begin{aligned}\text{Alignment}_{c,t} = &\text{ Democratic Share}_{c,t} \times \text{Dem Pres}_t \\ &+ (1 - \text{Democratic Share}_{c,t}) \times (1 - \text{Dem Pres}_t)\end{aligned} \tag{6}$$

where Dem Pres_t is an indicator equal to one if the current president is a Democrat and Democratic $\text{Share}_{c,t}$ is the share of votes cast for the Democratic candidate in county c in the most recent presidential election. By construction, $\text{Alignment}_{c,t}$ is high for Democratic (Republican) counties during Democratic (Republican) presidencies and low during Republican presidencies, where $\text{Alignment}_{c,t} > 0.5$ indicates that the county's vote share favored the party of the current president and vice versa.

We test whether counties adjust their economic activity in response to changes in Alignment by estimating regressions of the form

$$\text{Dep Var}_{c,t} = \delta \text{Alignment}_{c,t} + \Gamma X_{c,t} + \alpha_i + \tau_t + \epsilon_{c,t} \tag{7}$$

where $\text{Dep Var}_{c,t}$ is once again either the year-over-year growth in discretionary consumption or in equity holdings in county c in year t, α_i are county fixed effects, τ_t are year fixed effects, and $X_{c,t}$ contains county controls including lagged county income, county income growth, and county population growth. For ease of interpretation, we normalize all regressors to have mean zero and unit standard deviation. The key independent variable, $\text{Alignment}_{c,t}$, measures the degree to which county c's partisan lean matches the party of the sitting president in year t.

Because county fixed effects absorb all time-invariant differences between Republican and Democratic counties and year fixed effects absorb aggregate macroeconomic conditions common to all counties, the coefficient $\hat{\delta}$ is identified from within-county variation in $\text{Alignment}_{c,t}$. That is, $\hat{\delta}$ measures the degree to which a county alters its economic activity *in response* to a change in alignment. Two features of the political environment support the exogeneity of election outcomes as identifying shocks. First, no individual county can realistically affect the outcome of a national presidential election, so the realized alignment status of any given county is determined by forces largely outside its control. Second, Angrisani, Sias, Sobotka, and Turtle (2025) document that partisan agents do not update their economic expectations prior to election outcomes, regardless of polling odds or the objective probability that their preferred party wins, but do so sharply and immediately after the result is announced. If expectations—and the economic behavior they generate—are anchored to realized outcomes rather than anticipated ones, the alignment shock is cleanly dated at the

election.[22,23]

The results in Table III suggest that counties strongly adjust their consumption and investment in response to changes in alignment. For example, column (2) indicates that a one-standard deviation increase in the share of aligned voters in a county is associated with an additional 0.47 percentage point ($t = 12.79$) increase in year-over-year discretionary consumption growth. The effect size of alignment on consumption is economically large in relation to our unconditional year-over-year discretionary consumption growth of 1.84% and is comparable to the effect size of a one-standard deviation increase in population growth or a one-half-standard deviation increase in income growth. Results are similar for investment growth. For example, column (5) indicates that a one-standard deviation increase in the share of aligned voters in a county is associated with an additional 0.47 percentage point ($t = 8.01$) increase in year-over-year investment growth. Once again, this effect size is large in relation to standard county-level controls—alignment has an effect size more than twice that of population growth and approximately one-fourth that of income growth.

[Insert Table III about here.]

Angrisani, Sias, Sobotka, and Turtle (2025) find that political affective polarization has greater explanatory power over perceptions of the stock market and the economy than party affiliation alone, indicating that the intensity of partisan attachment, not just partisan identity, shapes economic beliefs. Importantly, polarization can influence perceptions both cross sectionally—between relatively more and less polarized individuals—and in the time series—between more or less polarized periods. Although our alignment variable proxies for cross sectional polarization, it does not capture relative changes in the intensity of such polarization over time. As such, we test whether the partisan economic responses to alignment we document depend on the level of national affective polarization. Accordingly, columns (3) and (6) add an interaction term between Affective Polarization (Equation 5) and our political alignment variable.

The coefficient on $\text{Alignment}_{c,t} \times \text{Aff Polar}_t$ in column (3) indicates that a one-standard deviation increase in Affective Polarization is associated with an additional 0.52 percentage point ($t = 13.42$) increase in discretionary consumption growth in aligned counties—that is, the relationship between partisan alignment and local consumption is significantly amplified during periods of high national affective polarization. We do not find such an effect for investment growth: the corresponding

[22] We replicate the relevant results in Angrisani, Sias, Sobotka, and Turtle (2025) in the Internet Appendix.

[23] To the extent that partisan expectations and thus behavior shift prior to election outcomes, our results should be biased downwards.

coefficient in column (6) is near zero and insignificant (0.10, $t = 1.49$). However, we show in the Internet Appendix that this null result is not robust to sample composition: when we drop extremely large and small counties we estimate a positive and significant coefficient on $\text{Alignment}_{c,t} \times \text{Aff Polar}_t$ for investment growth, suggesting that the full-sample result is driven by very large urban and or very small rural counties—which may not reflect representative household investment behavior—and that the rise of affective polarization has amplified the investment response for most counties.

Our results are robust to a battery of checks for alternative determinants of local economic activity and for sensitivity to sample composition. We show in the Internet Appendix that our results are robust to excluding extremely large and small counties and to excluding observations in the Great Financial Crisis (2008, 2009) and the COVID-19 pandemic (2020, 2021). We also show that our results are robust to replacing year fixed effects with state by year fixed effects, which absorb any shock common to a state in a given year, such as state fiscal or regulatory policy (Table IA-VIII). Moreover our results are still significant when we cluster standard errors at the county-year level. Because house price movements have substantial explanatory power over local consumption (e.g., Mian, Rao, and Sufi (2013); Guren, McKay, Nakamura, and Steinsson (2021)), we also show in Table IA-IX that our results are unchanged when we control for county-level home price growth. Finally, because household leverage is an important determinant of local consumption dynamics, with highly levered areas cutting spending most sharply in downturns (Mian, Rao, and Sufi (2013)), Table IA-X shows that our consumption results are unaffected by controlling for lagged household leverage and for stock market wealth effects, measured as lagged county dividend income as a share of income multiplied by the market return (Chodorow-Reich, Nenov, and Simsek (2021)).[24,25]

Our main specification assumes that partisan responses are increasing in the intensity of partisan alignment. It is possible, however, that the effect is approximately binary—driven entirely by counties becoming aligned—and that the intensity of alignment does not matter. As such, we estimate separately the effect of simply becoming aligned and the marginal effect of the degree of

[24] We do not include these controls in the investment regressions because both are constructed from the same dividend income data underlying our investment measure; for investment, the yearly cross-sectional demeaning removes the common market return by construction.

[25] We also show in the Internet Appendix that our consumption-alignment results are consistent when we replace the BEA consumption data with year-over-year growth in county employment in consumer-facing industries (retail trade, NAICS 44–45, and accommodation and food services, NAICS 72), an administrative measure from the Quarterly Census of Employment and Wages (Table IA-IV).

such alignment. To do so, we define a binary variable

$$\text{Aligned}_{c,t} = \begin{cases} 1, & \text{if Alignment}_{c,t} > 0.5 \\ 0, & \text{if Alignment}_{c,t} < 0.5 \end{cases} \tag{8}$$

and estimate

$$\text{Dep Var}_{c,t} = \beta \text{Aligned}_{c,t} + \delta(\text{Aligned}_{c,t} \times \text{Alignment}_{c,t}) + \Gamma X_{c,t} + \alpha_i + \tau_t + \epsilon_{c,t} \tag{9}$$

where $\text{Aligned}_{c,t}$ captures the binary effect of becoming aligned (the extensive margin) and $\text{Aligned}_{c,t} \times \text{Alignment}_{c,t}$ captures the marginal effect of the degree of alignment (the intensive margin).

[Insert Table IV about here.]

Table IV reveals that the effect is driven by the intensive margin. Columns (1) and (3) show that counties significantly increase their consumption and investment when they become aligned with the party of the sitting president. However, the coefficient on Aligned becomes much smaller and insignificant in columns (2) and (4), with large positive coefficients on the interaction $\text{Aligned}_{c,t} \times \text{Alignment}_{c,t}$, indicating that the response to alignment is driven by the intensity of partisan composition, not simply by becoming aligned. This pattern is consistent with the alignment effect reflecting the aggregation of individual partisan responses: if individual partisans adjust their consumption and investment when their preferred party wins or loses the presidency, then counties with larger shares of aligned partisans should exhibit larger aggregate responses.

4.2 Symmetric Responses

A natural question is whether the alignment effect is driven primarily by one side of the partisan divide—for example, whether Republican counties are highly responsive to alignment while Democratic counties are relatively unresponsive, or vice versa.[26] To investigate this, we test whether Democratic and Republican counties respond differently on both the extensive margin (becoming

[26] Chu, D'Acunto, Gao, Wang, and Wang (2026), for example, provide evidence that Democrats and Republicans behave heterogeneously in the real estate market.

aligned) and the intensive margin (the degree of alignment) by estimating

$$\begin{aligned}\text{Dep Var}_{c,t} = \delta_{Dem}(\text{Dem County}_{c,t} \times \text{Align Var}_{c,t}) + \delta_{Rep}(\text{Rep County}_{c,t} \times \text{Align Var}_{c,t}) \\ + \Gamma X_{c,t} + \alpha_i + \tau_t + \epsilon_{c,t} \qquad (10)\end{aligned}$$

$$\text{Align Var}_{c,t} \in \{\text{Aligned}_{c,t},\ \text{Alignment}_{c,t}\}$$

where $\text{Align Var}_{c,t}$ equals the binary $\text{Aligned}_{c,t}$ in Panel A of Table V (the extensive margin) and the continuous $\text{Alignment}_{c,t}$ in Panel B (the intensive margin).

In contrast to the coefficient on the binary $\text{Aligned}_{c,t}$ variable or the continuous $\text{Alignment}_{c,t}$ variable in Equations 9 and 7, which capture the overall response to changes in alignment, the coefficients δ_{Dem} and δ_{Rep} capture separately the average consumption and investment responses of Democratic and Republican counties. The null hypothesis $H_0 : \delta_{Dem} = \delta_{Rep}$ tests whether the average within-county response to alignment is symmetric across the two partisan groups, and the analogous null $\theta_{Dem} = \theta_{Rep}$ tests whether its amplification by affective polarization is symmetric.

[Insert Table V about here.]

Table V reports the results. Panel A columns (1) and (3) show that the extensive margin of becoming aligned is symmetric between Republican and Democratic counties—the difference in the baseline alignment coefficients ($\delta_{Dem} - \delta_{Rep}$) is statistically indistinguishable from zero for both consumption and investment, indicating that Republican and Democratic counties respond equally to becoming aligned with the sitting president. In column (2), conditioning on affective polarization reveals that this average symmetry masks a time-varying asymmetry: the baseline difference becomes negative and significant ($\delta_{Dem} - \delta_{Rep} = -0.51$, $t = -2.20$), indicating that at mean polarization levels Republican counties have a larger within-county consumption response than Democratic counties, while the polarization interaction difference is positive and significant ($\theta_{Dem} - \theta_{Rep} = 0.45$, $t = 3.70$), indicating that Democratic counties' response grows faster with rising polarization. For investment, we find that the baseline response remains symmetric regardless of polarization level ($\delta_{Dem} - \delta_{Rep} = -0.12$, $t = -0.31$). However, the polarization amplification is strongly asymmetric—Democratic counties show a large and significant response to rising polarization ($\theta_{Dem} = 0.85$, $t = 4.27$) while Republican counties show none ($\theta_{Rep} = -0.06$, $t = -0.48$), yielding a significant difference ($\theta_{Dem} - \theta_{Rep} = 0.91$, $t = 4.17$). Unlike consumption, there is no offsetting baseline asymmetry—the investment polarization amplification is a clean one-sided effect concentrated entirely in Democratic counties.

Panel B reveals significant heterogeneity in the intensive margins of alignment between Republican and Democratic counties. Column (1) shows that the intensive margin for consumption growth is indistinguishable from zero for Democratic counties (0.03, $t = 0.29$) while it is large and significant for Republican counties (0.63, $t = 11.80$). The difference between the two is large and statistically significant ($\delta_{Dem} - \delta_{Rep} = -0.61$, $t = -4.58$), indicating that the degree of partisan alignment significantly amplifies the consumption response among Republican counties but not among Democratic counties. Column (2) once again finds a significant difference in the intensive margin between Republican and Democratic counties at mean polarization levels, but also that these responses are significantly amplified by the level of polarization for both Republican and Democratic counties. Although the point estimate on the triple interaction is larger for Republican counties, we cannot reject the null that the two types of counties respond equally to polarization at the 5% ($\theta_{Dem} - \theta_{Rep} = -0.28$, $t = -1.88$) level. Column (3) shows that the intensive margin is significant for both Republican and Democratic counties with respect to investment, but that Democratic counties respond more strongly ($\delta_{Dem} - \delta_{Rep} = 0.59$, $t = 2.73$). Column (4) shows that the intensive margin is not affected by polarization for either Republican or Democratic counties.[27]

4.3 Within-Term Dynamics

Having provided evidence that political alignment affects local consumption and investment growth, we now examine the within-term dynamics of this effect. Three questions motivate this analysis. First, if the alignment effect operates through a real economic channel—for example, through government subsidies, targeted investment, or differential regulatory treatment—such effects should take time to materialize and should therefore be concentrated in the later years of a presidential term. An effect that is instead concentrated early in the term, when any policy changes are less likely to have affected local economic conditions, is more consistent with an expectations-based or behavioral channel. Second, the timing of the effect relative to midterm elections provides a test of the mechanism underlying the alignment effect. Aside from Bush's first term, in all of the presidencies in our sample the president's party suffered electoral losses at the midterm, reducing its effective governing power and thus the probability that aligned partisans' preferred policies will be

[27] A second dimension of symmetry concerns whether county partisan composition matters equally for aligned and unaligned counties. In the Internet Appendix (Table IA-XII) we decompose Alignment separately for aligned and unaligned county-years and find that the consumption response is driven by counties that become unaligned: the intensive margin is large and highly significant for unaligned counties, is absent for aligned counties, and is amplified more strongly by rising affective polarization. For investment the pattern reverses, with an incremental effect only among aligned counties, although the difference between the two is significant only at the 10% level and only once the polarization interactions are included.

enacted.[28] A rational expectations mechanism therefore predicts that the alignment effect should weaken after midterm losses. Angrisani, Sias, Sobotka, and Turtle (2025) provide direct evidence against this prediction at the individual level, documenting that partisan economic expectations do not update following midterm electoral losses—consistent with a motivated beliefs mechanism in which partisans maintain optimistic economic assessments regardless of the president's diminished governing capacity. Here, we test whether this same mechanism is reflected in realized local economic outcomes: if the alignment effect persists through the second half of the presidential term despite midterm losses, this corroborates the motivated beliefs interpretation at the aggregate level and is less consistent with a rational expectations channel. Third, while prior work has documented partisan consumption responses in the immediate aftermath of presidential elections (e.g., Kamdar and Ray (2022)), it is an open question whether these responses are short-lived or persist throughout the presidency. Persistence has substantially larger aggregate implications, as it suggests that partisan economic behavior is a sustained feature of the macroeconomic environment rather than a transitory post-election phenomenon.

To examine these questions, we estimate the alignment effect separately for the first and second halves of each presidential term. Because part of our interest is in partisan responses to midterm losses, we estimate our main specification on a panel that excludes Bush's first term.

[Insert Table VI about here.]

Table VI provides strong support for the behavioral channel while simultaneously establishing that partisan responses to alignment are not short-lived. Column (2) estimates the consumption alignment effect at 0.51 percentage points ($t = 9.46$) in the first half of the presidential term and an additional 0.06 percentage points ($t = 0.86$) for the second half interaction, implying a total effect of 0.57 percentage points in the second half. That is, the effect of alignment manifests in the first half of the presidency and does not decay in the second half, following midterm losses. The pre-midterm response is less consistent with policy changes increasing local employment and thus consumption and instead supports the expectations-based channel in which the election outcome triggers a behavioral response while the persistence of the effect throughout the second half of the term despite midterm losses is consistent with the motivated beliefs mechanism documented in Angrisani, Sias, Sobotka, and Turtle (2025). Relatedly, because our specifications control for contemporaneous income and population growth, any government subsidies or investment that did reach aligned counties and stimulate their local economies would be absorbed by these controls rather than

[28] We provide midterm election outcomes for each of the presidential terms in our sample in the Internet Appendix.

attributed to alignment. Column (4) adds interactions for national affective polarization. The polarization interaction is large and significant in the first half (0.67, $t = 11.25$), confirming that rising affective polarization has amplified the initial expectations-driven consumption response. However, this amplification is significantly offset in the second half (-0.33, $t = -4.23$), indicating that the polarization amplification is concentrated in the first half of the term. This pattern is consistent with polarization intensifying the initial behavioral response to election outcomes while the subsequent persistence of the effect through the second half is driven by the baseline alignment effect.

The investment results in columns (5)–(8) reveal similar patterns. Column (6) shows that the investment-alignment effect exists in the first half of the presidency and that it intensifies in the second half. The initial response and sustained response throughout the presidency are once again consistent with a motivated beliefs explanation. In columns (7) and (8) we find little evidence of a polarization effect on investment: the first-half interaction is significant only at the 10% level in column (7) (0.12, $t = 1.70$) and is indistinguishable from zero once the second-half interaction is added in column (8).

We reestimate these results both including Bush's first term and again excluding observations in 2008, 2009, 2020, and 2021 in the Internet Appendix. We find consistent results when we include Bush's first term. When we exclude crisis years, we find that the consumption response weakens slightly over the second half of the presidency and find no amplification of the investment response over the second half.

4.4 Cross-County Economic Activity

There is substantial heterogeneity in the geographic size of counties and thus in the likelihood that individuals work and consume across county borders. Thus, it is possible that our results are contaminated by cross-county spillovers.[29] To test whether this is the case, we reestimate our main results at the commuting zone level. Because they are designed to capture local labor market activity rather than political boundaries, commuting zones are less susceptible to such spillover effects (e.g., Dunn and Gholizadeh (2025)).[30] We aggregate our county-level variables to the commuting zone (CZ) level using data from the U.S. Department of Agriculture (USDA). We

[29]Consistent with this concern, Dunn and Gholizadeh (2025) document that cross-county spending potentially accounts for a non-trivial portion of within-county economic activity. On the other hand, Suárez Serrato and Wingender (2016) find limited spillover of local multiplier effects across counties, suggesting that within-county *shocks* to consumption do not spillover to neighboring counties.

[30]By the same hand, commuting zones will exhibit less political heterogeneity. As our main question regards how partisanship impacts household behavior, counties naturally remain our preferred level of aggregation.

provide additional details as to how commuting zones are formed as well as commuting zone-level summary statistics of our main variables in the Internet Appendix.[31]

[Insert Table VII about here.]

Table VII reports the results from estimating Equation 7 on our panel on commuting zone data. Our results are consistent with those estimated at the county level. In particular, we find that polarized CZs strongly adjust their consumption based on the party of the president and that this effect is amplified by the level of national affective polarization. For example, column (2) indicates that a one-standard deviation increase in the share of aligned voters in a CZ is associated with a 0.38 percentage point ($t = 7.77$) higher discretionary consumption growth, while column (3) indicates that the relationship between partisan alignment and consumption is significantly amplified during periods of high national affective polarization. Columns (4)–(6) reveal that our main results for the investment-alignment response are also consistent at the CZ level—CZs significantly increase their investment in response to changes in alignment. As at the county level, we find no effect of national affective polarization on investment at the CZ level (0.03, $t = 0.29$). Overall, our results are consistent at the commuting zone level, indicating that cross-county spillovers do not contaminate our county-level estimates.[32]

5 Identification of the Alignment Effect

We have shown that counties increase their discretionary consumption and investment when they become aligned with the sitting president. As we argue in Section IA-4, national election outcomes act as plausibly exogenous shocks to alignment: no individual county can affect the national result and partisan expectations update only once outcomes are realized. Despite this, we acknowledge the relationship could nevertheless reflect endogenous county vote shares, as when counties experiencing weak growth vote against the incumbent (who typically wins reelection), leaving those counties systematically less aligned; local shocks not captured by our controls that are correlated

[31] Counties are uniquely assigned to a commuting zone (i.e., a county never belongs to more than a single commuting zone). We use the 2020 USDA commuting zone definition. We provide additional details as to how commuting zones are formed in the Internet Appendix as well as how commuting zones compare to counties in terms of geographic coverage.

