Belief Disagreement and Portfolio Choice

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ABSTRACT

Using proprietary financial data on millions of households, we show that (likely) Republicans increased the equity share and market beta of their portfolios following the 2016 presidential election, while (likely) Democrats rebalanced into safe assets. We provide evidence that this behavior was driven by investors interpreting public information using different models of the world by using detailed controls to rule out the main non-belief-based channels like income hedging needs, preferences, and local economic exposures. These findings are driven by a small share of investors making big changes, and are stronger among investors who trade more ex ante.

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The canonical assumption in most economic models is that people have rational expectations with common priors. However, in both the laboratory and the real world, people tend to report different probabilities for the same event, and report updating these beliefs differently in response to common information.[1] A leading interpretation of this evidence is that people believe in different models of the world – they hold different possibly dogmatic priors – which leads them to disagree both about baseline probabilities and about the implications of common information for future outcomes. However, a major challenge in testing this proposition outside of artificial laboratory environments has been to measure behavioral responses that come from changes in beliefs separately from behavioral responses that come through other economic channels or are driven by other dimensions of preferences.[2] In this paper, we test this proposition not by inferring beliefs from reported beliefs or asset prices, but by showing that investors adjust their portfolios differently in response to the same public event and by ruling out the main non-belief channels through which the event may have caused this behavior.

Specifically, we posit that a person's political affiliation measures the model of the world that they use to interpret the economic impact of political outcomes. We then study portfolio differences by political affiliation (inferred from zip code) using anonymized, proprietary data on millions of *Retirement Investors*, defined as retail investors with retirement savings accounts in the middle 80% of the age-adjusted distribution of retirement wealth.[3] Consistent with Republicans (Democrats) reporting to become more optimistic (pessimistic) about future US economic performance following the US national election of 2016, we show that (likely) Republicans rebalanced their portfolios towards equity, while (likely) Democrats rebalanced their portfolios towards relatively safe assets after the unexpected outcome of the election, both over a six to nine-month horizon following the election.

The key challenge for identifying the role of beliefs in this portfolio rebalancing is that, even

[1] Brunnermeier et al. (2021) surveys both the research on, and importance of, non-rational beliefs for asset returns.

[2] We follow the Savage, revealed-preference terminology, and use the term *preferences* to refer to the elements of investor objective functions that are distinct from the *beliefs* that represent subjective probabilities about realizations of states.

[3] Retirement investors make up 40% of US households, and hold 47% of total retirement wealth and 41% of all household investable wealth (see Section III).

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if investors held common priors and updated rationally, the real impact of the election could still differentially change their hedging needs or overall wealth levels. For example, Republican and Democratic voters tend to work in different industries and live in different parts of the country so that their future incomes and costs of living might be differentially affected by the outcome of the election.[4] Such economic differences could generate different behavior not driven by different beliefs. Previous research (discussed subsequently) has documented both different economic impacts of election outcomes by political party and a relationship between reported beliefs and behavior. Our key contribution (along with more accurate measurement) is that we rule out the main channels through which the election may have differentially impacted individuals' portfolio behavior other than through beliefs.

Our main finding is that (likely) Republicans increased the equity share and market beta of their portfolios relative to *otherwise similar* (likely) Democrats, even conditional on detailed controls both for ex ante differences in preferences and endowments and for ex post differences in local economic conditions and income hedging needs. That is, differences in portfolio rebalancing between Republican and Democratic voters following the election are not explained by differences in ex ante wealth, income, age, or characteristics of portfolios. More importantly, rebalancing is also not explained by differential exposure to regional or firm-level policies due to the election outcome: we find different investment behavior comparing investors working at the same employer and living in the same county (that is, after controlling for employer-county-period fixed effects). Further, these changes in portfolio holdings are due to active rebalancing, net of any differences in passive portfolio appreciation. Finally, the outcome of the 2016 election was an unlikely event, the policy differences between presidential candidates were large, and the portfolio share of equity moves similarly for Republicans and Democrats in the year prior to the election, all suggesting that the differential rebalancing was a response to the election rather than other differences correlated with party affiliation.

Having controlled for the main channels through which rational theories operate, we conclude that this rebalancing behavior is due to differential updating of beliefs about the expected return of stock market investments, likely through the performance of the US economy. Consistent with this

[4] As in Pastor and Veronesi (forthcoming) and Autor et al. (2020).

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interpretation, people affiliated with either party reported little change in their views about their own individual economic situations, but reported significantly different changes by party about their expectations of macro-level economic prospects, consistent with Huberman et al. (2018). However, we cannot reject that *arbitrary* preference variation by party affiliation drove the differences in both reported beliefs and investment behavior.[5]

Quantitatively, the average effect of the election on portfolio allocations grows slowly over months following the election, suggesting that investors understand that they disagree with other market participants rather than believing that they have a temporary informational advantage. After a year, the quintile of most Republican zip codes increased the equity shares of their portfolios by roughly three quarters of 1% relative to the quintile of most Democratic zip codes. This relatively small average result masks substantial investor-level heterogeneity. Most of the investors that we observe did not rebalance at all; only a third of investors actively changed their portfolio in any given year and only a tenth of investors changed their allocations across asset classes (consistent with the passivity documented in Madrian and Shea, 2001; Choi et al., 2004). Among the investors who did rebalance, the election had a much larger effect: 75% of our effect comes from investors who changed the equity share of their portfolio by more than 25% after the election. Further, for those investors who actively reallocated their portfolios during the year before our main sample starts, the effect of the election on equity share is three times as large.

This heterogeneity in responses also varies by how actively investors were engaged in their portfolio choices. Wealthier and older investors, who tend to be generally more attentive to managing their savings, rebalanced more strongly in response to the election. And investors who delegate more – who have a larger fraction of their wealth invested in automatic investment products such as target date funds or accounts managed by an advisor – rebalanced by less in response to the election.

As corroborating evidence that differential updating of beliefs drives portfolio behavior, we

[5]Revealed preference theory implies that it is impossible to separate *arbitrary* preferences variation from beliefs. As an example, our findings could be due to Republicans and Democrats consuming different bundles of goods which have prices that are differentially exposed to the outcome of the election even after conditioning on observable factors. For instance (as suggested by a referee), gun control policies could affect the costs of firearms for those who have preference for owning guns.

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follow Curtin (2017) and show that Republicans in the University of Michigan Survey of Consumers (UMSC) reported becoming much more optimistic about the future of the US economy following the 2016 US national election, while Democrats reported becoming more pessimistic.[6] This pattern of reported beliefs is consistent with a substantial literature documenting that political affiliations lead people to report that they interpret public news differently.[7]

Our results also provide some validation that survey-reported beliefs on the macroeconomy contain substantial information about true beliefs that are independent of people's individual economic situations or preferences. That is, by identifying a specific event that changes beliefs and controlling for the main alternative channels through which the event could have changed portfolios other than through beliefs, we confirm that heterogeneous beliefs drive both reported beliefs and differential portfolio allocations. More specifically, our results are in line with the attenuation puzzle – the low correlation between portfolio holdings and reported beliefs – documented in Ameriks et al. (2020), and the finding that changes in reported beliefs do not predict the likelihood of trading but do predict the direction of trading conditional on a trade, documented in Giglio et al. (forthcoming).

Our results support models in which people agree to disagree even in asset markets where prices convey information. As noted, the rebalancing we find occurs over many months following the election, consistent with investors being aware that asset prices do not rapidly incorporate their party-specific beliefs and so knowing that they need not react immediately to this change in beliefs. As additional evidence that supports the predictions of theoretical models with heterogeneous (non-Bayesian) updating of beliefs (Miller, 1977; Harris and Raviv, 1993; Harrison and Kreps, 1978; Morris, 1996; Scheinkman and Xiong, 2003; Banerjee and Kremer, 2010; Simsek, 2013), we show that the total amount of trading among the investors in our sample rises following the election. Rational

[6] A different type of corroborating evidence is provided by research showing that "sentiment" is correlated with risk-taking (e.g. Edmans et al., 2007; Kamstra et al., 2003; Hirshleifer and Shumway, 2003) if one interprets sentiment as subjective expected present discounted utility rather than emotional risk tolerance.

[7] Bartels (2002) and Gaines et al. (2007) document a partisan bias in perceptions of changes in economic performance and military outcomes, and Alesina et al. (2018) shows that political party affiliation determines how new information differentially changes reported beliefs. Subsequent work studies the roles of selective exposure, selective attention, motivated processing, and respondent bias (cheerleading) and incentives (Prior, 2007; Prior et al., 2015; Bullock et al., 2015).

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expectations models of asset prices have been largely unable to match portfolio heterogeneity and the volume of trading.[8] Finally, in a back-of-the-envelope calculation, we show that the change in beliefs and behavior induced by the 2016 election only had a small quantitative effect on the net demand for stocks, on the order of a few billion dollars.

While there are many existing approaches to modelling differences in beliefs, our results are most directly supportive of models that link heterogeneity in beliefs to identity.[9] In particular, building on Akerlof and Kranton (2000), Bénabou and Tirole (2011) develops a model of stake-dependent beliefs in which people's investment choices are affected by what their allocations would imply about their political beliefs. A political identity comes with the belief that the set of policies associated with that identity are good policies. In this context, identity theory predicts that people's portfolios reflect their set of beliefs, so that Democrats (Republicans) trade on the belief that the change in policies following the election will be bad (good) for the national economy and the stock market.

By controlling for county of residence we control for differences in the supply of information, like differences in local newspapers or access to cable news. Thus our findings are consistent with differences in beliefs being "deep" like identity and causing differences in information sources rather than the reverse.

Related literature While there is growing literature on the effect of political polarization in the United States on both economic outcomes and reported beliefs, prior papers have found inconclusive or no results on how elections affect household behavior. Most closely related to our paper, Bonaparte et al. (2017) finds no significant or stable correlation during 1988 to 2000 between investors' party affiliations and the equity shares of their portfolios using self-reported data from the National Longitudinal Survey of Youth.[10] Kaustia and Torstila (2011) shows that left-wing voters in Finland

[8] The variation in portfolio positions and the volume of trade in assets are significantly underestimated by quantitative rational expectations models (Guiso et al., 2002; Calvet et al., 2007; Curcuru et al., 2010). Barber and Odean (2008) documents the transaction cost of over-trading among a sample of active traders.

[9] Examples of more general models include Brunnermeier and Parker (2005), Hansen and Sargent (2010), Gennaioli and Shleifer (2010), and Mailath and Samuelson (2020) for example.

[10] Using account-level data from a brokerage firm, Bonaparte et al. (2017) also finds differences in the composition of equity held related to differences in party affiliation, but without controlling for possible differences in real economic

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are less likely to hold stocks than right-wing voters, but again without controlling for differences in the exposure of labor income or local economic performance to political risks. We advance this line of research both by studying an historically large political shock and by using much larger and more detailed administrative data on millions of typical investors and controlling for the main direct channels through which changes in policies could differentially impact investors. Our work is also related to papers that document differences in professional behaviors by political affiliation (Hong and Kostovetsky, 2012; Kempf and Tsoutsoura, forthcoming). Also consistent with our findings but using professional situation as a measure of people's different models of the world, Linnainmaa et al. (2021) shows that professional portfolio advice correlates with personal portfolio choices across investment advisers (see also Cheng et al., 2014). Again, our contribution is to rule out many ways in which rational theories could rationalize observed differences in behavior unrelated to differences in beliefs.

Our results are also related to research showing that investors seem to incorrectly update in response to idiosyncratic or private information, typically by over-weighting information from their own past experiences or friends (Grinblatt and Keloharju, 2000; Vissing-Jørgensen, 2003; Malmendier and Nagel, 2011; Cookson and Niessner, 2020; Bailey et al., 2018).

There is also ongoing research on whether local aggregate consumer spending responds differently to electoral outcomes depending on the dominant local political affiliation (e.g. Gerber and Huber, 2009, 2010; Mian et al., forthcoming). These analyses focus on measuring local aggregate effects, and do not look to separate the beliefs channel from differences in real economic effects. We contribute (a little) to this literature by analyzing the role of beliefs in differences in saving rates in Section A.

Finally, our paper also contributes to an older literature providing indirect evidence that investors interpret public signals differently. The volume of asset trading rises after public signals (e.g. Kandel and Pearson, 1995), survey beliefs or stated forecasts widen (e.g. Bamber et al., 1999; Carlin et al., 2014), and stock prices move in predictable ways (see the survey Hong and Stein, 2007) that are correlated with economic disagreement (Li and Li, 2021).

exposure that should also cause such differences. We find no evidence of differential changes in the composition of equity held in our data.

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Outline The remainder of this paper is structured as follows. In Section I, we discuss the public event that we study. Section II presents survey evidence on changes in beliefs around the event. Section III describes the data that we use for the analysis of household portfolios. Section IV illustrates our main findings on portfolio reallocation. In Section V, we perform a quantitative analysis where we control for alternative, non-belief-based channels. Section VI discusses the role of portfolio inaction. Section VII examines variation in trading volume. In Section VIII, we study heterogeneity in portfolio rebalancing. Section IX presents additional results. Finally, Section X concludes.

I. The 2016 US Election

The 2016 US national election is almost uniquely suited for measuring how households who believe in different political models of the world update in response to a publicly observable signal.

First, the outcome of the 2016 national election was a very public event.[11] Thus, our results are unlikely to be due to differences in attention. Conditional on observing the outcome, households may process the information at different speeds or take different amounts of time to act, but there is no ex ante reason to believe that this differs by party affiliation.

Second, this election was not correlated with other significant events. Such a correlation would weaken our claim that the differences we uncover are due to different interpretations about a given piece of news about future economic policies and performance.

Third, the outcome of this presidential election, and the Republican party achieving a majority in the Senate, constitute a very large and unexpected change in likely future US economic policy. While all presidential elections affect policy, the 2016 election involved two candidates with quite different policy prescriptions. More importantly, the outcome of the election was unexpected. For most other types of news about the economy or other changes in governance, such as most elections, legislation, and court decisions, information percolates slowly into the economy and the timing of its arrival is hard to pinpoint.

[11] While the outcome of the presidential race was highly unlikely and the main news revealed by the election, we measure the effect of all the electoral outcomes including for example the fact that the Republican party won a majority in the US Senate, an event with roughly even odds prior to the election.

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For a measure of the probability of the outcome of the presidential election, we consider the market prices of two contracts traded on Betfair that pay $1 conditional on the respective party winning the election. State prices of course reflect marginal utilities as well as probabilities, but Betfair is a UK-based internet betting exchange and because of US regulations, it is hard for American investors to enter this market.[12] Thus, these prices differ from probabilities only to the extent that the marginal utility of the UK investors is different across outcomes of the US presidential election. The market predicted a Democratic victory the entire year before the election and with roughly 75% likelihood during the six months prior to the election rising to over 80% on the eve of the election (see Appendix Figure A.1). This stability contrasts slightly with polling data (which is survey-based hypothetical choice data) which has larger swings around events like national conventions and in which the odds appeared close to even at times.

Fourth, we can use differences in political affiliation as ex ante measures of differences in investors' models of the world. This measure is not derived from any economic behavior like portfolio allocation, for which differences might be directly due to differences in preferences like risk aversion or differences in income dynamics and resulting hedging needs. That said, a correlation between political affiliation and preference or hedging needs is of course possible, since beliefs about the economy are not randomly assigned but form endogenously. So our analysis will provide evidence that while there are ex ante differences in the portfolios of Republicans and Democrats on average, the changes in portfolio allocations in response to the election are not driven by these ex ante differences in economic exposure.[13] A related benefit is that we can use other household-level data that measures political affiliation to show how household responses differ along other dimensions, and in particular, we present evidence on survey measures of economic beliefs in Section II.

All four features of this event are important for our study and important to consider in any study

[12] Alternative sources of betting market data on elections are Intrade, the Iowa Electronic Market (IEM), and PredictIt. Intrade shut down in 2013. IEM is open to US households but is capped at $500 and does not trade a contract on the outcome of the election (only contracts based on vote shares). PredictIt also has capped trading, and like the IEM, is relatively small.

[13] It is also the case that political affiliation may be associated with different views of the likelihood of the outcomes of the 2016 election or the policies that are implemented conditional on the outcome. These are subsumed into what we measure.

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of the relationship between beliefs and behavior. In our case, people of different party affiliations are continually responding to all sorts of information and changes in their economic environment. And our hypothesis is that they may be responding differently along many dimensions of identity. Without the ability to differentiate households ex ante and without a large, public signal, we could not separate the belief-driven portfolio response of households to the election from the effects of other factors.[14] As a result, we would not expect to find the same (absolute value of) difference in rebalancing by party that we find in 2016 in response to every US presidential election. In Section D we discuss this point further in conjunction with an analysis of differences in portfolio behavior by party around the 2012 election.

II. Reported Beliefs

Prior research shows that Republicans and Democrats interpret political events differently and that this leads to different reported economic expectations.[15] This section builds directly on Curtin (2016, 2017) which shows that in the University of Michigan Survey of Consumers (UMSC), Republicans report much more optimism about the economy following the election while Democrats report more pessimism.[16] We use both the usual monthly data on reported economic expectations in the UMSC and special questions that the survey added about political affiliation from June to October 2016 and from February to April 2017. The survey does not contain zip code or county of residence, nor can we replicate our sample of Retirement Investors in the UMSC, so instead we analyze the subsample of households that hold stocks (63% of the sample weighted, 65% of the sample unweighted). The results are nearly identical for the entire UMSC sample.

Our first result is that the election appears to have had a dramatic effect on expectations of future national economic performance. Figure 1A shows the average response among people with

[14] To the extent that the arrival of other information also causes households with different political affiliations to behave differently, then it will bias our measured responses. How big is this bias? As we show, during the period before the election, the portfolios of households of different political affiliations behaved similarly, consistent with the arrival of little political news or other news that might be differentially interpreted. See our discussion in Section IV.

[15] See Conover et al. (1987); Bartels (2002); Gaines et al. (2007); Gillitzer and Prasad (2018); Alesina et al. (2018).

[16] Das et al. (2020) finds that reported beliefs about future macroeconomic outcomes are persistently different by socioeconomic status.

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different party affiliations to the question "Looking ahead, which would you say is more likely – that in the country as a whole we'll have continuous good times during the next 5 years or so, or that we will have periods of widespread unemployment or depression, or what?" and we index the answers as Good times = 5, Good with qualifications = 4, Pro-con = 3, Bad with qualifications = 2, and Bad times = 1. On average, Republicans changed from slightly pessimistic before the election to highly optimistic after, and Democrats the reverse. Confirming this difference, Figure 1B shows the same large changes in expectations of the unemployment rate in a year. The reported large changes in beliefs about future national economic performance generate a desire to change exposures to national economic performance in investment portfolios.

[FIGURE 1 ABOUT HERE]

Our second result is that there were no similar large changes in people's reported expectations about their own economic circumstances. Figure 2A plots by party affiliation the average answer to: "During the next 12 months, do you expect your income to be higher or lower than during the past year?" Democrats were more likely to expect higher income before the election, but the advantage is small. Following the election, Republicans on average reported higher expected income and Democrats lower, but the changes are moderate and the ultimate differences small. In contrast, Figure 2B shows the changes in expectations of whether business conditions overall will be better or worse in a year and shows a much larger swing. This result suggests that changes in people's behavior after the election were due to changes in beliefs about the aggregate economy and were not responses to changes in beliefs about their own individual economic situations, such as changes in expected labor income that might make them more or less likely to invest in equity.

[FIGURE 2 ABOUT HERE]

III. Data

A. Party Affiliation

The particular measure of different models of the world that we use is political party affiliation. Our main measure of likely political affiliation is based on publicly available data on individual campaign donations during the 2015 to 2016 election cycle from the Federal Election Commission

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aggregated to the zip code level. We restrict attention to contributions from individuals to political action committees associated with the two main parties or with their presidential nominees and with at least $20 million in donations (see Appendix A for further details). For each zip code, we count the number of donors to each party.[17] We limit our analysis to zip codes with at least 10 donating individuals and measure the *Republican contribution share* of a zip code as the number of donors to the Republican party or the Republican presidential nominee divided by the total number of donors in that zip code. Appendix Figure A.2 shows the geographical distribution of the Republican contribution share.

For robustness, we also confirm that our results hold for three alternative measures of likely political affiliation. First, we consider the dollar-weighted version of Republican contribution share defined as the dollars donated to the Republication party or the Republican presidential nominee divided by the total amount donated in that zip code to either party. Second, we use data on votes for the 2016 presidential election at the county level and define the Republican share of a county as the number of votes for the Republican candidate divided by the total number of votes to both parties' candidates.[18] Appendix Figure A.3 shows that the Republican contribution share and Republican vote share line up well at the county level, despite the limited number of individuals that make individual contributions (the median coverage by zip code is 0.3% of the population). Finally, we use county-level vote shares for the Republican presidential candidate in the 2012 election (eliminating any sui generis effects of particular candidates in 2016). All of our measures surely have some mismeasurement. To the extent that mismeasurement is classical, our main results are attenuated.

B. Household Portfolios

Our main data are anonymized, account-level data on financial holdings from a large US financial institution. We have access to anonymized information on all accounts held directly at the firm by

[17] We find that donations weighted by people are more precise and more differentiating than donations weighted by dollars. Our hypothesis is that this is because the value-weighted donations measure is sensitive to outliers; a single wealthy donor can swing the measure for the whole area.

[18] Data are from David Leip's Atlas of U.S. Presidential Elections at uselectionatlas.org.

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individual investors. For these accounts, we observe end-of-month account balances and holdings, and all inflows, outflows, and transfers at a daily frequency. We observe assets at the security level (identified by CUSIP) for 92% of wealth. For the remaining 8% we observe the characteristics of the fund the wealth is invested in. We aggregate all accounts – pre-tax, taxable, and untaxed – across all members of a household and track household portfolios.[19] The data cover millions of households and trillions of dollars in financial wealth.

We also have access to some information on the characteristics of the investors themselves. In addition to wealth information on all households, we observe age, gender, marital status, and zip code for the vast majority of individuals.[20] We define the head of the household by selecting the individual with the highest total assets (or the older individual if all assets are jointly held), and we use the head's characteristics as that of the household for non-financial information like age and for employment information. We can construct an employer indicator variable for a substantial subset of households. In addition, we use the employer's NAICS code to assign each such household to a three-digit industry. For a subset of these households, we also observe annual labor income of the head of household. Our main sample uses information from one year before to one year after the election, from October 31, 2015 to October 31, 2017.

While this data provides a unique view of retirement savings and the portfolio allocations of US households, there are two potential weaknesses of our data. First, while we observe a significant share of US households, this is obviously not a randomly selected sample. In particular, most observed household wealth is retirement savings and few households have a very high net worth (as we document subsequently). Our analysis does not require a random sample, but we would like to understand the relationship between our sample and the US population. The second potential weakness is that we do not necessarily observe all the investable wealth of the households in our sample. One way this occurs is that we only observe one member of the household for some households. The other is that some households have investable wealth at other institutions.

[19]Where there are multiple households that co-own a given retail account, we assign each account to a single individual by selecting the (oldest) owner with the highest total assets. This yields a unique mapping from investment accounts to households.

[20]The firm data include some variables obtained through a commercial consumer database. Part of the data on marital status comes from this database and we treat entries that were not collected at the household level as missing.

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To address these issues, we select a subsample of our data that meets a set of criteria that is likely to be reasonably representative of the US population that also meets this set of criteria. We focus on households with moderate levels of retirement wealth, which we call retirement investors (RIs). Specifically, we define RIs as households of working age – heads of households between the ages of 25 and 64 – without extremely high or low retirement wealth, defined as all wealth in retirement saving accounts of all types (excluding defined benefit plans and Social Security).

We use the 2016 Survey of Consumer Finances (SCF) to understand how the population of RIs compares to the US population and how well the wealth and portfolio holdings of the RI subsample of our account-level data compare to the RI subsample in the US. We focus on households with some retirement wealth. Using the 2016 SCF, we run quantile regressions of the log of retirement wealth on a second-order polynomial in age. We then drop households with retirement wealth below the estimated 10th percentile or above the 90th by age. This RI subsample of the SCF captures 31.4% of the US population, 32.0% of retirement wealth, and 24.8% of investable wealth held by households in the US according to the SCF.[21] Investable wealth is defined as money market funds (MMFs), non-money market funds, individual stocks and bonds, quasi-liquid retirement wealth, and other managed accounts.[22] Figure 3 shows the wealth distributions of the US population and of our subsample in the 2016 SCF.

