Elections and Household Policy Expectations*

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Abstract

I ask how much households revise policy expectations around elections relative to non-election years and whether revisions differ across policy groups. I pair August and December responses in the Public Policy Survey of the New York Fed's Survey of Consumer Expectations. Households revise their expectations substantially more around presidential elections, but much less so around midterm elections. Revisions around presidential elections are larger for federal than for state and local policies. Households that shift their expectations toward a higher average income tax rate also tend to raise their own tax expectations and to report fewer large purchases.

Keywords: Policy expectations; Elections; Belief revision; Subjective probabilities; Federalism

JEL Classification: D84; D72; H77

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1 Introduction

Expectations about future policy can shape household decisions today. An expected change in income taxes, for example, may affect how much households work or spend before any tax change takes effect. Elections can bring news about several policies at once. The outcome of a presidential election may tell households more about future federal income tax rates than about state minimum wages. I ask how much households revise their policy expectations around presidential and midterm elections relative to non-election years, and whether the additional revision is larger for federal policies than for state and local policies.

I use the Public Policy Survey, a module of the New York Fed's Survey of Consumer Expectations (SCE), to track revisions in household policy expectations from 2016 to 2024. The Public Policy Survey is fielded in April, August, and December and asks respondents about expected changes in twenty policies over the next twelve months. For each policy, respondents assign probabilities to an increase, no change, and a decrease. I pair each respondent's answers for the same policy in the August and December waves bracketing the November elections. I study the 2016, 2020, and 2024 presidential elections and the 2018 and 2022 midterm elections, using 2019, 2021, and 2023 as non-election benchmark years. To compare revisions across policy groups, I classify the twenty policies as federal or state and local, with four comparisons: two one-to-one comparisons of closely related federal and state policies, seven core federal versus three core state and local policies, and fourteen federal versus six state and local policies covering all twenty policies.

I use three measures to quantify to what extent households revise their expectations and to show which probabilities change. First, *total variation* measures the overall probability shift across the three outcomes. Second, the *no-change probability revision* shows whether households put more or less weight on policies staying the same. Third, *absolute net revision* captures the size of the change in the difference between the directional probabilities of an increase and a decrease.

Households revise their policy expectations substantially more around presidential elections than in non-election years, while the estimated difference from the benchmark is small around midterm elections. Total variation around presidential elections is about 20 percent higher than in non-election years. The no-change probability revision is 7.1 percentage points below the non-election benchmark. Absolute net revision is about 29 percent higher, reflecting larger shifts in the probability households assign to one direction of policy change relative to the other. As for midterm elections, total variation and absolute net revision are only about 5 and 6 percent above the

non-election benchmark, respectively, while the no-change probability revision is 1.9 percentage points higher.

Across policy groups, households make larger overall and directional revisions for federal policies than for state and local policies around presidential elections, relative to the gap in non-election years. The one-to-one comparisons show significantly larger directional revisions for the federal minimum wage than for the state minimum wage, and for federal student aid than for public college tuition in the respondent's state. Across the 7-versus-3 and 14-versus-6 comparisons, the gaps increase by 3.8–4.0 percentage points for total variation and 3.4–3.6 percentage points for absolute net revision, all statistically significant at the 5 percent level. Around midterm elections, the changes in the gap range from -1.0 to 0.5 percentage points across the three measures in both group comparisons, and none is statistically significant.

The additional updating around presidential elections extends to household fiscal expectations over a shorter window. I use the monthly SCE Core Questionnaire to track expectations of own taxes and U.S. government debt, two fiscal outcomes closely related to public policy. From October to December, households revise both expectations about 26 percent more around presidential elections than in non-election years, while the corresponding midterm differences are statistically insignificant.

Policy expectations are significantly related to households' own tax expectations and purchasing decisions. I link responses from the same individuals across three SCE modules: the Public Policy Survey, the Core Questionnaire, and the Household Spending Survey. Households that shift their policy expectations toward a higher average income tax rate raise their own tax expectations, and this association strengthens more around presidential elections than around midterm elections, relative to non-election years. Further, these policy expectation revisions are associated with a lower probability of making large purchases.

These findings point to policy expectations as a potential channel through which political events can matter for household economic decisions. A household's response to policy may begin when its expectations start to change rather than when a new policy takes effect. Therefore, evaluations centered on implementation dates may miss anticipatory behavior and mischaracterize the timing and size of policy responses. The larger revisions for federal policies around presidential elections further suggest that such responses may be concentrated in decisions most affected by the policies households expect to change with a specific election.

This paper makes three contributions, beginning with the measurement of revisions in policy expectations. Studies of policy expectations mostly use their levels or track how their averages shift around reforms and elections (Dominitz, Manski,

and Heinz, 2003; Luttmer and Samwick, 2018; van der Wiel, 2009; Bissonnette and van Soest, 2012; Ciani et al., 2023; Bram, 2023; Koşar, Smith, and van der Klaauw, 2019; Delehanty, Koşar, and van der Klaauw, 2025; Koh, Monninger, and Wang, 2026). Studies of household expectations around elections mainly concern general economic expectations and partisan differences (Gerber and Huber, 2010; Conlon and Zafar, 2017; Mian, Sufi, and Khoshkhou, 2023; Kamdar and Ray, 2024; DiGiuseppe, Garriga, and Kern, 2025; Guirola, 2025; Jeong, Jo, and Shim, 2025; Drobot and Kang, 2026; Niekamp, 2026; Koh et al., 2026).[1] Instead, I quantify how much each household revises its full probability distributions for twenty policies. I show that households revise these distributions substantially more around presidential elections than in the same months of non-election years, but much less so around midterm elections.