[32] We also replicate our analysis at the core-based statistical area (CBSA) level, the federal statistical standard for delineating local economies: counties anchored by an urban core of at least 50,000 population and linked to it by commuting ties, as delineated by the Office of Management and Budget. Following Bureau of Economic Analysis (2025), we aggregate all counties outside a metropolitan area to state non-metropolitan remainders, so that every county is covered. Internet Appendix Table IA-XVIII shows that our results are unchanged under this alternative geography: alignment remains a significant determinant of both consumption and investment growth, and the polarization interaction is again positive and significant for consumption and insignificant for investment.

with partisan composition; industries that generate income in Republican and Democratic counties performing differently across administrations; or estimates of this magnitude simply arising by chance. We test each of these possibilities directly in what follows and find the effect of alignment on consumption and investment unchanged, further supporting a causal interpretation of our results.

5.1 A Bartik Instrument Approach to Alignment

We first test whether endogenous changes in county partisan composition drive the alignment effect. Alignment is the product of two components: the party of the sitting president, which no county controls, and the county's own vote share, which is measured from contemporaneous ballots and may therefore respond to the same local economic conditions we study. Retrospective voting is the leading concern: counties experiencing weak growth vote against the incumbent party,[33] and because the incumbent party usually retains the presidency,[34] those counties systematically end up less aligned, generating a correlation between alignment and growth that runs from economic conditions to votes rather than from alignment to behavior. The same concern arises if households relocate across counties or if a county's partisan lean shifts over time in ways related to its economic prospects: in either case, the county's measured vote share changes with local conditions. Estimating the effect of alignment on discretionary consumption and investment growth therefore requires variation in alignment whose vote-share component predates the local economic conditions in our sample, which we obtain with a Bartik (shift-share) instrument.

Specifically, we instrument alignment with its value computed from predetermined partisan shares,

$$\begin{aligned}\text{Alignment}_{c,t}^{2000} = \text{Democratic Share}_{c,2000} \times \text{Dem Pres}_t \\ + (1 - \text{Democratic Share}_{c,2000}) \times (1 - \text{Dem Pres}_t)\end{aligned} \tag{11}$$

where $\text{Democratic Share}_{c,2000}$ is county c's two-party Democratic vote share in the 2000 presidential election and Dem Pres_t is defined as in Equation 6. The instrument is identical to Alignment except that the vote share is fixed at its 2000 value for the entire sample: it varies over time only through the party of the sitting president, and it varies across counties only through partisan composition measured before our sample begins. This is a Bartik instrument in the sense of Goldsmith-Pinkham, Sorkin, and Swift (2020): predetermined local shares, here partisan rather than industry shares,

[33] A large literature documents that voters punish incumbents for poor economic conditions; see, e.g., Kramer (1971), Fair (1978), and Fiorina (1981).

[34] Sitting presidents seeking reelection usually win. In our sample, Bush (2004) and Obama (2012) were reelected; Trump (2020) is the only sitting president to lose re-election.

interacted with aggregate shocks, here national election outcomes. Identification requires that partisan composition in 2000 be unrelated to subsequent changes in county consumption and investment growth except through alignment, conditional on county and year fixed effects and our controls; because the 2000 shares predate every observation in our sample, they cannot respond to the retrospective voting, migration, or realignment that occurs within it. We then estimate Equation 7 by two-stage least squares, instrumenting $\text{Alignment}_{c,t}$ with $\text{Alignment}_{c,t}^{2000}$.

[Insert Table VIII about here.]

The results in Table VIII show that, using a Bartik instrumental variables approach, the alignment effect is not driven by endogenous changes in county partisan composition. Columns (1) and (3) replicate our baseline OLS estimates on the instrument sample.[35] Column (2) indicates that, when alignment is instrumented with $\text{Alignment}_{c,t}^{2000}$, a one-standard deviation increase in the share of aligned voters is associated with an additional 0.27 percentage point ($t = 6.17$) increase in year-over-year discretionary consumption growth, and column (4) shows a corresponding 0.59 percentage point ($t = 8.26$) increase in investment growth. In the first-stage alignment loads on $\text{Alignment}_{c,t}^{2000}$ with a coefficient of 0.79, and the first-stage F-statistics far exceed conventional thresholds.[36] The smaller 2SLS estimate for consumption is expected: the instrument discards all variation arising from changes in partisan composition after 2000, so the 2000 shares measure a county's current partisanship with error that grows over the sample, and this drift attenuates the estimate precisely in the later years when rising affective polarization amplified the consumption response. The investment estimate is, if anything, somewhat larger than its OLS counterpart. If retrospective voting, migration, or realignment generated our results, instrumenting alignment with predetermined shares would eliminate the effect; instead, both estimates remain large and highly significant.

5.2 Contiguous County Pairs

We next test whether the alignment effect reflects local economic shocks, above and beyond our commuting zone results in Table VII, rather than partisan behavior. County fixed effects absorb

[35] The instrument requires a county's 2000 vote share, which two counties lack: Broomfield, Colorado, which was created in November 2001, and Oglala Lakota, South Dakota, which was assigned a new FIPS code in 2015. Together they account for 22 county-years, so the instrument sample is 66,047 rather than 66,069.

[36] The first stage F-statistics far exceed conventional thresholds for instrument relevance (Staiger and Stock (1997); Stock and Yogo (2005)). Their magnitude reflects the persistence of county partisanship: the instrument is the predetermined component of Alignment itself, so relevance is essentially guaranteed by construction, and the identifying content of the design comes from the variation the instrument excludes, namely post-2000 changes in county vote shares.

permanent differences across counties, year fixed effects absorb shocks common to all counties, and our controls absorb realized local income and population growth, but region-specific shocks, such as a commodity price boom in resource-dependent regions or a contraction of manufacturing demand in industrial regions, are neither permanent nor national and therefore survive all three. Because partisanship is spatially clustered, these regional shocks can be correlated with county partisan composition: when a regional economy booms while its counties happen to be aligned with the sitting president, discretionary consumption and investment rise in aligned counties for reasons unrelated to partisan behavior. Ruling out this channel requires comparing counties that share the same local economy but differ in partisan composition.

To implement this comparison, we construct all pairs of geographically adjacent counties from the Census Bureau's county adjacency file, yielding 8,993 unique pairs of neighboring counties.[37] Following Dube, Lester, and Reich (2010), for each pair and year we difference the outcome and all regressors between the two counties in the pair and estimate

$$\Delta \text{Dep Var}_{p,t} = \delta \Delta \text{Alignment}_{p,t} + \Gamma \Delta X_{p,t} + \alpha_p + \tau_t + \epsilon_{p,t} \tag{12}$$

where, for adjacent-county pair p, Δ denotes the difference between the two counties in the pair, α_p are pair fixed effects, and τ_t are year fixed effects. Differencing removes any shock common to both counties in a pair: because adjacent counties share a local economy, a regional boom or bust affects both members and drops out of the difference. Pair fixed effects absorb permanent differences between the two counties, and year fixed effects absorb aggregate conditions, so δ is identified solely from differences in partisan composition between neighboring counties.[38] We scale the within-pair alignment difference by the full-sample standard deviation of Alignment so that coefficients are directly comparable to Table III.[39] We estimate the specification on all adjacent pairs, on pairs that straddle a state border, which additionally hold state policy fixed, and on pairs within the same state.

[Insert Table IX about here.]

The results in Table IX show that local economic shocks not captured by our county and year fixed effects do not drive the alignment effect. Column (1) indicates that, within pairs of adjacent

[37] We use the 2023 vintage of the Census Bureau county adjacency file. A county adjacent to several counties appears in one pair with each of its neighbors. We retain a pair only if both counties are in our estimation sample.

[38] Neighboring counties differ meaningfully in partisan composition: the average absolute within-pair difference in two-party vote share is 9.0 percentage points, and about one in ten pairs differs by more than 20 percentage points.

[39] Because each county appears in as many pairs as it has neighbors, observations are not independent across pairs; we therefore cluster standard errors on both counties in the pair.

counties, a one-standard deviation increase in the share of aligned voters is associated with an additional 0.41 percentage point ($t = 8.73$) increase in relative year-over-year consumption growth, slightly smaller than our baseline estimate. The effect is present both for pairs that straddle a state border (0.28, $t = 2.59$), which additionally holds state policy fixed, and for pairs within the same state (0.43, $t = 8.86$). Results for investment growth are similar: the within-pair alignment effect is 0.35 percentage points ($t = 4.44$) across all adjacent pairs and 0.37 ($t = 4.24$) while the cross-state estimate is positive and significant at the 10% level (0.26, $t = 1.65$), less precisely estimated than the others because there are far fewer border pairs. If regional shocks correlated with partisan composition drove our results, the alignment effect would disappear among neighboring counties that share those shocks; instead, it is fully present.

5.3 Shift-Share Predicted County GDP Growth

Next we test whether the alignment effect reflects industrial composition rather than partisan behavior. County partisanship is correlated with industrial structure: in our sample, Republican counties derive a substantially larger share of GDP from agriculture, mining, and manufacturing, while Democratic counties derive a larger share from service industries.[40] At the same time, national industry performance may vary with the party of the sitting president, whether through policy, regulation, or the macroeconomic conditions each administration inherits. Together, these two patterns imply that aligned counties could experience systematically faster growth in local income whenever their preferred party holds office. Because household income finances both discretionary consumption and equity purchases, this channel could reproduce our findings in both outcomes without any change in partisan behavior. Ruling out this channel therefore requires accounting for each county's exposure to national industry performance through its complete industrial composition.

To capture this exposure, we construct a shift-share measure of predicted growth in the spirit of Bartik (1991) and Blanchard and Katz (1992):

$$\text{Predicted Growth}_{c,t} = \sum_{k} s_{ik}\, g_{kt}^{-i} \tag{13}$$

where s_{ik} is county c's share of GDP in two-digit industry k in the first year of our sample and

[40] Averaging across county-years, agriculture (NAICS 11), mining (NAICS 21), and manufacturing (NAICS 31–33) account for 26% of GDP in Republican counties versus 17% in Democratic counties, while information, finance, real estate, and professional and business services (NAICS 51–56) account for 24% versus 30%. Both differences are statistically significant at the 1% level ($t = 14.46$ and $t = 12.53$, respectively, with standard errors clustered at the county level).

g_{kt}^{-i} is the national growth rate of real GDP in industry k in year t, computed excluding county c. Predicted Growth is the growth in total county GDP that county c would experience if each of its industries grew at that industry's national rate. By construction, it captures the growth a county experiences when the industries concentrated there perform well or poorly under a given administration, which is precisely the channel through which industrial composition could feed into discretionary consumption and investment: stronger industry performance raises local income, and local income finances both outcomes. If the alignment effect reflected this channel, controlling for Predicted Growth would eliminate it. Because each county is excluded from the national growth rates it is assigned, local shocks cannot enter a county's own prediction. Our construction follows Chodorow-Reich, Nenov, and Simsek (2021), who include an analogous predicted-growth control in county-level regressions.[41]

[Insert Table X about here.]

The results in Table X show that industrial composition does not drive the alignment effect. Columns (1) and (4) replicate our baseline estimates for consumption and investment for comparison. Column (2) indicates that, after controlling for predicted industry Growth, a one-standard deviation increase in the share of aligned voters in a county is associated with an additional 0.36 percentage point ($t = 10.03$) increase in year-over-year discretionary consumption growth, essentially identical to the baseline estimate in column (1). Column (3) shows that the estimate is likewise unchanged when Predicted Industry Growth is constructed excluding the consumer-facing industries. Results are similar for investment growth: the alignment coefficients in columns (5) and (6) are essentially identical to the baseline estimate in column (4). If aligned counties consumed and invested more simply because their industries generate more income under their preferred administrations, controlling for Predicted Industry Growth would have eliminated the alignment effect; instead, it is unaffected.

5.4 Randomization Inference

Our final exercise asks whether coefficients of the magnitude we estimate could arise by chance. We randomly reassign each county's entire time series of alignment to another county and re-

[41] Because Predicted Growth enters our specifications as a control rather than as an instrument, our estimates do not require the share- or shock-exogeneity conditions developed for shift-share instrumental variables; see Goldsmith-Pinkham, Sorkin, and Swift (2020) and Borusyak, Hull, and Jaravel (2025). In robustness checks we also construct the measure using lagged rather than initial-year shares, and excluding the consumer-facing industries (NAICS 44–45 and 72) from the shares so that the prediction is built entirely from industries outside our consumption measure.

estimate our baseline specifications, repeating this procedure 1000 times. Only alignment is reassigned: each county retains its own dependent variables and controls, and because complete histories move together, the placebo series preserve the time-series structure of alignment, which changes only at presidential elections. The reassigned histories are unrelated to local economic conditions by construction, so the distribution of placebo coefficients shows what our regressions produce when the true effect is zero, without relying on any assumption about the correlation structure of the errors.

Figure 3 plots the distributions of the 1000 placebo coefficients and Table XI reports the corresponding statistics. The placebo distributions are centered at zero, with standard deviations of 0.033 for consumption and 0.056 for investment; the largest placebo coefficient in absolute value is 0.11 and 0.21, respectively. The actual estimates of 0.47 and 0.47 lie roughly fourteen and eight standard deviations from the placebo means, and not one of the 1000 permutations produces a coefficient as large as the actual estimate, implying an empirical p-value below 0.001. Randomly assigned partisan histories essentially never generate relationships approaching those we observe.

6 Conclusion

Political affiliation and polarization generate large differences in reported expectations about the economy and financial markets. The central question for finance is whether these differences are merely expressive or whether they alter realized household decisions. Existing evidence on this transmission is mixed, leaving unresolved whether partisan belief heterogeneity has economically meaningful consequences. Using county-level data covering nearly every U.S. county across multiple presidential administrations, we provide evidence that it does: political alignment with the sitting president affects both discretionary consumption and equity investment. These effects are economically large relative to standard determinants of local activity. Both responses operate primarily through the intensive margin, varying with the share of voters aligned with the president rather than simply whether a county's political majority is aligned. Moreover, the consumption response has strengthened as affective polarization has risen, consistent with the prediction that political alignment matters more for household behavior as partisan divisions deepen. These results establish a direct link between politically driven expectations and realized household financial behavior.

The relationship survives complementary identification tests designed to address the leading alternative explanations. We instrument alignment with pre-sample vote shares, compare neigh-

boring counties exposed to similar local economic conditions, and control for each county's exposure to national industry performance. These designs separate the alignment effect from contemporaneous changes in county partisanship, regional economic shocks, and differences in the industries concentrated in Republican and Democratic counties. The timing and persistence of the responses provide additional evidence on the underlying mechanism. The effects emerge before policy changes are likely to materially affect local conditions, remain after midterm losses reduce the president's governing capacity, and are unchanged when we control directly for federal grants and contracts. These patterns are more consistent with motivated beliefs than with a policy-driven channel or rational updating. By showing that politically driven beliefs alter consumption and equity investment, our results establish partisan alignment as an economically consequential determinant of household spending and equity exposure at levels detectable in the aggregate.

These findings have broad implications for household finance. The time-varying differences in equity investment associated with political alignment interact with realized market returns, so households with otherwise similar fundamentals may accumulate substantially different levels of wealth simply because periods of greater equity exposure coincide with strong or weak market performance. This mechanism can generate path-dependent wealth differences even when neither partisan group possesses superior information or investment skill. Geographically concentrated shifts in discretionary spending may also affect the revenues and cash flows of consumer-facing firms, with potential consequences for their investment, leverage, and employment decisions. Partisan expectations may therefore influence not only contemporaneous household consumption and portfolio choice, but also the subsequent distribution of household wealth and the real decisions of firms. We leave these important questions for future research.

References

Ahn, Michael, Michael Batty, and Ralf R. Meisenzahl, 2018, Household Debt-to-Income Ratios in the Enhanced Financial Accounts, FEDS Notes, Board of Governors of the Federal Reserve System.

Angrisani, Marco, Richard W Sias, Austin Sobotka, and Harry J Turtle, 2025, Political Polarization and Stock Market Expectations, *Available at SSRN 5691962*.

Aysheshim, Kirubel D., Jacob R. Hinson, and Sharon D. Panek, 2020, A Primer on Local Area Gross Domestic Product Methodology, *Survey of Current Business* 100(3).

Bartels, Larry M., 2002, Beyond the Running Tally: Partisan Bias in Political Perceptions, *Political Behavior* 24, 117–150.

Bartik, Timothy J., 1991, *Who Benefits from State and Local Economic Development Policies?* W.E. Upjohn Institute for Employment Research, Kalamazoo, MI.

Berry, Christopher R., Barry C. Burden, and William G. Howell, 2010, The President and the Distribution of Federal Spending, *American Political Science Review* 104(4), 783–799.

Blanchard, Olivier Jean, and Lawrence F. Katz, 1992, Regional Evolutions, *Brookings Papers on Economic Activity* 1992(1), 1–75.

Bonaparte, Yosef, Alok Kumar, and Jeremy K. Page, 2017, Political Climate, Optimism, and Investment Decisions, *Journal of Financial Markets* 34, 69–94.

Borusyak, Kirill, Peter Hull, and Xavier Jaravel, 2025, A Practical Guide to Shift-Share Instruments, *Journal of Economic Perspectives* 39(1), 181–204.

Boxell, Levi, Matthew Gentzkow, and Jesse Shapiro, 2017, Is the Internet Causing Political Polarization? Evidence from Demographics, working paper w23258, National Bureau of Economic Research, Cambridge, MA.

Brav, Alon, John R Graham, Campbell R Harvey, and Roni Michaely, 2005, Payout policy in the 21st century, *Journal of Financial Economics* 77(3), 483–527.

Bureau of Economic Analysis, 2025, Local Area Personal Income and Employment: Concepts and Methods, working paper, U.S. Department of Commerce.

Cameron, A. Colin, Jonah B. Gelbach, and Douglas L. Miller, 2011, Robust Inference with Multiway Clustering, *Journal of Business & Economic Statistics* 29(2), 238–249.

Campbell, John Y, and Robert J Shiller, 1988, The dividend-price ratio and expectations of future dividends and discount factors, *The Review of Financial Studies* 1(3), 195–228.

Chen, M. Keith, and Ryne Rohla, 2018, The effect of partisanship and political advertising on close family ties, *Science* 360(6392), 1020–1024.

Chodorow-Reich, Gabriel, 2019, Geographic cross-sectional fiscal spending multipliers: What have we learned?, *American Economic Journal: Economic Policy* 11(2), 1–34.

Chodorow-Reich, Gabriel, Plamen T Nenov, and Alp Simsek, 2021, Stock market wealth and the real economy: A local labor market approach, *American Economic Review* 111(5), 1613–1657.

Chu, Yongqiang, Francesco D'Acunto, Janet Gao, Cong Roman Wang, and Yichen Wang, 2026, Political Ideology and Durable Spending, *Yichen, Political Ideology and Durable Spending (March 16, 2026)*.

Cochrane, John H, 2008, The dog that did not bark: A defense of return predictability, *The Review of Financial Studies* 21(4), 1533–1575.

Cohen, Alma, and Crystal S. Yang, 2019, Judicial Politics and Sentencing Decisions, *American Economic Journal: Economic Policy* 11(1), 160–191.

Coibion, Olivier, Yuriy Gorodnichenko, and Michael Weber, 2020, Political Polarization and Expected Economic Outcomes, *NBER Working Paper Series*.

Conover, Pamela Johnston, Stanley Feldman, and Kathleen Knight, 1987, The Personal and Political Underpinnings of Economic Forecasts, *American Journal of Political Science* 31(3), 559–583.

Cookson, J Anthony, Joseph E Engelberg, and William Mullins, 2020, Does Partisanship Shape Investor Beliefs? Evidence from the COVID-19 Pandemic, *Review of Asset Pricing Studies* 10(4), 863–893.

Dube, Arindrajit, T. William Lester, and Michael Reich, 2010, Minimum Wage Effects Across State Borders: Estimates Using Contiguous Counties, *Review of Economics and Statistics* 92(4), 945–964.

Dunn, Abe, and Mahsa Gholizadeh, 2025, The Geography of Consumption and Local Economic Shocks: The Case of the Great Recession, *American Economic Journal: Macroeconomics* 17(2), 206–244.

Evans, Geoffrey, and Robert Andersen, 2006, The Political Conditioning of Economic Perceptions, *The Journal of Politics* 68(1), 194–207.

Fair, Ray C., 1978, The Effect of Economic Events on Votes for President, *Review of Economics and Statistics* 60(2), 159–173.

Fiorina, Morris P., 1981, *Retrospective Voting in American National Elections*. Yale University Press, New Haven.

Fiorina, Morris P., Samuel A. Abrams, and Jeremy C. Pope, 2008, Polarization in the American Public: Misconceptions and Misreadings, *The Journal of Politics* 70(2), 556–560.