[FIGURE 3 ABOUT HERE]

Applying the same cutoffs to our data provides a sample of millions of investors and well more than a trillion dollars in investable wealth.[23] The first panel of Figure 4 shows that the retirement

[21] The age cutoff selects 69.7% of the US population, 65.8% of retirement wealth, and 58.3% of investable wealth according to the SCF. Within the 25–64 age group, RIs are 45.1% of the population and hold 48.6% of retirement wealth (roughly 50% of retirement wealth is held by the top 10%) and 42.6% of investable wealth. For age 30, the 10th and 90th percentile cutoffs are roughly $1,500 and $75,000, and for age 64 they are roughly $8,000 and $1,000,000.

[22] In the SCF, "other managed accounts" includes personal annuities and trusts with an equity interest and managed investment accounts. Excluded categories of financial wealth are checking and savings accounts, saving bonds, certificates of deposit, cash value of life insurance, and other financial assets.

[23] For use in our sample, because we select our sample at the initial date October 31, 2015, we first translate the retirement wealth cutoffs from the SCF at the end of 2016 to cutoffs at the initial date by matching the corresponding quantiles of retirement wealth in our dataset between the two dates. We perform two additional screens in our sample.

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wealth distribution of our RI sample of households lines up well with that measured by the SCF. The second panel of Figure 4 shows that our data also match reasonably well the distribution of total investable wealth in the SCF, but that our data is missing some non-retirement wealth mainly for households with more than $500,000 in investable wealth.

[FIGURE 4 ABOUT HERE]

To characterize portfolio risk taking, we classify fund and security holdings into equity, bonds, cash, and alternative assets (e.g. real estate and precious metals).[24] We calculate market betas by regressing fund and security excess returns on the market excess return over the period from 2006 to 2019, requiring at least 24 months of return observations. We also use other security-level information, such as international and sector exposures. The details are in Appendix B.

Column 1 of Table I shows the details of the portfolios held by our RI sample, as of just prior to the election shock. Our average RI has $138,100 in investable wealth, of which 84% is in retirement accounts. More than 70% of wealth is invested in equity (the sum of directly held equity, equity funds, and the equity amount of funds that invest across asset types), and the portfolios have an average market beta of 0.74 (0.72 if households are weighted by wealth). The second column of Table I shows that, relative to the estimates from the SCF, our data capture most retirement wealth of RIs but, while we match median total investable wealth well, our data misses the mean of non-retirement investable wealth, primarily due to missing wealth among high net worth households (see Figure 4).

[TABLE I ABOUT HERE]

Column 1 of Table II shows that the average age of household heads in our RI sample is 46 years old, 44% of household heads are female, and 73% are married. Our sample of working age

First, we select households with at least 50% of investable wealth in observable portfolio assets. Because of account types, we cannot measure characteristics like market exposures for a limited set of assets. Average holdings in these assets are less than 1.3% of total investable wealth. This restriction excludes only 1.0% of households in our RI sample. Second, we limit ourselves to households that have portfolio holdings between 20% and 500% of initial assets in every month (83% of the full sample). This gives us a balanced panel and drops people who start or stop using the firm during this period.

[24]Holdings in alternative assets are on average less than 1% of total assets and we do not separately analyze this investment class.

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households has a similar age distribution as the corresponding SCF sample of RIs (see Appendix Figure A.4). The average household income is $101,200 and the median is $77,300.

[TABLE II ABOUT HERE]

Figure 5A shows a scrambled map of the share of households in each US county that are in our sample of RIs: we calculate the population share of households that are in our sample by county, and then randomly reallocate the shares across counties in each state to preserve anonymity. Figure 5B shows the density of the share of the population in each county that is in our RI dataset. We remind the reader that RI households represent only a third of the US population (according to the SCF) and that we are using the term household when we in fact may only observe one of two (or more) earning and investing members.

[FIGURE 5 ABOUT HERE]

Finally, we use each investor's zip code of residence one year prior to the election to link investors to our zip code or county measures of political party affiliation. Our sample is tilted towards households that live in Democratic zip codes according to our contributions measure.[25]

Tables I and II show the distribution of asset holdings and demographics by party affiliation of the zip code of the households just prior to the election. There are differences across zip codes associated with party affiliation, but they are generally small. The average equity share varies by less than 1% across groups, with Democratic zip codes having slightly higher equity shares on average. The differences are even smaller for the average market beta of investor portfolios. Nevertheless, in our analysis, we control for pre-election equity share, as well as many individual demographic characteristics.

While both the share of equity and the market beta of the portfolios of households with different political affiliations are quite similar prior to the election, we do find some differences in the composition of equity holdings. While equity portfolios are similar across zip codes in terms of market betas, the share allocated to funds, and the share of international equity holdings, there are differences in

[25] Appendix Figures A.5A and A.5B show the distribution of our households across areas with different political affiliations. While in the regression analysis all households are weighted equally, in the graphical analysis we construct groups symmetrically around the 50% mark. The findings are consistent.

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the sectoral composition of stock holdings and in particular with respect to industry performance immediately following the election.[26] Although allocations to specific sectors are a limited fraction of overall portfolios, we find that households in zip codes with different predominant political affiliations hold significantly different exposures to winning and losing sectors. Table I shows that the most Republican zip codes hold on average 6.5% of their total equity in winning sectors, relative to 4.8% for the most Democratic zip codes. For losing industries, the picture is reversed with the most Democratic zip codes holding more than 4.7% of their equity in losing industries and the most Republican zip codes holding less than 3.5%.

Given these differences it is important that our analysis in the next two sections shows that these ex ante sectoral differences in portfolio holdings do not explain our findings of differential post-election movements in equity shares, trading, and portfolio betas by party affiliation. We do this in three ways. First, we control for these differences in ex-ante portfolio compositions in regression analysis. Second, we analyze price-constant portfolios that exclude all post-election differences in returns from portfolios. Third, this difference in holdings is significantly due to people over-weighting their employer's stock in their portfolios, and much of our analysis compares workers at the same firm.

IV. Household Portfolio Reallocation

This section shows that likely Republican households increased the exposure of their portfolios to US economic growth after the 2016 presidential election relative to likely Democratic households. We show that this effect was not driven by passive appreciation but instead was driven by trading of households that actively reallocated their portfolios. The next section builds the case that this effect is caused by differential updating of beliefs by showing that this finding is robust to many

[26] We use Global Industry Classification Standard (GICS) codes to measure exposure to different sectors. We use the Morningstar benchmark for sector mutual funds and Compustat/CRSP for directly held equity. The sectors are energy, materials, industrials, consumer discretionary, consumer staples, health care, financials, information technology, telecommunication services, utilities, and real estate. We classify an industry as a winner or loser from its stock market response from the end of October through to the end of 2016. The winning industries are financials, telecommunication services, energy, materials, and industrials, while the losing ones are consumer staples, utilities, information technology, health care, and real estate. Consumer discretionary is in the middle and is considered neither.

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controls for differences in preferences or hedging needs across households.

To begin, we simply plot the data. Figure 6 shows the change in the average portfolio share of equities (relative to October 31, 2016) for households in zip codes with different shares of contributions to each party, relative to their baseline shares at the end of October 2016. Figure 6A displays equally-weighted portfolio shares, Figure 6B displays the value-weighted portfolio shares, which sum the value of equity across all households in each group before dividing by total investable wealth for that group.

[FIGURE 6 ABOUT HERE]

First, for all groups, the share of wealth invested in equities decreases prior to the election, which suggests that something caused reduced investment in the stock market regardless of party affiliation.[27] This rebalancing might have been election-related – due, for example, to information revealed during the political race or the approaching uncertainty of the election – or it could simply represent rebalancing after a half decade of relatively good stock market returns. After the election, the equity share rebounds for all groups, primarily due to high stock market returns.

Second, and our main result, the share of wealth invested in equities rose in predominantly Republican zip codes relative to predominantly Democratic zip codes following the election. Specifically, households living in Republican zip codes increased their equity exposure by roughly 0.4% of their wealth relative to those in the most Democratic zip codes, consistent with increased optimism about the economy by Republicans relative to Democrats.

Third, there were minimal differences in the evolution of the portfolio shares in equity across areas with different political affiliations prior to the election. During this time, there was information arriving about the likely outcome of the election, but the total change in probability over this period prior to the election was smaller than that on the one day of the election (see Appendix Figure A.1). The similarity of the movements in the equity share before the election is consistent with our evidence that there are only small prior differences in equity shares across zip codes with different political leanings. These zip codes are surely not the same, but are also not affected very

[27] Roughly two thirds of the pre-election decline comes from allocation decisions of households (see Figures 8A and A.10). Post election, allocation decisions on average are equity neutral.

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differently by economic news (and the limited political news) in the year prior to the election.[28]

We find similar results for long bonds and cash-like securities. Households in zip codes that are predominantly Republican decrease the shares of their portfolios invested in both bonds and cash-like assets (including MMMFs) following the election relative to those in zip codes that are predominantly Democratic (see Appendix Figures A.6 and A.7).

One concern with these results so far is the possibility that these movements may be due to inactivity, initial differences in holdings, and appreciation resulting from the post-election stock market increase following the election. Republicans may hold more equity or equities with higher beta, and so might have had a disproportionate increase in the equity share of their portfolios due to the post-election increase in stock prices. Like most wealth for the typical American household, the majority of the wealth that we observe is retirement wealth which is notably "sleepy." While we address these concerns in a regression framework subsequently, we present three transparent pieces of evidence that reject this hypothesis here.

First, we find larger differential effects by party affiliation when we focus on households that are more active in the past, where an "active" household is defined as one that had at least one monthly portfolio reallocation due to trading that changed the equity share by at least 5% (in absolute value) during the year November 2014 to October 2015 (the year prior to our main sample). This sample is 11.1% of our original sample (see Table II). Figure 7A shows that active investors in mostly Republican zip codes had a much larger relative increase in equity share after the election, on the order of 1.5% of their investable wealth on average. (Appendix Figures A.8A and A.8B show the corresponding plots for long bonds and safe assets, respectively, for the sample of active investors.) These effects translate into differences in the overall market betas of portfolios by likely political affiliation. Figure 7B shows that movements in market betas of active investors are also very similar prior to the election across political affiliations, and diverge post election, with market beta rising relatively more in Republican zip codes post election.

[FIGURE 7 ABOUT HERE]

[28] Figure 6A does show a small rise in equity share in Democratic zip codes relative to Republican zip codes prior to the election, which is consistent with the slowly increasing probability of a Democratic victory in the presidential election during this period.

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Second, we measure the difference in active rebalancing in two different ways. The composition of an investor's portfolio is changed by revaluations, reallocations, and inflows and outflows related to deposits and withdrawals. We measure rebalancing into equity by focusing on changes in equity share driven by either transfers of assets into or out of equity or withdrawals or inflows that change equity shares.

Our first method to measure rebalancing is simply to construct and track hypothetical portfolios as if there were no changes in valuations of funds or securities. Figure 8A plots the equity share of these hypothetical price-constant portfolios and shows that prior to the election, households were rebalancing out of equity in a nearly identical way across the distribution of zip codes by political affiliation. After the election, Republican zip codes actively increased their equity shares by reallocating wealth from safe assets into equity, while primarily Democratic zip codes decreased their equity shares by reallocating wealth out of equity into bonds and cash. The total reallocation is roughly the same size as the total difference shown in Figure 6. Note that in Figure 8A we also observe mostly Republican zip codes reducing their equity shares slightly more than other zip codes over the year prior to the election, which is consistent with the small amount of news that comes out pre-election as the chances of a Democratic victory rise. Figure 8B again shows similar patterns for market betas before and after the election. (Appendix Figures A.9A and A.9B show the corresponding plots for long bonds and safe assets, respectively, of price-constant portfolios.)

[FIGURE 8 ABOUT HERE]

It is worth noting that while Figure 6 shows that party affiliation and the election do not explain much of the temporal variation in zip code between the most Republican and most Democratic zip codes, most of this is due to passive behavior and market returns. Removing market return, Figure 8A shows a much higher share of the temporal variation in equity share between these groups is driven by political variation in beliefs.

Second, we measure excess equity trading for household i in month t as

$$Excess\ Equity\ Trades_{i,t} = \frac{Equity\ Trades_{i,t} - Equity\ Share_{i,t-1}\, Total\ Trades_{i,t}}{Assets_{i,0}}, \tag{1}$$

where $Equity\ Trades_{i,t}$ is the sum of all transactions in equity securities by household i in month

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t (positive if buying, negative if selling), $Equity\ Share_{i,t-1}$ is the equity share at the end of month $t-1$, $Total\ Trades_{i,t}$ is the sum of all transactions in all securities by household i in month t, and we scale by initial household assets, $Assets_{i,0}$. Appendix Figure A.10 shows cumulative excess equity trades from the end of October 2015. We again find that the relative movement in portfolio share was driven by rebalancing and not by ex ante differences in portfolios and differences in post-election performance. Appendix Figures A.11A and A.11B show the same plots for excess bond and cash trades (defined analogously).

While these results present the data in a transparent manner, they do not show that changes in beliefs were the driving force for these reallocations rather than differences in preferences or differential changes in hedging needs due to differential changes in the stochastic process of labor income or the local local economy. Much existing research relates reported beliefs to economic behaviors (see Manski, 2018) and to portfolio choice in particular (e.g. Vissing-Jørgensen, 2003; Ameriks et al., 2020; Giglio et al., forthcoming). In general, however, many factors change both behavior and beliefs, and so may contaminate any analysis of the effect of beliefs on behavior. Because we have identified a particular source of variation in beliefs, we can address the main ways in which the election may have directly impacted behavior other than through beliefs. For example, the election outcome raised the probability of reductions in personal and corporate tax rates, changes in personal tax deductions through itemization, increases in barriers to trade, and reductions in various regulations, and the stock prices of companies in different industries were impacted quite differently. To the extent that affiliates of one party or the other are impacted differently by these policies, or tend to work for or live near winning industries, then the political outcome may differentially affect their future incomes and cost of living. The next section provides evidence that the differences in rebalancing are not driven by the different economic effects of the election but rather by different interpretations of the election. These results thus lend credence to the informativeness of reported expectations data.

V. Quantitative Analysis Controlling for Alternative Channels

To control for differences in economic outcomes and hedging needs due to the election result and to quantify the differential changes in portfolio allocations across households, we now run a set

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of regressions of changes in household equity shares on the Republican share of donations at the zip code level. We continue to study portfolio changes between one year before the election and one year after the election. We estimate an equation of the form:

$$\Delta P_{i,t} = \sum_s (\beta_s R_{z(i)} + \theta_s' X_{i,t-1}) 1_{s=t} + \tau_t + \eta_{i,t}, \tag{2}$$

where $\Delta P_{i,t} \equiv P_{i,t} - P_{i,t-1}$ is the change in household i's portfolio share in equity between time $t-1$ and t, $R_{z(i)}$ is the time-invariant Republican share of donations in zip code $z(i)$, $1_{s=t}$ is an indicator variable which takes the value 1 when $s = t$, and τ_t is a period-specific intercept. The controls $X_{i,t-1}$ include the lagged equity share and other covariates that are either time-invariant or measured at $t-1$. Standard errors are clustered at the zip code level.

It is important to emphasize that the effects of our control variables are allowed to be different in every period, just like the effect of the Republican share. Thus, when we control for a variable, we are not controlling for its average effect pre-election and post-election, but we are controlling for differences related to this control variable in the specific period.

A. Quarterly Portfolio Changes

We start our quantitative analysis by running regressions in quarterly first differences. We use portfolios exactly one year before the election to construct initial positions and create a sample of portfolio holdings every three months, starting with the end of January 2016 and ending with the end of October 2017, so we have four observations of portfolio changes before the election (denoted $t = -3, -2, -1$, and 0 for October 31, 2016) and four observations after the election ($t = 1, 2, 3, 4$) for each individual.

To show that the differences in portfolio rebalancing that we observe are due to differences in beliefs rather than differential economic outcomes or hedging demands, we make use of many control variables, $X_{i,t-1}$, related to differences in preferences, incomes, other wealth, or the ways these factors are exposed to economic policies. Our baseline control variables include the lagged portfolio equity share, age, gender, marital status, log lagged financial wealth, and log labor income in 2015, which are as noted all interacted with quarterly indicator variables. These controls are

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designed to confirm that our quantitative results are not biased by initial differences in wealth and portfolios that might be due to heterogeneity in, for example, risk aversion, as investors that differ ex ante in endowments or risk preferences select different portfolios and are therefore differentially affected by the election outcome. Portfolio allocations are also significantly mean reverting, which is most obvious at the extremes of the distribution of initial portfolio shares, since shares are bounded by zero and one, but happens across the whole distribution. We additionally control for ex-ante differences in sectoral allocations (as discussed at the end of Section III) and for zip code house price growth between 2010 and 2015 in the baseline. We use the all-homes Zillow Home Value Index (ZHVI) as a measure of house prices at the zip code level.

Panel (a) of Figure 9 shows the cumulative change, relative to the end of October 2016, in the equity share of households in Republican zip codes relative to those in Democratic zip codes from this baseline regression.[29] During the first three months following the election, people in a zip code with only Republican donations increased their holdings of equity by 0.40% of their portfolio relative to a zip code with only Democratic donations, rising to 0.65% by the end of the second three-month period following the election. The cumulative effect further increase slightly between six and twelve months after the election.

[FIGURE 9 ABOUT HERE]

Since equity shares also reflect changes in asset valuations and do not only reflect active decisions by the account owners, we also report the results of the same baseline regression but now using the hypothetical price-constant equity share measure from Section IV as outcome variable. This measure is purely driven by active rebalancing decisions. Panel (b) of Figure 9 reports similar effects for this measure. We will use the transactions-based price-constant equity share measure as our main outcome variable in later tests.

Finally, we control for any possible channels through which the election outcome and the dynamics of future policy might impact investors differentially through their employer or locality and so change their portfolio behavior other than through beliefs. We do so by including a fixed effect for each employer-county pair at each point in time. This version of equation (2) then compares, in each quarter, investors working at the same firm and living in the same county while still controlling

[29] The cumulative change at time t, relative to just before the election, is defined as $\sum_{s=-3}^{t} \beta_s - \sum_{s=-3}^{0} \beta_s$.

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for the effect of all the individual-level characteristics in that quarter. Panel (c) of Figure 9 shows the effects of the election outcome on investors' equity shares controlling for these employer-county-period fixed effects. These quarterly regressions show the same impact of the election on portfolios: Democrats reduced their holdings of equity while Republicans increased theirs. Panel (d) of Figure 9 shows that these results are due to differences in active trading and not due to differences in passive appreciation.[30]

B. Annual Portfolio Changes

Having shown the dynamics of equity shares around the election in quarterly regressions, we now switch to regressions in annual first differences for our subsequent analyses for expositional convenience. Thus, the regression sample consists of two observations for each individual: the portfolio change in the year prior to the election and the portfolio change in the year after the election. We estimate the following version of equation (2):

$$\Delta P_{i,t} = \beta_0 R_{z(i)} + \beta_1 R_{z(i)} Post_t + \theta_0' X_{i,t-1} + \theta_1' X_{i,t-1} Post_t + \tau_t + \eta_{i,t}, \tag{3}$$

where $Post_t$ is an indicator variable that takes the value 1 at year $t = 1$.

Table III reports estimates of equation (3) for changes in the equity share of price-constant portfolios (the corresponding findings for realized equity shares are in Appendix Table A.IV). For consistency, we restrict the regression sample to RIs for which we observe a complete set of controls, so that sample sizes do not change across columns. This subsample of households with complete controls forms 27.7% of the full RI sample. In Appendix C.2, we show that our findings are not driven by this sample selection and that the results extend to less restrictive samples.

The first column of Table III reports estimates for the specification without any controls $X_{i,t-1}$. As before, we find that likely Republican households significantly increased their equity holdings relative to likely Democratic households after the election shock. In the remaining columns, we show that the main effect remains very similar as we control for a large number of variables that

[30] Appendix Tables A.II and A.III report the estimated coefficients in these regressions of quarterly changes in equity shares and price-constant equity shares, respectively, for various sets of controls.

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measure possible differences in wealth or hedging needs that might be correlated with the political affiliation of zip code of residence.

[TABLE III ABOUT HERE]

The second column shows the results for the baseline set of controls. We report the main coefficients on political affiliations, as well as the coefficients on the most important controls. In the year prior to the election, likely Republicans mildly reduced their equity share relative to likely Democrats. After the election, likely Republicans significantly increased their equity share relative to likely Democrats. The total post-election difference is 0.8, matching the previous cumulative effect from quarterly panel regression with individual fixed effects as reported in panel (b) of Figure A.17.

To control for ex-post differences in wealth growth, the third column includes ex-post realized income growth from 2016 to 2017 and house price growth between 2015 and 2017. Columns (4) and (5) control for any remaining differences in hedging needs due to employment opportunities. Column (4) includes an indicator variable for employer industry (3-digit NAICS).[31] Column (5) takes this control one step further by including indicators for each employer in each year. By controlling for employer effects, we absorb any differences across households that are due to Republicans and Democrats working for different firms and therefore are differentially affected by the election outcome in their expected labor income or due to differences in retirement investment menus.

Columns (6) and (7) show that we still find that Republicans increased their relative equity share when we control for detailed locational indicators. These indicators capture differences in local economic conditions and the local effects of various economic policies that lead to ex-post heterogeneity in the effects of the election outcome on wealth and hedging needs. Specifically, we control for regional variation with indicators for state and county. These indicators account for potential differences in hedging needs related to the real effects of trade or immigration policies, by absorbing differences in urbanicity, manufacturing shares, and firms' exposure to import competition (Feenstra, 1996; Barrot et al., 2019). It is worth emphasizing again that, as in the previous columns, all controls are interacted by annual indicators. The locational controls do not do much to decrease the magnitude of our main finding.

[31] The results are similar for 2-digit and for 4-digit NAICS controls.

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Ultimately in column (8), we control for the main ways in which rational hedging demand might change differentially in response to the election through the investor's employer or through their local economy. We still find the same differential trading behavior when we control for employer-county-period fixed effects by including an indicator for each county interacted with each employer interacted with each annual period. This specification compares people in the same year working for the same employer and living in the same county but living in zip codes with different party affiliations. The magnitudes of our main effects are highly significant although somewhat reduced in magnitude, but this may occur because these indicator variables may be absorbing some valid variation, since our main measure of the effect of beliefs is a noisy measure of party affiliation at the zip code level.

The results in this last column of Table III have interesting implications for the interpretation of our findings related to work on differences in media and political beliefs, as discussed in the introduction. By controlling for county, we are largely controlling for access to information since people in the same county likely have access to the same media for example. Thus, to the extent that the variation in beliefs across zip codes within a county is related to differences in the consumption of media, then, these differences in media represent differences in the demand for certain types of information. That is, we are not measuring the effect of exogenous differences in media consumption, but the expression of individual (or rather zip code level) beliefs.[32]

Our main results are quite similar for our alternative measures of likely political affiliation in 2016 and for the 2012 Republican vote share by county (see Appendix Table A.V). Thus, our results are not driven by something specific to donations (rather than votes) or by something particular to the candidates in 2016 (although we do not have a way to adjust for differences in turnout). At the one-year horizon with employer-state-period fixed effects, we find no statistically significant evidence that changes over time in the intensity of county support matter above and beyond the 2012 vote shares.

[32] Although beliefs may be amplified or perpetuated by choice of media, they still represent choices driven by initial political identity. Note however that our controls for supply are incomplete in the sense that new media platforms (e.g. Twitter, Facebook, etc.) target at the individual level with little relation to geography. See the related work of Gentzkow and Shapiro (2006), Mullainathan and Shleifer (2005), and Martin and Yurukoglu (2017).

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In sum, the regression analysis of this section confirms that the public signal of the 2016 election caused likely Republicans to increase their portfolio exposure to US growth relative to likely Democrats, and that we find this behavior even when controlling for many measures of differences in the real economic impact of the election, such as only using variation within people living in the same county and working for the same employer. These results are consistent with survey beliefs reported in Section II: households that are more optimistic on the future growth of the US economy increase their exposure to assets that load on future economic performance.[33]

The relative changes in portfolios that we find are not quantitatively large. In part this is because we do not observe individual-level political affiliations, so that even quite Republican or Democratic zip codes by our measures contain a mixture of Democratic and Republican investors. However, in part our effects are not large because most of the wealth of retirement investors is retirement wealth, and there is very little (active) trading in retirement accounts. As prior research has shown, retirement savers in the US largely stick to their default portfolio allocations and trade very rarely.