Second, I contribute to the literature on how the allocation of policy authority shapes households' responses to elections. Schneider, Jacoby, and Lewis (2011) and Arceneaux (2006) examine policy responsibility and electoral accountability, respectively. National politics increasingly shapes Americans' voting decisions (Hopkins, 2018), while partisan attribution of responsibility across levels of government can shift following a change in the presidency (Uttermark et al., 2022). Policy uncertainty also varies with the level of government involved in an election (Baker, Davis, and Levy, 2022). I extend this literature by showing that households make larger overall and directional revisions to their expectations for federal policies than for state and local policies around presidential elections, relative to the gap between the two policy groups in non-election years.

Third, I contribute to the literature on how anticipated policy changes and elections affect economic behavior. Households' willingness to buy durables responds to announced tax changes in advance, as do aggregate investment and hours (D'Acunto, Hoang, and Weber, 2022; Mertens and Ravn, 2012). Election-driven shifts in sentiment have been linked to car purchases (Gillitzer and Prasad, 2018), although the evidence on household spending is mixed (Mian et al., 2023; Kamdar and Ray, 2024). I provide another piece of suggestive empirical evidence linking anticipated policy changes to household economic behavior.

Section 2 describes the data, the measures, and the specifications. Section 3 reports the revisions around elections and their links to own tax expectations and purchases. Section 4 compares the revisions across policy groups, and Section 5 concludes. A separate Supplemental Appendix reports further results.

[1] Other papers compare partisan groups around elections without tracking the same respondents over time (Armantier, Conlon, and van der Klaauw, 2017; Huberman et al., 2018; Coibion, Gorodnichenko, and Weber, 2020).

2 Data and Empirical Design

2.1 Data and Samples

I combine three modules of the SCE, a rotating panel in which each respondent stays for a year (Armantier, Topa, van der Klaauw, and Zafar, 2017). The Public Policy Survey elicits policy expectations, the monthly Core Questionnaire measures fiscal expectations, and the Household Spending Survey records large purchases. I link the modules by respondent identifier and year.

The Public Policy Survey (Federal Reserve Bank of New York, 2026b) has been fielded in April, August, and December since 2016 and asks every respondent about expected changes in each of the twenty policies.[2] For each policy, respondents state the percent chances that over the next twelve months it will be increased or expanded, remain unchanged, or be decreased or reduced, with the three answers summing to 100. I pair each respondent's August and December answers for the same policy within a year and include a pair only if the probability triple is complete in both waves. The two waves bracket the November federal elections, and I use eight August-to-December windows from 2016 to 2024.[3] These windows cover the presidential elections of 2016, 2020, and 2024, the midterm elections of 2018 and 2022, and the non-election benchmark years of 2019, 2021, and 2023. Supplemental Appendix Table S1 lists the waves and the pairs by window. The paired sample contains 52,820 respondent–policy pairs from 3,267 respondents, and each respondent appears in only one sample year.[4]

The Core Questionnaire (Federal Reserve Bank of New York, 2026c) is fielded every month and asks respondents to suppose that their total household income twelve months from now is the same as now. It then asks whether they expect the total amount of taxes they will have to pay to have increased or decreased, and by about what percent.[5] Repeat respondents are also asked by about what percent the level of U.S. government debt will change over the next twelve months. I pair each respondent's October and December answers q to bracket the election month more narrowly, and for own taxes also the August and December answers. I drop an own tax pair when either answer is below -25 or above 40 percent, corresponding to the 1st and 99th percentiles

[2] The October and November 2015 launch waves asked each respondent about a random half of the policies, and I do not use these waves.

[3] There was no December wave in 2017. The benchmark years had no regularly scheduled federal elections. Some states held gubernatorial or state legislative elections in November, and special elections for the U.S. House took place during the August–December windows.

[4] The December 2020 probability questions are blank for 50 percent of the module respondents, so 2020 supplies 23 percent of the presidential-window pairs of the estimation sample. Supplemental Appendix Section S1.3 documents the wave.

[5] The question includes federal, state, and local income, property, and sales taxes.

of the 14,544 October and December answers from the 7,272 pairs pooled over the eight years. This leaves 7,043 pairs. Applying the same rule to debt gives thresholds of -15 and 100 percent, leaving 6,310 pairs.

The Household Spending Survey (Federal Reserve Bank of New York, 2026a) is fielded in April, August, and December, as is the Public Policy Survey. It asks whether any member of the household made each of several large purchases during the last four months and the percent chance of each purchase within the next four months. I use seven categories.[6] The December spending measure covers the four months before the December interview, overlapping with the August-to-December window of policy expectations.[7] I link each respondent's August and December purchase answers to the same respondent's August-to-December pair for the average income tax rate.