Fowler, Christopher S, 2024, New Commuting Zone delineation for the US based on 2020 data, *Scientific Data* 11(1), 975.

Gerber, Alan S., and Gregory A. Huber, 2009, Partisanship and Economic Behavior: Do Partisan Differences in Economic Forecasts Predict Real Economic Behavior?, *American Political Science Review* 103(3), 407–426.

——— , 2010, Partisanship, Political Control, and Economic Assessments, *American Journal of Political Science* 54(1), 153–173.

Giglio, Stefano, Matteo Maggiori, Johannes Stroebel, and Stephen Utkus, 2021, Five Facts about Beliefs and Portfolios, *American Economic Review* 111(5), 1481–1522.

Gillitzer, Christian, and Nalini Prasad, 2018, The Effect of Consumer Sentiment on Consumption: Cross-Sectional Evidence from Elections, *American Economic Journal — Macroeconomics* 10(4), 234–269.

Goldsmith-Pinkham, Paul, Isaac Sorkin, and Henry Swift, 2020, Bartik Instruments: What, When, Why, and How, *American Economic Review* 110(8), 2586–2624.

Guren, Adam M, Alisdair McKay, Emi Nakamura, and Jón Steinsson, 2021, Housing wealth effects: The long view, *The Review of Economic Studies* 88(2), 669–707.

Hersh, Eitan D., and Matthew N. Goldenberg, 2016, Democratic and Republican physicians provide different care on politicized health issues, *Proceedings of the National Academy of Sciences* 113(42), 11811–11816.

Hong, Harrison, and Leonard Kostovetsky, 2012, Red and blue investing: Values and finance, *Journal of Financial Economics* 103(1), 1–19.

Howell, William, Scott Adler, Charles Cameron, and Charles Riemann, 2000, Divided Government and the Legislative Productivity of Congress, 1945–94, *Legislative Studies Quarterly* 25(2), 285–312.

Huber, Gregory A., and Neil Malhotra, 2017, Political Homophily in Social Relationships: Evidence from Online Dating Behavior, *Journal of Politics* 79(1), 269–283.

Huberman, Gur, Tobias Konitzer, Masha Krupenkin, David Rothschild, and Shawndra Hill, 2018, Economic Expectations, Voting, and Economic Decisions around Elections, 108, 597–602.

Iyengar, Shanto, Tobias Konitzer, and Kent Tedin, 2018, The Home as a Political Fortress: Family Agreement in an Era of Polarization, *Journal of Politics* 80(4), 1326–1338.

Iyengar, Shanto, Yphtach Lelkes, Matthew Levendusky, Neil Malhotra, and Sean J. Westwood, 2019, The Origins and Consequences of Affective Polarization in the United States, *Annual Review of Political Science* 22(Volume 22, 2019), 129–146, Publisher: Annual Reviews.

Iyengar, Shanto, Gaurav Sood, and Yphtach Lelkes, 2012, Affect, Not Ideology, *Public Opinion Quarterly* 76(3), 405–431.

Kamdar, Rupal, and Walker Ray, 2022, Polarized Expectations, Polarized Consumption, *Working paper, Indiana University, London School of Economics.*

Kaustia, Markku, and Sami Torstila, 2011, Stock market aversion? Political preferences and stock market participation, *Journal of Financial Economics* 100(1), 98–112.

Ke, Da, 2024, Left behind: Partisan identity, stock market participation, and wealth inequality, *Journal of Banking & Finance* 164, 107201.

Kempf, Elisabeth, and Margarita Tsoutsoura, 2024, Political Polarization and Finance, *Annual Review of Financial Economics* 16, 413–434.

Kleibergen, Frank, and Richard Paap, 2006, Generalized Reduced Rank Tests Using the Singular Value Decomposition, *Journal of Econometrics* 133(1), 97–126.

Kramer, Gerald H., 1971, Short-Term Fluctuations in U.S. Voting Behavior, 1896–1964, *American Political Science Review* 65(1), 131–143.

Lee, Donghoon, and Wilbert Van der Klaauw, 2010, An introduction to the FRBNY consumer credit panel, *FRB of New York Staff Report*(479).

Lintner, John, 1956, Distribution of incomes of corporations among dividends, retained earnings, and taxes, *The American economic review* 46(2), 97–113.

McGrath, Mary C., 2017, Economic Behavior and the Partisan Perceptual Screen, *Quarterly Journal of Political Science* 11(4), 363–383.

Meeuwis, Maarten, Jonathan A. Parker, Antoinette Schoar, and Duncan Simester, 2022, Belief Disagreement and Portfolio Choice, *Journal of Finance* 77(6), 3191–3247.

Mian, Atif, Kamalesh Rao, and Amir Sufi, 2013, Household Balance Sheets, Consumption, and the Economic Slump, *Quarterly Journal of Economics* 128(4), 1687–1726.

Mian, Atif, and Amir Sufi, 2014, What explains the 2007–2009 drop in employment?, *Econometrica* 82(6), 2197–2223.

Mian, Atif, Amir Sufi, and Nasim Khoshkhou, 2023, Partisan Bias, Economic Expectations, and Household Spending, *Review of Economics and Statistics* 105(3), 493–510.

Milosh, Maria, Marcus Painter, Konstantin Sonin, David Van Dijcke, and Austin L. Wright, 2021, Unmasking partisanship: Polarization undermines public response to collective risk, *Journal of Public Economics* 204, 104538.

Nicholson, Stephen P., Chelsea M. Coe, Jason Emory, and Anna V. Song, 2016, The Politics of Beauty: The Effects of Partisan Bias on Physical Attractiveness, *Political Behavior* 38(4), 883–898.

Saez, Emmanuel, and Gabriel Zucman, 2016, Wealth inequality in the United States since 1913: Evidence from capitalized income tax data, *The Quarterly Journal of Economics* 131(2), 519–578.

Smith, Matthew, Owen Zidar, and Eric Zwick, 2023, Top wealth in America: New estimates under heterogeneous returns, *The Quarterly Journal of Economics* 138(1), 515–573.

Staiger, Douglas, and James H. Stock, 1997, Instrumental Variables Regression with Weak Instruments, *Econometrica* 65(3), 557–586.

Stock, James H., and Motohiro Yogo, 2005, Testing for Weak Instruments in Linear IV Regression, in Donald W. K. Andrews, and James H. Stock, eds.: *Identification and Inference for Econometric Models: Essays in Honor of Thomas Rothenberg* (Cambridge University Press), Cambridge, pp. 80–108.

Suárez Serrato, Juan Carlos, and Philippe Wingender, 2016, Estimating Local Fiscal Multipliers, .

Wintoki, M. Babajide, and Yaoyi Xi, 2020, Partisan Bias in Fund Portfolios, *Journal of Financial and Quantitative Analysis* 55(5), 1717–1754.

Wlezien, Christopher, Mark Franklin, and Daniel Twiggs, 1997, Economic Perceptions and Vote Choice: Disentangling the Endogeneity, *Political Behavior* 19(1), 7–17.

Yi, Yang, 2025, Does Partisanship Affect Household Consumption?, *Available at SSRN 4733331*.

Table I – County Summary Statistics

The table reports summary statistics for our main variables, computed on the estimation sample used throughout the paper, so that every row describes the same set of observations. Democratic Share is the county's two-party Democratic vote share in the most recent presidential election, and Alignment is defined in Equation 6. Local Consumption is the sum of the BEA's estimated county-level GDP in the retail trade (NAICS codes 44-45) and accommodations and food services (NAICS code 72) industries (in thousands). Dividend Income is the county-level total dividend income from the IRS (in thousands). All dollar variables are in constant 2023 dollars. Investment Growth is computed from nominal dividend income and then demeaned by the yearly cross-sectional mean, since inflation is common to all counties within a year and is removed by the demeaning; Dividend Income is therefore deflated in this table for presentation only. All growth variables are winsorized at the 5% and 95% levels of the estimation sample. The final two columns report means for Republican (Democratic Share$_{i,t} < 0.50$) and Democratic (Democratic Share$_{i,t} > 0.50$) counties. Our sample includes 3,058 unique counties (covering all 50 states plus DC) and runs from 2002 to 2023.

							Mean by County Type	
Variable	N	Mean	10^{th} Percentile	Median	90^{th} Percentile	Standard Deviation	Republican	Democrat
Democratic Share	66,069	38.21%	19.84%	37.30%	57.57%	14.65%	32.69%	59.69%
Alignment	66,069	50.64%	24.69%	51.30%	75.92%	18.80%	50.40%	51.56%
Population	66,069	102,889	5,647	26,613	209,323	325,808	59,336	272,392
Population Growth	66,069	0.24%	-1.19%	0.16%	1.81%	1.10%	0.26%	0.15%
Per Capita Income	66,069	$50,157	$36,789	$47,496	$65,718	$14,417	$49,225	$53,782
Income Growth	66,069	1.60%	-3.78%	1.60%	6.96%	3.96%	1.65%	1.43%
Local Consumption	66,069	$662,945	$11,370	$104,507	$1,278,895	$2,638,280	$329,123	$1,962,123
Local Consumption Growth	66,069	1.84%	-6.86%	1.68%	10.75%	6.93%	1.91%	1.58%
Dividend Income	66,069	$92,441	$1,286	$9,026	$142,891	$470,179	$38,214	$303,486
Investment Growth	66,069	0.00%	-16.01%	-0.87%	18.69%	14.21%	-0.02%	0.06%

Table II – Level Differences

The table reports unconditional mean year-over-year growth in both consumption and investment separately for Democratic and Republican counties under either Republican or Democratic presidencies. Standard errors are clustered at the county level. ***,**,* indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Consumption Growth			Investment Growth		
	Rep Pres	Dem Pres	Rep−Dem Pres	Rep Pres	Dem Pres	Rep−Dem Pres
	(1)	(2)	(3)	(4)	(5)	(6)
Rep County	1.05***	2.82***	-1.77***	0.15**	-0.20***	0.35***
	(23.78)	(70.94)	(-29.94)	(2.42)	(-2.95)	(3.86)
Dem County	0.20**	2.69***	-2.48***	-0.69***	0.67***	-1.36***
	(2.53)	(37.44)	(-22.94)	(-5.61)	(5.65)	(-7.70)
Rep − Dem	0.84***	0.13	0.71***	0.85***	-0.87***	1.72***
	(9.19)	(1.60)	(5.71)	(6.10)	(-6.38)	(8.57)

Table III – Local Economic Activity and Political Alignment

The table reports estimated coefficients from Equation 7. Columns (1)–(3) use discretionary consumption growth and columns (4)–(6) use investment growth as dependent variables. Columns (1) and (4) include only Alignment (Equation 6); columns (2) and (5) add beginning year per capita county income, county income growth, and county population growth; columns (3) and (6) add an interaction term between Alignment and the national level of affective polarization (Equation 5). Consumption growth, income levels, and income growth are based on constant 2023 dollars (i.e., inflation adjusted). Regressors are normalized to have mean zero and unit standard deviation. Coefficients are in percent (i.e., multiplied by 100). t-statistics are reported in parentheses. Standard errors are clustered at the county-level. ***,**,* indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Consumption Growth			Investment Growth		
	(1)	(2)	(3)	(4)	(5)	(6)
$\text{Alignment}_{i,t}$	0.46***	0.47***	0.36***	0.49***	0.47***	0.45***
	(12.37)	(12.79)	(10.01)	(8.41)	(8.01)	(7.63)
$\text{Income}_{i,t-1}$		0.18*	0.06		−0.11	−0.14
		(1.85)	(0.65)		(−0.81)	(−0.95)
$\text{Income Growth}_{i,t}$		0.84***	0.84***		1.84***	1.84***
		(19.12)	(19.13)		(21.16)	(21.15)
$\text{Population Growth}_{i,t}$		0.51***	0.54***		0.22**	0.22**
		(11.84)	(12.51)		(2.48)	(2.54)
$\text{Alignment}_{i,t} \times \text{Aff Polar}_t$			0.52***			0.10
			(13.42)			(1.49)
Year Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes
County Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.1878	0.2015	0.2043	0.0272	0.0392	0.0392
N	66,069	66,069	66,069	66,069	66,069	66,069

TABLE IV – EXTENSIVE AND INTENSIVE MARGINS

The table reports estimated coefficients from Equation 9. $\text{Aligned}_{i,t}$ is the binary indicator defined in Equation 8 and captures the extensive margin of becoming aligned with the sitting president. $\text{Aligned}_{i,t} \times \text{Alignment}_{i,t}$ captures the intensive margin, the marginal effect of the degree of alignment. Columns (1)–(2) use discretionary consumption growth and columns (3)–(4) investment growth as dependent variables. County controls include lagged county income, county income growth, and county population growth. Continuous regressors are normalized to have mean zero and unit standard deviation. Coefficients are multiplied by 100. $t-$statistics are reported in parentheses. Standard errors are clustered at the county-level. ***,**,* indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Consumption Growth		Investment Growth	
	(1)	(2)	(3)	(4)
$\text{Aligned}_{i,t}$	0.53***	−0.11	0.77***	0.22
	(8.76)	(−1.21)	(7.48)	(1.50)
$\text{Aligned}_{i,t} \times \text{Alignment}_{i,t}$		0.86***		0.73***
		(8.41)		(4.54)
County Controls?	Yes	Yes	Yes	Yes
Year Fixed Effects?	Yes	Yes	Yes	Yes
County Fixed Effects?	Yes	Yes	Yes	Yes
R^2	0.1999	0.2009	0.0390	0.0392
N	66,069	66,069	66,069	66,069

TABLE V – SYMMETRY - REPUBLICAN VS DEMOCRATIC COUNTIES

The table reports estimated coefficients from Equation 10. Panel A reports the extensive margin (Align $\text{Var}_{i,t} = \text{Aligned}_{i,t}$) and Panel B reports the intensive margin (Align $\text{Var}_{i,t} = \text{Alignment}_{i,t}$). Columns (2) and (4) additionally interact both terms with Aff Polar_t, with θ_{Dem} and θ_{Rep} denoting the coefficients on these interactions. $\delta_{Dem} - \delta_{Rep}$ reports the difference in coefficients between Dem $\text{County}_{i,t} \times$ Align $\text{Var}_{i,t}$ and Rep $\text{County}_{i,t} \times$ Align $\text{Var}_{i,t}$. $\theta_{Dem} - \theta_{Rep}$ reports the difference in coefficients between Dem $\text{County}_{i,t} \times$ Align $\text{Var}_{i,t} \times$ Aff Polar_t and Rep $\text{County}_{i,t} \times$ Align $\text{Var}_{i,t} \times$ Aff Polar_t. Continuous regressors are normalized to have mean zero and unit standard deviation. Coefficients are multiplied by 100. $t-$statistics are reported in parentheses. Standard errors are clustered at the county-level. ***,**,* indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Consumption Growth		Investment Growth	
	(1)	(2)	(3)	(4)
Panel A: Extensive margin				
Dem $\text{County}_{i,t} \times \text{Aligned}_{i,t}$	0.61***	0.26**	0.76***	0.57***
	(4.92)	(2.00)	(3.62)	(2.67)
Rep $\text{County}_{i,t} \times \text{Aligned}_{i,t}$	0.44***	0.77***	0.78***	0.69***
	(3.39)	(5.53)	(3.43)	(2.90)
Dem $\text{County}_{i,t} \times \text{Aligned}_{i,t} \times$ Aff Polar_t		1.14***		0.85***
		(10.94)		(4.27)
Rep $\text{County}_{i,t} \times \text{Aligned}_{i,t} \times$ Aff Polar_t		0.70***		−0.06
		(8.00)		(−0.48)
$\delta_{Dem} - \delta_{Rep}$	0.17	−0.51**	−0.01	−0.12
	(0.76)	(−2.20)	(−0.03)	(−0.31)
$\theta_{Dem} - \theta_{Rep}$		0.45***		0.91***
		(3.70)		(4.17)
County Controls?	Yes	Yes	Yes	Yes
Year Fixed Effects?	Yes	Yes	Yes	Yes
County Fixed Effects?	Yes	Yes	Yes	Yes
R^2	0.1999	0.2018	0.0390	0.0391
N	66,069	66,069	66,069	66,069
Panel B: Intensive margin				
Dem $\text{County}_{i,t} \times \text{Alignment}_{i,t}$	0.03	0.01	0.90***	0.88***
	(0.29)	(0.06)	(5.53)	(5.36)
Rep $\text{County}_{i,t} \times \text{Alignment}_{i,t}$	0.63***	0.50***	0.31***	0.28***
	(11.80)	(9.11)	(3.49)	(3.24)
Dem $\text{County}_{i,t} \times \text{Alignment}_{i,t} \times$ Aff Polar_t		0.31***		0.17
		(2.65)		(0.87)
Rep $\text{County}_{i,t} \times \text{Alignment}_{i,t} \times$ Aff Polar_t		0.59***		0.08
		(10.58)		(0.84)
$\delta_{Dem} - \delta_{Rep}$	−0.61***	−0.49***	0.59***	0.60***
	(−4.58)	(−3.80)	(2.73)	(2.75)
$\theta_{Dem} - \theta_{Rep}$		−0.28*		0.10
		(−1.88)		(0.37)
County Controls?	Yes	Yes	Yes	Yes
Year Fixed Effects?	Yes	Yes	Yes	Yes
County Fixed Effects?	Yes	Yes	Yes	Yes
R^2	0.2018	0.2046	0.0392	0.0393
N	66,069	66,069	66,069	66,069

Table VI – Economic Activity and Midterms

The table reports estimates of Equation 7 augmented with interactions between Alignment and an indicator for the second half of the presidential term, estimated on a panel that excludes Bush's first term. Columns (1)–(4) use discretionary consumption growth and columns (5)–(8) use investment growth as dependent variables. Columns (3)–(4) and (7)–(8) add interactions with the national level of affective polarization (Equation 5). County controls include beginning year per capita county income, county income growth, and county population growth. Continuous regressors are normalized to have mean zero and unit standard deviation. Coefficients are multiplied by 100. $t-$statistics are reported in parentheses. Standard errors are clustered at the county-level. ***,**,* indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Consumption Growth				Investment Growth			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\text{Alignment}_{i,t}$ (First Half)	0.54***	0.51***	0.48***	0.47***	0.47***	0.22**	0.21**	0.21**
	(13.04)	(9.46)	(8.91)	(8.78)	(7.06)	(2.31)	(2.23)	(2.25)
$\text{Alignment}_{i,t} \times$ Second Half		0.06	−0.04	0.01		0.52***	0.50***	0.48***
		(0.86)	(−0.60)	(0.11)		(3.37)	(3.21)	(2.99)
$\text{Alignment}_{i,t} \times \text{Aff Polar}_t$			0.52***	0.67***			0.12*	0.06
			(12.83)	(11.25)			(1.70)	(0.60)
$\text{Alignment}_{i,t} \times \text{Aff Polar}_t \times$ Second Half				−0.33***				0.13
				(−4.23)				(0.88)
County Controls?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.2219	0.2220	0.2249	0.2251	0.0443	0.0445	0.0445	0.0446
N	57,013	57,013	57,013	57,013	57,013	57,013	57,013	57,013

TABLE VII – ECONOMIC ACTIVITY AND COMMUTING ZONE ALIGNMENT

The table reports estimates of Equation 7 at the commuting zone (CZ) level. Columns (1)–(3) use discretionary consumption growth and columns (4)–(6) use investment growth as dependent variables. Columns (1) and (4) include only Alignment (Equation 6); columns (2) and (5) add prior year per capita income, year-over-year growth in per capita income, and population growth; columns (3) and (6) add an interaction term between Alignment and the national level of affective polarization (Equation 5). Regressors are normalized to have mean zero and unit standard deviation. Coefficients are multiplied by 100. $t-$statistics are reported in parentheses. Standard errors are clustered at the CZ-level. ***,**,* indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Consumption Growth			Investment Growth		
	(1)	(2)	(3)	(4)	(5)	(6)
$\text{Alignment}_{i,t}$	0.35***	0.38***	0.28***	0.60***	0.56***	0.55***
	(7.09)	(7.77)	(5.68)	(6.29)	(5.87)	(5.60)
$\text{Income}_{i,t-1}$		−0.12	−0.28**		−0.41*	−0.43*
		(−0.98)	(−2.16)		(−1.66)	(−1.66)
$\text{Income Growth}_{i,t}$		0.86***	0.86***		1.25***	1.25***
		(11.62)	(11.55)		(7.75)	(7.75)
$\text{Population Growth}_{i,t}$		0.83***	0.85***		0.16	0.17
		(10.66)	(11.03)		(0.97)	(0.97)
$\text{Alignment}_{i,t} \times \text{Aff Polar}_t$			0.45***			0.03
			(8.51)			(0.29)
Year Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes
CZ Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.3520	0.3888	0.3935	0.0330	0.0428	0.0428
N	12,441	12,441	12,441	12,441	12,441	12,441