Due to infrequent rebalancing, the economic magnitudes of the coefficients of portfolio changes are modest on all explanatory variables. The coefficients on demographic variables such as age, wealth, income, gender, and marital status are statistically significant, and likely pick up differences in economic impacts of the election outcome as well as differential changes in beliefs beyond what is captured by zip code affiliation. However, Table III shows that compared with these key investor characteristics, we find that the coefficient on likely political affiliation is large. For example, in column (8) of Table III, the magnitude of the post-election difference between likely Republicans and likely Democrats is equivalent to an age difference of 36 years, a difference in wealth of 377 log points, and a difference in income of 445 log points in the election response. Zip-code-level regressions in Table A.XIII (further described in Appendix C.3) also show that apart from initial equity shares, zip code political affiliations are the most important variable for explaining post-election changes in

[33]Consistent with the survey evidence on beliefs as well as with theoretical models of belief heterogeneity, we interpret our findings on portfolio allocations as the result of disagreement on expected stock market returns. It is however also possible that the results (partially) reflect heterogeneity in beliefs on higher-order moments such as expected volatility or tail risk in asset returns.

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portfolio allocations across zip codes. However, most of the variation in portfolio changes is due to idiosyncratic factors, especially at the individual level – the overall R^2 in Table III is 6.7% without fixed effects and 18.9% with employer by county fixed effects.

In the next section, we show that our estimated main effect is much larger among households that do reallocate their wealth.

VI. Inaction and Large Portfolio Changes

Having established our main results, we now show that the portfolio reallocation is due to a small share of active investors who make large reallocations in their portfolios.

First, we focus on the effect among active investors. Table IV performs the same analyses as Table III on the subset of the population with an active portfolio reallocation during the year prior to the pre-election year. An active portfolio reallocation is defined as an equity share change of at least 5% caused by trading in a particular month. As in our graphical analysis, we find much larger differential portfolio movements among active households. The relative increase in equity share among likely Republicans who had made active reallocations is three times as large as that for the general population of RIs.

[TABLE IV ABOUT HERE]

We also analyze several other categorizations of more active or engaged investors, such as those invested less in target date funds and those with greater past access and trading behavior in their accounts. We relegate these results to Appendix Table A.VIII, but we find that the more active the investors are that we include in the sample, the greater are the differences in reallocation. Notably, relative reallocations were much greater – five times the baseline effect – among investors who made active reallocations in each of the three years prior to the election (1.7% of RIs). Investors who were less engaged, for example those who have a significant part of their assets in target date funds or advised accounts, responded less than the average investor.[34]

[34]Instead of classifying investors by their engagement ex ante, we can also look at portfolio reallocation conditional on active rebalancing ex post. Appendix Table A.IX shows the results of regressing annual portfolio changes on political affiliation and various controls for the sample of investors that actively reallocated their portfolios in that particular year. Not surprisingly, we find even larger effects.

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We next turn to measuring the size of the portfolio adjustments made by the investors that actively rebalance. As in our previous analysis, we continue to focus on hypothetical portfolios constructed as if there were no changes in valuations of funds or securities. This allows us to cleanly avoid changes in equity share driven by different returns on different investments. Figures 10A and 10B show the changes in equity shares as in Figure 8A, decomposed into small and large portfolio changes. In the upper panel we take the average of (hypothetical) equity share changes relative to October 2016 interacted by an indicator for the change being at least 10%. In the lower panel we multiply the (hypothetical) equity share change by an indicator for the change being at most 10%. There is no noticeable differential active adjustment among the investors who made only small adjustments and effectively our entire measured effect comes from investors who made adjustments of greater than 10%. In terms of what share of investors are making these large trades, in each month following the election, roughly 1% of investors made a trade of greater than 10%.

[FIGURE 10 ABOUT HERE]

To further characterize the extent to which our results are driven by large reallocations, we re-run our main analysis on the subsample of investors whose cumulative adjustment over the year following the election exceeds X%, for different values of X. Strikingly, only about a quarter of the total relative portfolio reallocation came from investors who adjusted the equity shares of their portfolios by less than 25%. A full half of the measured effect comes from the few households that increased or decreased the equity share of their portfolio by 50% or more. We display the complete set of results in Appendix Figure A.13A.

What share of investors were making these large changes? Aggregated across the full year, 9.6% of our RI sample reallocated so that their equity share changed by at least 10%, 4.3% reallocated to change their equity share by at least 25%, and 1.7% made a change of at least 50%.

We further find that rebalancing at the individual level is lumpy. Investors tend to make only one large trade. Examining the cumulative difference in reallocation between the most Republican zip codes and the most Democratic zip codes, we experiment with dropping all adjustments made by investors after their first rebalancing of 10% or greater. Almost the entire effect comes from the first large adjustment made by each investor (as shown in Appendix Figure A.13B). In sum, effectively no Republican and Democratic investors actively changed their equity shares by only a

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few percentages in response to the election. Instead, a small share of investors made very large changes in their portfolios in response to their different changes in their beliefs.

These results are consistent with previous studies which show that the typical American investor, whose wealth is primarily in retirement funds, trades very rarely (e.g. Choi et al., 2004). The interesting question is: why? The majority of wealth in our sample is retirement wealth, which has no fees associated with trading. Thus large monetary costs are an implausible explanation. Instead, inaction seems to be due to psychological or informational costs. This interpretation is supported by our finding that investors who were more engaged with their portfolio allocations respond more strongly, and that those who had delegated or were less engaged responded less. While it is possible that only a few households updated their beliefs following the election, this interpretation is inconsistent with the evidence from survey beliefs in Section II.

VII. Trading Volume

A main motivation and implication of models with time-varying heterogeneous beliefs is that they generate trade among agents, something that the canonical model is either silent about or, when calibrated, tends to vastly under-predict. In this section we show that trading volume rose significantly following the election for our RI sample, consistent with these models and our interpretation of the election as a public shock that caused different movements in different households' beliefs.

We measure trades as the absolute value of the dollar amount of every purchase or sale of any security s, and define the trading volume for household i in month t as:[35]

$$Trading\ Volume_{i,t} = \frac{\frac{1}{2}\sum_s(|Buy_{i,s,t}| + |Sell_{i,s,t}|)}{Assets_{i,0}}, \tag{4}$$

where $Buy_{i,s,t}$ is the sum of all buying transactions for household i in month t of security s, $Sell_{i,s,t}$ is the sum of all selling transactions for that security by the household in that month, and the total amount of trading is scaled by initial household assets $Assets_{i,0}$. Since this trading includes

[35] Trading volume is winsorized at 100% to remove sensitivity to a small number of extreme outliers. This affects less than 0.5% of the sample.

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purchases and sales from inflows and outflows, we also construct a measure of active trades, which are trades limited to rebalancing across assets, excluding those associated with inflows and outflows.

Figure 11A plots average trading volume over time and shows that the volume of trade by our sample of retirement investors rose significantly following the election. Their average trading volume was low (relative to turnover in the overall stock market), roughly 2.25% per month prior to the election. Active trades were roughly 1% per month. Trading increased significantly in the month following the election, reaching more than 3% per month in March 2017. Figure 11B shows that there was little difference in trading volume across the distribution of political affiliation in zip codes, including those that are most politically balanced. This similarity suggests that the Republicans and Democrats in non-homogeneous zip codes were both trading, just in opposite directions.

[FIGURE 11 ABOUT HERE]

In contrast, there was no noticeable increase in trading activity in US equity markets overall, even in the submarket for ETFs. We confirm this both with total US equity market volume and with the narrower ETF market volume, both from the CBOE (we plot these series in Appendix Figures A.14A and A.14B). In some sense, this is not surprising because our typical American retail investors trade very little. Thus, while the increase in trading that we study was a noticeable increase in their trading volume, it made only a trivial contribution to total trading volume. This observation also implies that the political-party-driven trading in and out of the stock market by institutional or high net worth individuals is also a trivial share of total trading volume.

We conclude that the evidence on trading volume supports our hypothesis that this public event was interpreted differently by people with different models of the world, but we also note that this particular source of different beliefs can generate only a small amount of observed equity trades.

VIII. Differences across Households and Accounts

Which characteristics of investors are associated with the largest differential responses to the election news? We characterize differences in the effect of the election by running triple difference regressions to measure differential effects by prior-year active rebalancing, age, wealth, and starting equity share. We report the coefficients on the Republican contribution share and its interactions in Table V, where age, log wealth, and lagged equity share are all demeaned.

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[TABLE V ABOUT HERE]

The interaction of Republican contribution share with active reallocation in the prior year in column (2) highlights and confirms our previous results. The response in the equity shares of households with active rebalancing in the prior year is almost four times as strong as those who were not active. Turning to age in column (3), older households – those closer to retirement but also with more wealth and income – have more differences in their portfolios by political affiliation than younger households. Relative to a base difference of 0.87%, a ten year higher age is associated with an additional difference in equity share changes of 0.51% between likely Republicans and likely Democrats. Column (4) shows that equity share moves relatively more for wealthier households, with a doubling of financial wealth implying a 0.25% greater relative increase in equity shares by likely Republicans, and a doubling of income implying a 0.43% greater relative increase in equity shares. The difference in reallocation is larger for investors with relatively low starting equity shares, as shown in column (5). With the full set of demographic interactions in column (6), age becomes less important, while heterogeneity by financial wealth, income, and initial equity allocation remains highly significant.

Next, do households rebalance differently depending on the type of account they own, or do we see differences across households that own different types of accounts? We find that there are significant responses across all types of accounts, with larger responses for people with non-retirement accounts but more rebalancing in retirement accounts. Table VI shows how the responses differ across investors or different types of accounts held by RIs at the firm. The first column presents our main results again, which largely represent the response in retirement accounts since this is the vast majority of wealth held by our RIs. In the second column, we restrict the sample to households that own personally advised accounts.[36] Both pre- and post-election coefficients on political affiliation are somewhat larger, but with very large standard errors when we insist on including employer-county-year fixed effects. The third column shows a slightly lower but still highly significant estimate for the subset of households with a single, unmarried household head. Because we may only observe one member of a household, we are more confident that we observe the full portfolio of unmarried investors. A possible explanation for the lower magnitude in this

[36] A common financial adviser line is "keep your politics out of your portfolio."

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sample is that these are typically younger investors with lower balances and a higher initial equity allocation.

[TABLE VI ABOUT HERE]

In the final five columns, we draw a distinction between retirement and non-retirement account behavior and owners. Given the prominence of retirement wealth in our sample, we find similar results when we restrict attention to only retirement wealth (column (4)). We find that investors that own non-retirement accounts (column (5)) tended to trade more than people without (column (6)) and therefore show bigger responses, but for these investors we find less reallocation in their non-retirement accounts (column (7)) than in their retirement accounts (column (8)), presumably for reasons of both lower transaction costs and no immediate tax ramifications (see Dammon et al., 2001). Hence, we find no evidence that reallocation happens more in non-retirement accounts that may offer more flexibility in choosing asset allocations or have shorter investment horizons.

IX. Extensions

A. Household Saving Behavior

Gerber and Huber (2009, 2010) began a literature in political science studying whether local economic activity is affected differently by elections in localities with different political affiliations. These papers and the subsequent literature has primarily focused on consumption spending (McGrath, 2017; Benhabib and Spiegel, 2019; Gillitzer and Prasad, 2018; Mian et al., forthcoming). While there is disagreement across the papers, the balance of this evidence suggests little to no differential effect of the election on local consumption levels across areas with different dominant political affiliations. This finding is striking in part because one would expect differential real effects of the election outcome through policy on the current and future economic circumstances of people with different political affiliations. In contrast to this literature, we measure individual-level behavior controlling for the real effects on the local economy and labor income.

In this section, we investigate saving using three measures of inflows into accounts. We show that there is some weak evidence that the typical Republican retirement investor increased their savings in investment accounts by a small amount following the election, relative to their Democratic

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counterparts.

First, we investigate a measure that exists for all RIs in our sample, the net flow rate, defined as net account inflows less account outflows as a share of total initial balance:

$$Net\ Flow\ Rate_{i,t} = \frac{Deposits_{i,t} - Withdrawals_{i,t}}{Financial\ Wealth_{i,0}}. \tag{5}$$

Figure 12 shows the average of this saving rate by zip code bin at a monthly frequency and reveals no noticeable differences by likely party affiliation. We also run a regression similar to that in Section V, but using the year-on-year change in annual net flow rate as the dependent variable. Columns (1) and (2) of Table VII show that Republican households, who become more optimistic, increased their saving rate relative to Democratic households following the election. Including employer-county-period fixed effects has a big impact on the estimates – the pre-election trend disappears and the overall effects are smaller.

[FIGURE 12 ABOUT HERE]

Second, at some loss of sample size, we can instead measure a saving rate more directly by defining the net saving rate as net inflows divided by income:

$$Net\ Saving\ Rate_{i,t} = \frac{Deposits_{i,t} - Withdrawals_{i,t}}{Income_{i,t}}. \tag{6}$$

This has the obvious advantage of being a more standard measure of the saving rate but limits our sample to RIs for which we observe annual income for the head of the household. Columns (3) and (4) of Table VII again show evidence that Republican households saved more relative to Democratic households following the election, with no pre-election differences once we control for employer by county by year fixed effects. The effect on political affiliation is economically small but not trivial, on the order of three quarters of 1% of income. Columns (5) and (6) of Table VII show similar results when we limit financial flows to accounts owned by the head of the household for whom income is measured.

[TABLE VII ABOUT HERE]

A disadvantage of these first two measures of saving rates is that they have a lot of variation over time and across people because of large withdrawals and large inflows, presumably both significantly

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due to transfers out of and into the financial institution rather than due to actual saving. To address this issue, we measure the retirement saving rate as the contribution rate (as a percentage of income) chosen by the head of the household in his or her active retirement account. The advantage of this measure is that it avoids account inflows and outflows that represent transfers from and to other institutions. This measure still does not avoid the possibility of substitution between retirement saving and non-retirement. Columns (7) and (8) of Table VII show that in the year prior to the election, the contribution rates of Democratic investors rose faster. Most of this difference can be explained by employer effects. There is no statistically significant difference in contribution rate changes post election by party affiliation. The effects are also economically small, on the order of 0.05% of income.

We take this evidence as suggestive of only very small effects of the election on households' saving rates. If this were also true of consumption spending, this would have two implications. First, and less important given our extensive controls, a non-response of consumption further mitigates the concern that differences in the economic effects of the policy changes cause the differences in portfolio responses that we find. Second, only certain utility functions are consistent with differences in beliefs that cause differences in portfolio exposures to risk without causing differences in consumption changes. Specifically, these findings are consistent with offsetting income and substitution effects.

The UMSC data also supports this interpretation. As we showed in Figures 2A and 2B, in contrast to the large changes in beliefs about the future of the US economy, there are only small changes in what households reported expecting about their own personal economic situations. Here we show some evidence that Republicans and Democrats did not change their reported views about whether it is a good time to consume.

The UMSC shows only small differential changes in responses to the questions: "Generally speaking, do you think now is a good or a bad time for people to buy major household items / a house?" (see Appendix Figures A.15A and A.15B). Consistent with the strong economy, most people believed it was a good time to buy a house or major durable item before the election, with Democrats being slightly more optimistic than Republicans only for the purchase of a durable item. This difference went away after the election, with beliefs about the purchase of a durable item similar by party affiliation. For the purchase of a house, Republicans and Democrats held similar

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views prior to the election and Republicans became slightly more optimistic following the election.

B. Differences in the Composition of Equity Holdings

The quantitative analysis so far has concentrated on the equity share as a summary measure of investor portfolios. Figure 8B shows that the market beta of price-constant portfolios of Republicans also rose relative to Democrats after the election. Consistent with these raw effects, columns (1)–(3) of Table VIII find that likely Republicans increased the market beta of their portfolios relative to likely Democrats in the post-election year under the full set of controls. Next, we turn to a more detailed decomposition of investor portfolios. We find that differential portfolio reallocation after the election was concentrated on rebalancing across asset classes.

[TABLE VIII ABOUT HERE]

First, to what extent were differences in the portfolio market beta of people with different political affiliations due to differences in the market beta of the equity portfolio? Figure 13A shows both minimal differences in the market beta of equity by party affiliation and only small relative changes in the beta of equity following the election. Columns (4)–(6) of Table VIII quantify these differences. Within equity we do not see an increase in the market exposure of Republicans relative to Democrats, controlling for ex-ante differences. This finding is consistent with our focus on the rise in the equity share of Republicans relative to Democrats in our main set of results.

[FIGURE 13 ABOUT HERE]

Second, do we see differences in the post-election reallocation across sectors? Stock market performance differs substantially across stocks and sectors expected to benefit from the unexpected change in party control (Wagner et al., 2018). We have seen that there are differences in the ex-ante composition of household equity holdings, but controlling for this heterogeneity does not alter our main results. In addition, we find no evidence of heterogeneous reallocation after the election. Columns (7)-(10) of Table VIII show insignificant coefficients for changes in the winning and losing sectors share of equity on the Republican contribution share. These results suggests that investors of different political affiliations do not disagree on the relative impact of the election on different sectors of the economy, and are inconsistent with the explanations of our main finding that equity

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share changes differ by party affiliation through differences in information or tastes. If Republicans were better able to identify firms or sectors that do well after the election or have more taste for holding those types of assets, we should observe significant differential changes in the composition of equity portfolios following the election. We do not observe such changes (see also the plots in Appendix Figure A.16).

Third, do differences in beliefs about the future state of the US economy translate into differential shifts into and out of international equity investments? We find no evidence of differential changes in international equity allocations.[37] Figure 13B shows the share of international equity across households sorted into quintiles by party affiliation of their zip code, relative to the end of October 2016. Prior to the election there was a small relative decrease in the international share held by the most Democratic zip codes and there was a similar small relative decrease following the election. Columns (11) and (12) of Table VIII show that in the regression analysis with full controls, there were no significant differential changes in the international equity share of Republican areas relative to Democratic areas.

C. The Impact of Changes in Beliefs on Net Asset Demand

The public signal of the election changed beliefs and caused trade and portfolio allocation across households. Did this change in disagreement change the net demand for equity of the retirement investor sample that we observe? Did the heterogeneous changes in beliefs and the resulting trade contribute to the high returns on the stock market following the election?

We can provide only a very rough answer to these questions. We begin by defining the baseline relative to which we measure the effect of differences in the updating of beliefs. We assume that there would have been no change in the demand for equity from the election had the Democratic candidate won the election, that is, had the much more likely outcome occurred. Further, we base our calculation on our regressions using county vote share as the measure of political affiliation (see Appendix Table A.V) and assume there was no net change in the demand for equity in a county in which investable wealth is evenly split between Democrats and Republicans. Finally, we assume

[37] Individual equity is classified as international if it is traded on an international exchange or if the company is incorporated outside the US. Equity funds are classified as domestic or international based on their product description.

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that wealth is uncorrelated with party affiliation within counties.

Under these three significant assumptions, the change in net demand for equity from each county is our regression coefficient times county wealth times the difference between county vote share and 0.5. Summing across counties leads to a decrease of $660 million in the demand for equity by the investors that we observe over the year following the election.

What about the aggregate demand for equity? We only observe a fraction of the retirement investors and wealth in each county. To scale our estimate to a measure for all retirement investors, we scale up the demand in each county by multiplying by the share of the population that we observe in that county (based on US Census data) times the national share of households that are retirement investors (from the 2016 SCF). Again, summing across counties, this crude estimate implies that the demand for equity declined by $1.09 billion among retirement investors in the US. Finally, while we do not know much about the political affiliations of very wealthy households, if we simply scale this number up by the inverse of the share of the investable wealth in the US held by retirement investors in the 2016 SCF, this back-of-the-envelope calculation implies that the election caused a decrease in the net demand for equity of $4.37 billion in the year following the election. This is obviously a very rough estimate, but far too small a number to have had a noticeable effect on returns during the year following the election.

D. Differences in Reported Beliefs and Portfolio Reallocation in Other Elections

Are our findings relevant for other elections? Would we expect Democrats and Republicans to trade differently following other major US elections? We hypothesize that the answer is yes, although we expect these differences to be smaller for elections in which the candidates are more similar, in which the outcome is more likely ex ante, or (more speculatively) in which the nation is not as obsessed with the singular issue of the election (e.g. at times of less political polarization or during times of other major national news). In this section, we analyze reported beliefs around several US elections, and provide one observation related to these hypotheses by repeating our analysis of changes in portfolio holding but around the 2012 election.

We consider four US presidential elections, 2008, 2012, 2016, and 2020. Other than in 2016, these elections contain no major upsets: in 2008, neither candidate was an incumbent president, and

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the heavily favored candidate won; in 2012, the heavily favored incumbent president won; and in 2020, the non-incumbent favored candidate won (although uncertainty was not resolved immediately following the election). To analyze reported beliefs in each of these elections we use the University of Michigan Survey of Consumers as we did in Section II but using data from around each of these elections.[38]

Our first result is that the 2008 and 2012 elections were associated with smaller changes in reported beliefs by party, consistent with less movement in reported beliefs when the more likely outcome of the election occurs. Following the election of 2008, which the Democratic candidate was predicted to be highly likely to win and did, Republicans report becoming slightly more pessimistic and Democrats report becoming slightly more optimistic (about both future national economic conditions and, less so, their own future incomes). This movement in average reports is about one half to one third the size of the movement in response to the 2016 election (see Appendix Figures A.18 and A.19). In 2012, the (incumbent) winning candidate in 2012 had a two thirds chance of victory right before the election according to the election odds from Betfair (inferred as we did for the 2016 election). The Consumer Confidence Survey only surveyed expectations by party after the election in November 2012. These expectations immediately after the election show no change in reported beliefs associated with this likely outcome (Appendix Figures A.20 and A.21).[39]

Our second result is that there was a smaller change (and in the opposite direction) in portfolios by party following the 2012 election, and this difference is not present in our specifications with more controls, and in particular those that compare households in the same location working for the same employer. Ex ante, one would expect smaller differential changes in portfolios by party affiliation in 2012 because the election resolved less objective uncertainty and showed no changes in reported beliefs by party (at least in November 2012). We perform the same analysis that we conducted on data covering the time around 2016 election but using data from our provider covering the 2012 election. With realized equity shares and few controls, we find that Democrats increased

[38]The conclusions of the following paragraphs can be seen in Appendix Figures A.18 to A.23 which replicate Figures 1 and 2 but for the periods around the 2008, 2012, and 2020 elections and using only the months for which the survey collects political affiliation information.

[39]Polling by Gallup also shows virtually no differential change in economic expectations around the 2012 election (Mian et al., forthcoming, Appendix Figure A3).

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their equity shares relative to Republicans following the 2012 election, consistent with the changes in reported beliefs (see the first few columns of Appendix Table A.VI). However, we find no evidence of differential rebalancing either when we include more controls so as to compare similar households or when we look at differences in price-constant equity shares that remove changes due to differences in appreciation across portfolios (Appendix Table A.VII).

In sum, our more robust and careful analyses reveal no measurable differential effect of party affiliation on rebalancing following the 2012 election. If Democrats became (relatively) more optimistic and bought stocks and sold bonds, they did so quite limitedly and/or in ways that were correlated with our control variables (like industry and county). The natural interpretation of this weak evidence is that there was not enough news in the election outcome to cause much movement in portfolios and that there may have been information arriving during this same period about other factors that differentially impacted (or was differentially interpreted by) Democrats and Republicans. In addition to suggesting that elections which have the expected outcome lead to little differences in portfolio reallocation by party, this analysis also rules out some perhaps naive hypotheses, such as that there is a general tendency for Republicans to increase their equity shares following elections or when the market does well (as it did after both the 2012 and 2016 elections).

Third, reported beliefs move a lot (but in the opposite direction) following the 2020 election, much as they did following the 2016 election, as we show in Appendix Figures A.22 and A.23. On the one hand, the 2020 outcome was not a big upset in that the candidate that won was expected to do so according to (aggregated) polls and election betting platforms. On the other hand, in 2020, political polarization was high. And the candidate with lower odds in both 2016 and 2020 had won the previous election, which, combined with polarization, may have undermined faith in predictions. In sum, it is not simply the case that likely election outcomes are associated with little movements in reported beliefs. The role of polarization and/or the difference in candidates also seem to play a large role based on these few datapoints.

Finally, it is an open and interesting question whether there were similar changes in portfolios by party following the 2020 election.

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X. Concluding Discussion

We study the unexpected outcome of the 2016 US election as a public signal that was widely-observed, well-measured, and for which we have ex ante measures of how different investors should interpret this signal given their identities, or models that they use to interpret the world. We find that people with different political affiliations updated their beliefs and rebalanced their portfolios differentially in response to a common public signal. A correlation between differences in beliefs and differences in behavior may not be causal, and an important contribution of our analysis comes from our controls for an extensive array of other factors that may have affected rebalancing differentially such as through the real effects of changes in economic policies on incomes or local risk exposures. Consistent with dynamic models of heterogeneous beliefs, we show that the heterogeneity in updating across households led to an increase in trading volume.