The estimation samples keep the respondents whose household and regional controls are known. The policy-revision sample contains 52,235 pairs from 3,230 respondents. The analyses of own tax and debt revisions use 5,170 and 5,208 respondents, respectively. When I link own tax revisions to policy expectations, the samples contain 2,676 respondents for the revisions from August and 2,577 for those from October, while the purchase analysis uses 2,495 respondents. Most of the reduction in the own tax revision sample is due to respondents who did not complete a Core interview in August of that year, when I measure the controls. Supplemental Appendix Table S3 traces each sample, and Table S4 defines the controls.

2.2 Measures and Policy Groups

The Public Policy Survey records probabilities on a 0–100 scale. Let p^{up}, p^{same}, and p^{down} denote the probabilities of an increase, no change, and a decrease, respectively, and $p^{\text{net}} = p^{\text{up}} - p^{\text{down}}$ represents the net balance of directional probabilities. For each respondent–policy pair, Δ denotes the December value minus the August value. I define three measures in percentage points to characterize the policy expectation revision.

First, total variation is defined as:

$$\text{TV} = \tfrac{1}{2}\left(\left|\Delta p^{\text{up}}\right| + \left|\Delta p^{\text{same}}\right| + \left|\Delta p^{\text{down}}\right|\right). \tag{1}$$

It measures how much the whole distribution changes between August and December. Second, the no-change probability revision is $\Delta p^{\text{same}} = p^{\text{same}}_{\text{Dec}} - p^{\text{same}}_{\text{Aug}}$. Negative Δp^{same} reflects that the respondent puts less weight on the policy staying the same, and pos-

[6] The seven categories are "Home appliances," "Electronics, computers or cell phones," "Furniture," "Home repairs, improvements or renovations," "Car or other vehicles," "Trips and vacations," and "A house or apartment." The question on past purchases also offers "Other," which I do not use.

[7] Supplemental Appendix Section S1.5 measures this overlap.

itive Δp^{same} reflects that the respondent puts more weight on it. Third, absolute net revision is defined as:

$$\text{ANR} = \tfrac{1}{2}\left|\Delta p^{\text{up}} - \Delta p^{\text{down}}\right| = \tfrac{1}{2}\left|\Delta p^{\text{net}}\right|. \tag{2}$$

It measures how far the balance between an increase and a decrease moves. The factor of one half puts it on the scale of TV, which it never exceeds and equals only when p^{same} does not change.

A single example shows what each measure adds. Suppose a respondent answers $(p^{\text{up}}, p^{\text{same}}, p^{\text{down}}) = (20, 60, 20)$ in August. If the December answer is $(30, 40, 30)$, total variation is 20 points, the no-change probability revision is -20 points, and absolute net revision is zero. Probability has left the status quo without favoring either direction. If the December answer is instead $(40, 60, 0)$, total variation is again 20 points, but the no-change probability revision is zero and absolute net revision is also 20 points. Probability has moved from a decrease to an increase, and the probability of no change is untouched.

The Core Questionnaire measures fiscal expectations as the expected percent change q over the next twelve months in own total taxes at unchanged household income or in the level of U.S. government debt. Positive values indicate an expected increase and negative values an expected decrease. For own taxes, I define $\Delta q = q_{\text{Dec}} - q_{\text{Aug}}$, matching the August-to-December window of Δp^{net} for the average income tax rate. For both fiscal expectations, I also define $\Delta^o q = q_{\text{Dec}} - q_{\text{Oct}}$, where o denotes October. The signed revision measures the direction of updating, while its absolute value measures the size. Both are expressed in percentage points of the expected rate of change, not changes in actual taxes or debt.

The Household Spending Survey records purchases over the previous four months and purchase probabilities over the next four months for seven categories. For category j, $B_{ijt,\text{Dec}}$ equals 100 if respondent i reports a purchase in that category in the December survey of year t and zero otherwise, and $B_{ijt,\text{Aug}}$ is defined in the same way for August. The August purchase probability, $\pi_{ijt,\text{Aug}}$, is the reported percent chance of purchasing in that category over the next four months. I examine purchases using both the seven category indicators stacked for each respondent and $\max_j B_{ijt,\text{Dec}}$, which equals 100 if the respondent reports at least one purchase across the seven categories and zero otherwise. Regression coefficients are thus expressed in percentage points of the probability of making a purchase per unit increase in the corresponding regressor.