TABLE VIII – A BARTIK INSTRUMENT FOR ALIGNMENT

The table reports two-stage least squares estimates in which Alignment is instrumented with $\text{Alignment}_{i,t}^{2000}$, alignment computed fixing each county's partisan shares at its two-party vote share in the 2000 presidential election so that it varies over time only through the party of the sitting president. Because the 2000 vote shares predate the sample, the instrument is purged of endogenous changes in county partisan composition. Columns (1) and (3) report OLS estimates of Equation 7 on the instrument sample; columns (2) and (4) report 2SLS estimates. First Stage F is the Kleibergen-Paap rank Wald F statistic (Kleibergen and Paap (2006)). Because a county's residuals are correlated over time, we cluster standard errors by county, and the conventional weak-identification statistic, which assumes independently and identically distributed errors, no longer applies; the Kleibergen-Paap statistic remains valid under this form of dependence. Regressors are normalized to have mean zero and unit standard deviation. Coefficients are multiplied by 100. $t-$statistics are reported in parentheses. ***,**,* indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Consumption Growth		Investment Growth	
	OLS	2SLS	OLS	2SLS
	(1)	(2)	(3)	(4)
$\text{Alignment}_{i,t}$	0.46***	0.27***	0.47***	0.59***
	(12.75)	(6.17)	(8.01)	(8.26)
$\text{Income}_{i,t-1}$	0.18*	0.13	−0.12	−0.09
	(1.84)	(1.35)	(−0.81)	(−0.60)
$\text{Income Growth}_{i,t}$	0.84***	0.84***	1.84***	1.84***
	(19.10)	(19.00)	(21.14)	(21.17)
$\text{Population Growth}_{i,t}$	0.51***	0.51***	0.22**	0.22**
	(11.87)	(11.89)	(2.48)	(2.48)
First Stage F		9,614.1		9,614.1
Year Fixed Effects?	Yes	Yes	Yes	Yes
County Fixed Effects?	Yes	Yes	Yes	Yes
N	66,047	66,047	66,047	66,047

TABLE IX – CONTIGUOUS-COUNTY-PAIR ESTIMATES

The table reports estimates of Equation 12, in which, for every pair of adjacent counties, the within-pair difference in year-over-year growth is regressed on the within-pair difference in Alignment, pair fixed effects, and year fixed effects (Dube, Lester, and Reich (2010)). Columns (1)–(3) use discretionary consumption growth and columns (4)–(6) investment growth as dependent variables. Columns (2) and (5) restrict the sample to pairs that straddle a state border; columns (3) and (6) restrict the sample to pairs within the same state. $\Delta\text{Alignment}_{p,t}$ is scaled by the full-sample standard deviation of Alignment so that coefficients are directly comparable to Table III; control differences are normalized to have mean zero and unit standard deviation. The level of affective polarization is national and is absorbed by the year fixed effects; its interaction with $\Delta\text{Alignment}_{p,t}$ survives the within-pair differencing. Coefficients are multiplied by 100. $t-$statistics, based on standard errors two-way clustered on both counties in the pair, are reported in parentheses. ***,**,* indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Consumption Growth			Investment Growth		
	(1)	(2)	(3)	(4)	(5)	(6)
$\Delta\text{Alignment}_{p,t}$	0.41***	0.28***	0.43***	0.35***	0.26*	0.37***
	(8.73)	(2.59)	(8.86)	(4.44)	(1.65)	(4.24)
$\Delta\text{Income}_{p,t-1}$	−0.00	0.28	−0.06	−0.09	0.05	−0.12
	(−0.01)	(1.12)	(−0.61)	(−0.61)	(0.14)	(−0.79)
$\Delta\text{Income Growth}_{p,t}$	0.72***	0.84***	0.69***	2.37***	2.16***	2.41***
	(15.04)	(7.69)	(14.34)	(26.07)	(11.13)	(25.05)
$\Delta\text{Population Growth}_{p,t}$	0.26***	0.35***	0.24***	0.16	−0.08	0.20*
	(5.45)	(3.23)	(4.95)	(1.55)	(−0.42)	(1.88)
$\Delta\text{Alignment}_{p,t} \times \text{Aff Polar}_t$	0.53***	0.52***	0.53***	−0.06	−0.28	−0.02
	(11.02)	(4.58)	(10.51)	(−0.64)	(−1.42)	(−0.22)
Sample	All	Cross-state	Within-state	All	Cross-state	Within-state
Year Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes
Pair Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.0567	0.0588	0.0568	0.0366	0.0357	0.0377
N	191,599	26,673	164,926	191,599	26,673	164,926

TABLE X – ALIGNMENT AND SHIFT-SHARE PREDICTED INDUSTRY GROWTH

The table reports estimates of Equation 7 augmented with a shift-share (Bartik) control for industry-mix exposure to aggregate shocks (Equation 13). Industry shares are fixed at their 2001 values, the first year of the BEA county series. Predicted Industry Growth^{excl} excludes the consumer-facing industries (NAICS 44–45, 72) from the shares. Columns (1)–(3) use discretionary consumption growth and columns (4)–(6) investment growth as dependent variables. Regressors are normalized to have mean zero and unit standard deviation. Coefficients are multiplied by 100. $t-$statistics are reported in parentheses. Standard errors are clustered at the county-level. ***,**,* indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Consumption Growth			Investment Growth		
	(1)	(2)	(3)	(4)	(5)	(6)
Alignment$_{i,t}$	0.36***	0.36***	0.36***	0.45***	0.46***	0.46***
	(10.01)	(10.03)	(10.11)	(7.65)	(7.78)	(7.77)
Income$_{i,t-1}$	0.06	0.06	0.05	−0.14	−0.15	−0.15
	(0.64)	(0.64)	(0.56)	(−0.95)	(−1.05)	(−1.04)
Income Growth$_{i,t}$	0.84***	0.84***	0.86***	1.84***	1.87***	1.87***
	(19.11)	(18.82)	(19.16)	(21.13)	(20.87)	(20.91)
Population Growth$_{i,t}$	0.54***	0.54***	0.54***	0.22**	0.22**	0.22**
	(12.53)	(12.53)	(12.50)	(2.54)	(2.50)	(2.50)
Predicted Industry Growth$_{i,t}$		−0.00			−0.19*	
		(−0.11)			(−1.88)	
Predicted Industry Growth$^{excl}_{i,t}$			−0.11***			−0.19**
			(−2.67)			(−2.00)
Alignment$_{i,t}$ × Aff Polar$_t$	0.52***	0.52***	0.51***	0.10	0.09	0.08
	(13.39)	(13.37)	(13.16)	(1.50)	(1.30)	(1.24)
Year Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes
County Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.2043	0.2043	0.2044	0.0391	0.0392	0.0392
N	66,050	66,050	66,050	66,050	66,050	66,050

TABLE XI – PERMUTATION TEST OF POLITICAL ALIGNMENT AND ECONOMIC ACTIVITY

The table reports permutation tests of the relation between political alignment and county economic activity estimated in Table III, columns (2) and (5). In each of 1,000 permutations, we randomly reassign each county's entire time series of Alignment (Equation 6) to another county and re-estimate the column (2) and (5) specifications. Only alignment is reassigned: each county retains its own dependent variable and control variables, and reassigning complete alignment histories preserves the time-series structure of alignment, which changes only at presidential elections. The Actual columns report the estimated Alignment coefficients from Table III, with $t-$statistics in parentheses. Mean Alignment is the average of the Alignment coefficient across the 1,000 permutation regressions; Standard Deviation is the standard deviation of those 1,000 coefficients; and Permutations $\geq$ Actual Estimate is the number of the 1,000 permutation coefficients at least as large as the actual estimate. All specifications include prior year per capita income, income growth, population growth, and year and county fixed effects. Coefficients are multiplied by 100. Standard errors are clustered at the county-level. ***,**,* indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Consumption Growth		Investment Growth	
	Actual	Placebo	Actual	Placebo
$\text{Alignment}_{i,t}$	0.47***		0.47***	
	(12.79)		(8.01)	
Mean Alignment		−0.0013		−0.0009
Standard Deviation		0.0333		0.0564
Permutations $\geq$ Actual Estimate		0		0
Controls?	Yes	Yes	Yes	Yes
Year Fixed Effects?	Yes	Yes	Yes	Yes
County Fixed Effects?	Yes	Yes	Yes	Yes

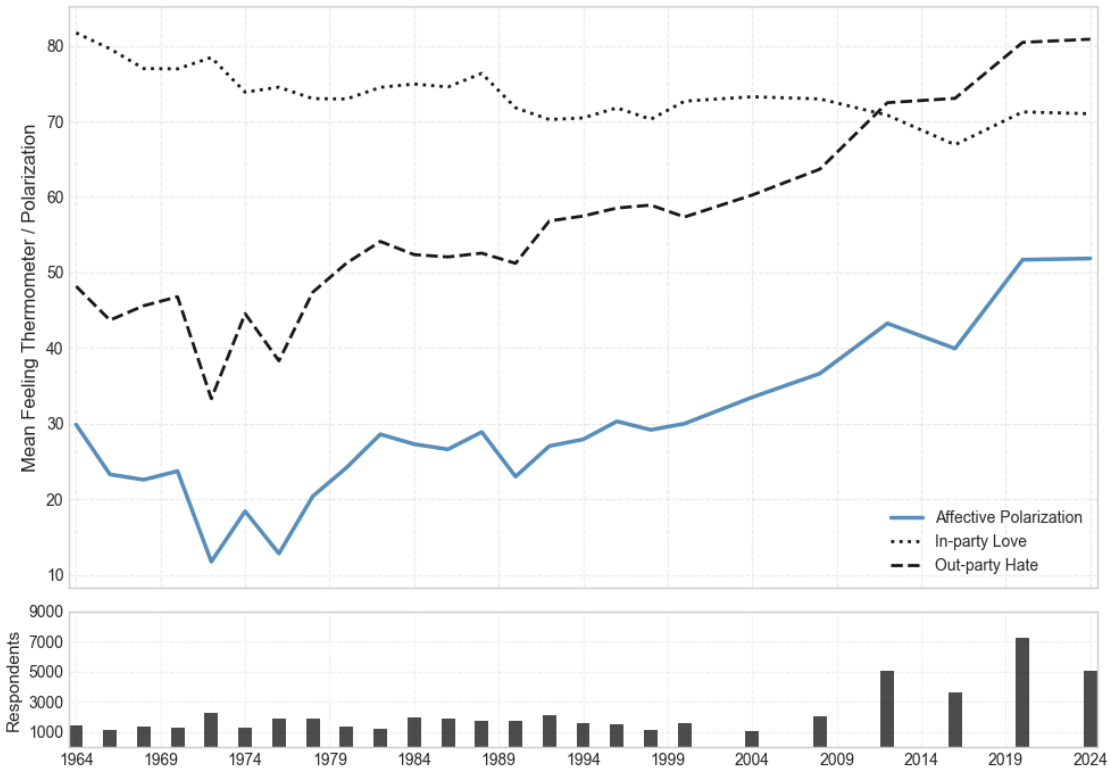
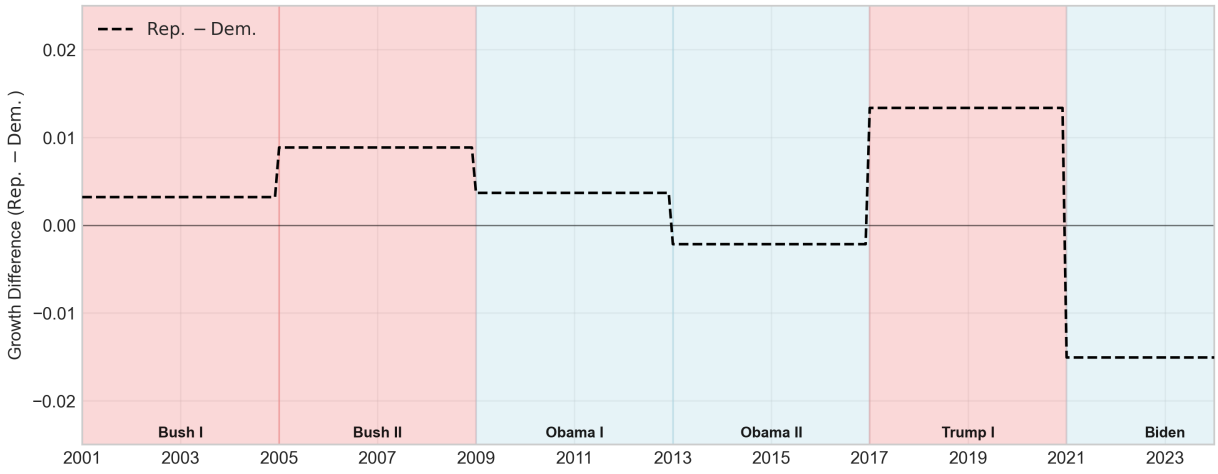


FIGURE 1 – AFFECTIVE POLARIZATION OVER TIME

The figure plots average yearly Affective Polarization (Equation 5) from 1964 to 2024. Affective polarization potentially ranges from -100 (individuals fully despise their own party and love the opposition party) to 100 (individuals fully despise the opposition party and love their own party). The subplot reports number of respondents per survey used to compute Affective Polarization.

Panel A: Consumption growth



Panel B: Investment growth

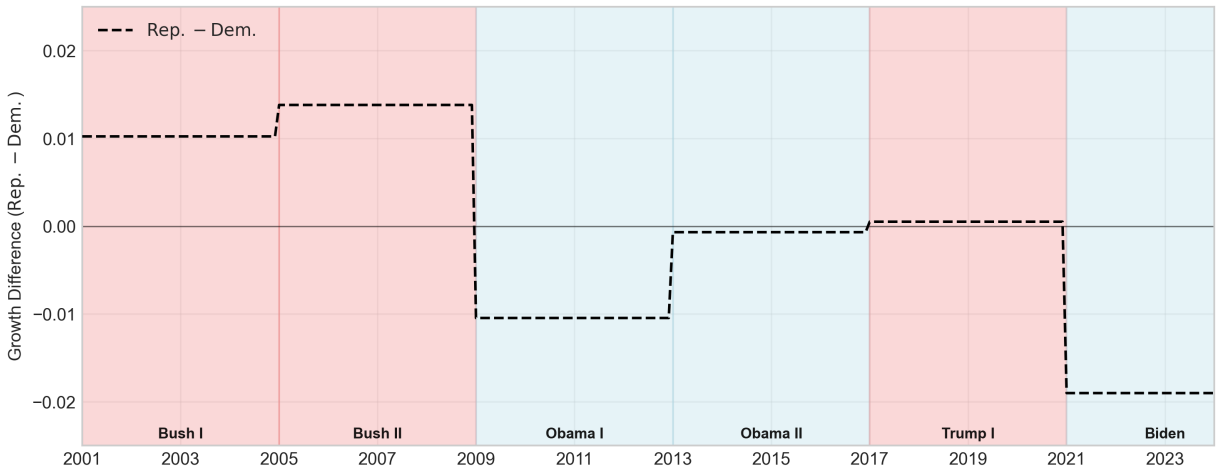
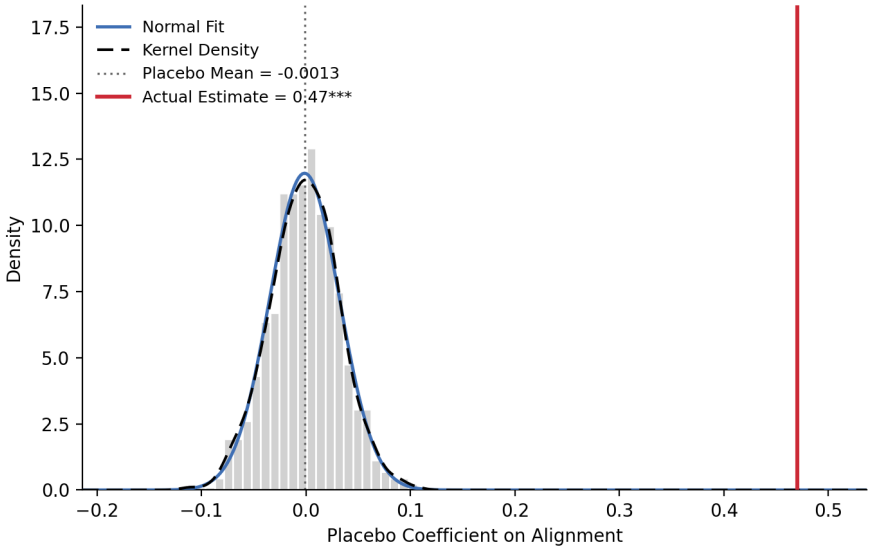


FIGURE 2 – DIFFERENCES IN ECONOMIC ACTIVITY

Differences in average year-over-year local economic activity for Republican and Democratic counties by presidential term. Panel A plots differences in consumption growth. Panel B plots differences in investment growth. Positive values indicate higher growth in Republican counties while negative values indicate higher growth in Democratic counties.

Panel A: Consumption Growth



Panel B: Investment Growth

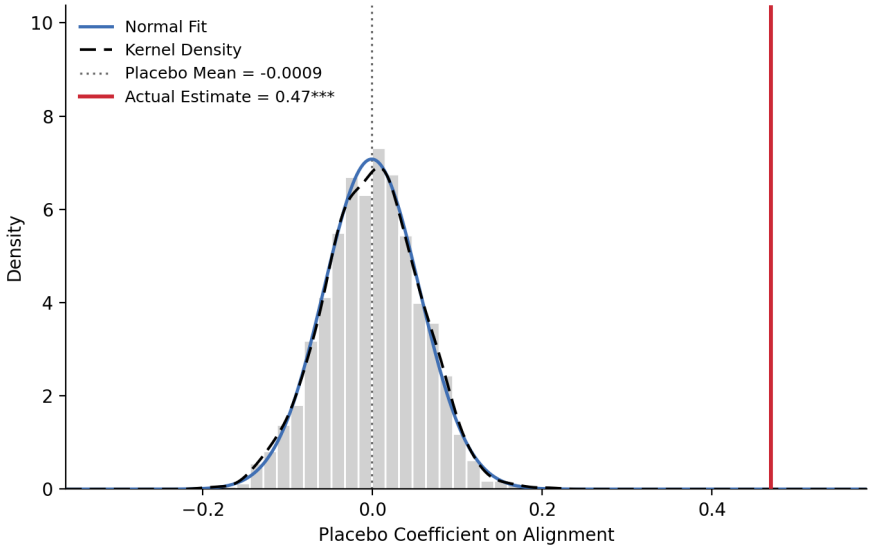


FIGURE 3 – PERMUTATION TEST OF POLITICAL ALIGNMENT AND ECONOMIC ACTIVITY
The figure plots the distribution of placebo Alignment coefficients from 1000 permutations in which each county's entire time series of Alignment (Equation 6) is randomly reassigned to another county and the Table III column (2) and (5) specifications are re-estimated. Only alignment is reassigned: each county retains its own dependent variable and control variables. Panel A plots the 1000 placebo coefficients for discretionary consumption growth and Panel B for investment growth, together with a normal fit, a kernel density, and the mean of the placebo distribution (dotted line). The red line marks the actual estimate from Table III. Coefficients are multiplied by 100.

Internet Appendix for "Political Polarization and Partisan Economic Cycles"

Contents

IA-1 Bureau of Economic Analysis Data A2
IA-1.1 County-level GDP A2
IA-1.2 County-level Personal Income A4
IA-1.3 Industry classifications A4
IA-2 IRS Data A5
IA-3 Additional Data A6
IA-3.1 Zillow A6
IA-3.2 Federal Reserve Enhanced Financial Accounts A7
IA-3.3 Federal government grants and contracts A7
IA-4 Elections as unanticipated shocks A8
IA-5 Employment growth and political alignment A9
IA-6 Discretionary GDP growth - alternative standard errors A10
IA-7 Discretionary GDP growth - anomalous years, county size, and state level controls . A10
IA-8 Discretionary GDP growth - the role of home prices and household leverage A11
IA-9 Discretionary GDP growth - the role of federal spending A12
IA-10 Aligned versus unaligned counties A12
IA-11 Within-term dynamics A13
IA-12 Commuting zones A14
IA-13 Core-based statistical areas A15

IA-1 Bureau of Economic Analysis Data

IA-1.1 County-level GDP

Our primary data source is the BEA file "CAGDP2 Gross domestic product (GDP) by county and metropolitan area", which provides county level GDP and a breakdown by industry components[42]. The BEA first officially released these statistics in December 2019, covering 2001 through 2018, and has extended them annually since.[43]

County level GDP is computed using the income approach, which sums the income payments and other costs incurred in producing goods and services within the county. GDP is more commonly expressed through the expenditure approach by the familiar formula $C+I+G+NX$, and because every dollar spent on a good or service becomes income to the factors that produced it, the two are representations of the same underlying economic activity. In practice, however, the two approaches do not coincide exactly: even at the national level, where income and expenditure are estimated independently, the two differ by a statistical discrepancy, which has averaged 0.8% of GDP since 1947 and has ranged from near zero to as much as 2.7%.[44] At the county level the comparison cannot be made at all, since expenditures are not independently observed at this geographic resolution. The BEA relies on the income approach because geographically detailed expenditure and production source data are limited, whereas wages (Quarterly Census of Employment and Wages; BLS), proprietors' income (IRS), and capital and tax components (Economic Census) can be constructed from direct observables. We therefore treat county GDP in consumer-facing industries as a proxy for local discretionary consumption rather than as a direct measure of household expenditure.