Reported beliefs data also suggest that Democrats indeed became more pessimistic about the US economy following the election and that Republicans became more optimistic. In contrast, people did not report the same polarized expectations about their own personal situations. Relatedly, our retirement savings data also do not show differences in saving rates across households with different affiliations.

Finally, while we find small average differences in rebalancing between Democrats and Republicans following the election, these small averages were driven by very large rebalancing by a small share of investors. Across both investors and accounts, the trading responses we find are consistent with the inertia generally found for typical American retirement investors. We also find that differences in rebalancing persisted over many months after the election, which is consistent with investors being aware that there was disagreement and that prices did not rapidly incorporate their own views.

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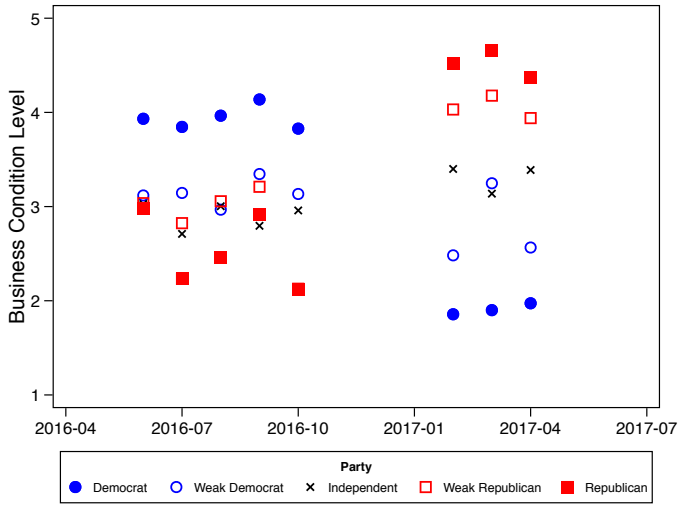
Panel A. Expected Business Conditions in 5 Years



Panel B. Expectations on Unemployment in a Year

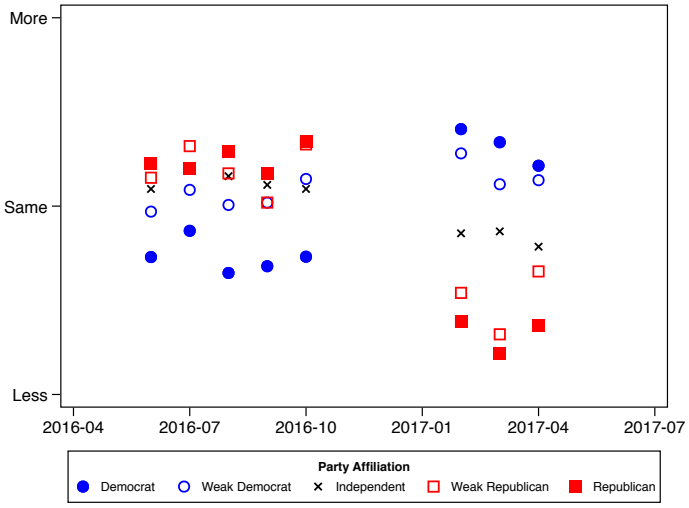


Figure 1. Survey Expectations on Future State of the Economy. These graphs plot survey expectations on the future state of the economy by political affiliation. The data is from the University of Michigan Survey of Consumers. The upper panel shows the average response to the question "Looking ahead, which would you say is more likely – that in the country as a whole we'll have continuous good times during the next 5 years or so, or that we will have periods of widespread unemployment or depression, or what?" Responses range from 1 (worst) to 5 (best). The lower panel shows expectations on unemployment in a year relative to the current unemployment rate.

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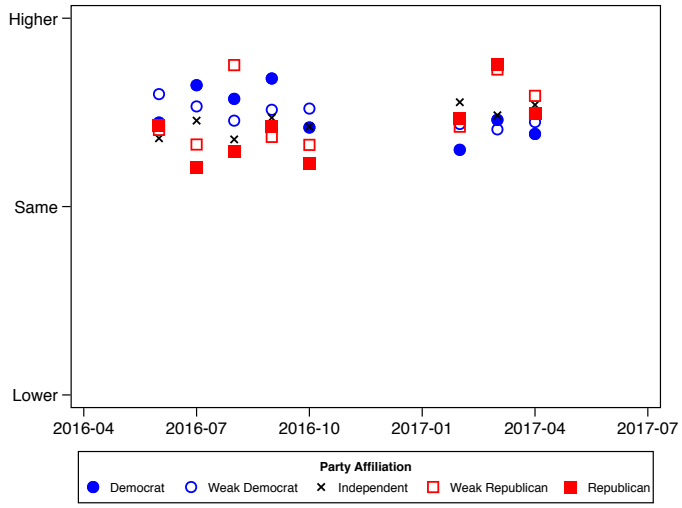
Panel A. Expected Own Income in 1 Year



Panel B. Expected Business Conditions in 1 Year

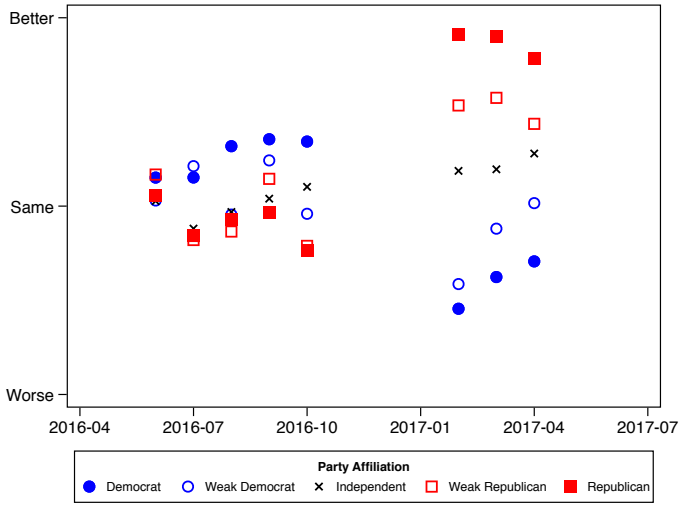


Figure 2. Survey Expectations on Own Economic Circumstances Versus Overall Conditions. These graphs plot survey expectations on the future state of own economic circumstances and the overall economy by political affiliation. The data is from the University of Michigan Survey of Consumers. The upper panel shows the average response to the question "During the next 12 months, do you expect your income to be higher or lower than during the past year?" The lower panel shows expectations on whether business conditions overall will be better or worse in a year.

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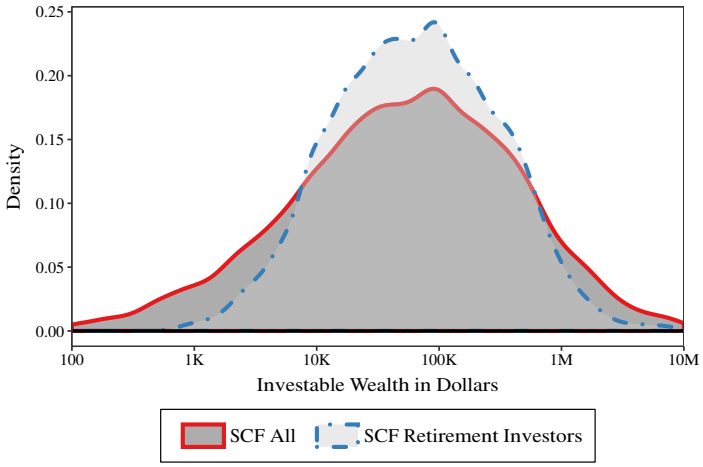

Figure 3. SCF Wealth Distribution in Population and RI Subsample. This figure plots the distribution of investable wealth (conditional on positive) in the full population and in the subsample of retirement investors (RIs) in the public version of the 2016 Survey of Consumer Finances (SCF). Investable wealth is defined as the sum of money market funds, stocks, bonds, pooled investment funds, retirement accounts, and other managed assets. To construct the RI subsample, we select households with age of the head between 25 and 64 and with quasi-liquid retirement wealth, and run quantile regressions of log retirement wealth on a second-order polynomial in age. We use the fitted 10th and 90th percentiles by age as retirement wealth cutoffs.

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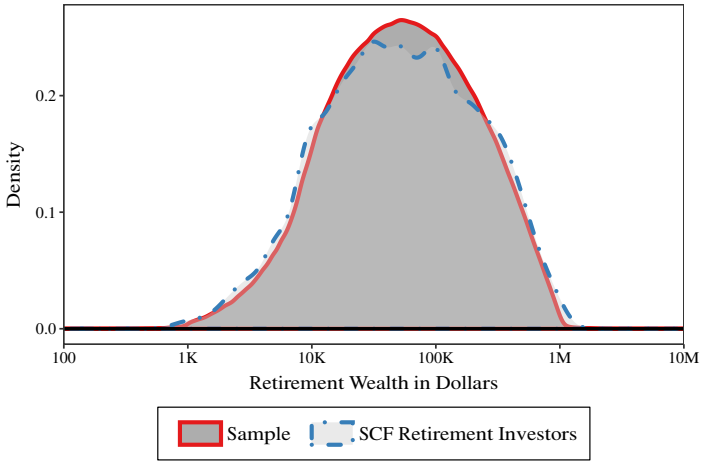
Panel A. Retirement Wealth



Panel B. Total Investable Wealth

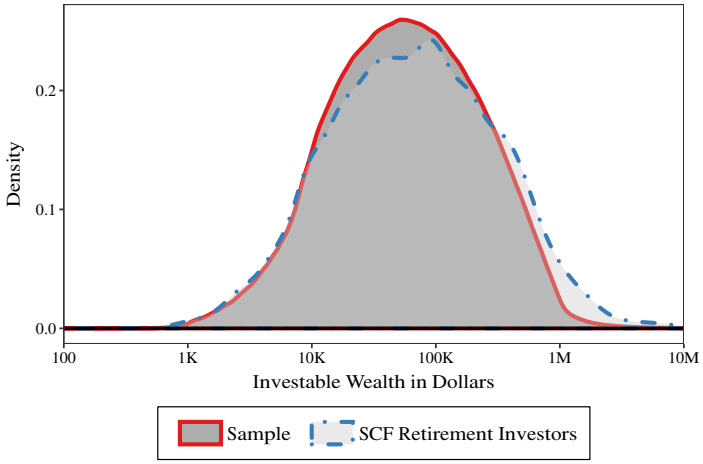


Figure 4. Wealth Distribution in Comparison to SCF. These figures plot the distributions of retirement wealth and total investable wealth, respectively, in our sample compared to the equivalent sample of RIs in the public version of the 2016 Survey of Consumer Finances (SCF). We select households with positive quasi-liquid retirement wealth and run quantile regressions of log retirement wealth on a second-order polynomial in age for households in the 2016 SCF. We use the fitted 10th and 90th percentiles by age as retirement wealth cutoffs in both datasets. We include households with age of the head between 25 and 64 and filter our sample on households that have portfolio holdings between 20% and 500% of initial assets in every month in the sample.

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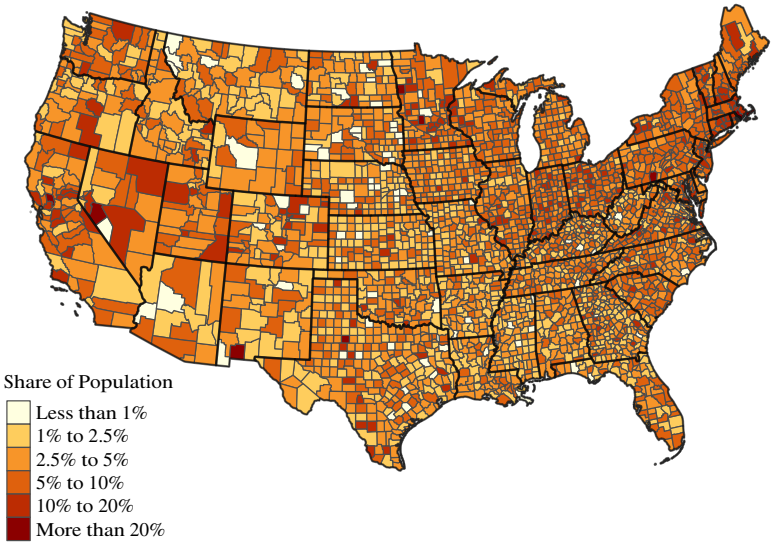
Panel A. Map of Household Coverage in Sample (Scrambled)



Panel B. Distribution of Household Coverage by County

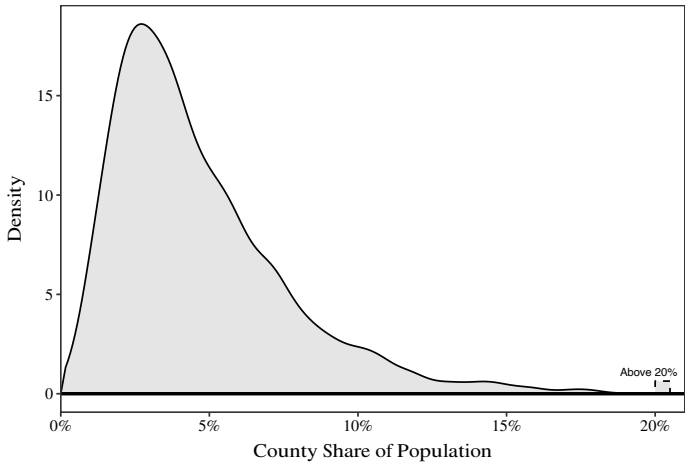


Figure 5. Household Coverage in Sample. Panel (a) shows the geographical distribution of household coverage in our sample of RIs relative to the population total number of households by county from the 2011–2015 American Community Survey. We calculate the share for every county in the US and then randomly reallocate the shares across counties in each state for confidentiality reasons. Panel (b) plots the density of household coverage by county in our sample of RIs as a fraction of the population total number of households by county.

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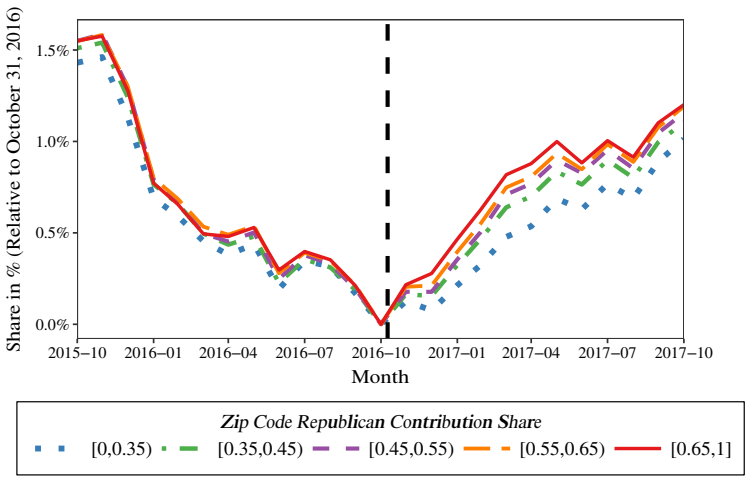
Panel A. Equity Share, Equally Weighted Across Households



Panel B. Equity Share, Value Weighted Across Households

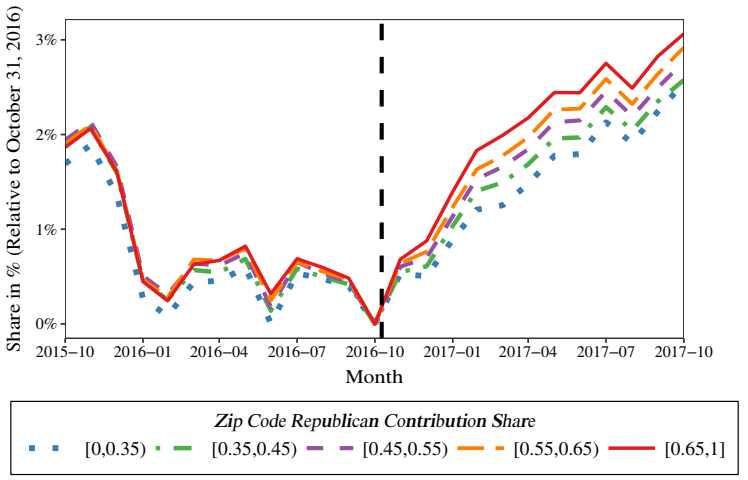


Figure 6. Portfolio Equity Share by Zip Code Party Affiliation. These graphs plot the average equity share of household portfolio assets in five groups by zip code party affiliation measured from political contributions, relative to the share by the end of October 2016. The sample is our full set of RI households. Average shares by group are equally weighted and asset weighted across households, respectively.

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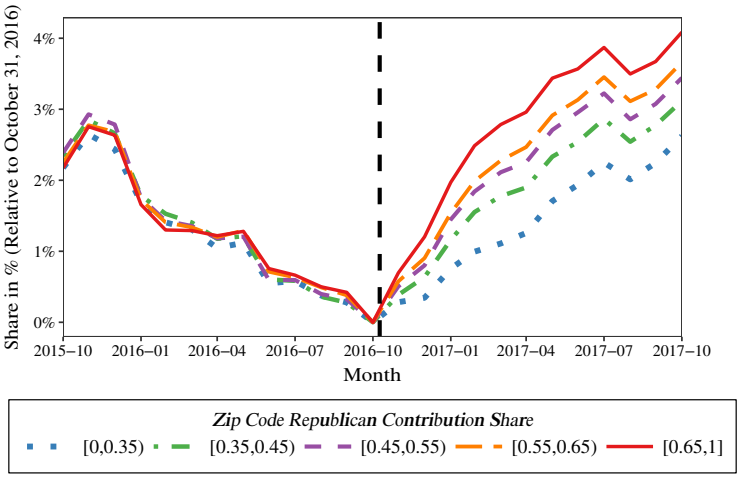
Panel A. Equity Share for Previously Active Sample



Panel B. Market Beta for Previously Active Sample

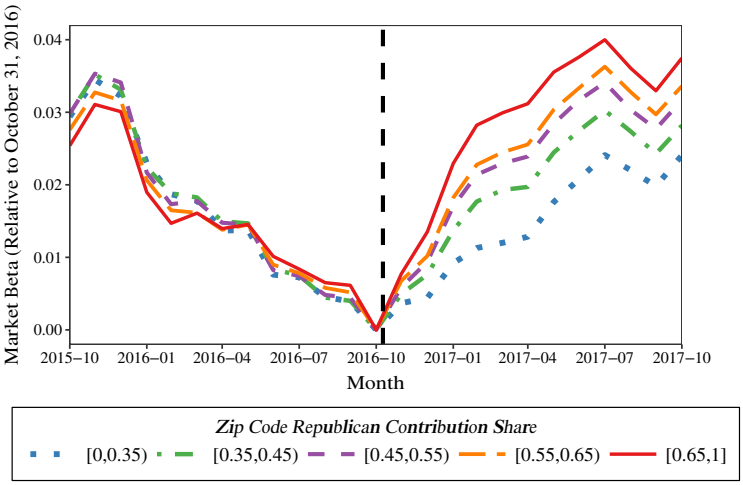


Figure 7. Portfolio Allocations for Previously Active Sample. This figure plots the average equity share and market beta, respectively, of household portfolio assets in five groups by zip code party affiliation measured from political contributions, relative to the share by the end of October 2016. The sample is the subset of RI households with an active portfolio reallocation in the prior year (11.1% of all RIs). Average allocations by group are equally weighted across households.

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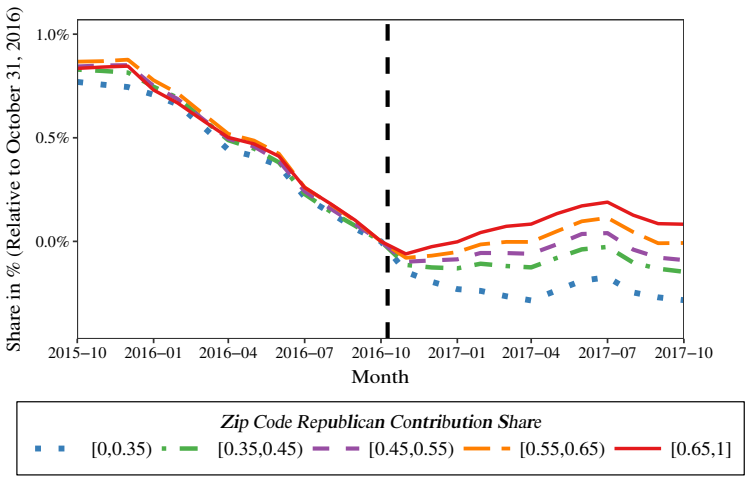
Panel A. Equity Share of Price-Constant Portfolios



Panel B. Market Beta of Price-Constant Portfolios

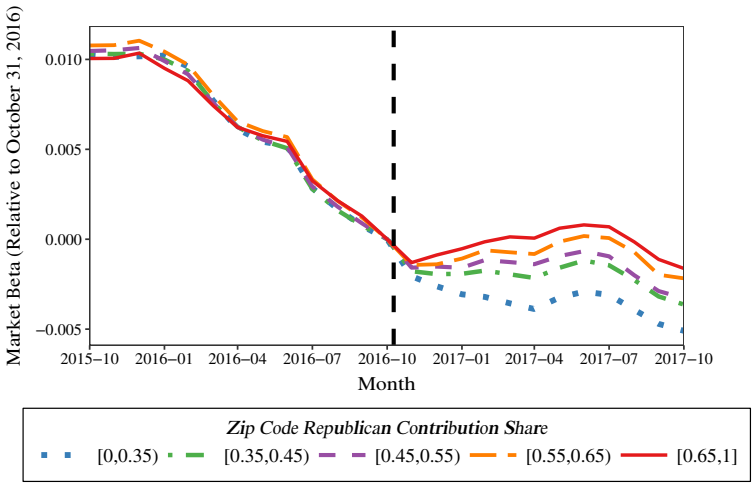


Figure 8. Active Portfolio Rebalancing by Zip Code Political Affiliation. In this figure, we plot the average equity share and average portfolio market beta, respectively, of hypothetical price-constant household portfolios in five groups by zip code party affiliation measured from political contributions, relative to the share by the end of October 2016. The portfolio measures are calculated for a hypothetical portfolio that is insensitive to passive appreciations. To construct these price-constant portfolios, we start with initial household holdings as of October 2015, assume there are no price changes, and keep track of cumulative monthly dollar inflows and outflows at the asset level. For each month we then calculate the equity share from this hypothetical portfolio. The sample is our full set of RI households. Average allocations by group are equally weighted across households.