I classify each of the twenty policies as federal or state and local according to which level of government primarily sets or changes the policy parameter asked about in the survey. The assignments follow the survey wording and the allocation of author-

Table 1: Policy Classification

Panel A. Federal (14)	
Core (7)	*Additional (7)*
Federal student loan debt forgiveness	Payroll tax rate
Federal student aid/Pell grants[b]	Mortgage interest tax deduction
Federal welfare benefits	Capital gains tax rate
Federal minimum wage[a]	Average income tax rate
Retirement age to claim social security benefits	Income tax rate for highest income bracket
Social security retirement benefits	Unemployment benefits
Medicare benefits	Affordable housing/housing assistance
Panel B. State and local (6)	
Core (3)	*Additional (3)*
Public college tuition in your state[b]	Gasoline tax
State minimum wage[a]	Free/subsidized public preschool education
Cost of public transportation	Mandatory paid parental leave

Notes: Core means that the level of government in the panel title sets the policy. *Additional* means that both the federal and the state governments play a role in the policy, and the panel title gives the side on which I place it. [a] and [b] mark the two pairs of closely related federal and state policies.

ity. Two pairs provide comparisons between closely related federal and state policies: (a) the federal minimum wage versus the state minimum wage and (b) federal student aid versus public college tuition in the respondent's state. The federal group includes seven core policies set by the federal government and seven additional policies in which the federal government plays a major role. The state and local group includes three core policies set by state or local governments and three additional policies for which states primarily set the relevant parameters. I refer to the comparison of the core policies as 7 versus 3 and the comparison of all twenty policies as 14 versus 6. Table 1 lists the full classification of fourteen federal policies and six state and local policies. Supplemental Appendix Section S2 discusses the policy classification in detail and reproduces the survey wording for all twenty policies.

2.3 Empirical Specifications

Every main specification includes the same household and regional controls X_{it} of respondent i in year t. These controls include age, gender, education, household income, labor status, and Census region.[8] Each specification also has versions without controls,

[8]The indicators are age 40–59 and age 60 or older against under 40; female against male; a bachelor's degree or higher against less education; household income of $50,000–99,999 and of $100,000 or more against under $50,000; working, and retired and not working, against the other labor statuses; and the Midwest, South, and West Census regions against the Northeast. I take income, labor status, and the region of the state of residence from the August Core interview of the window year, and age, gender, and education from the earliest Core answers at or before it.

with state indicators in place of the regions, with a control for the initial level of each expectation at the start of its window, and with extended controls of homeownership, household composition, race, ethnicity, numeracy, and financial decision-making. All further results are reported in Supplemental Appendix Sections S3, S4, and S5.

To examine whether households revise their policy expectations more around elections, I compare revisions in presidential and midterm election windows separately with those in benchmark windows, using all twenty policies. Let Y_{ikt} denote one of the three revision measures for respondent i, policy k, and year t. For each measure, I consider the following equation:

$$Y_{ikt} = \alpha_k + \beta_P P_t + \beta_M M_t + X_{it}'\lambda + \varepsilon_{ikt}, \tag{3}$$

where P_t and M_t indicate presidential and midterm years and the benchmark years are the omitted group. With policy fixed effects α_k, the coefficients β_P and β_M measure how much Y_{ikt} for a given policy differs in presidential or midterm windows from the non-election benchmark windows.

To examine revisions in fiscal expectations over a narrower window around elections, I compare October-to-December revisions in own tax and government debt expectations across presidential, midterm, and non-election years using the following equation:

$$|\Delta^o q_{it}| = \alpha + \beta_P P_t + \beta_M M_t + X_{it}'\lambda + u_{it}, \tag{4}$$

where β_P and β_M measure how much larger the fiscal revision is around presidential and midterm elections than in the same October–December window of the non-election benchmark years.

To examine whether households connect their expectations about tax policy $\Delta p_{it}^{\text{net}}$ to their own tax expectations Δq_{it}, I relate revisions in expectations for the average income tax rate to revisions in expected own taxes using the following specification:

$$\Delta q_{it} = \gamma_t + \theta_0 \Delta p_{it}^{\text{net}} + \theta_P \Delta p_{it}^{\text{net}} P_t + \theta_M \Delta p_{it}^{\text{net}} M_t + X_{it}'\lambda + u_{it}, \tag{5}$$

where γ_t are year fixed effects, θ_0 is the slope in benchmark years, and θ_P and θ_M are the differences of the slopes in presidential and midterm years from the benchmark years. The dependent variable is either Δq_{it}, the August-to-December revision in own tax expectations matching the window of $\Delta p_{it}^{\text{net}}$, or $\Delta^o q_{it}$, the narrower October-to-December revision.[9]

[9]The October-to-December window brackets each Election Day and the date on which the Associated Press called the election, one to eight days later.

To examine whether revisions in income tax rate expectations are associated with household purchasing behavior, I first estimate the following specification using the seven purchase categories stacked for each respondent:

$$B_{ijt,\text{Dec}} = \alpha_{jt} + \mu_j \pi_{ijt,\text{Aug}} + \rho_j B_{ijt,\text{Aug}} + \eta\, p^{\text{net}}_{it,\text{Aug}} + \psi \Delta p^{\text{net}}_{it} + X'_{it}\lambda + e_{ijt}, \tag{6}$$

where α_{jt} are category-by-year fixed effects that absorb shocks common to purchases in each category and year. The August purchase probability and purchase indicator control for prior plans and recent purchases, with category-specific coefficients μ_j and ρ_j. The August value $p^{\text{net}}_{it,\text{Aug}}$ controls for initial policy expectations. I then estimate the following specification for the indicator of at least one purchase, using one observation per respondent:

$$\max_j B_{ijt,\text{Dec}} = \gamma_t + \sum_j \mu_j \pi_{ijt,\text{Aug}} + \rho \max_j B_{ijt,\text{Aug}} + \eta\, p^{\text{net}}_{it,\text{Aug}} + \psi \Delta p^{\text{net}}_{it} + X'_{it}\lambda + e_{it}. \tag{7}$$

In both equations, ψ measures the association between revisions in income tax rate expectations and purchases, pooled across all eight years.