At the county level, the income approach implies:

$$GDP_{county,t} = COMP_{county,t} + TOPI_{county,t} - SUB_{county,t} + GOS_{county,t} \tag{IA-1}$$

Here $COMP$ is the sum of wages, salaries, and supplements to such (employer contributions for government social insurance and for employee pension and insurance funds). $TOPI$ less SUB is taxes on production and imports minus subsidies. It is included because production taxes are part of the income generated in the economy that accrues to government, whereas subsidies are transfers from government and therefore not income earned by the factors of production. Gross operating surplus (GOS) is the capital-income component of GDP, capturing the returns to capital owners rather than labor: consumption of fixed capital (depreciation), proprietors' income, corporate profits, rental income of persons, and net business current transfer payments (payments for which no current services are performed).

While the above can be thought of as the accounting identity that represents the income approach, given data availability this identity is operationalized by grouping all other income payments and costs, $OIC = TOPI - SUB + GOS - PI$ (where PI is proprietors' income), which then yields a formula in three quantities available at the county level:

$$GDP_{county,t} = COMP_{county,t} + PI_{county,t} + OIC_{county,t} \tag{IA-2}$$

The BEA applies this decomposition to every industry except farming, which it estimates using the

[42] https://www.bea.gov/data/gdp/gdp-county-metro-and-other-areas

[43] Aysheshim, Hinson, and Panek (2020), "A Primer on Local Area Gross Domestic Product Methodology," *Survey of Current Business* 100(3), https://apps.bea.gov/scb/issues/2020/03-march/0320-county-level-gdp.htm

[44] https://www.bls.gov/opub/mlr/2026/article/gdp-gdi-and-gdo-an-evaluation-of-output-measures-for-productivity-analysis.htm

production approach, and mining and government, which carry no proprietors' income component.[45]

$COMP$ and PI are built from county level source data rather than allocated down from state totals. County level compensation of employees is constructed primarily from the QCEW, a BLS program that compiles employer unemployment insurance filings by county and six-digit NAICS industry on a quarterly basis and that accounts for 94.2% of the wages and salaries the BEA estimates. Supplements to wages are derived from BEA estimates of wages and employment and from state unemployment insurance contribution records. Proprietors' income for nonfarm industries is measured using IRS tax data on the net profits of sole proprietorships and partnerships. Since compensation is the largest component of GDP for most counties, the bulk of the measure rests on these county level filings.

All other income payments and costs (OIC) represent the portion of income generated in production that accrues to corporations and other capital owners. OIC is estimated at the state level and then allocated across counties using company level information from the Economic Census, conducted every five years by the U.S. Census Bureau, which is the source for 49 of the 65 detailed industries.[46] For each industry, a county is assigned the same share of state OIC as its share of state value added (for goods producing industries) or of state receipts (for services producing industries), where both series are first rescaled by the ratio of BEA compensation of employees to Economic Census payroll in order to adjust for coverage differences between the Economic Census and the QCEW data underlying the compensation estimates.[47] Because the Economic Census is quinquennial, National Establishment Time Series (NETS) sales data are used to interpolate the allocator between benchmark years.[48]

For a particular industry, the county level Economic Census measure is available in benchmark years, and in those years the corresponding indicator is also available in NETS, although the two measures are not expected to be equal. The BEA then considers the ratio:

$$r_t = \frac{S_t}{EC_t}$$

where S_t is the county level sales measure from NETS and EC_t is the measure from the Economic Census. When EC_{t+5} is observed five years later, the annual geometric factor $\Delta_{t,t+5} = \sqrt[5]{r_{t+5}/r_t}$, one plus the average annual growth rate in the ratio, allows the intervening years to be filled in:

$$EC_{t+1} = \frac{S_{t+1}}{\Delta_{t,t+5} \cdot r_t}$$

In the numerator is the observed value from NETS in year $t+1$, which is then scaled so that the interpolated series preserves the growth implied between the two Census benchmarks and reproduces the observed Economic Census levels in years t and $t+5$. In extrapolation (years following the most recent Economic Census but preceding the next), setting Δ to 1 holds the ratio between

[45] "The farm industry is not estimated as the sum of *COMP*, *PI*, and *OIC*. For the farm industry, GDP or value added by county is estimated directly using the production approach, in which GDP is measured as gross output minus intermediate inputs" (Aysheshim, Hinson, and Panek 2020, p. 7). Farm proprietors' income is drawn from USDA rather than IRS data and accounted for approximately 2.3% of proprietors' income in 2018.

[46] The remaining industries rely on industry-specific sources, including EIA generation data for utilities, FDIC branch deposits for banking, and BTS traffic data for air transportation.

[47] The rescaling "ensure[s] consistency between the BEA income components and the Economic Census data, which are derived from different sample sets" (Aysheshim, Hinson, and Panek 2020, p. 12).

[48] NETS is an establishment level database from Dun & Bradstreet covering over 59 million establishments.

the Economic Census measure and NETS sales constant.

To summarize, state level *OIC* is projected to counties using each county's share of state economic activity within an industry, with the quinquennial Economic Census setting the benchmarks and NETS data interpolating between them in a manner that preserves the levels *and* the growth rates the Census implies. The published county files report GDP by industry rather than by income component, so this allocation is not separately observable in the data we use.[49]

IA-1.2 County-level Personal Income

We employ controls for county level personal income from the Bureau of Economic Analysis (BEA) CAINC1 file, *County and MSA Personal Income Summary.* Personal income is a residence based measure that captures the income received by county residents from all sources, regardless of where that income is earned. The BEA defines personal income as the sum of wages and salaries, supplements to wages and salaries (including employer contributions for pensions and health insurance), proprietors' income, dividends, interest, and rental income, and personal current transfer receipts, net of contributions for government social insurance. This measure is distinct from county level GDP, which is production based, as personal income excludes corporate retained earnings and depreciation while incorporating government transfers and income flows to households.[50]

County personal income estimates are constructed primarily from administrative records, with the largest component, wages and salaries, drawn from state unemployment insurance (UI) programs via the Quarterly Census of Employment and Wages (QCEW). Additional components are assembled from federal administrative sources, including Internal Revenue Service tax records and Social Security Administration data, which are used to measure proprietors' income, asset income, and government transfer payments such as retirement, disability, and medical benefits. Because several income components, particularly wages and salaries, are initially recorded on a place of work basis, the BEA applies a residence adjustment that reallocates labor income across counties to account for inter-county commuting flows. As a result, county personal income reflects the income available to residents where they live rather than where production occurs, making it an appropriate control for local purchasing power. From the same file, we also retain county-level population estimates, which are derived from U.S. Census Bureau population controls.

IA-1.3 Industry classifications

The BEA reports county-level GDP data by industry using two-digit NAICS codes. Because our interest is in whether political polarization affects local discretionary consumption, we restrict our sample to non-tradable consumer-facing industries. We keep 'Retail trade' (NAICS codes 44 and 45) and 'Accommodation and food services' (NAICS code 72). All NAICS industries are reported in Table IA-I.

[Insert Table IA-I about here.]

Table IA-II reports all sub-industries within the two-digit NAICS industries used in our main analysis along with the total number of businesses entities in those industries and the two-digit industry contribution to GDP. As of 2025, our collection of consumer-facing industries collectively

[49] https://www.bea.gov/data/gdp/gdp-county-metro-and-other-areas

[50] https://www.bea.gov/sites/default/files/methodologies/BEA-Local-Area-Personal-Income-and-Employment-Concepts-and-Methods.pdf

account for approximately 15% of all US business entities and accounted for approximately 10% of total 2024 fiscal year GDP.

[Insert Table IA-II about here.]

IA-2 IRS Data

We measure county-level dividend income using the Internal Revenue Service's Statistics of Income (SOI) county data for tax years 2000 through 2023.[51] These are population tabulations rather than samples: the IRS aggregates administrative records of individual income tax returns (Form 1040) from its Individual Master File to the county level. Counties are assigned from the address reported on the return, so the data reflect the county in which filers report residing.[52] The universe is the fifty states and the District of Columbia; returns filed from Army and Fleet Post Office addresses, foreign addresses, Puerto Rico, and the U.S. territories are excluded, and state total rows (county FIPS code 000) are dropped from our panel. All dollar amounts are reported in thousands of nominal dollars.[53]

The definition of the filing population changes once during our sample. Through tax year 2009, the tabulations include returns processed through the thirty-ninth week of the IRS processing year ("cycle 39," in late September), which the IRS documents as covering 95 to 98 percent of all returns filed for a given tax year. Beginning with tax year 2010, the tabulations instead include all returns filed during the twelve-month calendar year following the tax year, together with a limited number of prior-year returns to proxy for late filers.[54] The IRS also cautions that tax data do not represent the full population, as individuals below the filing threshold need not file a return.

Our variable of interest is total dividend income reported by county. For tax years 2000 through 2009, the SOI files report a single dividend series, "Dividends before exclusion," defined as taxable distributions of money, stock, or other property received from domestic and foreign corporations, excluding non-taxable distributions and distributions treated as interest income. From tax year 2010 onward, the files report two dividend series, Ordinary Dividends and Qualified Dividends. Qualified dividends are a subset of ordinary dividends rather than a separate category: they are the portion of ordinary dividends that meets holding-period and other conditions and therefore receives preferential tax rates.[55,56] We therefore use ordinary dividends alone for 2010 onward, which

[51] https://www.irs.gov/statistics/soi-tax-stats-county-data

[52] From tax year 2010 onward the IRS assigns state and county using the ZIP code shown on the return and excludes returns filed without a ZIP code or with a ZIP code inconsistent with the reported state. The IRS notes that the address on a return may differ from the filer's actual residence.

[53] The IRS notes that state totals computed from the county data may not match state totals it publishes elsewhere, both because of the disclosure procedures described below and because returns whose ZIP code cannot be matched to a state are excluded from the county product.

[54] The IRS publishes no coverage percentage for the post-2009 files. Because our tests use year-over-year growth within county and include year fixed effects, a change in the filing window common to all counties does not affect our estimates.

[55] On Form 1099-DIV, box 1b (qualified dividends) reports the portion of box 1a (total ordinary dividends) eligible for reduced rates, so qualified dividends cannot exceed ordinary dividends. On Form 1040, the two appear as lines 9a and 9b through tax year 2017 and as lines 3b and 3a following the 2018 redesign of the form. See IRS Instructions for Form 1099-DIV.

[56] Preferential rates on qualified dividends were introduced for tax years beginning after 2002. To the extent that this change affected corporate payout policy, it affected all counties in the same year and is absorbed by our year fixed effects.

corresponds to the pre-2010 "dividends before exclusion" concept and avoids double counting; summing the two series would count qualified dividends twice. In the data, qualified dividends do not exceed ordinary dividends in 99.93% of county-years, with the remaining cases reflecting rounding in very small counties.

The file format changes at the same point in the sample. For 2000 through 2009 the IRS distributes one Excel workbook per state within an annual archive, and we read dividend income by column position, verifying each state file against its published state total. For 2010 onward the IRS distributes a single national file per year in which each income item appears as a pair of columns, the number of returns reporting the item followed by the dollar amount; we take the dollar amount.[57]

The IRS suppresses county cells that could disclose individual taxpayers. Through 2009, counties with fewer than ten returns are suppressed and their values folded into a successor county; from 2010 onward, items with fewer than twenty returns are excluded, and an undisclosed dominance rule excludes items in which a single return accounts for too large a share of the cell. In practice this affects three counties with negligible population: Kalawao County, Hawaii (FIPS 15005), Kenedy County, Texas (FIPS 48261), and Loving County, Texas (FIPS 48301). Rather than impute these values, we leave them missing; they account for less than 0.04% of the panel. The SOI data covers approximately 3,140 counties per year, and our estimation sample retains the 3,058 of the 3,063 counties for which we also observe presidential vote shares and controls.[58]

Prior work uses these tabulations to locate financial wealth geographically. Mian, Rao, and Sufi (2013) allocate national household asset totals across zip codes in proportion to SOI dividend and interest income, and Chodorow-Reich, Nenov, and Simsek (2021) capitalize county-level SOI dividend income to construct county stock market wealth. We use the same underlying series but measure changes rather than levels: as described in Section 3.1, we compute year-over-year growth in county dividend income, winsorize at the 5% and 95% levels, and subtract the yearly cross-sectional mean to obtain our investment measure.[59]

IA-3 Additional Data

IA-3.1 Zillow

Three additional data sources appear in our robustness tests. The first is county-level home prices from Zillow's home value index, which we use to control for local house price movements in Table IA-IX.[60] We deflate the annual series to constant dollars using the same price index applied to our other dollar variables and compute year-over-year growth for counties observed in consecutive years, winsorizing at the 5% and 95% levels. Zillow's county coverage widens considerably over our sample,

[57] Three file-level irregularities in the source archives require correction. The 2001 Kentucky workbook contains one fewer leading blank row than every other file, shifting its header row; Pulaski County, Georgia (FIPS 13235) appears in both the Georgia and Missouri workbooks in 2003; and the 2008 archive contains a duplicate Delaware workbook. We correct the header row, deduplicate by county FIPS code, and drop the duplicate file, respectively.

[58] County definitions change slightly over the sample as the Census Bureau revises boundaries and county-equivalent codes; we compute growth only from consecutive-year observations sharing a county FIPS code, so county-years affected by a code change enter as missing rather than as growth.

[59] We use the county files that aggregate over all returns rather than the tabulations by adjusted gross income class. Because dividend income is a county aggregate, growth in it partly reflects growth in the number of filers; our specifications control for county population growth and income growth, and the cross-sectional demeaning removes any change in filing behavior common to all counties in a year.

[60] https://www.zillow.com/research/data/. Zillow reports the index monthly for each county; we average the twelve monthly observations within each calendar year to obtain an annual series.

from roughly 1,100 counties in 2002 to roughly 2,950 by 2023, so the home price specifications are estimated on 45,825 county-years covering 2,946 counties rather than on the full panel.[61]

IA-3.2 Federal Reserve Enhanced Financial Accounts

The second is county-level household leverage from the Federal Reserve's Enhanced Financial Accounts, which publishes household debt-to-income ratios built from the Federal Reserve Bank of New York Consumer Credit Panel, an anonymized five percent random sample of individuals with Equifax credit files together with their household members (Lee and Van der Klaauw (2010)).[62] The numerator is total household debt excluding student loans; the denominator is annual income of workers covered by state unemployment insurance programs, reported by the Bureau of Labor Statistics.[63] The public file reports five fields: year, quarter, county FIPS, and the lower and upper bound of the county's debt-to-income interval. Precise values are not released, and each county-quarter is instead assigned to one of ten intervals bounded at 0, 0.78, 1.01, 1.19, 1.37, 1.58, 1.82, 2.15, 2.61, and 3.43. These cutoffs are deciles of the pooled distribution and are fixed in every year of the release, so a county moving across intervals reflects a change in its absolute leverage rather than in its rank. We take the midpoint of the reported interval, assign the open top interval its lower bound of 3.43, and average the four quarters to obtain an annual ratio, which we lag one year and use as a control in Table IA-X. The coding is coarse: two counties with debt-to-income ratios of 1.40 and 1.57 receive the same value. The measure nonetheless varies meaningfully both across counties and within county over time.

IA-3.3 Federal government grants and contracts

The third is county-level federal spending from USAspending, the government-wide award database mandated by the Federal Funding Accountability and Transparency Act, which records every federal contract and grant with its place of performance.[64] We aggregate obligations to the county-fiscal year level, summing contracts and grants into a single measure of federal spending, and map fiscal year t, which runs from October of $t-1$ through September of t, to calendar year t. We exclude direct payments to individuals, which are transfers already contained in BEA county personal income and therefore in our income controls, and loans, whose face value does not measure resources delivered.[65] We likewise exclude direct payments to businesses and federal farm subsidies, as both enter county personal income through proprietors' income, and federal insurance and other miscellaneous assistance, whose recorded amounts represent the value of coverage extended rather than

[61] Coverage is thinnest for small rural counties, where few transactions are available in the early years of the sample. Because the home price regressions include county and year fixed effects, this changing composition affects which counties contribute to identification but not the interpretation of the alignment coefficient, which we report alongside the baseline estimate on the same restricted sample for comparison.

[62] Data and documentation are available at https://www.federalreserve.gov/releases/z1/dataviz/household_debt/county/map/. The construction is described in Ahn, Batty, and Meisenzahl (2018).

[63] Debt covers first mortgages, home equity installment and revolving loans, auto loans, bankcard, consumer finance, retail trades, and other trades. Student loans are excluded because they are not consistently reported before 2006.

[64] County-level coverage begins in fiscal year 2008. Contracts comprise award types A through D (blanket purchase agreement calls, purchase orders, delivery orders, and definitive contracts); grants comprise assistance types 02 through 05 (block grants, formula grants, project grants, and cooperative agreements). Obligations record when an award is made rather than when funds are disbursed.

[65] Direct payments include Social Security, Medicare, and unemployment benefits. The BEA's methodology confirms that county personal income includes personal current transfer receipts, so retaining these categories in our federal spending measure would double count them; see BEA, County Personal Income: Concepts and Methods.

funds delivered.[66,67] What remains is the discretionary federal money whose geographic allocation an administration could plausibly steer.

IA-4 Elections as unanticipated shocks

It is well documented that political affiliation and polarization have substantial explanatory power over individuals' expectations about the future state of the economy. However, most studies that document these relationships rely on infrequently collected survey data, which makes it difficult to determine the timing of expectation updates. Specifically, it remains unclear from these studies whether individuals adjust their beliefs immediately following election outcomes or gradually over time (e.g., in anticipation of the outcome).

To examine the dynamics of economic expectations around elections, we replicate findings in Angrisani, Sias, Sobotka, and Turtle (2025) using daily data from Civiqs, an online polling and analytics company.[68,69] Specifically, Civiqs asks respondents, "Do you think the nation's economy is getting better or worse?" and respondents select getting better, staying about the same, getting worse, or unsure. We compute the difference between Republicans' and Democrats' net favorable views of future economic conditions each day as,

$$\begin{aligned}\text{Diff Favorable Future Views}_t = \\ &\big(\%\text{Getting Better}_{Rep,t} - \%\text{Getting Worse}_{Rep,t}\big) \\ &- \big(\%\text{Getting Better}_{Dem,t} - \%\text{Getting Worse}_{Dem,t}\big)\end{aligned} \tag{IA-3}$$

where the Rep, t (Dem, t) subscript identifies the fraction of Republicans (Democrats). Thus, the potential range for the difference in net favorable views, as defined in Equation IA-3, is from -2 (e.g., 100% of Republicans believe things are getting worse and 100% of Democrats believe things are getting better) to 2, where positive values indicate Republicans are more optimistic about the future than Democrats.

Civiqs also ask respondents, "How would you rate the condition of the national economy right now?" and respondents select between very good, fairly good, unsure, fairly bad, and very bad. We compute the difference between Republicans' and Democrats' net favorable views of current economic conditions each day as,

$$\begin{aligned}\text{Diff Favorable Current Views}_t = \\ &\big(\%\text{Very Good}_{\text{Rep},t} + \%\text{Fairly Good}_{\text{Rep},t} - \%\text{Fairly Bad}_{\text{Rep},t} - \%\text{Very Bad}_{\text{Rep},t}\big) \\ &- \big(\%\text{Very Good}_{\text{Dem},t} + \%\text{Fairly Good}_{\text{Dem},t} - \%\text{Fairly Bad}_{\text{Dem},t} - \%\text{Very Bad}_{\text{Dem},t}\big)\end{aligned} \tag{IA-4}$$

Panel A of Figure IA-1 reports the daily difference in Republicans' and Democrats' views of

[66]The BEA's methodology details that gross farm receipts, and thus farm proprietors' income, include payments to farmers under federal farm subsidy programs.

[67]Insurance obligations record the insured amount under programs such as federal crop insurance, not federal spending, and are therefore not commensurable with contract and grant obligations.