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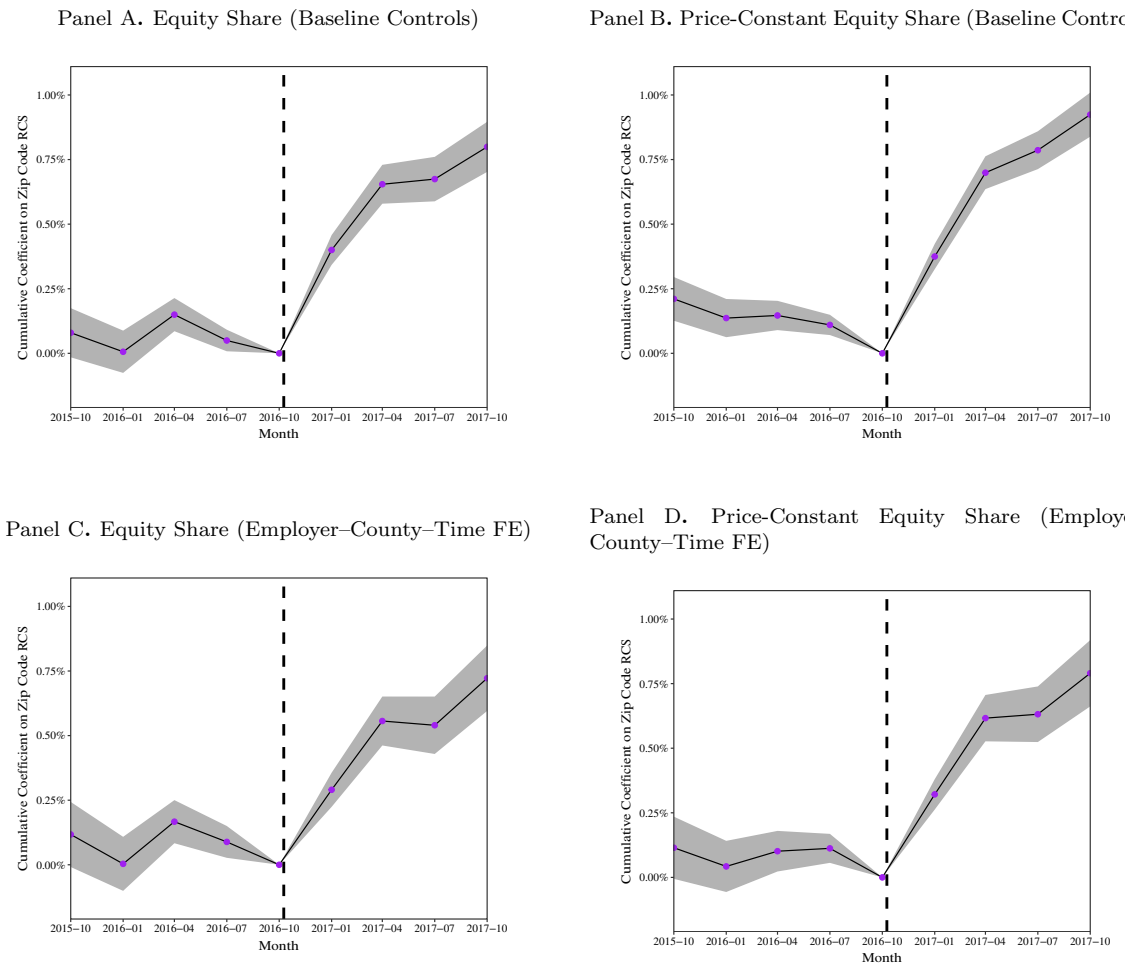

Figure 9. Cumulative Regression Coefficients of Equity Share Changes on Likely Political Affiliation. This figure plots cumulative regression coefficients of quarterly changes in household portfolio equity shares on the zip code Republican contribution share, between four quarters prior to the election and four quarters after the election, normalized to zero just before the election. In panels (a) and (c) we report the results for the observed equity share, and in panels (b) and (d) we report the results for the equity share of hypothetical price-constant portfolios that are insensitive to passive appreciations and are driven by trading only. The baseline controls are the lagged equity share, age, gender, marital status, log lagged financial wealth, log labor income in 2015, the lagged winning and losing sectors shares of equity, and zip code house price growth (2010–2015), all interacted by quarterly indicators. In panels (c) and (d) we additionally control for employer × county × quarter fixed effects. The sample is our full set of RI households between October 2015 and October 2017, for which we observe the complete set of controls (27.7% of all RIs). Standard errors are clustered at the zip code level.

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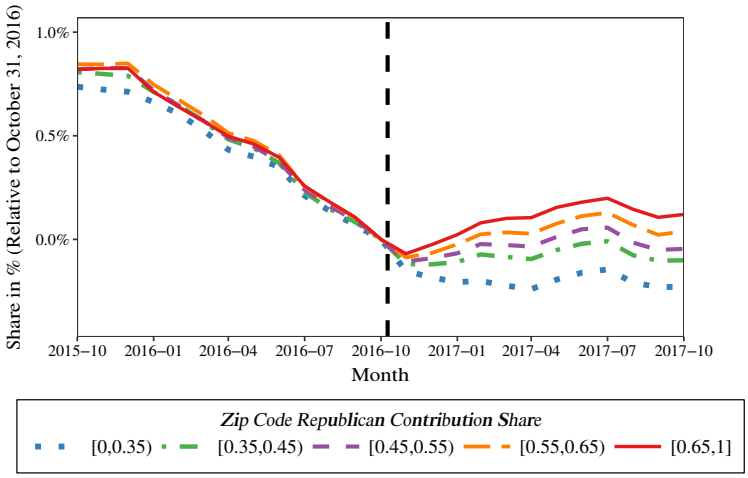
Panel A. Restricting to Cumulative Changes of at Least 10%



Panel B. Restricting to Cumulative Changes of at Most 10%

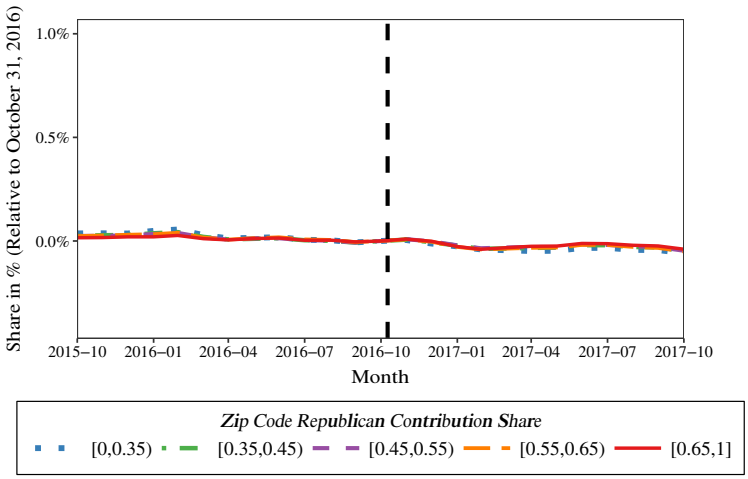


Figure 10. Equity Share of Price-Constant Portfolios by Zip Code Party Affiliation, Small Versus Large Changes. These graphs plot the average equity share of hypothetical price-constant household portfolios in five groups by zip code party affiliation measured from political contributions, relative to the share by the end of October 2016. The price-constant equity share is calculated for a hypothetical portfolio that is insensitive to passive appreciations and is driven by trading only. In panel (a), we only include cumulative changes relative to October 2016 of at least 10%, and set portfolio changes to zero otherwise. In panel (b), we only include cumulative changes of at most 10%. The sample is our full set of RI households. Average shares by group are equally weighted across households.

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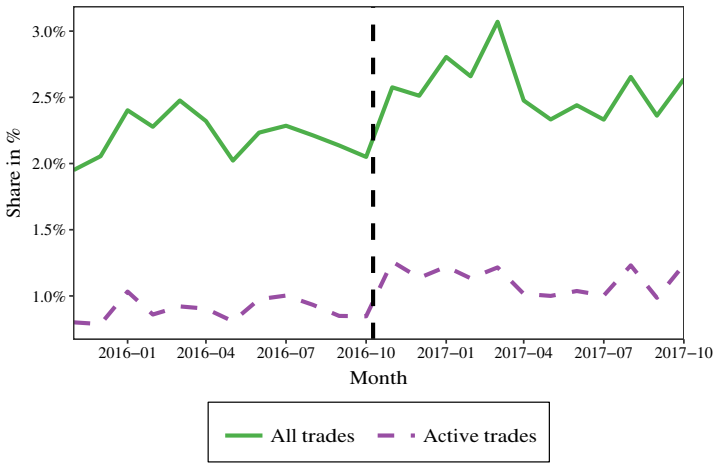
Panel A. All and Active Trading Volume for Aggregate Sample



Panel B. Trading Volume by Zip Code Political Affiliation

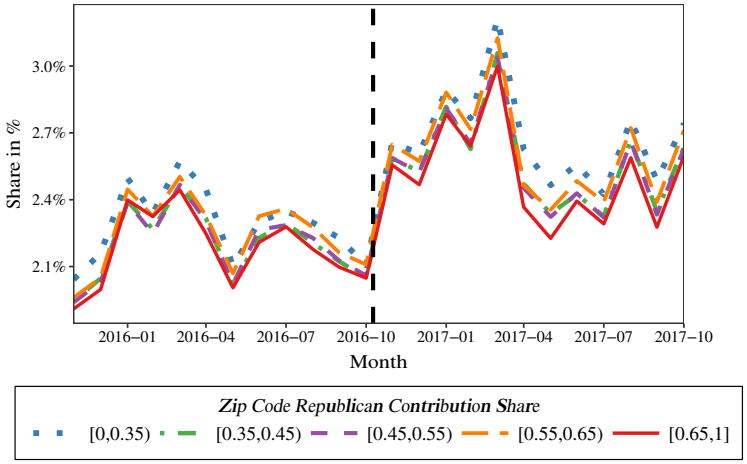


Figure 11. Trading Volume Relative to Initial Balance. This figure plots average trading volume as a fraction of initial balance, where volume is defined as one half times the sum of the absolute values of buy and sell transactions. The upper panel plots the volume of all trades and of active (investor-initiated) trades or exchanges. The lower panel plots the volume of all trades in five groups by zip code party affiliation measured from political contributions. The sample is our full set of RI households. Averages are equally weighted across households.

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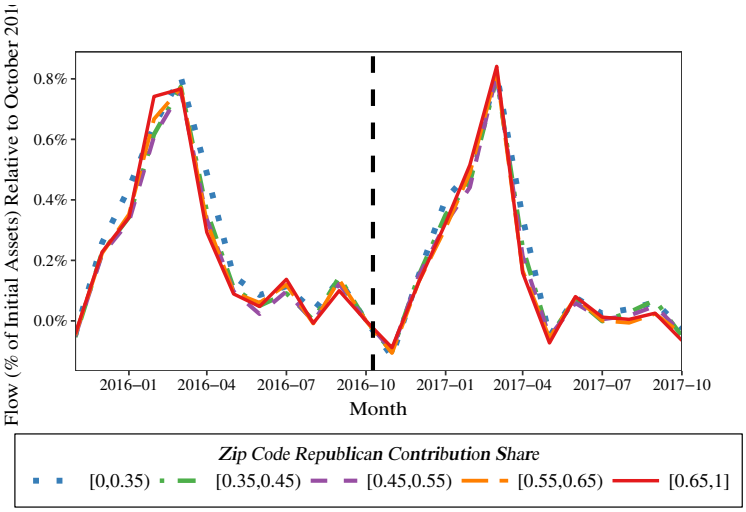

Figure 12. Household Net Flow Rates by Zip Code Party Affiliation. This graph plots average net flows as a fraction of initial financial wealth in five groups by zip code party affiliation measured from political contributions, relative to the savings rate in October 2016. Net flows are defined as total deposits minus withdrawals. The sample is our full set of RI households. Average flow rates by group are equally weighted across households.

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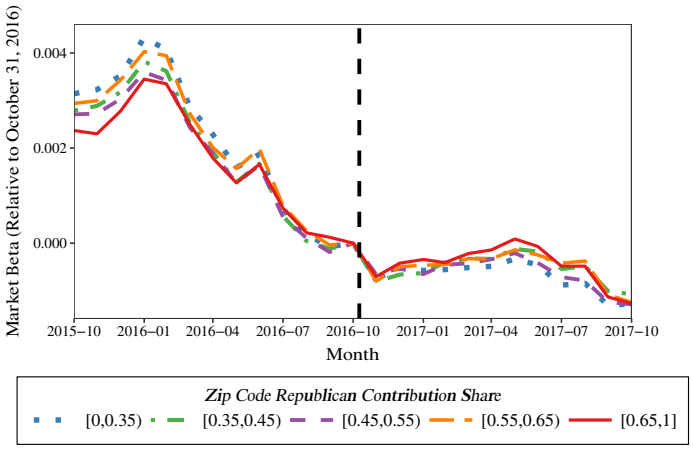
Panel A. Market Beta of Equity by Zip Code Party Affiliation



Panel B. International Share of Equity by Zip Code Party Affiliation

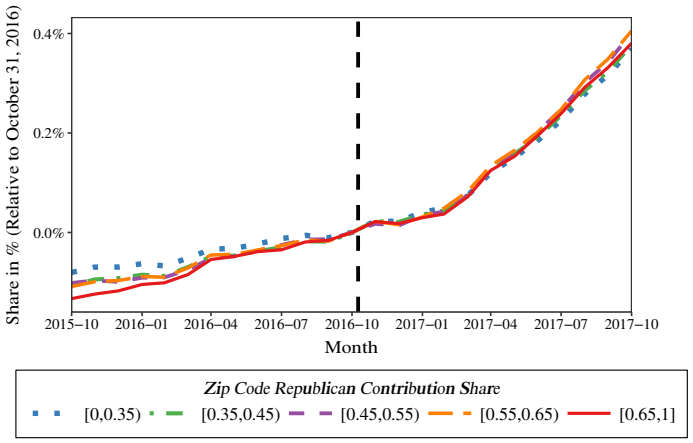


Figure 13. Equity Composition of Price-Constant Portfolios by Zip Code Party Affiliation. These graphs plot the average price-constant market beta of household equity assets and the average price-constant international share of household equity products, respectively, in five groups by zip code party affiliation measured from political contributions, relative to the share by the end of October 2016. The price-constant portfolio measures are calculated for a hypothetical portfolio that is insensitive to passive appreciations and is driven by trading only. Market betas are obtained by regressing monthly fund or security excess returns on the value-weighted CRSP market excess return over the period 2006–2019 with at least 24 observations. International equity holdings consist of individual foreign company stocks and funds that invest in international equity. The sample is our full set of households. Averages by group are equally weighted across households.

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Table I
Summary Statistics on Portfolios of Retirement Investors Sample

This table presents summary statistics on wealth and portfolio allocations of our retirement investors (RI) sample as of October 31, 2016, for the full sample and for five groups by zip code party affiliation measured from political contributions. We select households with quasi-liquid retirement wealth and run quantile regressions of log retirement wealth on a second-order polynomial in age for households in the 2016 Survey of Consumer Finances (SCF). We use the fitted 10th and 90th percentiles by age as retirement wealth cutoffs at the initial date. We include households with age of the head between 25 and 64 years and create a balanced panel by filtering on households that have portfolio holdings between 20% and 500% of initial assets over the sample period from October 31, 2015 to October 31, 2017. All funds and individual securities are characterized as equity, bonds, cash or cash-like assets, or alternative assets. Mixed funds are subdivided into equity and bonds based on their asset holdings. Market betas are obtained by regressing monthly fund or security excess returns on the value-weighted CRSP market excess return over the period 2006–2019 with at least 24 observations. Winning (losing) sectors are sectors that did relatively well (poorly) between the election and the end of 2016. International equity holdings consist of individual foreign company stocks and funds that invest in international equity.

			Republican contribution share				
	All	SCF	[0,0.35)	[0.35,0.45)	[0.45,0.55)	[0.55,0.65)	[0.65,1]
Share of sample	100.0%		44.8%	18.0%	14.8%	12.5%	9.9%
Average wealth (in 1,000 USD)							
Total investable wealth	138.1	232.8	147.9	138.3	136.4	140.1	128.7
Retirement wealth	116.4	122.5	118.2	119.3	118.6	121.1	114.5
Median wealth (in 1,000 USD)							
Total investable wealth	56.7	66.0	58.2	58.1	57.7	59.5	55.0
Retirement wealth	53.7	51.0	54.1	55.1	55.1	56.8	53.0
Average allocation							
Equity share	71.2%	53.0%	71.5%	71.2%	71.1%	71.1%	70.7%
Bond share	19.5%	44.5%	18.2%	19.6%	20.0%	20.0%	20.8%
Cash share	8.6%	2.5%	9.5%	8.5%	8.3%	8.2%	7.8%
Market beta	0.743		0.746	0.743	0.743	0.744	0.741
Average allocation (weighted)							
Equity share	68.6%	59.8%	69.0%	68.8%	68.5%	68.2%	67.7%
Bond share	20.6%	36.8%	19.1%	20.8%	21.3%	21.3%	22.5%
Cash share	9.7%	3.4%	10.7%	9.3%	9.1%	9.5%	8.8%
Market beta	0.717		0.721	0.718	0.715	0.713	0.709
Average allocation of equity							
Market beta	1.007		1.007	1.006	1.006	1.008	1.011
Fund share	90.3%		90.5%	90.3%	90.3%	89.7%	89.7%
Winning sectors share	5.4%		4.8%	5.4%	5.6%	6.1%	6.5%
Losing sectors share	4.2%		4.7%	4.3%	4.1%	4.0%	3.5%
International share	6.9%		6.9%	7.2%	7.0%	7.1%	6.9%

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Table II
Summary Statistics on Demographics of Retirement Investors Sample

This table presents summary statistics on demographics and composition of our retirement investors (RI) sample as of October 31, 2016, for the full sample and for five groups by zip code party affiliation measured from political contributions. We select households with quasi-liquid retirement wealth and run quantile regressions of log retirement wealth on a second-order polynomial in age for households in the 2016 Survey of Consumer Finances (SCF). We use the fitted 10th and 90th percentiles by age as retirement wealth cutoffs at the initial date. We include households with age of the head between 25 and 64 years and create a balanced panel by filtering on households that have portfolio holdings between 20% and 500% of initial assets over the sample period from October 31, 2015 to October 31, 2017.

			Republican contribution share				
	All	SCF	[0,0.35)	[0.35,0.45)	[0.45,0.55)	[0.55,0.65)	[0.65,1]
Percentage of sample with observed							
Gender	94.8%		94.8%	95.1%	95.0%	95.1%	94.7%
Marital status	87.0%		85.7%	87.3%	87.8%	87.9%	88.7%
Non-retirement assets	16.8%		19.0%	16.8%	16.0%	16.6%	14.7%
Employer industry	66.6%		63.3%	67.0%	68.0%	68.1%	70.1%
Labor income in 2015	49.9%		45.6%	50.8%	51.5%	52.6%	55.1%
Income growth over 2016-17	44.2%		39.7%	45.2%	45.9%	47.1%	49.3%
Active change in prior year	11.1%		11.7%	11.1%	11.1%	11.4%	10.7%
Average age (in years)	46.0	46.3	45.2	46.3	46.6	46.7	46.8
% Female	43.8%	19.9%	46.5%	44.6%	43.2%	41.2%	38.0%
% Married	73.1%	69.5%	69.3%	73.9%	75.3%	77.0%	78.1%
Labor income in 2016 (in 1,000 USD)							
Average	101.2	102.8	115.7	99.1	96.1	98.6	91.6
Median	77.3	81.0	84.0	77.9	76.3	78.0	74.6
10th percentile	37.2	33.4	38.3	37.6	37.2	38.0	37.3
90th percentile	172.4	182.3	200.3	169.1	162.1	166.3	152.2

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Table III
Regressions of Price-Constant Equity Share Changes on Likely Political Affiliation

This table presents regression coefficients of annual changes in price-constant household portfolio equity shares on the zip code Republican contribution share, before and after the election, for various sets of controls. The price-constant equity share is calculated for a hypothetical portfolio that is insensitive to passive appreciations and is driven by trading only. The baseline controls are the lagged equity share, age, gender, marital status, log lagged financial wealth, log labor income in 2015, the lagged winning and losing sectors shares of equity, and zip code house price growth (2010–2015), interacted by annual indicators. In specifications (3)–(8) we consider additional sets of controls (interacted by yearly dummies) that include ex post income growth (2016–2017) and house price growth (2015–2017), employer industry indicators (3-digit NAICS), employer indicators, state indicators, county indicators, and employer × county indicators. The sample is our full set of RI households between October 2015 and October 2017, for which we observe the complete set of controls (27.7% of all RIs). Standard errors are clustered at the zip code level.

	One-year difference in price-constant equity share (in %), all households							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Zip code Republican contribution share	-0.343	-0.225	-0.232	-0.097	-0.070	-0.148	-0.221	-0.160
	(0.042)	(0.039)	(0.039)	(0.037)	(0.038)	(0.043)	(0.054)	(0.059)
Zip code Republican contribution share × Post	1.071	1.061	1.062	0.932	0.900	0.929	0.969	0.859
	(0.066)	(0.057)	(0.057)	(0.056)	(0.059)	(0.064)	(0.084)	(0.091)
Lagged equity share		-10.273	-10.273	-10.392	-10.798	-10.291	-10.342	-10.805
		(0.060)	(0.060)	(0.060)	(0.062)	(0.060)	(0.060)	(0.068)
Lagged equity share × Post		-3.888	-3.890	-3.856	-3.815	-3.880	-3.870	-3.799
		(0.077)	(0.077)	(0.077)	(0.079)	(0.077)	(0.077)	(0.085)
Age		-0.104	-0.104	-0.104	-0.106	-0.104	-0.104	-0.105
		(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Age × Post		-0.021	-0.020	-0.021	-0.023	-0.022	-0.021	-0.024
		(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Female		0.299	0.301	0.166	0.140	0.284	0.271	0.144
		(0.012)	(0.012)	(0.013)	(0.013)	(0.012)	(0.012)	(0.015)
Female × Post		-0.509	-0.502	-0.415	-0.394	-0.495	-0.487	-0.378
		(0.019)	(0.019)	(0.021)	(0.021)	(0.019)	(0.019)	(0.024)
Married		0.037	0.037	0.047	0.051	0.044	0.043	0.048
		(0.014)	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)	(0.016)
Married × Post		0.120	0.121	0.096	0.092	0.107	0.101	0.092
		(0.021)	(0.021)	(0.021)	(0.022)	(0.021)	(0.021)	(0.025)
Log wealth		-0.233	-0.234	-0.224	-0.215	-0.234	-0.230	-0.211
		(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)
Log wealth × Post		0.245	0.243	0.236	0.227	0.247	0.240	0.228
		(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.012)
Log labor income 2015		0.045	0.044	0.062	0.087	0.049	0.058	0.085
		(0.013)	(0.013)	(0.014)	(0.015)	(0.013)	(0.013)	(0.017)
Log labor income 2015 × Post		0.282	0.286	0.261	0.227	0.284	0.273	0.193
		(0.021)	(0.021)	(0.022)	(0.024)	(0.021)	(0.021)	(0.027)
Controls by year								
Baseline		Y	Y	Y	Y	Y	Y	Y
Labor income growth 2016-17			Y					
Zip code house price growth 2015-17			Y					
Employer industry				Y				
Employer					Y			
State						Y		
County							Y	
Employer × County								Y
R^2	0.001	0.067	0.067	0.068	0.080	0.067	0.068	0.189

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Table IV
Regressions of Price-Constant Equity Share Changes on Likely Political Affiliation for Active Investors

This table presents regression coefficients of annual changes in price-constant household portfolio equity shares on the zip code Republican contribution share, before and after the election, for various sets of controls. The price-constant equity share is calculated for a hypothetical portfolio that is insensitive to passive appreciations and is driven by trading only. The baseline controls are the lagged equity share, age, gender, marital status, log lagged financial wealth, log labor income in 2015, the lagged winning and losing sectors shares of equity, and zip code house price growth (2010–2015), interacted by annual indicators. In specifications (3)–(8) we consider additional sets of controls (interacted by yearly dummies) that include ex post income growth (2016–2017) and house price growth (2015–2017), employer industry indicators (3-digit NAICS), employer indicators, state indicators, county indicators, and employer × county indicators. The sample is the subset of households with an active portfolio reallocation in the prior year (11.1% of RIs). The sample period is October 2015 to October 2017, and we include RIs for which we observe the complete set of controls (23.6% of active RIs). Standard errors are clustered at the zip code level.