To examine differences across policy groups, I compare revisions for federal policies with those for state and local policies around presidential and midterm elections, relative to the gap between the two groups in non-election years. I consider the following specification:

$$Y_{ikt} = \alpha_k + \gamma_t + \phi_P F_k P_t + \phi_M F_k M_t + X'_{it}\lambda + F_k X'_{it}\kappa + \varepsilon_{ikt}. \tag{8}$$

The indicator F_k equals one for federal policies and zero for state and local policies. The coefficients ϕ_P and ϕ_M, which I call the presidential and midterm differentials, measure how the gap in revisions between the two policy groups differs from its non-election benchmark. The year fixed effects γ_t absorb shifts common to both groups within a year. The controls X_{it} and their interactions with F_k account for household characteristics and allow their associations with revisions to differ between the two policy groups. I apply Equation (8) first to the two one-to-one policy comparisons, then to the 7-versus-3 comparison of core policies, and finally to the 14-versus-6 comparison of all twenty policies.

2.4 Inference and Robustness

I estimate every equation by pooled least squares, giving each observation equal weight. In Equations (3) and (8), I cluster standard errors by respondent and policy for the twenty-policy sample and the 7-versus-3 comparison. I use a t distribution

with $G - 1$ degrees of freedom, where G is the number of policies, because there are fewer policy clusters than respondent clusters. For the one-to-one comparisons and in Equations (4)–(7), I cluster standard errors by respondent and use a t distribution with one less degree of freedom than the number of respondents.

In the Supplemental Appendix, I assess robustness in inference, sample composition and controls, and weighting and comparison windows. First, I examine inference using alternative clustering, election-label permutations, and a year-level wild cluster restricted bootstrap with Webb weights for the overall slopes of own tax revisions and purchases on Δp^{net} to account for within-year error correlation (Cameron, Gelbach, and Miller, 2008; Webb, 2023). Second, I examine sensitivity to sample composition and controls using the original samples, extended household controls, state indicators, initial levels of the expectations, winsorized Core answers, and common samples across modules, windows, and policy pairs, and to the composition of the policy groups. Third, I assess sensitivity to weighting and comparison windows by applying survey weights, omitting one election or one benchmark year at a time, and dropping August answers that put all probability on one outcome, and by comparing April-to-August revisions with those of the main windows for the same respondents. All these robustness checks are summarized in Sections S3, S4, and S5.

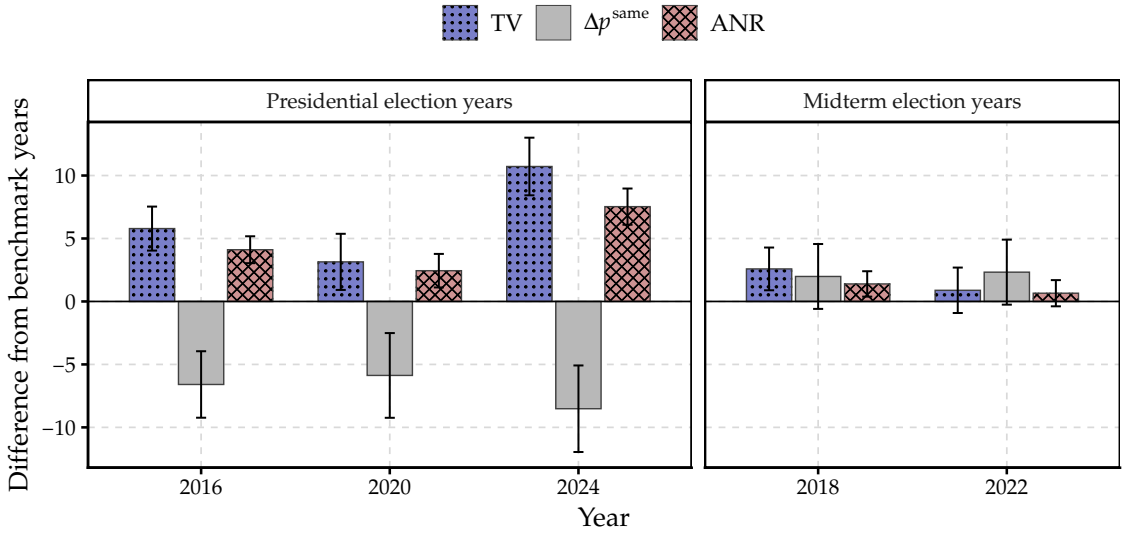
3 Revisions Around Elections

3.1 Policy Expectations from August to December

Households revise their policy expectations more around each presidential election than in non-election benchmark years, while the differences around midterm elections are smaller. Figure 1 compares the average August-to-December revision in each election year with the non-election benchmark. Total variation averages 33.2 percentage points in non-election years and is 3.1 to 10.7 points above that level around presidential elections and 0.9 to 2.6 points above it around midterms. The no-change probability revision is below its benchmark in all three presidential election years and above it in both midterm years. Absolute net revision is also above its benchmark in all three presidential election years.