[68]Civiqs surveys approximately 200-300 individuals a day and uses a Bayesian dynamic linear model to create a daily tracking value that updates the previous day's value based on the demographics of that day's respondents to ensure the index captures day to day shifts in the views of a representative sample of Americans. Civiqs data is available for the 3,784 days between January 15, 2015 and August 23, 2025, which reflects approximately 1.1 million surveys (284/day).

[69]We thank Drew Linzer for his help with the Civiqs data. As Civiqs points out, they own and run their own panel to ensure "that every respondent has verified contact information, every response is legitimate, and that the demographic, geographic, and political characteristics of every respondent can be trusted for accuracy." Details of the Civiqs panel and methodology are available at https://civiqs.com/content/methodology.

expected changes in the economy (i.e., Equation IA-3). The results reveal a dramatic shift in relative expectations around both election dates (identified by the change in background color) and inauguration dates (identified by the vertical dashed lines). Consistent with extant literature, individuals become significantly more optimistic about the future state of the economy when the sitting president belongs to "their" party. Importantly, we find that individuals seem to be surprised by election outcomes—that is, their perceptions of the future state of the economy do not adjust in anticipation of election outcomes but instead shift dramatically upon the election announcement and then again on the inauguration date.

[Insert Figure IA-1 about here]

Panel B of Figure IA-1 reports the difference in Republicans' and Democrats' view of the current state of the economy (i.e., Equation IA-4). Although the pattern is similar to Panel A, the effects on perceptions of current conditions are smoother. For example, Republicans remain more bearish than Democrats about *current* economic conditions between Trump's second election and his second inauguration (while Biden is the lame duck president). This contrast supports the interpretation that Panel A primarily reflects expectation-driven responses, rather than changes in individuals' experienced current economic conditions.

Overall, Figure IA-1 provides evidence that election outcomes constitute plausibly exogenous shocks to economic expectations: individuals do not gradually adjust their expectations in anticipation of the results, but instead update sharply upon learning the election outcome and then again when the newly elected president takes office. Moreover, perceptions of the current state of the economy do not adjust contemporaneously with expectations, reinforcing the interpretation that Panel A reflects belief updating rather than immediate changes in lived economic experiences.[70]

IA-5 Employment growth and political alignment

As noted, prior work has used Quarterly Census of Employment and Wages (QCEW) employment data as a proxy for local consumption. Though our BEA data offers a more complete picture of local consumption and is thus our preferred measure, we reestimate our main consumption-alignment results here using the QCEW data as robustness. The QCEW reports county-level employment data (number of jobs) by NAICS code quarterly. These data are compiled from state level UI programs which cover about 97% of US workers.[71] Keeping with our main analysis, we keep observations for NAICS codes in 44–45 and 72. We aggregate employment in these industries and use observations from the third month of the third quarter to compute year-over-year percent changes in employment in consumer-facing industries.[72] Our variable of interest is

$$\text{Employment Growth}_{i,t} = \frac{\text{Employment}_{i,t}}{\text{Employment}_{i,t-1}} - 1 \tag{IA-5}$$

[70] As additional evidence that election outcomes come as shocks to individuals, Coibion, Gorodnichenko, and Weber (2020) find that regardless of actual odds, participants from both parties are confident their candidate will win. Specifically, in the 2020 election, 87% of Democrats expected a Biden victory while 84% of Republicans predicted a Trump victory. If this pattern holds generally, then losing an election comes as a surprise to both Democrats and Republicans. Further, as detailed by Meeuwis, Parker, Schoar, and Simester (2022), the 2016 election result was inconsistent with objective expectations.

[71] https://www.bls.gov/opub/hom/cew/concepts.htm

[72] We use the third quarter to avoid any potentially problematic seasonality patterns associated with, for example, temporary workers during the Christmas season. Our results are qualitatively and quantitatively similar using employment data in the fourth quarter.

where Employment$_{i,t}$ is the total number of jobs in consumer-facing industries in county i in year t. Employment Growth$_{i,t}$ is winsorized at the 5% and 95% levels. Our data runs from 2001 to 2023 and covers all 3,063 counties in our main BEA sample. Table IA-III reports summary statistics for our employment growth variable.

[Insert Table IA-III about here.]

Table IA-IV shows that our main consumption-alignment results are consistent using the QCEW data—changes in alignment have an economically and statistically significant effect on local consumption growth and this effect is amplified by the level of national affective polarization.

[Insert Table IA-IV about here.]

IA-6 Discretionary GDP growth - alternative standard errors

Our baseline standard errors are clustered by county, which permits arbitrary correlation within a county over time but treats two counties observed in the same year as independent. That assumption is strong in our setting. Alignment is a predetermined county vote share interacted with a single national variable, the party of the president, so counties with similar partisan composition receive nearly identical treatment paths and their residuals are correlated across counties within a year. We therefore reestimate Equation 7 clustering by state, and clustering two-way by county and year following Cameron, Gelbach, and Miller (2011), which additionally permits arbitrary correlation across all counties within a year and is therefore robust to the common national component of alignment.

Table IA-V reports the results. The specification and the point estimates are identical across columns; only the estimated standard errors change. Clustering by state has a modest effect, reducing the t-statistic on alignment from 12.79 to 6.48 for consumption growth and from 8.01 to 4.73 for investment growth, because counties in different states within the same year are still treated as independent. Two-way clustering matters considerably more, reducing the t-statistics to 2.31 and 2.68 respectively. Both remain significant at the 5% level.

[Insert Table IA-V about here.]

IA-7 Discretionary GDP growth - anomalous years, county size, and state level controls

Our sample includes both the Great Financial Crisis (GFC) as well as the COVID-19 pandemic. It is possible that these events impacted highly Democratic counties differentially from highly Republican counties. To test whether these events are driving our results, we reestimate Equation 7 on a sample that excludes observations from 2008-2009 and from 2020-2021. Table IA-VI reports the results. Overall, our results are consistent with those generated from the full sample, indicating that our baseline results are not driven by the GFC or by COVID-19. Note that this filter also excludes discretionary GDP growth in the first year of each of the presidential party switches in our sample (our first observation of GDP growth is from 2001 to 2002) and thus demonstrates that our results are not wholly driven by an initial change in spending following a switch in the party of the president.

[Insert Table IA-VI about here.]

Another potential worry is that our results are driven by a handful of super-large or super-small counties. To test whether this is the case, we reestimate Equation 7 on a panel of data that excludes counties above the 90^{th} percentile and below the 10^{th} percentile of population. This process excludes 576 of our 3,058 counties. Table IA-VII reveals that our results are not significantly affected by this filter.

[Insert Table IA-VII about here.]

A final concern is that state-level economic policies, which could be correlated with partisan composition, rather than the behavior of partisans themselves, drive our results. Such policies are set by legislatures whose partisanship may mirror that of their counties, and potentially move with the federal political cycle: fuel taxes, tax cuts following changes in party control, and the differing state responses to the COVID-19 pandemic.[73] We therefore reestimate Equation 7 replacing our year fixed effects with state by year fixed effects, which absorb any shock common to a state in a given year. Identification then comes only from differences in partisan composition between counties in the same state and year, which mechanically attenuates our estimates because partisanship is spatially clustered. Table IA-VIII reveals that our results are nonetheless not significantly affected: the alignment coefficient falls from 0.47 to 0.38 for consumption growth and is essentially unchanged at 0.48 for investment growth, and both remain significant at the 1% level.

[Insert Table IA-VIII about here.]

IA-8 Discretionary GDP growth - the role of home prices and household leverage

Local consumption is known to respond strongly to house price movements and to household balance sheet conditions (e.g., Mian, Rao, and Sufi (2013); Guren, McKay, Nakamura, and Steinsson (2021)). Because partisan composition is correlated with the geography of the housing cycle, a natural concern is that our alignment effect reflects these determinants rather than partisan behavior. We address this by adding each in turn as a control.

Table IA-IX adds year-over-year county home price growth, constructed as described in Section IA-3. Column (1) reports the baseline specification on the subsample of counties covered by Zillow, where the alignment coefficient is 0.66 ($t = 16.07$); column (2) adds home price growth and leaves it essentially unchanged at 0.67 ($t = 16.39$), even though home price growth is itself a strong predictor of consumption growth (0.52, $t = 13.52$). Column (3) additionally excludes the crisis and pandemic years, when house prices moved most violently, and the alignment effect remains highly significant. Results for investment growth in columns (4) through (6) are likewise unaffected.[74]

[Insert Table IA-IX about here.]

Table IA-X adds two further controls for consumption growth. The first is a stock market wealth shock, measured as the county's lagged dividend income as a share of personal income multiplied

[73] State fuel taxes and refinery access are the principal sources of geographic variation in retail gasoline prices. State fiscal policy is a particular concern because tax and spending changes are enacted by legislatures whose partisanship is correlated with the partisan composition of the counties within them.

[74] The alignment coefficient is larger on the Zillow sample than in our full-sample baseline because Zillow coverage is concentrated in larger counties and in the later, more polarized years of our sample. The relevant comparison is between columns (1) and (2), which hold the sample fixed and differ only in whether home price growth is controlled for.

by the annual market return, which captures the passive revaluation of the county's existing equity holdings as distinct from the active purchases our investment measure is designed to capture. The second is the county's lagged household debt-to-income ratio, described in Section IA-3. The alignment coefficient is 0.47 ($t = 12.79$) in the baseline and remains 0.47 in every specification, whether we add the wealth shock, lagged leverage, or both. Leverage itself enters significantly and with the expected negative sign (-0.26, $t = -3.22$), so the invariance of the alignment coefficient is not simply the result of adding a control with no explanatory power. The investment columns behave the same way: the alignment coefficient is 0.47 ($t = 8.01$) in the baseline and remains 0.47 once lagged leverage is added, though leverage itself is insignificant for investment growth (0.16, $t = 1.23$).

[Insert Table IA-X about here.]

IA-9 Discretionary GDP growth - the role of federal spending

Federal spending is a direct determinant of local economic activity: a dollar of federal money spent in a county raises local income by substantially more than a dollar.[75] Its allocation is also correlated with partisan alignment, since counties represented by legislators in the president's party receive systematically more federal outlays.[76] A coincident increase in alignment and local economic activity could therefore reflect federal money arriving in newly aligned counties rather than any change in the behavior of the partisans who live there. To test this directly, we control for the growth of federal money flowing into each county, measured from the USAspending obligations described in Section IA-3: year-over-year growth in a county's total real federal contract and grant obligations, winsorized at the 5% and 95% levels. Because the obligations data begin in fiscal year 2008, the sample runs from 2009 to 2023.

Table IA-XI reports the results. Columns (1) and (3) reestimate our baseline on this sample, where the alignment effect is larger than in the full sample, at 0.60 ($t = 12.44$) for consumption growth and 0.44 ($t = 5.96$) for investment growth, consistent with the amplification of partisan responses as polarization rose over the later years of our sample. Columns (2) and (4) add federal spending growth. The control enters positively and significantly for consumption growth (0.09, $t = 2.21$), confirming that federal money does reach local economic activity, but the alignment coefficient is unchanged. If federal spending directed toward aligned counties generated our results, controlling for these flows would attenuate the alignment effect; instead it is unaffected. Together with the timing evidence in Section 4, where the response appears before policy could plausibly have changed local conditions, this indicates that the alignment effect does not operate through the geographic allocation of federal spending.

[Insert Table IA-XI about here.]

IA-10 Aligned versus unaligned counties

Section 4 asks whether Republican and Democratic counties respond equally to alignment. A second dimension concerns whether the degree of partisan composition matters equally for counties

[75] Suárez Serrato and Wingender (2016) use variation in federal program allocations driven by Census population counts to estimate the local effect of federal spending, finding "a local income multiplier of government spending between 1.7 and 2," so that a dollar of federal spending in a county raises local income by roughly two dollars. Chodorow-Reich (2019) surveys the geographic cross-sectional literature and reaches a similar benchmark.

[76] Berry, Burden, and Howell (2010) find that "the federal government spends approximately 4–5% more in districts and counties when members of the president's party represent them."

aligned with the sitting president and for those that are unaligned. To test this, we decompose Alignment separately for aligned and unaligned county-years,

$$\text{Alignment}_{i,t} = \text{Aligned}_{i,t} \times \text{Alignment}_{i,t} + \text{Unaligned}_{i,t} \times \text{Alignment}_{i,t} \tag{IA-6}$$

where $\text{Aligned}_{i,t}$ is defined in Equation 8 and $\text{Unaligned}_{i,t} = 1 - \text{Aligned}_{i,t}$, and we estimate the resulting specification with our standard county controls and county and year fixed effects.
Table IA-XII reports the results. For consumption growth, the intensive margin is large and highly significant among unaligned counties (0.98, $t = 8.26$) but indistinguishable from zero among aligned counties (-0.04, $t = -0.33$), and the difference between the two is significant (-1.01, $t = -4.58$). Column (2) shows that rising affective polarization amplifies the response of unaligned counties more strongly than that of aligned counties (difference of -0.30, $t = -2.91$). For investment growth the pattern reverses: the intensive margin is significant only among aligned counties (0.67, $t = 3.70$), although we cannot reject that the two coefficients are equal (0.41, $t = 1.15$). The consumption response to the degree of partisan composition is therefore concentrated among counties that have become unaligned with the sitting president, while the investment response is concentrated among those that have become aligned.

[Insert Table IA-XII about here.]

IA-11 Within-term dynamics

Our analysis of within-term dynamics in Section 4 turns on the fact that the president's party generally loses ground at the midterm. Table IA-XIII reports which chambers of Congress the president's party controlled before and after the midterm election of each presidential term in our sample. In every term except Bush's first, the president's party lost control of at least one chamber, reducing its capacity to enact the policies aligned partisans anticipate.

[Insert Table IA-XIII about here.]

In Table VI, we provide evidence that the partisan consumption gap persists over the entire presidency by estimating the effect of Alignment on consumption growth separately for the first and second halves of the presidency. Here, we estimate the effect of Alignment on consumption growth separately for each year of the presidency by interacting Alignment with indicators for years two through four, with the first year serving as the omitted category. We additionally interact Affective Polarization with a Term Year variable to test if the effect of polarization differentially impacts consumption as the presidency evolves. Our Term Year variable is constructed as,

$$\text{Term Year}_t = \begin{cases} 0 & \text{if year } t \text{ is the first year of a presidency} \\ 1 & \text{if year } t \text{ is the second year of a presidency} \\ 2 & \text{if year } t \text{ is the third year of a presidency} \\ 3 & \text{if year } t \text{ is the fourth year of a presidency} \end{cases} \tag{IA-7}$$

Table IA-XIV reports our results. Panel A, which includes all years in the sample, shows a large increase in the partisan consumption gap in the first year of a presidency (0.45, $t = 6.57$). This effect persists into the second year, is moderately attenuated in the third, and rises again in the fourth year. Because our sample includes the Global Financial Crisis and the COVID-19 recession—both of which coincide with first and fourth years of presidential terms—we reestimate the model excluding 2008, 2009, 2020, and 2021. Panel B shows that, after excluding these episodes, the consumption gap remains significant in the first year (0.19, $t = 2.14$), widens in the second year,

remains stable in the third, and attenuates in the fourth. Affective polarization significantly amplifies the first-year consumption gap in the full sample (0.53, $t = 13.49$) but not once the crisis years are excluded (0.02, $t = 0.56$), and in neither panel does polarization materially change how the gap evolves over the term.

Importantly, Panel B contains relatively few transitions contributing to first- and fourth-year observations, occurring only in 2004–2005 (Bush's second term), 2012–2013 (Obama's second term), and 2016–2017 (when Trump replaced Obama), only one of which resulted in a change in the party of the president. Consequently, we interpret these year-by-year estimates with caution: in the full sample they may be confounded by major macroeconomic shocks (the GFC and COVID-19), while estimates in the restricted sample are based on relatively limited identifying variation. Instead, we place greater weight on our main specification comparing the first and second halves of presidential terms, which aggregates across years and is less sensitive to idiosyncratic macroeconomic shocks. Regardless, consistent with Table VI, the results in Table IA-XIV indicate that the partisan consumption gap remains economically large throughout the entire presidency.

[Insert Table IA-XIV about here.]

Because we are generally concerned that the GFC and COVID-19 may impact our results, we reestimate Table VI on a panel of data that excludes observations in 2008, 2009, 2020, and 2021. Table IA-XV provides evidence that the partisan consumption gap is attenuated in the second half of the presidency. However, the attenuation of 0.21 percentage points (column (3)) still indicates that aligned counties have 0.16 percentage point higher ($0.37 - 0.21$) year-over-year discretionary GDP growth than unaligned counties throughout the second half of the presidency. Column (4) suggests that the amplifying effect of national affective polarization on the initial partisan consumption gap is fully offset in the second half of the presidency. The investment response behaves differently: the alignment effect is large in the first half (0.42, $t = 3.62$, column (6)) and shows no second-half attenuation. Overall, excluding the GFC and COVID-19 does not significantly alter the interpretation of our preferred specification—the partisan consumption gap does not reflect a one-time shock to consumption following the outcome of an election, but rather remains economically large throughout the presidency.

[Insert Table IA-XV about here.]

IA-12 Commuting zones

From the USDA website: "County boundaries are not always adequate confines for a local economy and often reflect political boundaries rather than meaningful boundaries for economic activity. Commuting Zones (CZs) are geographic units of analysis that are intended to more closely reflect the labor markets where people live and work... [They are] designed to group small, rural counties with nearby larger counties to identify where residents of rural counties are likely to travel for work. This grouping allows rural researchers and analysts to examine the variation in socioeconomic conditions across labor markets." Commuting zones are defined via hierarchical cluster analysis of commuting flows between counties as described in Fowler (2024). We use the most recent (2020) commuting zone map. Figure IA-2 plots commuting zones (dark borders) and counties (light borders) for the contiguous United States. We plot county lines as of 2023, corresponding to the end of our sample.

[Insert Figure IA-2 about here.]

Table IA-XVI reports CZ-level summary statistics of our main variables. Our sample includes 12,441 commuting zone-year observations covering 568 unique commuting zones (CZs). The average CZ has a Democratic vote share of 39.59%, 542k residents, per capita income of $51,589, annual discretionary consumption of $3.5B, and annual dividend income of $482M. Average year-over-year discretionary consumption growth is 1.87% and average population growth is 0.35%. The final two columns report means for Republican (Democratic Share < 0.50) and Democratic (Democratic Share > 0.50) CZs. Democratic CZs have a Democratic vote share of 57.94% against 34.31% in Republican CZs, are roughly four times larger in population, have higher per capita income, and have higher dividend income, while Republican CZs have slightly higher consumption growth and income growth.

[Insert Table IA-XVI about here.]

IA-13 Core-based statistical areas

Commuting zones assign every county in the country to a labor market. An alternative is the federal statistical standard for delineating local economies: the core-based statistical area (CBSA), defined by the Office of Management and Budget as one or more counties anchored by an urban core of at least 10,000 residents together with adjacent counties tied to that core by commuting. Metropolitan statistical areas are the subset anchored by a core of at least 50,000 residents; micropolitan areas are those with smaller cores. Because CBSAs do not cover counties outside any urban core, we follow the Bureau of Economic Analysis convention (Bureau of Economic Analysis (2025)) and aggregate counties in metropolitan statistical areas to their CBSA while aggregating all remaining counties within a state to a state non-metropolitan remainder, so that every county in our sample is covered exactly once. We use the 2023 OMB delineation. This yields 426 units, comprising 378 metropolitan statistical areas and 48 state non-metropolitan remainders.[77,78]

Table IA-XVII reports CBSA-level summary statistics of our main variables. Our sample includes 9,369 unit-year observations running from 2001 to 2023. The average unit has a Democratic vote share of 45.07%, 736k residents, per capita income of $53,835, annual discretionary consumption of $4.7B, and annual dividend income of $653M. Average year-over-year discretionary consumption growth is 2.00% and average population growth is 0.73%. The final two columns report means for Republican (Democratic Share < 0.50) and Democratic (Democratic Share > 0.50) units. As at the county and commuting zone levels, Democratic units are substantially larger than Republican units, with roughly twice the population, higher per capita income, and higher dividend income, while Republican units have slightly higher consumption growth and higher population growth.

[Insert Table IA-XVII about here.]

Table IA-XVIII reports estimates of Equation 7 at the CBSA level. Our results are consistent with those estimated at the county and commuting zone levels: alignment remains a significant

[77] Aggregation is performed on the underlying dollar quantities: we sum consumption, dividend income, personal income, population, and party votes across the counties in each unit and then compute unit-level vote shares, alignment, and year-over-year growth exactly as we do at the county level.