	One-year difference in price-constant equity share (in %), active households							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Zip code Republican contribution share	-0.172	-0.581	-0.559	-0.148	-0.216	-0.570	-0.983	-0.533
	(0.220)	(0.218)	(0.218)	(0.212)	(0.228)	(0.251)	(0.324)	(0.443)
Zip code Republican contribution share × Post	3.574	3.113	3.139	2.447	2.292	2.824	2.751	1.850
	(0.358)	(0.314)	(0.315)	(0.310)	(0.335)	(0.358)	(0.472)	(0.638)
Lagged equity share		-25.543	-25.541	-25.765	-26.292	-25.548	-25.666	-26.807
		(0.205)	(0.205)	(0.206)	(0.214)	(0.205)	(0.206)	(0.280)
Lagged equity share × Post		-5.078	-5.081	-4.893	-4.631	-5.074	-4.981	-4.246
		(0.272)	(0.272)	(0.273)	(0.284)	(0.272)	(0.274)	(0.373)
Age		-0.225	-0.224	-0.226	-0.226	-0.224	-0.225	-0.228
		(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.007)
Age × Post		0.012	0.012	0.010	0.007	0.010	0.010	0.001
		(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.010)
Female		1.337	1.343	0.969	0.851	1.264	1.231	0.836
		(0.075)	(0.075)	(0.078)	(0.081)	(0.075)	(0.076)	(0.110)
Female × Post		-1.966	-1.963	-1.547	-1.398	-1.869	-1.813	-1.220
		(0.109)	(0.109)	(0.113)	(0.119)	(0.108)	(0.110)	(0.160)
Married		-0.008	-0.006	-0.012	0.002	0.030	0.026	0.005
		(0.085)	(0.085)	(0.085)	(0.090)	(0.085)	(0.085)	(0.122)
Married × Post		0.386	0.387	0.348	0.228	0.318	0.303	0.296
		(0.125)	(0.125)	(0.125)	(0.132)	(0.125)	(0.127)	(0.177)
Log wealth		-0.639	-0.639	-0.584	-0.573	-0.620	-0.608	-0.588
		(0.039)	(0.039)	(0.039)	(0.041)	(0.039)	(0.039)	(0.057)
Log wealth × Post		0.253	0.256	0.178	0.091	0.229	0.205	0.161
		(0.059)	(0.059)	(0.058)	(0.062)	(0.058)	(0.058)	(0.084)
Log labor income 2015		0.332	0.343	0.311	0.397	0.306	0.336	0.397
		(0.070)	(0.071)	(0.072)	(0.079)	(0.071)	(0.072)	(0.110)
Log labor income 2015 × Post		-0.077	-0.067	0.020	0.059	-0.025	-0.019	0.016
		(0.103)	(0.104)	(0.105)	(0.117)	(0.104)	(0.106)	(0.162)
Controls by year								
Baseline		Y	Y	Y	Y	Y	Y	Y
Labor income growth 2016-17			Y					
Zip code house price growth 2015-17			Y					
Employer industry				Y				
Employer					Y			
State						Y		
County							Y	
Employer × County								Y
R^2	0.011	0.156	0.157	0.160	0.207	0.158	0.165	0.417

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Table V
Regressions of Price-Constant Equity Share Changes on Likely Political Affiliation, Heterogeneous Treatment Effects

This table presents regression coefficients of annual changes in price-constant household portfolio equity shares on the zip code Republican contribution share, before and after the election. To estimate heterogeneous treatment effects, we also interact the zip code Republican contribution share each year by a dummy for active portfolio reallocation in the preceding year, age (demeaned), log initial wealth (demeaned), log labor income in 2015 (demeaned), and the initial equity share (demeaned). The price-constant equity share is calculated for a hypothetical portfolio that is insensitive to passive appreciations and is driven by trading only. The baseline controls are the lagged equity share, age, gender, marital status, log lagged financial wealth, log labor income in 2015, the lagged winning and losing sectors shares of equity, and zip code house price growth (2010–2015), interacted by annual indicators. In addition, we control for employer × county × period fixed effects. The sample is our full set of RI households between October 2015 and October 2017, for which we observe the complete set of controls (27.7% of all RIs). Standard errors are clustered at the zip code level.

	One-year difference in price-constant equity share (in %), all households					
	(1)	(2)	(3)	(4)	(5)	(6)
Zip code Republican contribution share	-0.160	-0.133	-0.167	-0.204	-0.162	-0.187
	(0.059)	(0.057)	(0.059)	(0.060)	(0.059)	(0.060)
Zip code Republican contribution share	0.859	0.620	0.868	0.910	0.858	0.879
× Post	(0.091)	(0.088)	(0.091)	(0.091)	(0.091)	(0.091)
Active before		-0.222				
× Zip code Republican contribution share		(0.226)				
Active before × Post		2.535				
× Zip code Republican contribution share		(0.359)				
Age			-0.045			-0.036
× Zip code Republican contribution share			(0.004)			(0.005)
Age × Post			0.051			0.003
× Zip code Republican contribution share			(0.005)			(0.007)
Log wealth				-0.238		-0.074
× Zip code Republican contribution share				(0.035)		(0.038)
Log wealth × Post				0.355		0.254
× Zip code Republican contribution share				(0.056)		(0.063)
Log income				-0.216		-0.273
× Zip code Republican contribution share				(0.082)		(0.082)
Log income × Post				0.626		0.707
× Zip code Republican contribution share				(0.130)		(0.131)
Lagged equity share					0.590	-0.001
× Zip code Republican contribution share					(0.316)	(0.350)
Lagged equity share × Post					-3.261	-3.001
× Zip code Republican contribution share					(0.389)	(0.437)
Controls by year						
Baseline	Y	Y	Y	Y	Y	Y
Employer × County	Y	Y	Y	Y	Y	Y
R^2	0.189	0.191	0.189	0.189	0.189	0.189

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Table VI
Regressions of Price-Constant Equity Share Changes on Likely Political Affiliation for Subsamples

This table presents regression coefficients of annual changes in price-constant household portfolio equity shares on the zip code Republican contribution share, before and after the election, in various subsamples of the population: households with advised accounts (column 2), households with a single (not married) head of household (column 3), wealth in retirement accounts only (4), households with a non-retirement account (column 5), households with only retirement accounts (column 6), wealth in non-retirement accounts only (column 7), and wealth in retirement accounts for households with a non-retirement account (column 8). The price-constant equity share is calculated for a hypothetical portfolio that is insensitive to passive appreciations and is driven by trading only. The baseline controls are the lagged equity share, age, gender, marital status, log lagged financial wealth, log labor income in 2015, the lagged winning and losing sectors shares of equity, and zip code house price growth (2010–2015), interacted by annual indicators. In addition, we control for employer × county × period fixed effects. The sample period is October 2015 to October 2017, and we include RIs for which we observe the complete set of controls (27.7% of all RIs in the full sample). Standard errors are clustered at the zip code level.

	One-year difference in price-constant equity share (in %)							
	All	Advised account	Single	Ret. wealth	Non-ret. owner	Only ret. owner	Non-ret. wealth	Ret. wealth, non-ret. owner
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Zip code Republican contribution share	-0.160	-0.618	-0.245	-0.168	-0.325	-0.138	0.134	-0.428
	(0.059)	(0.677)	(0.103)	(0.058)	(0.213)	(0.061)	(0.343)	(0.215)
Zip code Republican contribution share × Post	0.859	0.923	0.670	0.835	1.252	0.796	0.198	1.283
	(0.091)	(1.050)	(0.162)	(0.090)	(0.328)	(0.094)	(0.490)	(0.326)
Controls by year								
Baseline	Y	Y	Y	Y	Y	Y	Y	Y
Employer × County	Y	Y	Y	Y	Y	Y	Y	Y
R^2	0.189	0.570	0.257	0.188	0.307	0.197	0.312	0.304
Share of observations	100.0%	1.5%	28.2%	100.0%	12.6%	87.4%	12.7%	12.6%

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Table VII
Regressions of Changes in Saving Rates on Likely Political Affiliation

This table presents regression coefficients of annual changes in household saving measures on the zip code Republican contribution share, before and after the election. The net flow rate is constructed as yearly deposits minus withdrawals as a fraction of initial balances. The net saving rate is defined as yearly deposits minus withdrawals as a fraction of income (derived from dividing annual income evenly over the year). The contribution rate applies only to households actively contributing to a retirement account. The baseline controls are the lagged saving rate measure, lagged equity share, age, gender, marital status, log lagged financial wealth, log labor income in 2015, the lagged winning and losing sectors shares of equity, and zip code house price growth (2010–2015), interacted by annual indicators. In addition, we control for employer × county × period fixed effects in even columns. In column (7)–(8), we also control for the personalized default annual increases of contribution rates. The sample is our full set of RI households between October 2015 and October 2017, for which we observe the complete set of controls (27.7% of all RIs). Standard errors are clustered at the zip code level.

	Change in net flow rate (%)		Change in net saving rate (%), household		Change in net saving rate (%), individual		Change in contribution rate (%)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Zip code Republican contribution share	-1.847	-0.417	-0.869	0.041	-0.732	0.073	-0.379	-0.118
	(0.196)	(0.239)	(0.101)	(0.143)	(0.121)	(0.171)	(0.018)	(0.026)
Zip code Republican contribution share × Post	1.682	0.833	0.700	0.751	0.597	0.635	-0.012	0.061
	(0.192)	(0.254)	(0.159)	(0.277)	(0.213)	(0.308)	(0.022)	(0.037)
Controls by year								
Baseline	Y	Y	Y	Y	Y	Y	Y	Y
Employer × County		Y		Y		Y		Y
R^2	0.643	0.706	0.267	0.360	0.263	0.375	0.065	0.188
Share of observations	100.0%	100.0%	96.6%	96.6%	96.6%	96.6%	87.8%	87.8%

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Table VIII
Regressions of Changes in Portfolio Measures on Likely Political Affiliation

This table presents regression coefficients of annual changes in price-constant household portfolio measures on the zip code Republican contribution share, before and after the election, for the full sample and for the subsample of households with an active portfolio reallocation in the prior year. The price-constant portfolio measures are calculated for a hypothetical portfolio that is insensitive to passive appreciations and is driven by trading only. Market betas are obtained by regressing monthly fund or security excess returns on the value-weighted CRSP market excess return over the period 2006–2019 with at least 24 observations. Winning (losing) sectors are sectors that did relatively well (poorly) between the election and the end of 2016. International equity holdings consist of individual foreign company stocks and funds that invest in international equity. The outcome variables that describe the composition of equity (columns (4)–(12)) are conditional on having strictly positive equity holdings. The baseline controls are the lagged portfolio measure, age, gender, marital status, log lagged financial wealth, log labor income in 2015, the lagged winning and losing sectors shares of equity, and zip code house price growth (2010–2015), interacted by annual indicators. In addition, we control for employer × county × period fixed effects in most columns. The sample period is October 2015 to October 2017, and we include RIs for which we observe the complete set of controls (27.7% of all RIs in the full sample), 97.3% of which have strictly positive equity shares. Standard errors are clustered at the zip code level.

A. Market betas	Beta of portfolio			Beta of equity		
(one-year difference in price-constant beta, ×100)	All	Active	Active	All	Active	Active
	(1)	(2)	(3)	(4)	(5)	(6)
Zip code Republican contribution share	-0.120	-0.263	0.235	-0.007	0.226	0.778
	(0.068)	(0.550)	(0.265)	(0.051)	(0.512)	(0.247)
Zip code Republican contribution share × Post	0.831	1.416	2.011	-0.014	-1.151	-0.624
	(0.113)	(0.845)	(0.398)	(0.082)	(0.765)	(0.358)
Controls by year						
Baseline	Y	Y	Y	Y	Y	Y
Employer × County	Y	Y		Y	Y	
R^2	0.175	0.392	0.119	0.174	0.358	0.104

B. Sector and global allocations	Winning sectors share of equity		Losing sectors share of equity		International share of equity	
(one-year difference in price-constant measures, in %)	All	Active	All	Active	All	Active
	(7)	(8)	(9)	(10)	(11)	(12)
Zip code Republican contribution share	0.017	0.053	0.004	0.052	-0.068	-0.497
	(0.032)	(0.257)	(0.025)	(0.215)	(0.030)	(0.217)
Zip code Republican contribution share × Post	-0.067	-0.359	-0.041	-0.588	-0.014	0.503
	(0.048)	(0.367)	(0.038)	(0.327)	(0.047)	(0.343)
Controls by year						
Baseline	Y	Y	Y	Y	Y	Y
Employer × County	Y	Y	Y	Y	Y	Y
R^2	0.149	0.343	0.209	0.392	0.152	0.348

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Online Appendix for

"Belief Disagreement and Portfolio Choice"

by

Maarten Meeuwis, Jonathan A. Parker, Antoinette Schoar,

and Duncan Simester*

September 30, 2021

Appendix A. Political Contributions Data

We construct a measure of likely political party affiliation using publicly available campaign finance data from the Federal Election Commission. We consider individual contributions to party committees, campaign committees, and political action committees during the 2015 to 2016 election cycle and aggregate to the zip code level to calculate the zip code Republican share of donations.

Individual contributions. We use donations from the FEC individual contributions file and limit the sample to contributions of individuals with a valid zip code on record. We impose a standard filter to select actual contribution transactions (transaction types 10, 11, 15, 15E, 21Y, and 22Y) and impose transaction amounts for refunds (types 21Y and 22Y) to be negative.

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Party committees. We consider individual contributions to the main party and candidate committees by selecting committees with at least $20 million in contributions, supporting a party or presidential nominee. The restriction to more than $20 million in contributions yields a set of 32 committees for a total of $2.3 billion in individual contributions from 7.8 million transactions. Further restricting the list of committees to those not related to a senator or losing presidential primary candidate leaves 21 committees. Appendix Table A.I provides an overview of the selected and discarded committees with more than $20 million in contributions by individuals. The resulting individual contributions sample includes 1.0 million distinct donors with a total of $1.8 billion in contributions. Of those donors, 672 thousand contribute to the Democratic party or candidate, 340 thousand contribute to the Republican party or candidate, and two thousand to both.

Republican contribution share. We select zip codes with at least 10 donors and construct the zip code Republican contribution share as the number of donors to the Republican party or candidate divided by the number of donors to either party. For robustness checks, we consider two alternative measures of likely party affiliations. First, we also construct the dollar-weighted version of the zip code Republican contribution share. Second, we calculate the county-level Republican vote share as the number of votes for the Republican candidate Donald J. Trump divided by the number of votes for either Trump or the Democratic candidate Hillary Clinton. Aggregating donations from zip codes to counties, the correlation between the Republican contribution share and the Republican vote share across counties is 0.68 (see Figure A.3). For the dollar-weighted contribution share aggregated to the county level, the correlation with the Republican vote share is 0.51.

Likely party affiliations in sample. Appendix Figure A.5A plots the distribution of likely political affiliations measured by the zip code Republican contribution share in our sample of RIs. Appendix Figure A.5B plots the distribution of county vote shares in the sample and population. Republican shares measured by donations are typically lower than Republican shares measured by votes. Relative to the population, our sample is tilted towards Democrats.

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Appendix B. Household Portfolios Data

Asset classes. Investor portfolios consist of positions in funds, individual securities, and annuities. For some holdings (e.g. some annuities), we do not observe sufficient detail to categorize holdings. Average holdings in these assets are less than 1.3% of total (investable) assets. For 92% of all remaining assets in investor portfolios we observe the CUSIP, and for the other 13% we observe basic characteristics of the fund the wealth is invested in. We assign holdings to four different asset classes based on product descriptions: equity, long-term bonds, short-term bonds, and alternative assets. Equity holdings consist of pure equity funds, directly held equity, and the equity portion of funds that invest across asset classes. The bond category includes bond funds, individual government and corporate bonds, and the portion of funds that invest across asset classes that is not allocated to equity. The cash and cash-like securities category is composed of cash and money market mutual funds. Alternative assets include real estate (REITs), precious metals, and royalty funds.

We split mixed-assets funds, such as lifecycle funds, into equity and long-term bond holdings based on fund equity shares. We use quarterly data on fund asset compositions from the CRSP Survivor-Bias Free US Mutual Fund database if available, and complement this with internally available quarterly target equity shares on other mixed-asset funds.

International exposure. To characterize international equity exposures in investor portfolios, we divide equity holdings into a domestic and an international component. Pure equity funds are characterized as either domestic or international based on internal product descriptions. We consider the equity portion of mixed-asset funds to be a domestic equity investment. For individual securities, we set the location to international if it is a foreign security (i.e., has a foreign ISIN) or if the company is incorporated outside of the US according to Compustat, and to domestic otherwise. We define the international share of equity as the ratio of international equity to total portfolio equity holdings.

Sector exposures. Investors can explicitly load on industries by investing in sector funds or by holding individual equities. We identify sector funds as funds that have a sector index as Morningstar benchmark. These sector indices are defined based on 11 Global Industry Classification Standard

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(GICS) sectors: energy, materials, industrials, consumer discretionary, consumer staples, health care, financials, information technology, telecommunication services, utilities, and real estate. For individual securities, we assign GICS industry codes to stocks by linking them to Compustat and CRSP data. If a stock can be linked to a Compustat record, we use the Compustat GICS sector code. If no Compustat record is available, we use the North American Industry Classification System (NAICS) code from CRSP and get the corresponding GICS code from a crosswalk table.[1]

Returns. We link observed portfolio holdings at the CUSIP level to external data on realized returns from CRSP stock, treasury, and mutual fund return files, as well as WRDS corporate bond returns. When we do not observe an asset's return in external data, we use internal data to compute realized returns.

Market betas. To calculate CAPM market betas, we use all available return data from 2006 to 2019. We estimate betas from monthly regressions of excess asset returns on excess market returns. We assign a market beta to funds and securities that have at least 24 monthly return observations. We use public return data on funds and securities if available, and otherwise use returns computed from internal data.

Appendix C. Additional Results

C.1. ***Panel Regressions in Levels***

In our main regression specification (2), we regress changes in equity shares on likely political affiliation. We find very similar effects if instead of running this regression in first differences, we run a panel regression in levels with individual fixed effects. In particular, we now estimate an equation of the form:

$$P_{i,t} = \sum_s (\beta_s R_{z(i)} + \theta_s' X_i) 1_{s=t} + \tau_t + \tau_i + \eta_{i,t}, \tag{A.1}$$

[1] We use the concordance from NAICS to GICS provided by Alison Weingarden available (July 2018) at sites.google.com/site/alisonweingarden/links/industries.

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where X_i is a vector of time-invariant controls and τ_i is an individual-specific intercept.[2] As before, standard errors are clustered at the zip code level.

Appendix Figure A.17 plots the estimated coefficients on the Republican share in each three-month period before and after the election, relative to October 31, 2016 (because we impose $\beta_0 = 0$). Panel (a) shows the results for the realized equity share and panel (b) shows the results for the price-constant equity share, using the baseline set of controls. Panels (c) and (d) additionally control for employer-county-period fixed effects. The resulting paths for the difference in equity shares between likely Republicans and likely Democrats match the cumulative coefficients of the first-differences regressions in Figure 9. Appendix Tables A.X and A.XI report the corresponding estimated coefficients of these panel regressions of equity shares and price-constant equity shares, respectively, for the various sets of controls.

C.2. ***Sample Selection***

Due to the size of the dataset with millions of investors and trillions in assets, our coefficients of interest are precisely estimated, even when we restrict our analysis to the sample of Retirement Investors (RIs) for whom we observe complete information on the full set of controls. However, since our final regression sample is selected on several dimensions, this raises the question of whether our point estimates extend to the full dataset or whether they are affected by the type of investors that are included in the regressions.

To address this question, we construct a non-selected and unbalanced sample by drawing a random sample of one million households that are between the ages of 25 and 84 and have positive asset holdings on October 31, 2015. As in the main dataset, we track the portfolios of these households over the year prior to the election and the year following the election.

We estimate equation (3) of annual changes in price-constant equity shares on zip code political affiliation for various subsamples of this new random sample. As a basic set of controls, we include the lagged equity share, age, log wealth, and the lagged winning and losing sectors share of equity – these are the controls that are available for the full sample. We ask whether our main results are affected by the two key selection criteria for our baseline regression sample: (i) RIs of working age

[2] We impose $\beta_0 = 0$ and $\theta_0 = 0$ to avoid collinearity with the individual effects.

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(25–64), for whom (ii) we observe complete observations on the control variables.

Table A.XII reports the estimated coefficients for various subsamples. We find that effect on political affiliation is robust to alternative and less restrictive samples. We report coefficients for the full sample, as well as subsamples restricted to age below 65 (88% of the full sample), the RI sample (70%), and the RI sample with observations of gender and marital status (57%), employer (47%), industry (46%), income over 2015–2017 (29%), and all controls (21%), respectively. In fact, we find that the point estimate is lowest under the baseline and most restrictive criteria: RI investors with complete observations on all control variables.

C.3. ***Zip-Code-Level Regressions***

In our main analysis, we relate household portfolios to political affiliations that are measured at the zip code level, with individual-level controls to maximize the precision of our estimates. Since we are working off between-zip-code-level variation in political affiliations, we can only hope to explain between-zip-code variation in portfolios. As reported in Table III and later tables, political affiliation does not explain a large share of the overall variation in household portfolio changes over time. But neither do the other investor characteristics that we observe.

To examine the explained variation across zip codes and as a robustness check, we run a purely zip-code-level regression. We average individual portfolios and characteristics by zip code, and estimate the following specification:

$$\Delta P_z = \beta R_z + \theta' X_z + \eta_z, \tag{A.2}$$

where variables observed at the individual level are averaged by zip code z. We weight zip codes by the number of individuals in our sample and estimate this regression on post-election annual changes. Note that this is a purely cross-sectional regression, so that we can immediately see how much of the post-election portfolio reallocation is explained by political orientation and by the control variables.

Table A.XIII report the estimated coefficients for the post-election year. We uncover the same relation between portfolio changes and the zip code Republican contribution share as in household-

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level regressions. Between zip codes, political affiliation explains 3.5% of the variation in equity shares.[3] Due to strong mean reversion in portfolios, the average initial equity share explains 5.6% of the variation in equity share changes. All other demographic variables, such as age, financial wealth, and income, explain less of the post-election variation in portfolio changes across zip codes than political affiliation.

*C.4. **Education Controls***

One limitation of the investor dataset is that we do not observe information on education. Since political affiliations may be correlated with education and investors with different educational attainment may face different wealth effects or hedging needs in response to the election outcome, education is a potential omitted variable. We therefore run a robustness check where we control for zip-code-level education measures from the 2015 American Community Survey in our baseline regression. We collect the share of the population without a high school diploma, the share of high school graduates, and the share of people with a college degree by zip code.

In Table A.XIV, we repeat the main regression of changes in price-constant equity shares on likely political affiliation with these additional zip-level controls for educational attainment. We pick having a high school diploma as the baseline and control for the share of people in the zip code without a high school diploma and the share of people with a college degree. Since the results are very similar to Table III, our main findings are robust to including controls for education.

*C.5. **Instrumental Variables Approach***

Political affiliation is not exogenously assigned and can be driven by whether a particular candidate and the candidateâs policies financially benefit the voter personally more than the other partyâs candidate. Even after including detailed controls for households' economic exposures and hedging demands, it is therefore possible that we still pick up some residual responses of investors to direct financial effects of the change in governance.

As an alternative to controlling for observable heterogeneity, we consider an instrumental vari-

[3] At the individual level, the share of explained variation is approximately zero due to idiosyncratic variation in individual portfolios.

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ables (IV) approach to address potential endogeneity, where we estimate our main regression specification by instrumenting for political affiliation. It is well known that after controlling for other observables like age, income, and education, average political affiliations differ by race. The explanation for these differences is more likely historical or due to non-economic issues so they are less likely to be driven by voters choosing a party based on who will help their financial situation. Since the investor dataset does not contain information on race, we collect data on the racial composition of zip codes from the American Community Survey. We use this composition to construct instrumental variables for political affiliation. As instruments, we use the zip code population shares of white, black, and Asian individuals, and the share of the population of Hispanic or Latino origin. Indeed, the first stage is highly significant, both under the baseline controls and with additional fixed effects.

Table A.XV reports the results when we instrument the zip code Republican contribution share by these zip code demographic variables. Without employer controls, we find effects with a very similar magnitude as our baseline estimates. With employer or employer-county fixed effects (by period), we even find larger point estimates that are still highly statistically significant.

C.6. ***Change in Equity Share Relative to Initial Share***

In the frictionless Merton (1969) model with agents that have constant relative risk aversion preferences, the optimal portfolio share is the myopic allocation that scales by the expected excess return on the market. In that case, a change in expected returns would lead to a proportional change in the equity share.

To study relative changes in equity shares, we run a version of the main regression where the outcome variable is the log equity share (excluding the roughly 5% of households with zero equity share). The coefficient then directly gives the change in equity share as a percentage of the initial equity share. Table A.XVI reports the estimated coefficients, comparing the sample of all households to the sample of active traders in the prior year. In the version with employer-county-year fixed effects, we find that Republicans increase their equity share by 1.4% more than Democrats, while for active traders the increase in equity share for Republicans is 4.8% more than for Democrats as a fraction of the initial equity share. Hence, we derive similar conclusions when looking at

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proportional changes across households.

C.7. ***Tails of Political Affiliation Measure***

In our analysis, political affiliation is inferred probabilistically from party affiliations at the zip code level. Should we expect to find similar effect sizes if we had individual-level party affiliations? We provide suggestive evidence by measuring effects in the subsample of households that live in zip codes with a strong affiliation to either political party. For these zip codes, the measurement error of individual political affiliations is substantially smaller.

Table A.XVII shows the results when we estimate the regression of annual portfolio changes on political orientation for households that live in zip codes with a pronounced political affiliation: zip codes that have a Republican contribution share below 35% or above 65%. We measure effects for this subsample that are very similar to those for the full sample. We would therefore expect to find that our estimated regression coefficients apply to individual-level affiliation data.