The pooled estimates show that households revise their policy expectations more around presidential elections and shift probability away from policies staying the same. In Table 2, total variation is 6.64 percentage points higher than in non-election years, about 20 percent above the raw benchmark-year mean of the same sample. The no-change probability revision is 7.13 percentage points below the benchmark. Abso-

Figure 1: Revisions Around Elections Relative to the Benchmark Years



Notes: Unadjusted August-to-December revisions of all twenty policies, in percentage points, computed directly from the data without a regression and without controls. Each bar is, for a presidential or midterm window, the mean revision over the window's respondent–policy pairs, every pair weighted equally, minus the mean over all pairs of the benchmark years. The bars use every valid pair of the paired sample, 52,820 pairs from 3,267 respondents, which includes the respondents without known controls who are not in Table 2. Vertical lines show 95 percent confidence intervals of that difference, based on standard errors clustered by respondent. Blue bars with black dots are TV, gray bars Δp^{same}, and red bars with a black lattice ANR.

Table 2: Revisions Around Elections

	TV	Δp^{same}	ANR
β_P	6.6373^{***}	-7.1326^{***}	4.7808^{***}
	(1.1724)	(1.2871)	(0.9114)
β_M	1.8094^{**}	1.9063^{*}	1.0304^{**}
	(0.7209)	(1.0869)	(0.4596)
Benchmark-year mean	33.2498	1.1238	16.5849
Number of pairs		52,235	
Number of respondents		3,230	

Notes: Estimates of Equation (3) on all twenty policies. All three columns include the household and regional controls X_{it} and the policy fixed effects and use the respondents whose controls are known. The benchmark-year mean is the raw mean over the benchmark-year pairs of this sample. Standard errors clustered by respondent and by policy are in parentheses. *, **, and *** denote significance at the 10, 5, and 1 percent levels, using the t distribution with 19 degrees of freedom, one less than the number of policies.

lute net revision is 4.78 percentage points higher, about 29 percent above its benchmark mean, reflecting larger directional shifts in households' policy expectations. All three differences are statistically significant at the 1 percent level.

Around midterm elections, policy expectation revisions differ little from the non-election benchmark. In Table 2, total variation and absolute net revision are 1.81 and 1.03 percentage points higher, respectively, only about 5 and 6 percent above their benchmark means. The no-change probability revision is 1.91 percentage points higher. The estimates for total variation and absolute net revision are statistically significant at

the 5 percent level, and the estimate for the no-change probability revision is significant at the 10 percent level.

The presidential results are robust across specifications and years, while the midterm results are not robust across years. The estimates change little without controls, with state indicators or initial levels, and with extended controls in Supplemental Appendix Table S6. Election-label permutations yield two-sided p-values of 0.004 for presidential and at least 0.63 for midterm differences in total variation and absolute net revision in Table S8. All three presidential estimates remain significant at the 1 percent level without any one election or benchmark year, under survey weights, and without the pairs whose August answers put all probability on one outcome in Table S9, whereas the midterm estimates in total variation and absolute net revision are insignificant at the 10 percent level without 2018, without the benchmark year 2019, or without these pairs. On the respondents who answer in April, August, and December, Table S10 shows that additional presidential updating in total variation is concentrated in August–December rather than April–August.

3.2 Fiscal Expectations and Household Purchases

From October to December, households revise their expectations of own total taxes and U.S. government debt more around presidential elections than in the same months of non-election years, but not significantly more around midterm elections. Table 3 reports absolute revisions of fiscal expectations in Panel A. Around presidential elections, these revisions are 0.87 and 1.67 percentage points larger for own taxes and debt, each about 26 percent above its raw benchmark-year mean and significant at the 1 percent level.

Households' own tax expectations move with their expectations for the average income tax rate, more so around presidential elections. Panel B of Table 3 links income tax rate expectations to own tax expectations. In its first column, both revisions run from August to December. The full slope in presidential years, $\theta_0 + \theta_P$, is 0.0161, significant at the 1 percent level. This slope is 0.0114 steeper than in benchmark years, significant at the 5 percent level, whereas the full midterm slope of 0.0113 is not significantly steeper than in benchmark years at the 10 percent level. With Δp^{net} from August to December and the own tax revision from October to December, the second column again gives a positive full presidential slope of 0.0141, significantly steeper than in benchmark years at the 5 percent level.