[78] The remainder units number 48 rather than 51 because the District of Columbia, New Jersey, and Rhode Island have no counties outside a metropolitan statistical area, so no residual unit exists for them. A remainder unit pools all counties in a given state that lie outside any metropolitan area, and its composition is fixed over our sample by the 2023 delineation. Because these units are internally heterogeneous, aggregating them attenuates variation in alignment relative to the county panel; we nonetheless retain them so that our CBSA-level sample covers the same counties as our county-level sample rather than only metropolitan America.

determinant of both discretionary consumption growth and investment growth. Column (2) indicates that a one standard deviation increase in the share of aligned voters in a unit is associated with an additional 0.22 percentage point ($t = 7.63$) increase in discretionary consumption growth, and column (5) indicates a corresponding 0.30 percentage point ($t = 3.99$) increase in investment growth. These estimates are smaller than their county-level counterparts, which is expected given that aggregation pools counties of differing partisan composition and thereby compresses the variation in alignment. Notably, the interaction between alignment and national affective polarization is positive and significant for consumption growth (0.18, $t = 4.69$) but insignificant for investment growth (0.10, $t = 1.31$), mirroring the pattern in the full county sample.

[Insert Table IA-XVIII about here.]

Table IA-I – Bureau of Economic Analysis Industries

The table reports industry names and corresponding NAICS codes from the BEA database. The third column indicates whether the industry is used in our analysis.

Industry Name	NAICS Codes	Used
Agriculture, forestry, fishing and hunting	11	
Mining, quarrying, and oil and gas extraction	21	
Utilities	22	
Construction	23	
Manufacturing	31-33	
Nondurable goods manufacturing	311-316,322-326	
Durable goods manufacturing	321,327-339	
Wholesale trade	42	
Retail trade	44-45	✓
Transportation and warehousing	48-49	
Information	51	
Finance and insurance	52	
Real estate and rental and leasing	53	
Professional, scientific, and technical services	54	
Management of companies and enterprises	55	
Administrative and support and waste management and remediation services	56	
Educational services	61	
Health care and social assistance	62	
Arts, entertainment, and recreation	71	
Accommodation and food services	72	✓
Other services (except government and government enterprises)	81	
Government and government enterprises	92	

Table IA-II – Bureau of Economic Analysis Sub-Industries

The table reports each sub-industry of the broad industries included in our analysis. 'Business Count' reports the total number of businesses within an industry reported on the NAICS website (https://www.naics.com/search-naics-codes-by-industry/) as of 10/2025. 'GDP' reports industry contribution to fiscal year 2024 GDP (in billions).

NAICS Codes	Industry Title	Business Count	GDP
44-45	Retail Trade	1,818,112	$1,849
4411	Automobile Dealers	105,713	
4412	Other Motor Vehicle Dealers	44,894	
4413	Automotive Parts, Accessories, and Tire Stores	75,426	
4421	Furniture Stores	44,509	
4431	Electronics and Appliance Stores	75,961	
4441	Building Material and Supplies Dealers	87,483	
4442	Lawn and Garden Equipment and Supplies Stores	18,268	
4451	Grocery Stores	142,847	
4452	Specialty Food Stores	95,159	
4453	Beer, Wine, and Liquor Stores	33,621	
4461	Health and Personal Care Stores	124,618	
4471	Gasoline Stations	63,745	
4481	Clothing Stores	180,650	
4482	Shoe Stores	18,839	
4483	Jewelry, Luggage, and Leather Goods Stores	41,632	
4511	Sporting Goods, Hobby, and Musical Instrument Stores	102,729	
4512	Book Stores and News Dealers	14,814	
4522	Department Stores	19,387	
4523	General Merchandise Stores, including Warehouse Clubs and Supercenters	41,369	
4531	Florists	27,992	
4532	Office Supplies, Stationary, and Gift Stores	58,835	
4533	Used Merchandise Stores	38,807	
4530	Other Miscellaneous Store Retailers	203,907	
4541	Electronic Shopping and Mail-Order Houses	32,866	
4542	Vending Machine Operators	9,866	
4543	Direct Selling Establishments	41,025	
72	Accommodation and Food services	899,728	$960
7211	Traveler Accommodation	120,021	
7212	RV(recreational Vehicle) Parks and Recreational Camps	15,409	
7213	Rooming and Boarding Houses, Dormitories, and Workers' Camps	2,361	
7223	Special Food Services	42,341	
7224	Drinking Places (Alcoholic Beverages)	75,590	
7225	Restaurants and Other Eating Places	644,006	
44-45, 72	Total US Consumer Facing Business Entities	2,717,840	$2,809
All	Total US Business Entities	17,566,030	$29,298

Table IA-III – Employment Growth Summary Statistics

The table reports summary statistics for our QCEW employment growth variable. Employment data are from the Bureau of Labor Statistics Quarterly Census of Employment and Wages (QCEW), an administrative census of establishments covered by state unemployment insurance programs that spans more than 95 percent of U.S. jobs. For each county and year, we observe September employment levels at private-sector establishments in retail trade (NAICS 44–45) and accommodation and food services (NAICS 72), and sum employment across the two sectors. Δ Employment is the year-over-year percentage change in this total, winsorized at the 5% and 95% levels and reported in percent.

Variable	N	Mean	10^{th} Percentile	Median	90^{th} Percentile	Standard Deviation
Δ Employment	72,980	0.79	-8.33	0.49	9.65	7.97

Table IA-IV – Employment Growth and Political Alignment

The table reports estimates of Equation 7 using year-over-year growth in county-level employment in consumer-facing industries, described in Table IA-III, as the dependent variable. Column (1) includes only Alignment (Equation 6); column (2) adds prior year per capita income, income growth, and population growth; column (3) adds an interaction term between Alignment and the national level of affective polarization (Equation 5). Regressors are normalized to have mean zero and unit standard deviation. Coefficients are multiplied by 100. $t-$statistics are reported in parentheses. Standard errors are clustered at the county-level. ***,**,* indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Employment Growth		
	(1)	(2)	(3)
$\text{Alignment}_{i,t}$	0.39***	0.41***	0.32***
	(12.74)	(13.58)	(10.87)
$\text{Income}_{i,t-1}$		0.35***	0.26***
		(4.44)	(3.24)
$\text{Income Growth}_{i,t}$		0.34***	0.34***
		(8.25)	(8.24)
$\text{Population Growth}_{i,t}$		0.39***	0.42***
		(8.97)	(9.55)
$\text{Alignment}_{i,t} \times \text{Aff Polar}_t$			0.43***
			(12.32)
Year Fixed Effects?	Yes	Yes	Yes
County Fixed Effects?	Yes	Yes	Yes
R^2	0.0893	0.0930	0.0950
N	66,178	66,178	66,178

TABLE IA-V –

LOCAL ECONOMIC ACTIVITY AND POLITICAL ALIGNMENT: ALTERNATIVE STANDARD ERRORS

The table reports estimates of Equation 7 under alternative assumptions about the correlation structure of the residuals. Columns (1)–(3) use discretionary consumption growth and columns (4)–(6) use investment growth as dependent variables. Within each dependent variable the specification and the point estimates are identical; only the estimated standard errors differ. Columns (1) and (4) cluster by county, the convention used elsewhere in the paper, which permits arbitrary correlation within a county over time but treats distinct counties within a year as independent. Columns (2) and (5) cluster by state. Columns (3) and (6) cluster two-way by county and year following Cameron, Gelbach, and Miller (2011), which additionally permits arbitrary correlation across all counties within a year and is therefore robust to the common national component of political alignment. There are 3,058 counties, 51 states (we treat The District of Columbia as a state for the purpose of this exercise), and 22 years. Regressors are normalized to have mean zero and unit standard deviation. Coefficients are multiplied by 100. t-statistics are reported in parentheses. ***,**,* indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Consumption Growth			Investment Growth		
	(1)	(2)	(3)	(4)	(5)	(6)
$\text{Alignment}_{i,t}$	0.47***	0.47***	0.47**	0.47***	0.47***	0.47**
	(12.79)	(6.48)	(2.31)	(8.01)	(4.73)	(2.68)
$\text{Income}_{i,t-1}$	0.18*	0.18	0.18	−0.11	−0.11	−0.11
	(1.85)	(1.50)	(0.82)	(−0.81)	(−0.56)	(−0.28)
$\text{Income Growth}_{i,t}$	0.84***	0.84***	0.84***	1.84***	1.84***	1.84***
	(19.12)	(10.10)	(7.30)	(21.16)	(10.11)	(7.27)
$\text{Population Growth}_{i,t}$	0.51***	0.51***	0.51***	0.22**	0.22**	0.22
	(11.84)	(7.47)	(6.82)	(2.48)	(2.23)	(1.10)
Year Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes
County Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes
Standard Errors Clustered by	County	State	County & Year	County	State	County & Year
R^2	0.2015	0.2015	0.2015	0.0392	0.0392	0.0392
N	66,069	66,069	66,069	66,069	66,069	66,069

TABLE IA-VI

– DISCRETIONARY GDP GROWTH AND POLITICAL ALIGNMENT - ANOMALOUS YEARS

The table reports estimates of Equation 7 on a panel that excludes the Great Financial Crisis (2008-2009) and the COVID-19 pandemic (2020-2021). Columns (1)–(3) use discretionary consumption growth and columns (4)–(6) use investment growth as dependent variables. Columns (1) and (4) include only Alignment (Equation 6); columns (2) and (5) add beginning year per capita county income, county income growth, and county population growth; columns (3) and (6) add an interaction term between Alignment and the national level of affective polarization (Equation 5). Regressors are normalized to have mean zero and unit standard deviation. Coefficients are multiplied by 100. $t-$statistics are reported in parentheses. Standard errors are clustered at the county-level. ***,**,* indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Consumption Growth			Investment Growth		
	(1)	(2)	(3)	(4)	(5)	(6)
$\text{Alignment}_{i,t}$	0.23***	0.25***	0.24***	0.52***	0.50***	0.51***
	(6.09)	(6.76)	(6.14)	(8.26)	(7.96)	(7.83)
$\text{Income}_{i,t-1}$		0.33***	0.31***		-0.19	-0.18
		(3.15)	(2.97)		(-1.12)	(-1.05)
$\text{Income Growth}_{i,t}$		0.79***	0.79***		1.56***	1.57***
		(17.04)	(17.02)		(16.45)	(16.47)
$\text{Population Growth}_{i,t}$		0.55***	0.56***		0.13	0.13
		(11.79)	(11.85)		(1.34)	(1.30)
$\text{Alignment}_{i,t} \times \text{Aff Polar}_t$			0.07*			-0.03
			(1.94)			(-0.41)
Year Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes
County Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.1540	0.1690	0.1691	0.0392	0.0480	0.0480
N	54,075	54,075	54,075	54,075	54,075	54,075

TABLE IA-VII

– DISCRETIONARY GDP GROWTH AND POLITICAL ALIGNMENT - ANOMALOUS COUNTIES

The table reports estimates of Equation 7 on a panel that excludes counties above the 90$^{\text{th}}$ percentile and below the 10$^{\text{th}}$ percentile of population. Columns (1)–(3) use discretionary consumption growth and columns (4)–(6) use investment growth as dependent variables. Columns (1) and (4) include only Alignment (Equation 6); columns (2) and (5) add beginning year per capita county income, county income growth, and county population growth; columns (3) and (6) add an interaction term between Alignment and the national level of affective polarization (Equation 5). Regressors are normalized to have mean zero and unit standard deviation. Coefficients are multiplied by 100. $t-$statistics are reported in parentheses. Standard errors are clustered at the county-level. ***,**,* indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Consumption Growth			Investment Growth		
	(1)	(2)	(3)	(4)	(5)	(6)
$\text{Alignment}_{i,t}$	0.41***	0.40***	0.34***	0.36***	0.34***	0.29***
	(9.41)	(9.47)	(8.16)	(5.05)	(4.78)	(4.07)
$\text{Income}_{i,t-1}$		0.06	-0.00		-0.28**	-0.32**
		(0.61)	(-0.01)		(-1.99)	(-2.29)
$\text{Income Growth}_{i,t}$		0.90***	0.90***		1.92***	1.92***
		(18.96)	(19.00)		(21.08)	(21.06)
$\text{Population Growth}_{i,t}$		0.50***	0.52***		0.22**	0.23***
		(10.93)	(11.32)		(2.51)	(2.67)
$\text{Alignment}_{i,t} \times \text{Aff Polar}_t$			0.35***			0.29***
			(7.28)			(3.82)
Year Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes
County Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.1936	0.2088	0.2100	0.0283	0.0414	0.0416
N	52,844	52,844	52,844	52,844	52,844	52,844

TABLE IA-VIII

– LOCAL ECONOMIC ACTIVITY AND POLITICAL ALIGNMENT - STATE BY YEAR FIXED EFFECTS

The table reports estimated coefficients from Equation 7 estimated with state-by-year fixed effects in place of year fixed effects, so that any shock common to a state in a given year, such as state tax policy or pandemic restrictions, is absorbed and Alignment is identified from differences in partisan composition between counties in the same state and year. Columns (1)–(3) use discretionary consumption growth and columns (4)–(6) use investment growth as dependent variables. Columns (1) and (4) include only Alignment (Equation 6); columns (2) and (5) add beginning year per capita county income, county income growth, and county population growth; columns (3) and (6) add an interaction term between Alignment and the national level of affective polarization (Equation 5). Consumption growth, income levels, and income growth are based on constant 2023 dollars (i.e., inflation adjusted). Regressors are normalized to have mean zero and unit standard deviation. Coefficients are in percent (i.e., multiplied by 100). t-statistics are reported in parentheses. Standard errors are clustered at the county-level. ***,**,* indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Consumption Growth			Investment Growth		
	(1)	(2)	(3)	(4)	(5)	(6)
$\text{Alignment}_{i,t}$	0.39***	0.38***	0.28***	0.50***	0.48***	0.47***
	(9.07)	(8.94)	(6.87)	(6.88)	(6.60)	(6.38)
$\text{Income}_{i,t-1}$		0.01	-0.05		0.02	0.01
		(0.15)	(-0.46)		(0.11)	(0.07)
$\text{Income Growth}_{i,t}$		0.80***	0.80***		2.11***	2.11***
		(16.71)	(16.66)		(21.61)	(21.61)
$\text{Population Growth}_{i,t}$		0.33***	0.36***		0.16*	0.17*
		(7.50)	(8.09)		(1.74)	(1.76)
$\text{Alignment}_{i,t} \times \text{Aff Polar}_t$			0.47***			0.04
			(10.38)			(0.52)
Year by State Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes
County Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.2702	0.2786	0.2801	0.0851	0.0967	0.0967
N	66,055	66,055	66,055	66,055	66,055	66,055

TABLE IA-IX – DISCRETIONARY GDP GROWTH AND LOCAL HOME PRICES

The table reports estimates of Equation 7 on the sample of counties covered by Zillow home price data, adding year-over-year growth in the county average home price as a control. Home prices are from Zillow, deflated to constant dollars, with growth winsorized at the 5% and 95% levels. Columns (1)–(3) use discretionary consumption growth and columns (4)–(6) investment growth as dependent variables. Columns (1) and (4) report the baseline specification on the Zillow sample; columns (2) and (5) add home price growth; columns (3) and (6) additionally exclude the Great Financial Crisis (2008, 2009) and the COVID-19 pandemic (2020, 2021). Regressors are normalized to have mean zero and unit standard deviation. Coefficients are multiplied by 100. $t-$statistics are reported in parentheses. Standard errors are clustered at the county-level. ***,**,* indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Consumption Growth			Investment Growth		
	(1)	(2)	(3)	(4)	(5)	(6)
$\text{Alignment}_{i,t}$	0.66***	0.67***	0.30***	0.50***	0.50***	0.51***
	(16.07)	(16.39)	(7.40)	(7.82)	(7.75)	(7.52)
$\text{Income}_{i,t-1}$	−0.03	−0.03	0.12	−0.33	−0.33	−0.62***
	(−0.31)	(−0.31)	(1.06)	(−1.64)	(−1.64)	(−2.94)
$\text{Income Growth}_{i,t}$	0.84***	0.81***	0.78***	2.18***	2.19***	1.87***
	(16.93)	(16.41)	(15.32)	(21.84)	(21.81)	(17.36)
$\text{Population Growth}_{i,t}$	0.43***	0.33***	0.46***	0.32***	0.35***	0.28**
	(8.43)	(6.63)	(8.23)	(3.07)	(3.30)	(2.38)
$\text{Home Price Growth}_{i,t}$		0.52***	0.29***		−0.17**	−0.22**
		(13.52)	(7.53)		(−2.01)	(−2.46)
Year Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes
County Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.2730	0.2765	0.2231	0.0636	0.0637	0.0799
N	45,825	45,825	37,300	45,825	45,825	37,300

TABLE IA-X –
DISCRETIONARY GDP GROWTH AND POLITICAL ALIGNMENT – WEALTH EFFECTS AND HOUSEHOLD LEVERAGE

The table reports estimates of Equation 7, adding controls for stock market wealth effects and household leverage. Wealth Shock$_{i,t}$ is county i's lagged dividend income as a share of county personal income multiplied by the annual S&P 500 return in year t, capturing the passive revaluation of the county's equity holdings. Debt-to-Income$_{i,t-1}$ is the county's lagged household debt-to-income ratio. Columns (1)–(4) use discretionary consumption growth and columns (5)–(6) use investment growth as dependent variables. Columns (1) and (5) report the baseline specification on the common sample; column (2) adds the wealth shock; columns (3) and (6) add lagged debt-to-income; column (4) adds both. The wealth shock is omitted from the investment columns because it is constructed from the same county dividend income data that underlies our investment measure. Regressors are normalized to have mean zero and unit standard deviation. Coefficients are multiplied by 100. $t-$statistics are reported in parentheses. Standard errors are clustered at the county-level. ***,**,* indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Consumption Growth				Investment Growth	
	(1)	(2)	(3)	(4)	(5)	(6)
Alignment$_{i,t}$	0.47***	0.47***	0.47***	0.47***	0.47***	0.47***
	(12.79)	(12.78)	(12.81)	(12.79)	(8.01)	(8.00)
Income$_{i,t-1}$	0.18*	0.18*	0.15	0.16	-0.11	-0.10
	(1.85)	(1.86)	(1.59)	(1.60)	(-0.80)	(-0.70)
Income Growth$_{i,t}$	0.85***	0.85***	0.84***	0.84***	1.84***	1.84***
	(19.13)	(19.13)	(19.06)	(19.05)	(21.15)	(21.17)
Population Growth$_{i,t}$	0.51***	0.51***	0.51***	0.51***	0.21**	0.22**
	(11.89)	(11.89)	(11.79)	(11.79)	(2.47)	(2.50)
Wealth Shock$_{i,t}$		-0.03		-0.03		
		(-0.56)		(-0.60)		
Debt-to-Income$_{i,t-1}$			-0.26***	-0.26***		0.16
			(-3.22)	(-3.23)		(1.23)
Year Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes
County Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.2015	0.2015	0.2017	0.2017	0.0391	0.0392
N	66,067	66,067	66,067	66,067	66,067	66,067

TABLE IA-XI – LOCAL ECONOMIC ACTIVITY, POLITICAL ALIGNMENT, AND FEDERAL SPENDING
The table reports estimates of Equation 7 adding a control for federal spending flowing to counties, constructed from USAspending county-level obligations as described in Section IA-3. Federal Spending Growth$_{i,t}$ is the year-over-year growth in total real federal contract and grant obligations performed in county i, winsorized at the 5% and 95% levels. Growth is undefined for counties with no obligations in the prior year, and because obligations data begin in fiscal year 2008 the sample runs from 2009 to 2023; columns (1) and (3) report the baseline specification on this sample. Columns (1)–(2) use discretionary consumption growth and columns (3)–(4) use investment growth as dependent variables. Regressors are normalized to have mean zero and unit standard deviation. Coefficients are multiplied by 100. t-statistics are reported in parentheses. Standard errors are clustered at the county-level. ***,**,* indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Consumption Growth		Investment Growth	
	(1)	(2)	(3)	(4)
Alignment$_{i,t}$	0.60***	0.60***	0.44***	0.44***
	(12.44)	(12.42)	(5.96)	(5.96)
Income$_{i,t-1}$	-0.18	-0.18	-0.21	-0.21
	(-1.19)	(-1.21)	(-0.88)	(-0.88)
Income Growth$_{i,t}$	0.73***	0.73***	2.08***	2.08***
	(13.23)	(13.22)	(18.31)	(18.31)
Population Growth$_{i,t}$	0.48***	0.49***	0.31***	0.31***
	(9.24)	(9.28)	(2.80)	(2.80)
Federal Spending Growth$_{i,t}$		0.09**		0.03
		(2.21)		(0.36)
Year Fixed Effects?	Yes	Yes	Yes	Yes
County Fixed Effects?	Yes	Yes	Yes	Yes
R^2	0.2243	0.2244	0.0536	0.0536
N	43,550	43,550	43,550	43,550