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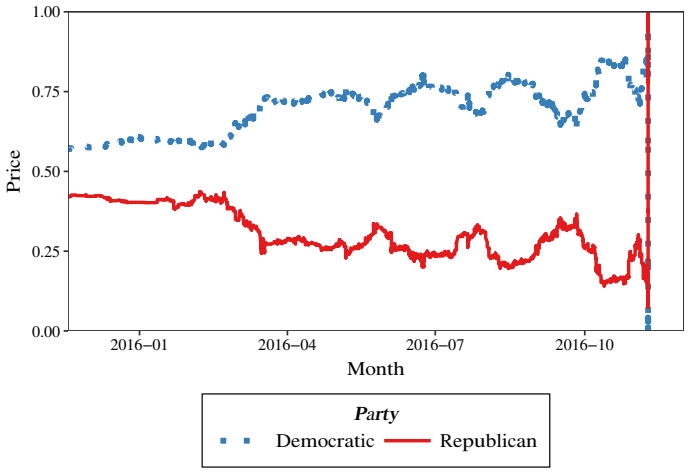

Figure A.1. Probability of Party Winning the 2016 Presidential Election. This figure plots the betting market-implied probabilities of a Democratic versus a Republican win over time. It shows the prices of two contracts traded on UK-based betting exchange Betfair, obtained through PredictWise, that pay $1 conditional on the respective party winning the election.

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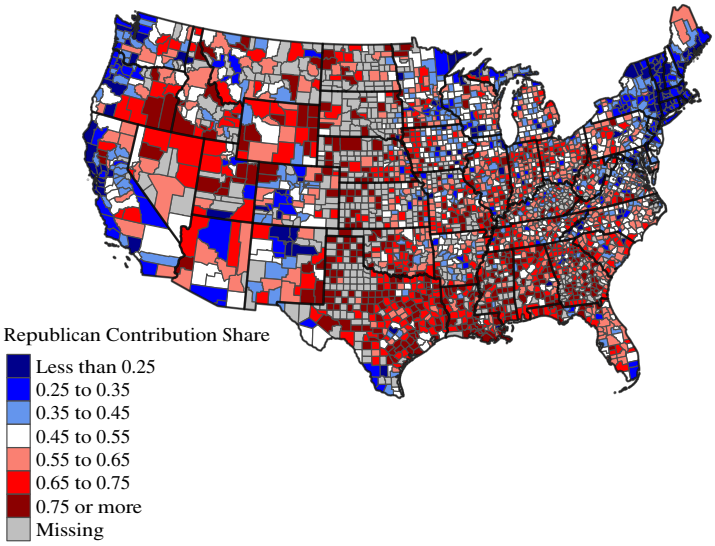

Figure A.2. Map of Republican Contribution Share. This figure shows the geographical distribution of the Republican contribution share over the 2015-2016 election cycle. The Republican contribution share is defined as the number of individuals with campaign donations to the main Republican party and candidate committees as a fraction of the total number of individuals with campaign donations to the main committees of either party. We aggregate zip-level donations to the county level for geographical illustration and include locations with at least 10 donors.

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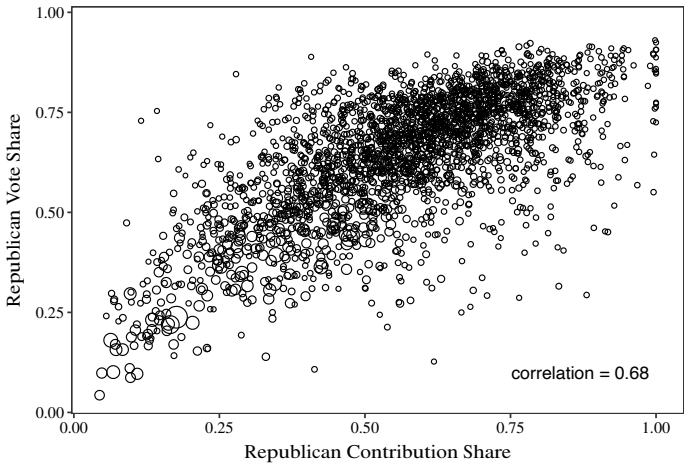

Figure A.3. Republican Contribution Share Versus Republican Vote Share by County. This figure plots the county-level Republican contribution share against the Republican vote share of the county for the 2016 presidential election. The county-level Republican contribution share is obtained by aggregating zip-code-level donations by county. The size of the point reflects the number of households that live in that county.

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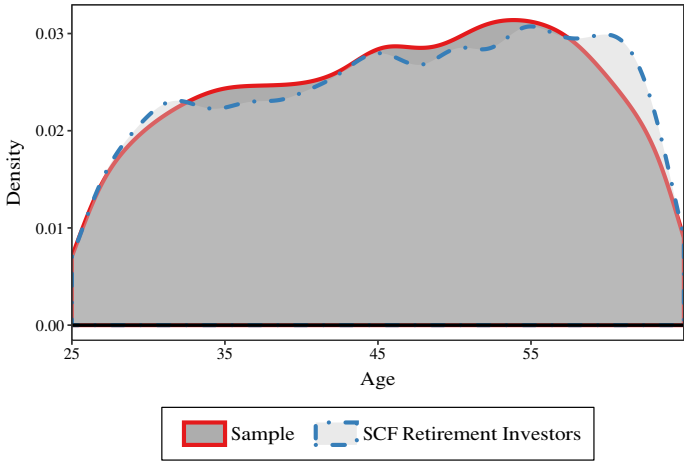

Figure A.4. Age Distribution in Comparison to SCF. This figure plots the age distribution in our sample compared to the equivalent sample of RIs in the public version of the 2016 Survey of Consumer Finances (SCF). We select households with quasi-liquid retirement wealth and run quantile regressions of log retirement wealth on a second-order polynomial in age for households in the 2016 SCF. We use the fitted 10th and 90th percentiles by age as retirement wealth cutoffs in both datasets. We include households with age of the head between 25 and 64 and filter our sample on households that have portfolio holdings between 20% and 500% of initial assets in every month in the sample.

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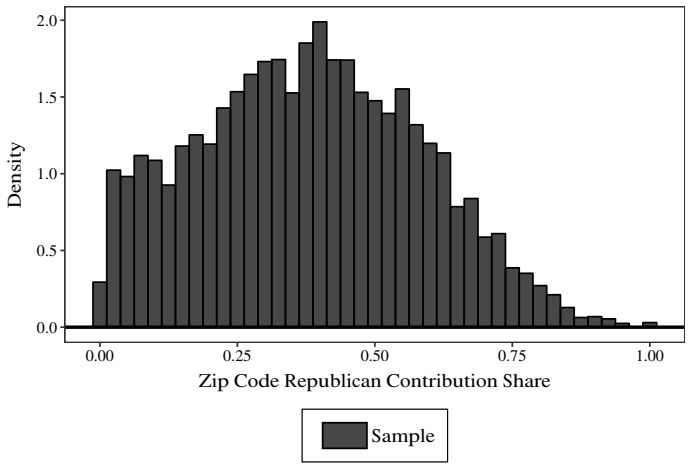
Panel A. Republican Contribution Share



Panel B. Republican Vote Share

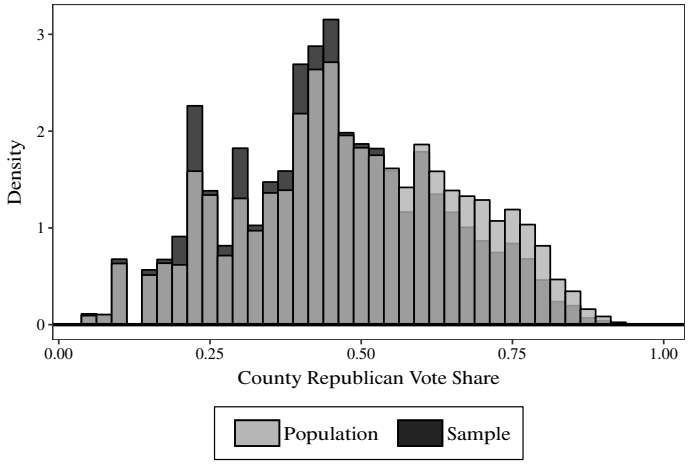


Figure A.5. Distribution of Likely Political Affiliation Measures. These graphs plot the distribution of the zip code Republican contribution share and the county Republican vote share, respectively. Panel (a) plots the distribution of the zip code Republican contribution share, defined as the number of individuals with campaign donations to the main Republican party and candidate committees as a fraction of the total number of individuals with campaign donations to the main committees of either party, in our RI sample. We include zip codes with at least 10 donors. Panel (b) plots the county Republican vote share, defined as the number of votes for Republican candidate Donald J. Trump divided by the total number of votes for Trump and for the Democratic candidate Hillary Clinton, in the population and in our RI sample.

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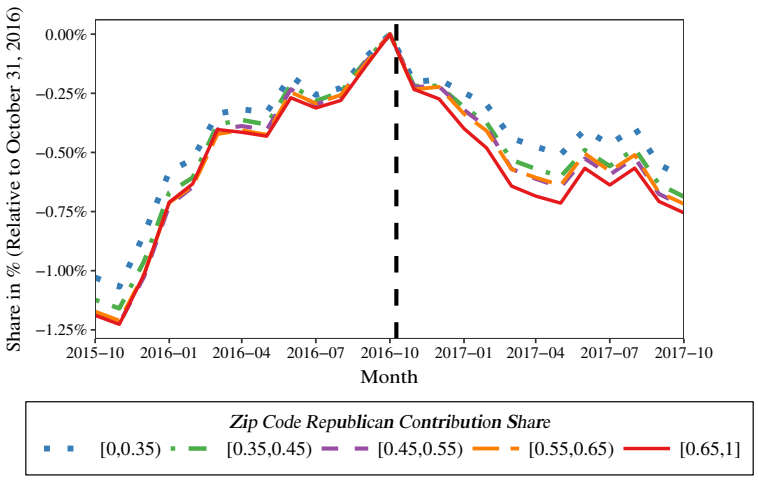
Panel A. Bond Share (Equally Weighted Across Households)



Panel B. Cash Share (Equally Weighted Across Households)

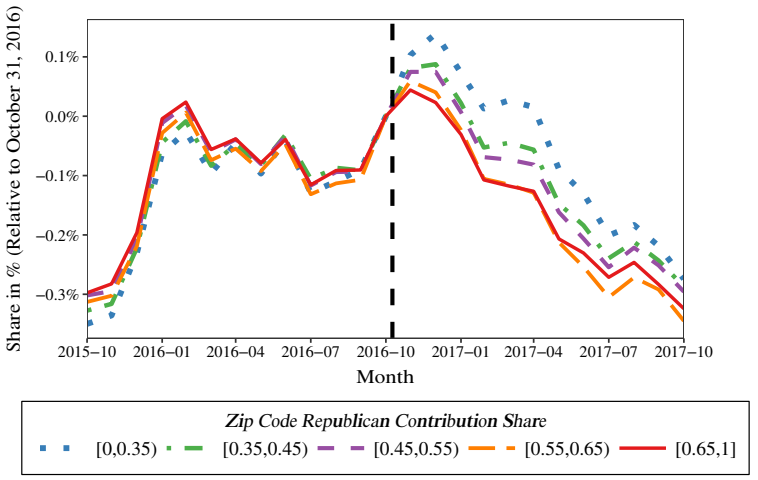


Figure A.6. Portfolio Shares by Zip Code Party Affiliation. These graphs plot the average bond share and cash share, respectively, of household portfolio assets in five groups by zip code party affiliation measured from political contributions, relative to the share by the end of October 2016. The sample is our full set of RI households. Average shares by group are equally weighted across households.

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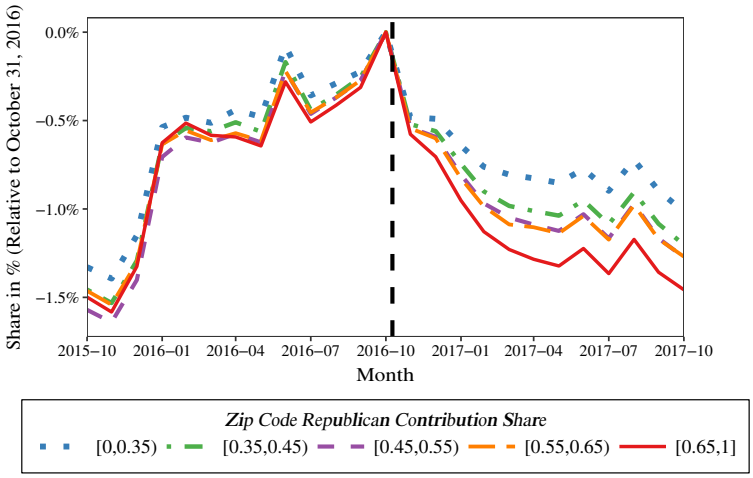
Panel A. Bond Share (Value Weighted Across Households)



Panel B. Cash Share (Value Weighted Across Households)

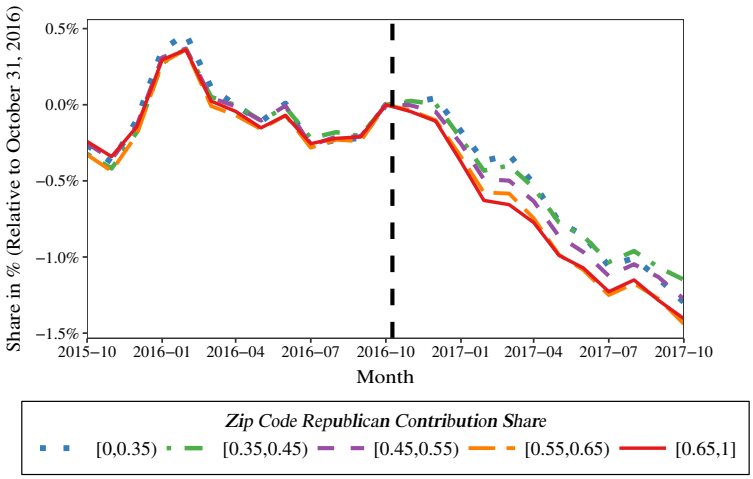


Figure A.7. Value-Weighted Portfolio Shares by Zip Code Party Affiliation. These graphs plot the average bond share and cash share, respectively, of household portfolio assets in five groups by zip code party affiliation measured from political contributions, relative to the share by the end of October 2016. The sample is our full set of RI households. Average shares by group are asset weighted across households.

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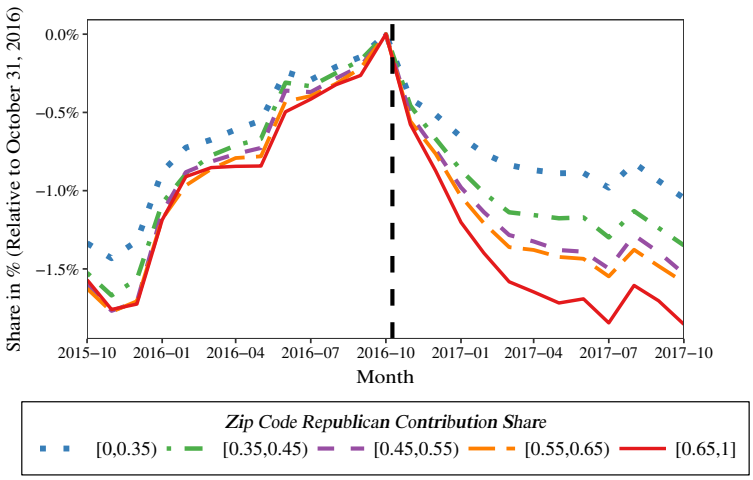
Panel A. Bond Share (Equally Weighted Across Households)



Panel B. Cash Share (Equally Weighted Across Households)

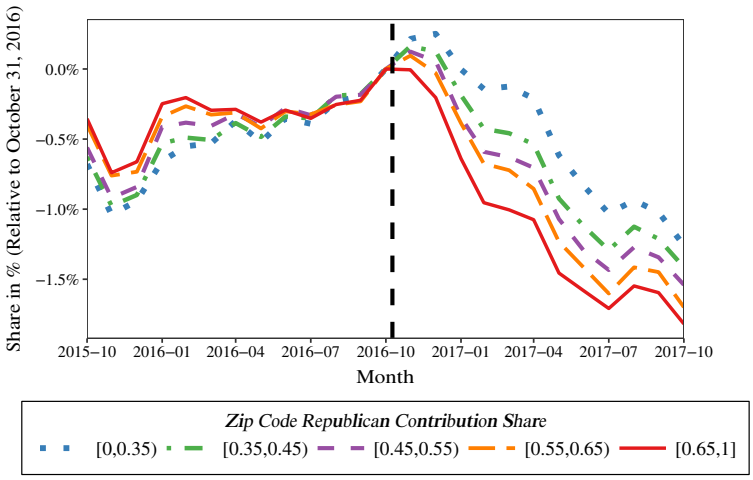


Figure A.8. Portfolio Shares by Zip Code Party Affiliation for Previously Active Sample. These graphs plot the average bond share and cash share, respectively, of household portfolio assets in five groups by zip code party affiliation measured from political contributions, relative to the share by the end of October 2016. The sample is the subset of RI households with an active portfolio reallocation in the prior year (11.1% of all RIs). Average shares by group are equally weighted across households.

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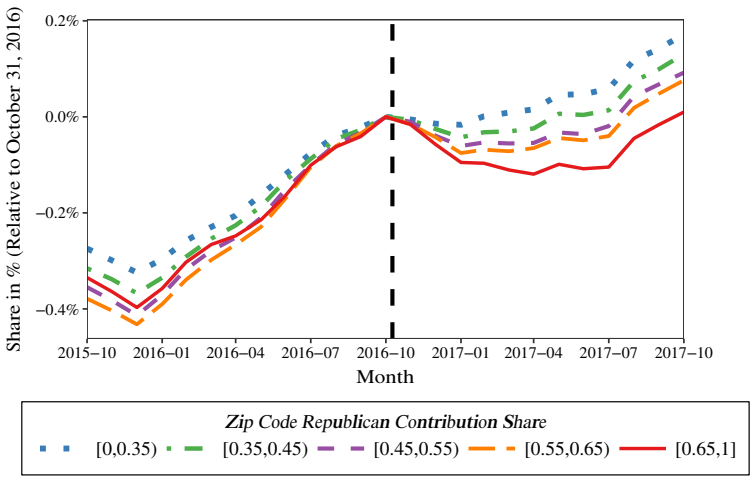
Panel A. Bond Share of Price-Constant Portfolios



Panel B. Cash Share of Price-Constant Portfolios

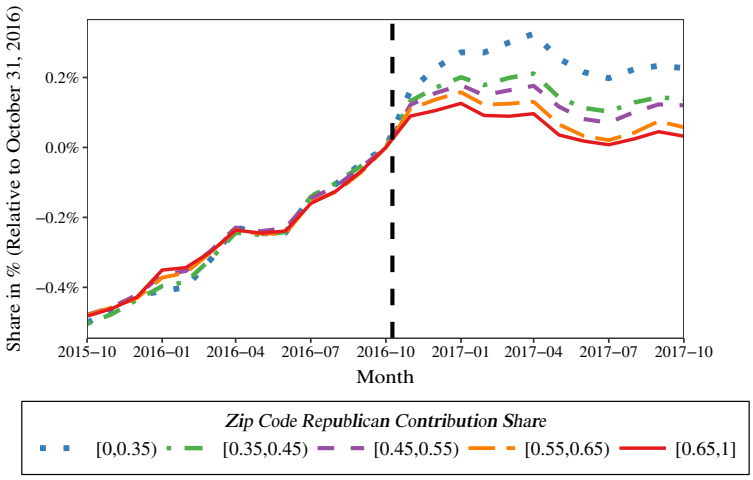


Figure A.9. Price-Constant Portfolio Shares by Zip Code Party Affiliation. These graphs plot the average bond share and cash share, respectively, of hypothetical price-constant household portfolios in five groups by zip code party affiliation measured from political contributions, relative to the share by the end of October 2016. The price-constant equity share is calculated for a hypothetical portfolio that is insensitive to passive appreciations and is driven by trading only. The sample is our full set of RI households. Average shares by group are equally weighted across households.

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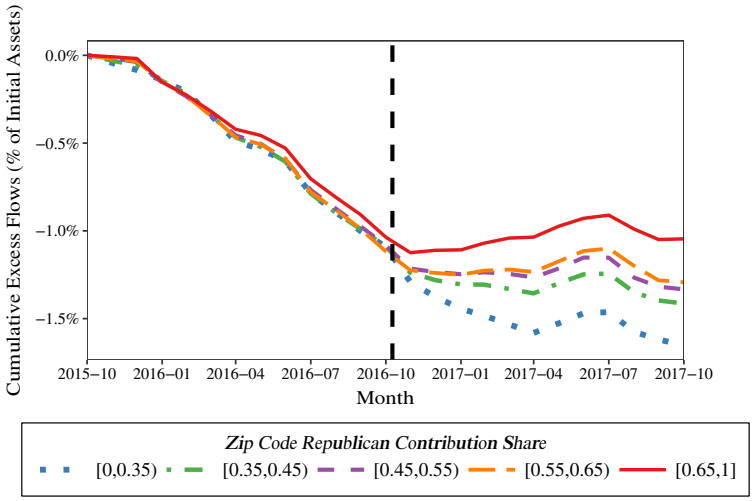

Figure A.10. Cumulative Excess Flows into Equity by Zip Code Party Affiliation. This figure plots cumulative excess flows into equity in five groups by zip code party affiliation measured from political contributions, starting from October 31, 2015. Excess flows are scaled by initial assets, and are defined as net equity flows minus the equity share from the previous month multiplied by total portfolio net flows. The sample is our full set of RI households. Average flow rates by group are equally weighted across households.

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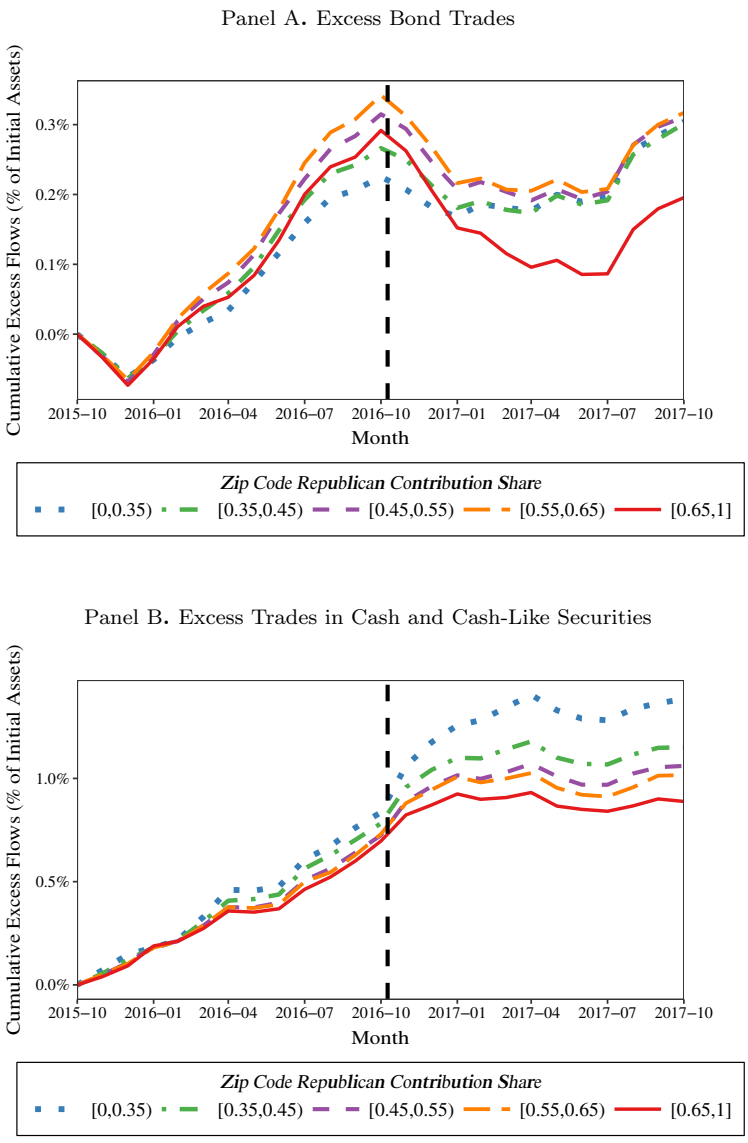

Figure A.11. Cumulative Excess Flows into Bonds and Cash by Zip Code Party Affiliation. These graphs plot cumulative excess flows into bonds and cash, respectively, in five groups by zip code party affiliation measured from political contributions, starting from October 31, 2015. Excess flows are scaled by initial assets, and are defined as net flows in the asset class minus the asset class share from the previous month multiplied by total portfolio net flows. The sample is our full set of RI households. Average flow rates by group are equally weighted across households.