Households that revise their expectations toward a higher average income tax rate are less likely to report large purchases. Panel C of Table 3 relates income tax rate ex-

Table 3: Fiscal Expectations and Household Purchases

Panel A. Absolute revisions of fiscal expectations	Own taxes	Government debt
β_P	0.8660***	1.6664***
	(0.1798)	(0.3564)
β_M	0.2486	0.3524
	(0.1854)	(0.3731)
Benchmark-year mean	3.2975	6.2926
Number of respondents	5,170	5,208
Panel B. Income tax rate expectations and own tax expectations	August–December	October–December
θ_0	0.0047	0.0006
	(0.0036)	(0.0034)
θ_P	0.0114**	0.0135**
	(0.0055)	(0.0055)
θ_M	0.0066	0.0021
	(0.0058)	(0.0056)
$\theta_0 + \theta_P$	0.0161***	0.0141***
	(0.0041)	(0.0043)
$\theta_0 + \theta_M$	0.0113**	0.0027
	(0.0045)	(0.0045)
Number of respondents	2,676	2,577
Panel C. Income tax rate expectations and purchases	Seven categories	Any purchase
ψ	−0.0230***	−0.0512***
	(0.0065)	(0.0188)
Benchmark-year mean purchase rate	17.7189	69.8574
Number of observations	17,465	2,495
Number of respondents	2,495	2,495

Notes: Estimates of Equations (4) to (7) with equal weights and the household and regional controls X_{it}, each column on its own sample. Panel A uses absolute October-to-December revisions in percentage points. In Panel B, Δp^{net} runs from August to December in both columns, which share most respondents and months, and the column heads give the window of the own tax revision. Panels B and C report percentage points of the outcome per percentage point of Δp^{net}. Each respondent gives one pair in each column of Panels A and B, seven observations in the first column of Panel C, and one observation in the second. Benchmark-year means are raw outcome means in benchmark years. Standard errors clustered by respondent are in parentheses. *, **, and *** denote significance at the 10, 5, and 1 percent levels, using the t distribution with one less degree of freedom than the number of respondents.

pectations to purchases. With the seven categories stacked, the slope ψ is -0.023, significant at the 1 percent level, so a 10-percentage-point increase in Δp^{net} is associated with a probability of making a purchase in a given category that is 0.23 percentage points lower, equivalent to 1.3 percent of the raw benchmark-year mean rate of 17.7 percent. With at least one purchase as the outcome, the slope is -0.051, also significant at the 1 percent level. The two outcomes capture different aspects of purchasing behavior using the same underlying responses.

The presidential estimates of Panel A, the full presidential slopes of Panel B, and the purchase slopes of Table 3 remain evident with other samples and controls in Supplemental Appendix Table S11. Election-label permutations in Table S12 give p-values of 0.054 and 0.068 for the Panel A presidential estimates and 0.007 for both Panel B presidential interactions, and a year-level bootstrap with Webb weights gives 0.011

and 0.044 for the purchase slopes. The presidential estimates of Panel A, the presidential interaction from October, and the seven-category purchase slope stay significant at the 10 percent level in every variant of Tables S13 and S14, while the interaction from August and the slope with at least one purchase lose significance in some. On the respondents who also answer in April and August, Table S15 shows that the additional presidential revision appears after August for own tax expectations but already before August for debt expectations. On the respondents valid in both windows in Table S16, both presidential interactions stay significant at the 5 percent level, and on one sample shared by the own tax column of Panel A, the October-to-December column of Panel B, and the stacked column of Panel C in Table S17, the presidential interaction from October and the stacked purchase slope are significant at the 1 percent level.

4 Revisions Across Policy Groups

Presidential elections may be more informative about federal policy than about state and local policy. Table 4 compares the gap between revisions for federal policies and those for state and local policies in each type of election year with the corresponding gap in non-election years. Panels A and B report the two one-to-one comparisons. Panels C and D report the 7-versus-3 comparison of core policies and the 14-versus-6 comparison of all twenty policies, respectively.[10]

The two one-to-one comparisons show larger directional revisions for the federal policy around presidential elections, while the two comparisons differ in total variation and the no-change probability revision. In Panel A, the presidential differential in absolute net revision is 2.04 points for the minimum wage, significant at the 1 percent level. The differential in the no-change probability revision is 3.46 points, positive and significant at the 5 percent level. In Panel B, the presidential differential in absolute net revision is 3.38 points for student aid against tuition, significant at the 1 percent level, and that in total variation is 3.16 points, significant at the 10 percent level.

Around presidential elections, households make larger overall and directional revisions for federal policies than for state and local policies, relative to the gap between the two groups in non-election years. In Panel C, the 7-versus-3 comparison of core policies, the gap is 3.79 percentage points larger for total variation and 3.39 points larger for absolute net revision, both significant at the 5 percent level, while the differential in the no-change probability revision, -2.91 points, is not significant at the 10 percent level. In Panel D, the 14-versus-6 comparison, the gap is 4.02 points larger for

[10] In Supplemental Appendix Figures S1 and S2, the no-change probability revision falls around presidential elections for both the federal and the state and local policies. The differentials of Panels C and D net out this common fall.