TABLE IA-XII – SYMMETRY - ALIGNED VS UNALIGNED COUNTIES

The table reports estimates of regressions of growth on Alignment decomposed separately for aligned and unaligned county-years, $\text{Alignment}_{i,t} = \text{Aligned}_{i,t} \times \text{Alignment}_{i,t} + \text{Unaligned}_{i,t} \times \text{Alignment}_{i,t}$, where $\text{Aligned}_{i,t}$ is defined in Equation 8 and $\text{Unaligned}_{i,t} = 1 - \text{Aligned}_{i,t}$, along with county controls and county and year fixed effects. Columns (1)–(2) use discretionary consumption growth and columns (3)–(4) investment growth as dependent variables. Columns (2) and (4) additionally interact both terms with Aff Polar_t, with $\theta_{Aligned}$ and $\theta_{Unaligned}$ denoting the coefficients on these interactions. $\delta_{Aligned} - \delta_{Unaligned}$ reports the difference in coefficients between $\text{Aligned}_{i,t} \times \text{Alignment}_{i,t}$ and $\text{Unaligned}_{i,t} \times \text{Alignment}_{i,t}$. $\theta_{Aligned} - \theta_{Unaligned}$ reports the difference in coefficients between their interactions with Aff Polar_t. Continuous regressors are normalized to have mean zero and unit standard deviation. Coefficients are multiplied by 100. $t-$statistics are reported in parentheses. Standard errors are clustered at the county-level. ***,**,* indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Consumption Growth		Investment Growth	
	(1)	(2)	(3)	(4)
$\text{Aligned}_{i,t} \times \text{Alignment}_{i,t}$	-0.04	0.12	0.67***	0.75***
	(-0.33)	(0.97)	(3.70)	(4.02)
$\text{Unaligned}_{i,t} \times \text{Alignment}_{i,t}$	0.98***	0.56***	0.26	0.10
	(8.26)	(4.37)	(1.38)	(0.50)
$\text{Aligned}_{i,t} \times \text{Alignment}_{i,t} \times \text{Aff Polar}_t$		0.37***		-0.02
		(5.88)		(-0.22)
$\text{Unaligned}_{i,t} \times \text{Alignment}_{i,t} \times \text{Aff Polar}_t$		0.67***		0.26**
		(10.19)		(2.28)
$\delta_{Aligned} - \delta_{Unaligned}$	-1.01***	-0.45*	0.41	0.65*
	(-4.58)	(-1.90)	(1.15)	(1.75)
$\theta_{Aligned} - \theta_{Unaligned}$		-0.30***		-0.28*
		(-2.91)		(-1.75)
County Controls?	Yes	Yes	Yes	Yes
Year Fixed Effects?	Yes	Yes	Yes	Yes
County Fixed Effects?	Yes	Yes	Yes	Yes
R^2	0.2017	0.2045	0.0392	0.0392
N	66,069	66,069	66,069	66,069

Table IA-XIII – US Election Results, 2000–2024

The table reports congressional control changes for each of the six presidential terms in our sample. Because the Vice President casts the tie-breaking vote, we consider the mixed senate during the start of Bush's first term as aligned.

	Aligned?			
	Pre-Midterm		Post-Midterm	
President	Senate	House	Senate	House
Bush I	✓	✓	✓	✓
Bush II	✓	✓	×	×
Obama I	✓	✓	✓	×
Obama II	✓	×	×	×
Trump I	✓	✓	✓	×
Biden	✓	✓	✓	×

TABLE IA-XIV – DISCRETIONARY GDP GROWTH OVER THE PRESIDENCY

The table reports estimates of Equation 7 for discretionary consumption growth, augmented with interactions between Alignment and indicators for years two through four of the presidential term, with the first year serving as the omitted category. Column (1) reports the baseline specification; column (2) adds the year-of-term interactions; column (3) adds an interaction between Alignment and the national level of affective polarization (Equation 5); column (4) adds an interaction between Alignment, affective polarization, and Term Year (Equation IA-7). Panel A includes all years in the sample. Panel B excludes the Great Financial Crisis (2008, 2009) and the COVID-19 pandemic (2020, 2021). County controls include prior year per capita income, income growth, and population growth. Regressors are normalized to have mean zero and unit standard deviation. Coefficients are multiplied by 100. $t-$statistics are reported in parentheses. Standard errors are clustered at the county-level. ***,**,* indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: All years	(1)	(2)	(3)	(4)
$\text{Alignment}_{i,t}$	0.47***	0.45***	0.37***	0.35***
	(12.79)	(6.57)	(5.29)	(4.92)
$\text{Alignment}_{i,t} \times$ Year 2		0.02	0.03	0.04
		(0.24)	(0.34)	(0.50)
$\text{Alignment}_{i,t} \times$ Year 3		-0.17*	-0.24***	-0.21**
		(-1.90)	(-2.66)	(-2.31)
$\text{Alignment}_{i,t} \times$ Year 4		0.23**	0.22**	0.25**
		(2.22)	(2.09)	(2.28)
$\text{Alignment}_{i,t} \times \text{Aff Polar}_t$			0.53***	0.62***
			(13.49)	(8.97)
$\text{Alignment}_{i,t} \times \text{Aff Polar}_t \times \text{Term Year}_t$				-0.06*
				(-1.70)
County Controls?	Yes	Yes	Yes	Yes
Year Fixed Effects?	Yes	Yes	Yes	Yes
County Fixed Effects?	Yes	Yes	Yes	Yes
R^2	0.2015	0.2017	0.2046	0.2047
N	66,069	66,069	66,069	66,069
Panel B: Excluding anomalous years				
$\text{Alignment}_{i,t}$	0.25***	0.19**	0.18**	0.19**
	(6.76)	(2.14)	(2.10)	(2.12)
$\text{Alignment}_{i,t} \times$ Year 2		0.27**	0.26**	0.26**
		(2.55)	(2.52)	(2.45)
$\text{Alignment}_{i,t} \times$ Year 3		0.08	0.08	0.08
		(0.81)	(0.74)	(0.77)
$\text{Alignment}_{i,t} \times$ Year 4		-0.35**	-0.34**	-0.35**
		(-2.55)	(-2.44)	(-2.50)
$\text{Alignment}_{i,t} \times \text{Aff Polar}_t$			0.02	0.06
			(0.56)	(0.70)
$\text{Alignment}_{i,t} \times \text{Aff Polar}_t \times \text{Term Year}_t$				-0.03
				(-0.51)
County Controls?	Yes	Yes	Yes	Yes
Year Fixed Effects?	Yes	Yes	Yes	Yes
County Fixed Effects?	Yes	Yes	Yes	Yes
R^2	0.1690	0.1696	0.1696	0.1696
N	54,075	54,075	54,075	54,075

TABLE IA-XV – ECONOMIC ACTIVITY AND MIDTERMS - ROBUSTNESS

The table reports estimates of the impact of Alignment on economic activity separately for the first and second halves of the presidency. Panel A includes all observations including Bush I. Panel B excludes observations in 2008, 2009, 2020, and 2021. County controls include population growth and per capita income. Continuous regressors are normalized to have mean zero and unit standard deviation. Coefficients are multiplied by 100. $t-$statistics are reported in parentheses. Standard errors are clustered at the county-level. ***,**,* indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Consumption Growth				Investment Growth			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Including Bush I								
$\text{Alignment}_{i,t}$ (First Half)	0.47***	0.47***	0.39***	0.37***	0.47***	0.22**	0.20**	0.21**
	(12.79)	(9.56)	(7.91)	(7.42)	(8.01)	(2.49)	(2.34)	(2.38)
$\text{Alignment}_{i,t} \times$ Second Half		-0.01	-0.06	-0.02		0.52***	0.52***	0.50***
		(-0.08)	(-0.81)	(-0.25)		(3.63)	(3.58)	(3.36)
$\text{Alignment}_{i,t} \times \text{Aff Polar}_t$			0.52***	0.63***			0.08	0.04
			(13.46)	(11.28)			(1.27)	(0.44)
$\text{Alignment}_{i,t} \times \text{Aff Polar}_t \times$ Second Half				-0.21***				0.08
				(-2.94)				(0.56)
County Controls?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.2015	0.2015	0.2043	0.2045	0.0392	0.0394	0.0394	0.0394
N	66,069	66,069	66,069	66,069	66,069	66,069	66,069	66,069
Panel B: Excluding anomalous years								
$\text{Alignment}_{i,t}$ (First Half)	0.28***	0.37***	0.37***	0.37***	0.47***	0.42***	0.41***	0.42***
	(6.52)	(6.69)	(6.61)	(6.54)	(6.48)	(3.62)	(3.59)	(3.64)
$\text{Alignment}_{i,t} \times$ Second Half		-0.21***	-0.21***	-0.17**		0.13	0.12	0.05
		(-2.66)	(-2.74)	(-2.14)		(0.62)	(0.59)	(0.24)
$\text{Alignment}_{i,t} \times \text{Aff Polar}_t$			0.04	0.13**			0.04	-0.12
			(1.03)	(2.19)			(0.44)	(-0.97)
$\text{Alignment}_{i,t} \times \text{Aff Polar}_t \times$ Second Half				-0.19**				0.34*
				(-2.46)				(1.74)
County Controls?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.1901	0.1902	0.1903	0.1904	0.0588	0.0588	0.0588	0.0589
N	45,019	45,019	45,019	45,019	45,019	45,019	45,019	45,019

Table IA-XVI – Commuting Zone Summary Statistics

The table reports summary statistics for our main variables aggregated at the commuting zone (CZ) level, computed on the estimation sample used throughout the paper, so that every row describes the same set of observations. Counties are assigned to commuting zones using the 2020 USDA delineation and county-level quantities are summed within each CZ. Democratic Share is the CZ's two-party Democratic vote share in the most recent presidential election, and Alignment is defined in Equation 6. Local Consumption is the sum of the BEA's estimated county-level GDP in the retail trade (NAICS codes 44-45) and accommodations and food services (NAICS code 72) industries (in thousands). Dividend Income is the county-level total dividend income from the IRS (in thousands). All dollar variables are in constant 2023 dollars, and Per Capita Income is aggregated by population weighting. Investment Growth is computed from nominal dividend income and then demeaned by the yearly cross-sectional mean, since inflation is common to all commuting zones within a year and is removed by the demeaning; Dividend Income is therefore deflated in this table for presentation only. All growth variables are winsorized at the 5% and 95% levels of the estimation sample. The final two columns report means for Republican (Democratic Share$_{i,t} < 0.50$) and Democratic (Democratic Share$_{i,t} > 0.50$) commuting zones. Our sample includes 568 unique commuting zones (covering all 50 states plus DC) and runs from 2002 to 2023.

							Mean by CZ Type	
Variable	N	Mean	10^{th} Percentile	Median	90^{th} Percentile	Standard Deviation	Republican	Democrat
Democratic Share	12,441	39.59%	21.46%	39.74%	57.07%	13.59%	34.31%	57.94%
Alignment	12,441	50.98%	27.52%	51.59%	73.47%	17.09%	50.75%	51.79%
Population	12,441	542,342	18,435	160,086	1,311,183	1,281,132	318,620	1,319,457
Population Growth	12,441	0.35%	-0.80%	0.28%	1.66%	0.88%	0.34%	0.39%
Per Capita Income	12,441	$51,589	$39,527	$49,450	$65,681	$12,240	$50,148	$56,593
Income Growth	12,441	1.72%	-3.02%	1.71%	6.47%	3.47%	1.75%	1.62%
Local Consumption	12,441	$3,492,476	$84,462	$783,850	$8,232,565	$9,450,626	$1,854,335	$9,182,673
Local Consumption Growth	12,441	1.87%	-4.38%	1.67%	8.16%	4.79%	1.87%	1.86%
Dividend Income	12,441	$482,494	$7,467	$69,529	$966,996	$1,614,435	$223,213	$1,383,125
Investment Growth	12,441	0.00%	-10.74%	-0.44%	13.37%	10.54%	-0.06%	0.21%

Table IA-XVII – Core-Based Statistical Area Summary Statistics

The table reports summary statistics for our main variables aggregated at the core-based statistical area (CBSA) level, computed on the estimation sample used throughout the paper, so that every row describes the same set of observations. Following the Bureau of Economic Analysis convention, counties in metropolitan statistical areas (2023 OMB delineation) are aggregated to their CBSA and all remaining counties are aggregated to a state non-metropolitan remainder unit, so that every county is covered. Democratic Share is the unit's two-party Democratic vote share in the most recent presidential election, and Alignment is defined in Equation 6. Local Consumption is the sum of the BEA's estimated county-level GDP in the retail trade (NAICS codes 44-45) and accommodations and food services (NAICS code 72) industries (in thousands). Dividend Income is the county-level total dividend income from the IRS (in thousands). All dollar variables are in constant 2023 dollars, and Per Capita Income is total personal income divided by population within each unit. Investment Growth is computed from nominal dividend income and then demeaned by the yearly cross-sectional mean, since inflation is common to all units within a year and is removed by the demeaning; Dividend Income is therefore deflated in this table for presentation only. All growth variables are winsorized at the 5% and 95% levels of the estimation sample. The final two columns report means for Republican (Democratic Share$_{i,t} < 0.50$) and Democratic (Democratic Share$_{i,t} > 0.50$) units. Our sample includes 426 units (378 metropolitan statistical areas and 48 state non-metropolitan remainders) and runs from 2002 to 2023.

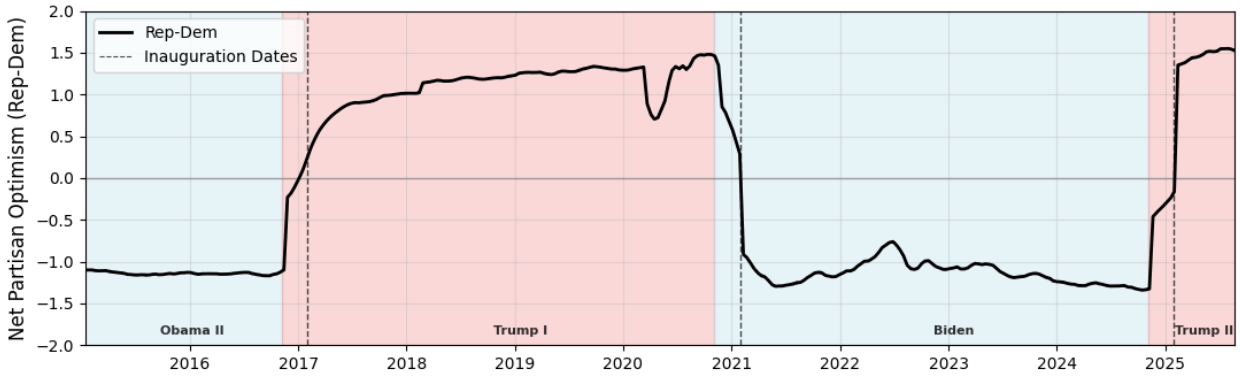
							Mean by CBSA Type	
Variable	N	Mean	10^{th} Percentile	Median	90^{th} Percentile	Standard Deviation	Republican	Democrat
Democratic Share	9,369	45.07%	29.37%	44.60%	60.96%	12.26%	38.23%	58.62%
Alignment	9,369	51.36%	33.44%	51.85%	68.18%	13.14%	51.11%	51.84%
Population	9,369	736,305	105,258	267,322	1,721,861	1,519,355	515,146	1,174,400
Population Growth	9,369	0.73%	-0.32%	0.60%	2.04%	0.86%	0.81%	0.57%
Per Capita Income	9,369	$53,835	$42,499	$51,925	$67,047	$11,475	$52,007	$57,456
Income Growth	9,369	1.92%	-1.76%	1.87%	5.83%	2.80%	2.05%	1.68%
Local Consumption	9,369	$4,746,030	$559,409	$1,587,860	$9,639,238	$11,134,085	$2,963,317	$8,277,424
Local Consumption Growth	9,369	2.00%	-3.60%	1.84%	7.43%	4.12%	2.02%	1.94%
Dividend Income	9,369	$652,599	$41,549	$162,066	$1,172,560	$2,118,082	$337,874	$1,276,042
Investment Growth	9,369	0.00%	-9.43%	-0.48%	9.98%	8.71%	0.02%	-0.04%

Table IA-XVIII – Economic Activity and CBSA Alignment

The table reports estimates of Equation 7 at the core-based statistical area (CBSA) level. Following the Bureau of Economic Analysis convention, counties in metropolitan statistical areas (2023 OMB delineation) are aggregated to their CBSA and all remaining counties are aggregated to a state non-metropolitan remainder unit, so that all counties are covered. Columns (1)–(3) use discretionary consumption growth and columns (4)–(6) use investment growth as dependent variables. Columns (1) and (4) include only Alignment (Equation 6); columns (2) and (5) add prior year per capita income, income growth, and population growth; columns (3) and (6) add an interaction term between Alignment and the national level of affective polarization (Equation 5). Regressors are normalized to have mean zero and unit standard deviation. Coefficients are multiplied by 100. $t-$statistics are reported in parentheses. Standard errors are clustered at the CBSA-level. ***,**,* indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Consumption Growth			Investment Growth		
	(1)	(2)	(3)	(4)	(5)	(6)
$\text{Alignment}_{i,t}$	0.29***	0.22***	0.18***	0.38***	0.30***	0.27***
	(9.11)	(7.63)	(6.15)	(5.41)	(3.99)	(3.70)
$\text{Income}_{i,t-1}$		-0.20	-0.28**		-0.44	-0.48
		(-1.55)	(-2.11)		(-1.31)	(-1.43)
$\text{Income Growth}_{i,t}$		1.08***	1.09***		1.94***	1.95***
		(18.99)	(19.13)		(11.47)	(11.50)
$\text{Population Growth}_{i,t}$		0.69***	0.71***		-0.13	-0.11
		(10.63)	(10.94)		(-0.89)	(-0.80)
$\text{Alignment}_{i,t} \times \text{Aff Polar}_t$			0.18***			0.10
			(4.69)			(1.31)
Year Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes
CBSA Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.5386	0.5906	0.5921	0.0405	0.0647	0.0648
N	9,369	9,369	9,369	9,369	9,369	9,369

Panel A: Perception of expected state of economy



Panel B: Perception of current state of economy

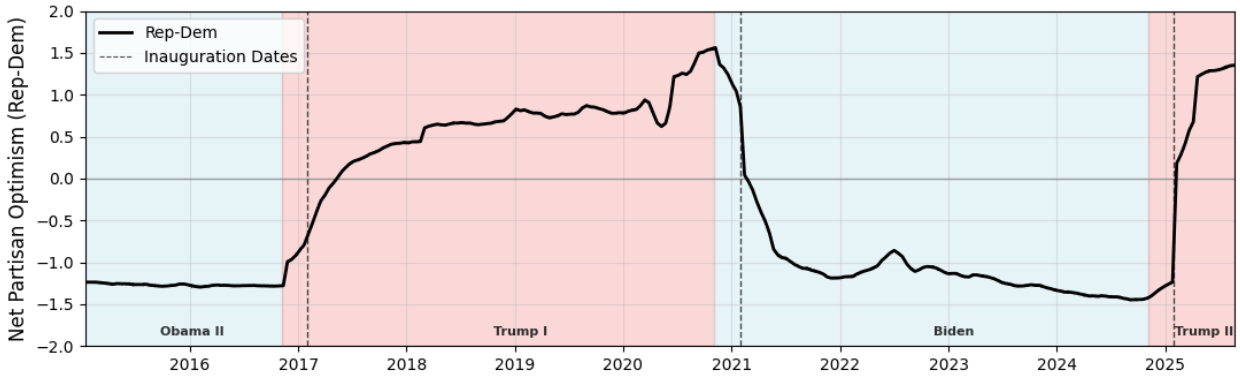


FIGURE IA-1 – CIVIQS SURVEY - POLITICAL PARTY AND ECONOMIC OUTLOOK

The figure shows the daily difference in net optimism about the perceived future state of the economy (Panel A) and perceived current state of the economy (Panel B) between Republicans and Democrats over time (Equations IA-4 and IA-3). Rep-Dem is bounded in $[-2, 2]$. Vertical dashed lines indicate the inauguration date of the given presidency. Blue shaded regions correspond to Democratic presidencies (Obama II, Biden) while red shaded regions correspond to Republican presidencies (Trump I, Trump II).



Figure IA-2 – Commuting Zones and Counties

The figure plots commuting zones (dark borders) and counties (light borders) for the contiguous United States. We use the 2020 USDA commuting zone definition, which is the most recent, and the 2023 USDA county map (corresponding to our sample end)