Table 4: Differences in Revisions for Federal Versus State and Local Policies

	TV	Δp^{same}	ANR
Panel A. Federal minimum wage versus state minimum wage			
ϕ_P	2.1044	3.4588**	2.0438***
	(1.3086)	(1.6065)	(0.7307)
ϕ_M	0.3964	−0.8743	0.4247
	(1.3759)	(1.6539)	(0.7397)
Benchmark-year gap	0.1513	0.7838	−0.2750
Number of pairs		5,647	
Number of respondents		2,881	
Panel B. Federal student aid versus public college tuition			
ϕ_P	3.1585*	−1.7446	3.3822***
	(1.8668)	(2.4032)	(1.2014)
ϕ_M	−0.3504	−2.2663	−0.4045
	(2.0075)	(2.5634)	(1.1755)
Benchmark-year gap	−1.5985	1.4519	−2.2008
Number of pairs		5,025	
Number of respondents		2,695	
Panel C. Core federal (7) versus core state and local (3)			
ϕ_P	3.7898**	−2.9079	3.3949**
	(1.6348)	(1.5981)	(1.1986)
ϕ_M	0.5168	−1.0282	0.4997
	(0.9557)	(1.5816)	(0.5323)
Benchmark-year gap	−0.0140	−0.4388	−0.2749
Number of pairs		26,872	
Number of respondents		3,156	
Panel D. Federal (14) versus state and local (6)			
ϕ_P	4.0219**	−2.5691*	3.5940**
	(1.6346)	(1.4309)	(1.2750)
ϕ_M	0.4552	0.1811	0.2696
	(0.8428)	(0.9594)	(0.5928)
Benchmark-year gap	0.3863	−0.3735	0.2372
Number of pairs		52,235	
Number of respondents		3,230	

Notes: Estimates of Equation (8). Panel A uses the two policies marked [a] in Table 1, Panel B the two marked [b], Panel C the core policies, and Panel D all twenty policies. All columns include the household and regional controls and their interactions with F_k. The benchmark-year gap is the raw mean over the benchmark-year pairs of the federal policies minus that of the state and local policies. In each panel, all three columns use the same respondents and include the policy and year fixed effects, and Panels A and B keep every respondent who has a valid pair on at least one of the two policies. In Panels A and B, standard errors clustered by respondent are in parentheses, and the stars use the t distribution with one less degree of freedom than the number of respondents. In Panels C and D, standard errors clustered by respondent and by policy are in parentheses, and the stars use the t distribution with 9 degrees of freedom in Panel C and 19 in Panel D, one less than the number of policies. *, **, and *** denote significance at the 10, 5, and 1 percent levels.

total variation and 3.59 points larger for absolute net revision, both significant at the 5 percent level. The gap in the no-change probability revision is 2.57 points lower than in non-election years and significant at the 10 percent level. In benchmark years, the gaps of Panels C and D are within half a percentage point of zero. Thus, these differentials in overall and directional revisions mainly reflect a gap between the two policy groups that opens around presidential elections.

Around midterm elections, the gap in revisions between the two policy groups changes little relative to the non-election benchmark. Across all three measures and both the 7-versus-3 and the 14-versus-6 comparisons in Panels C and D, the estimated differentials range from -1.03 to 0.52 percentage points. No midterm differential of Table 4 is statistically significant.

The presidential differentials of the 14-versus-6 comparison in total variation and absolute net revision keep their signs across elections, benchmark years, weights, samples, and policies, although their significance varies. The presidential differentials in absolute net revision of all four comparisons and in total variation of the 7-versus-3 and 14-versus-6 comparisons stay significant at the 5 percent level in every specification of Supplemental Appendix Tables S18 and S19. Each policy group has larger total variation around presidential elections than in its own benchmark years in Figures S1 and S2 and Table S20. Without any one election or benchmark year, under survey weights, and without the pairs whose August answers put all probability on one outcome in Tables S21 and S22, the presidential 14-versus-6 differentials in total variation and absolute net revision stay significant at least at the 10 percent level, while the one-to-one and 7-versus-3 differentials lose significance in some variants. Dropping any one state or local policy leaves the 14-versus-6 differential in total variation between 3.09 and 4.90 points, each significant at the 5 percent level, in Table S23, and reassigning the state and local label to every set of six policies gives permutation p-values of 0.057 and 0.039 for the two measures in Table S24.

5 Concluding Remarks

Households revise their policy expectations substantially more around presidential elections than in non-election years, and the estimated difference from the benchmark is small around midterm elections. Revisions toward a higher average income tax rate are associated with higher own tax expectations and fewer large purchases. Across policy groups, the additional revisions around presidential elections are concentrated more in federal policies than in state and local policies, whereas the change in the gap between the policy groups is insignificant around midterm elections.

The results suggest that households' policy expectations shift selectively around presidential elections, and changes in the perceived policy environment can in turn prompt households to adjust their economic decisions in response to anticipated policy changes before those policies take effect. These anticipatory responses may therefore be more pronounced for economic decisions that are directly exposed to the policies households expect to change.

The comparisons in this paper describe revisions around elections relative to non-election windows, and several extensions could take them further. Each presidential election in the sample changed the party in the White House, and pandemic legislation in 2020 and other policy news reached respondents in the same windows. With more elections, including some that keep the same party in the White House, and with interviews closer to Election Day, future work could separate the election from contemporaneous news and identify its effect on expectations. Information experiments that shift expectations about specific policies could show whether revisions of policy expectations cause the revisions of own tax expectations and the purchases. Asking respondents which level of government they hold responsible for each policy could also complement the assignment of the twenty policies to two groups.

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