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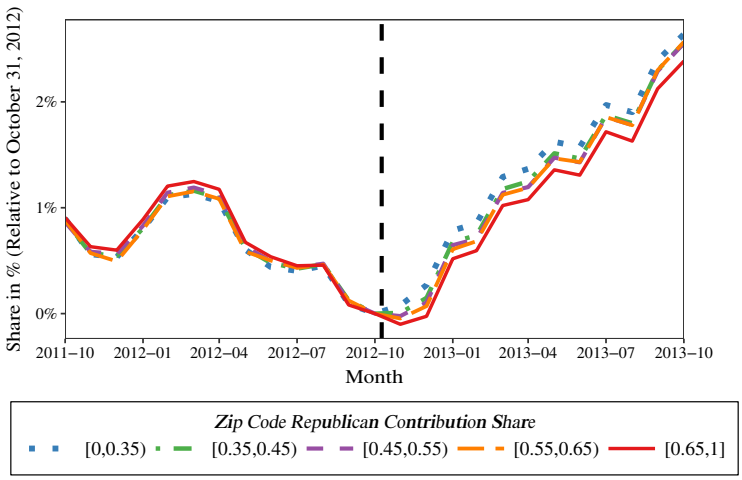
Panel A. Equity Share



Panel B. Equity Share of Price-Constant Portfolios

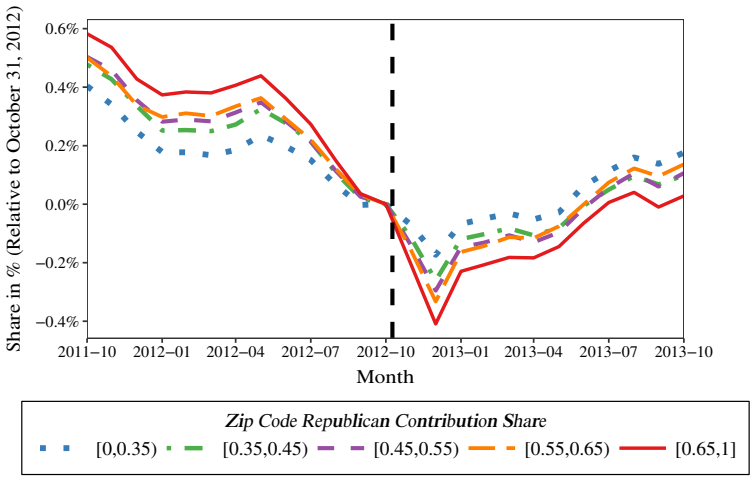


Figure A.12. Portfolio Equity Share by Zip Code Party Affiliation for 2012 Election. These graphs plot the average equity share of realized household portfolios and of hypothetical price-constant portfolios, respectively, in five groups by zip code party affiliation measured from political contributions for the 2012 presidential election, relative to the share by the end of October 2012. The price-constant equity share is calculated for a hypothetical portfolio that is insensitive to passive appreciations and is driven by trading only. The sample is the full set of RI households using our same procedure applied to the 2012 election. Average equity shares are equally weighted across households.

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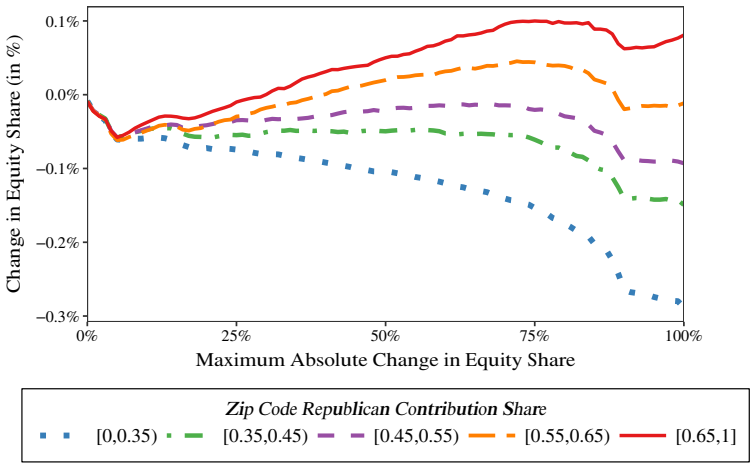
Panel A. Average Change in Equity Share by Maximum Size of Change



Panel B. Cumulative Difference in Equity Share of Republicans Versus Democrats by Type of Adjustment

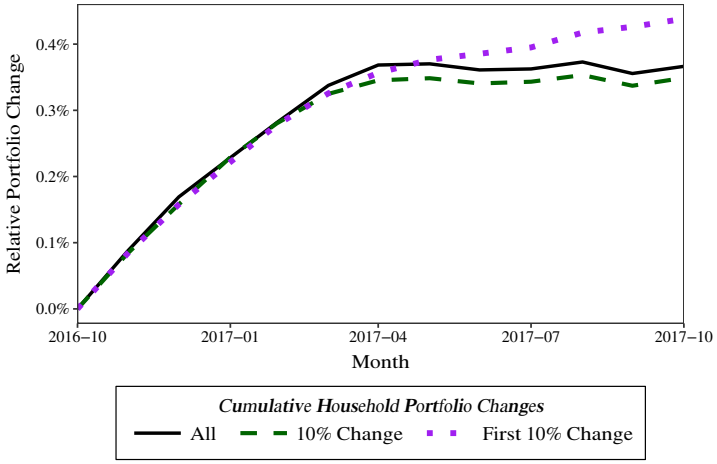


Figure A.13. Decomposition of Price-Constant Equity Share Changes by Zip Code Party Affiliation. The graph in panel (a) breaks down changes in the equity share of hypothetical price-constant household portfolios over the year following the election, for five groups by zip code party affiliation measured from political contributions. We plot average changes in equity shares, only including changes that are smaller than k%, as a function of k. Changes bigger than k% are set to zero. In panel (b) we plot the average cumulative change in the equity share of price-constant portfolios after the election for households in zip codes with a Republican contribution share of at least 65% relative to the average cumulative change for households in zip codes with a Republican contribution share of at most 35%. The solid line includes all changes in portfolio equity shares, the dashed line includes only changes that are at least 10% relative to the share at the end of October 2016 (and sets the change to zero otherwise), and the dashed line includes only the first change of at least 10% since October 2016. Price-constant equity shares are calculated for hypothetical portfolios that are insensitive to passive appreciations and are driven by trading only. The sample is our full set of RI households. Average shares by group are equally weighted across households.

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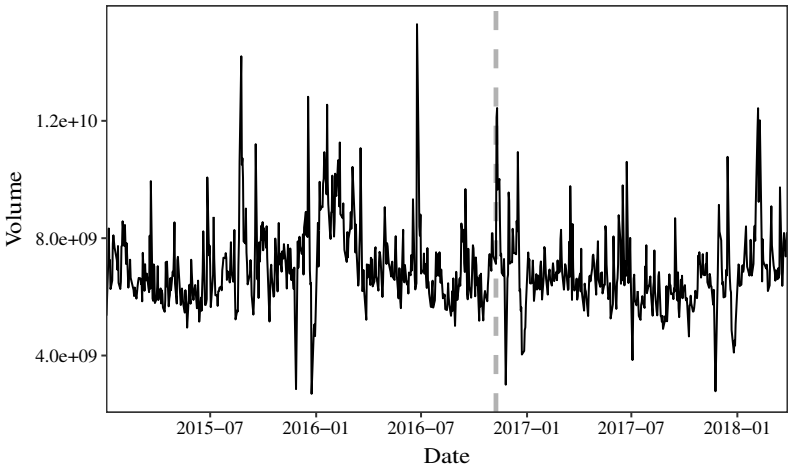
Panel A. US Equity Market Volume



Panel B. US ETF Market Volume

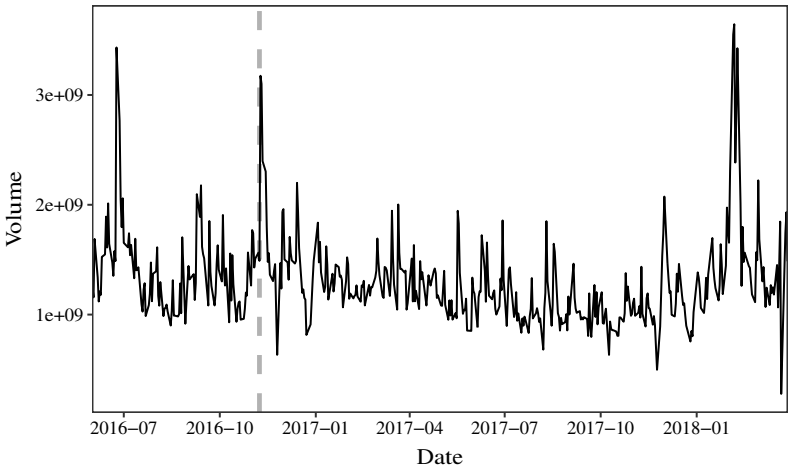


Figure A.14. Trading Activity in US Markets. This figure plots total trading volume on US markets. The upper panel plots total US equity market volume. The lower panel plots the narrower ETF market volume. The data is sourced from the CBOE.

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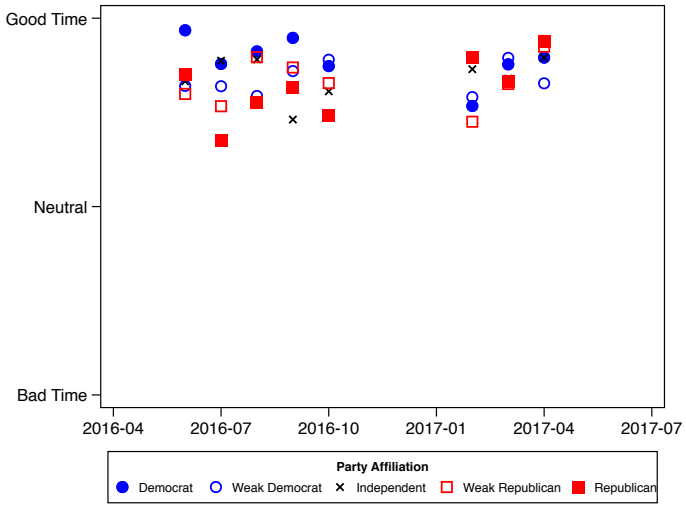
Panel A. Conditions for Buying Major Household Items



Panel B. Conditions for Buying a House

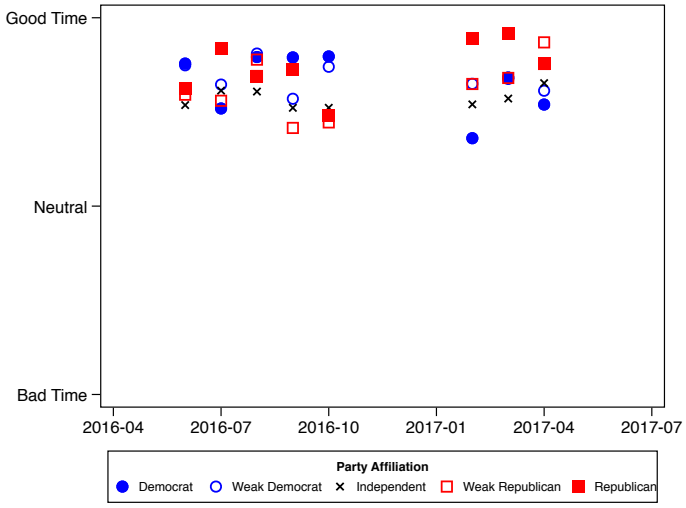


Figure A.15. Survey Evidence on Expenditures. These graphs plot survey evidence on spending behavior by political affiliation. The data is from the University of Michigan Survey of Consumer Confidence (SCC). The upper panel shows the average response to the question "Generally speaking, do you think now is a good or a bad time for people to buy major household items?" The lower panel shows the response to the same question on buying a house.

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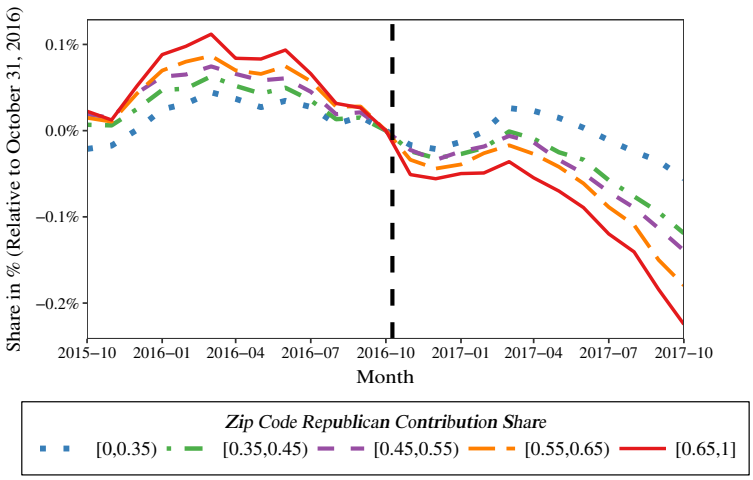
Panel A. Share of Equity in Winning Sectors



Panel B. Share of Equity in Losing Sectors

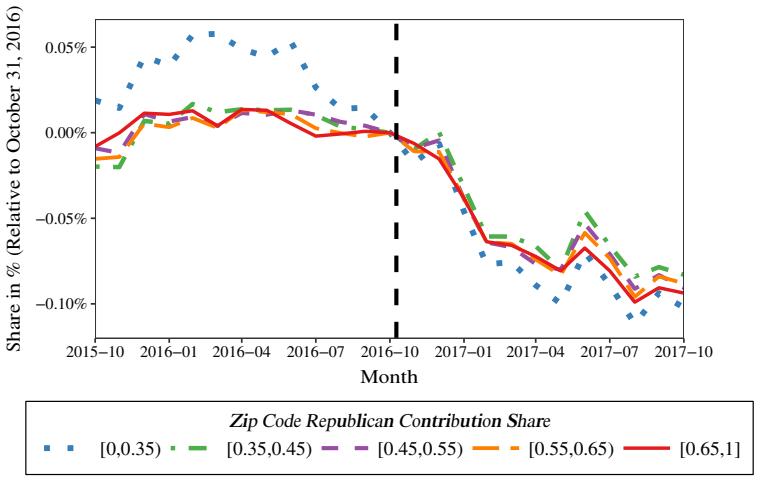


Figure A.16. Sector Allocations of Price-Constant Equity Portfolios by Zip Code Party Affiliation. These graphs plot the average price-constant shares of household equity in winning and losing sectors, respectively, in five groups by zip code party affiliation measured from political contributions, relative to the share by the end of October 2016. The price-constant portfolio measures are calculated for a hypothetical portfolio that is insensitive to passive appreciations and is driven by trading only. Winning (losing) sectors are sectors that did relatively well (poorly) between the election and the end of 2016. The sample is our full set of households. Averages by group are equally weighted across households.

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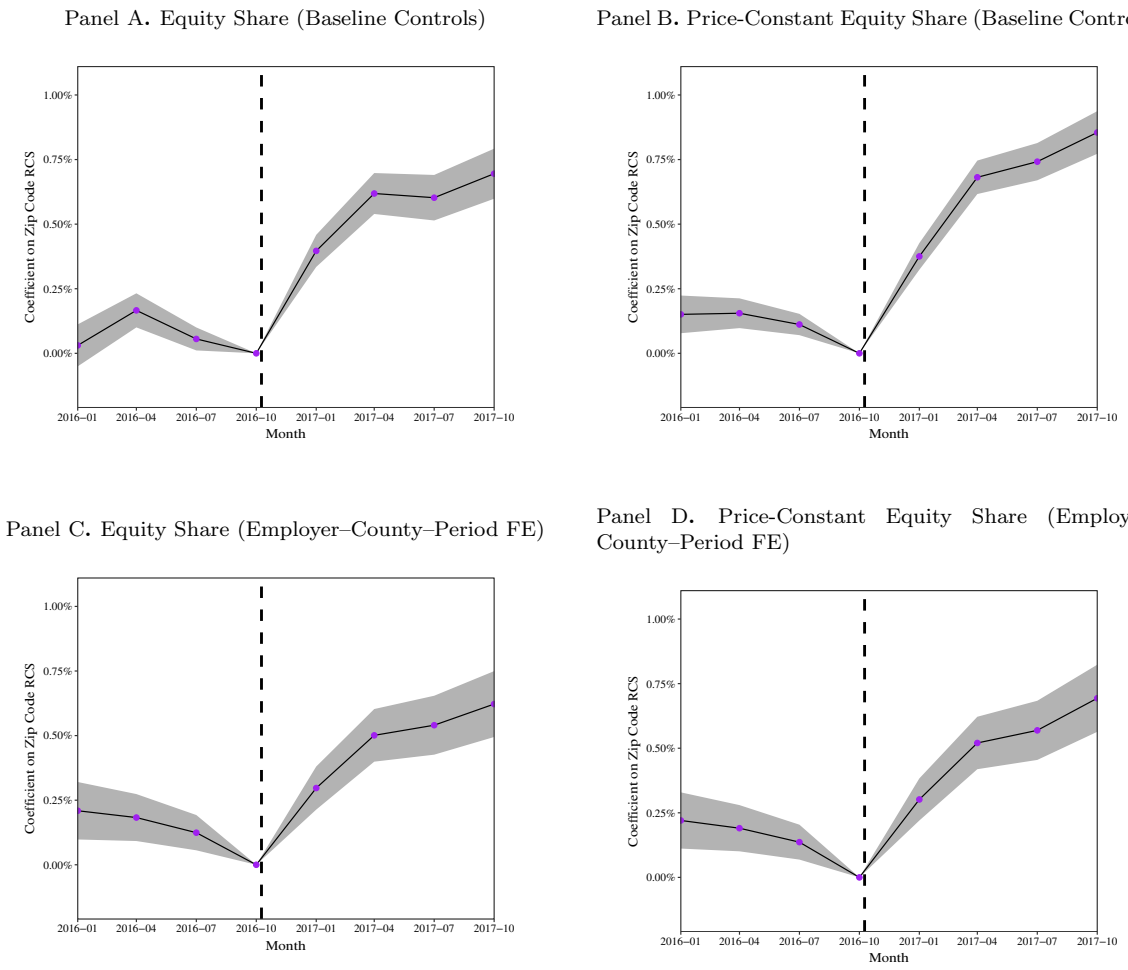

Figure A.17. Panel Regression Coefficients of Equity Shares on Likely Political Affiliation. This figure plots the estimated regression coefficients of quarterly household portfolio equity shares on the zip code Republican contribution share, for the three quarters prior to the election and the four quarters following the election, relative to allocations just before the election. In panels (a) and (c) we report the results for the observed equity share, and in panels (b) and (d) we report the results for the equity share of hypothetical price-constant portfolios that are insensitive to passive appreciations and are driven by trading only. The baseline controls are the initial equity share, age, gender, marital status, log initial financial wealth, log labor income in 2015, the initial winning and losing sectors shares of equity, and zip code house price growth (2010–2015), all interacted by quarterly indicators, as well as individual fixed effects. In panels (c) and (d) we additionally control for employer × county × quarter fixed effects. The sample is our full set of RI households between October 2015 and October 2017, for which we observe the complete set of controls (27.7% of all RIs). Standard errors are clustered at the zip code level.

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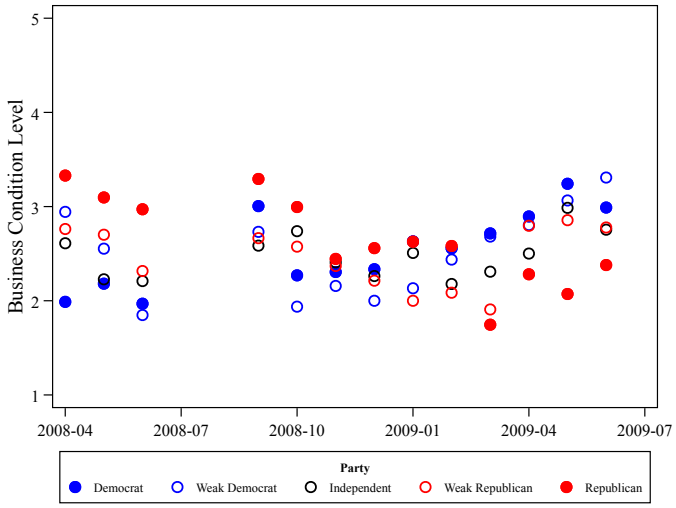
Panel A. Expected Business Conditions in 5 Years



Panel B. Expectations of Unemployment in a Year

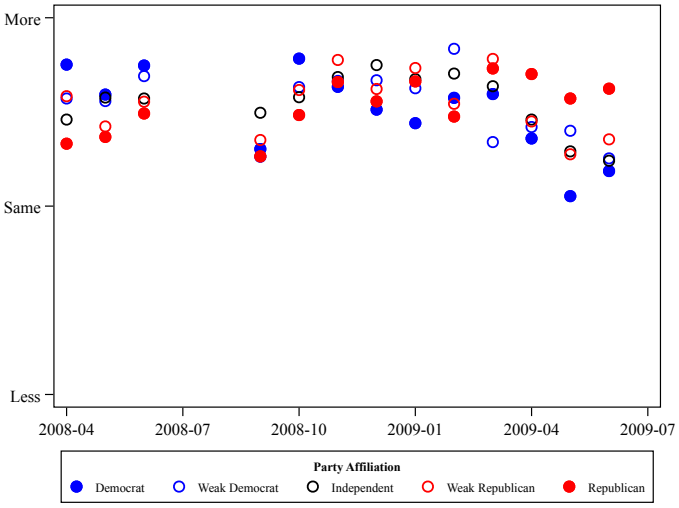


Figure A.18. Survey Expectations on Future State of the Economy in 2008. These graphs plot survey expectations on the future state of the economy by political affiliation around the 2008 election (November 4). The data is from the University of Michigan Survey of Consumers. The upper panel shows the average response to the question "Looking ahead, which would you say is more likely – that in the country as a whole we'll have continuous good times during the next 5 years or so, or that we will have periods of widespread unemployment or depression, or what?" Responses range from 1 (worst) to 5 (best). The lower panel shows expectations of unemployment in a year relative to the current unemployment rate.

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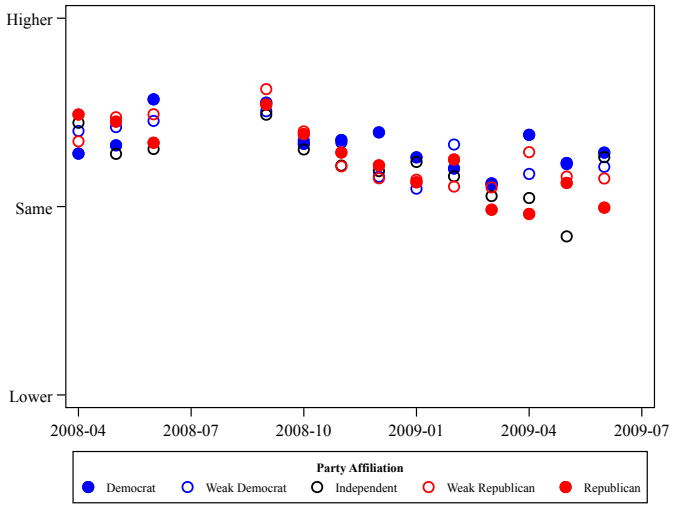
Panel A. Expected Own Income in 1 Year



Panel B. Expected Business Conditions in 1 Year

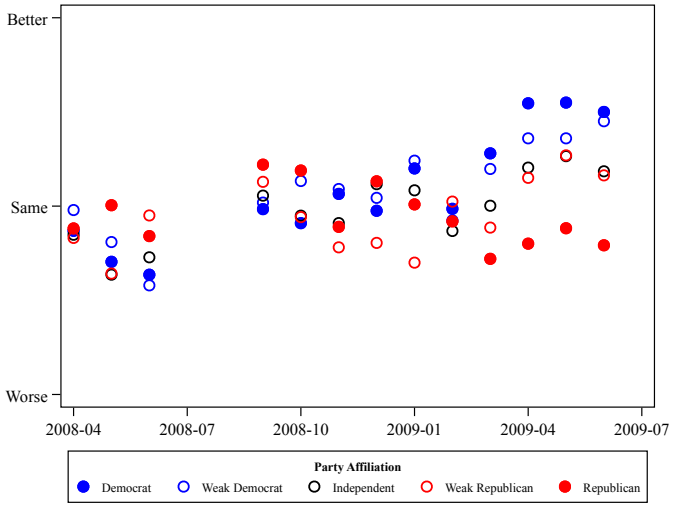


Figure A.19. Survey Expectations on Own Economic Circumstances Versus Overall Conditions in 2008. These graphs plot survey expectations on the future state of own economic circumstances and the overall economy by political affiliation around the 2008 election (November 4). The data is from the University of Michigan Survey of Consumers. The upper panel shows the average response to the question "During the next 12 months, do you expect your income to be higher or lower than during the past year?" The lower panel shows expectations on whether business conditions overall will be better or worse in a year.

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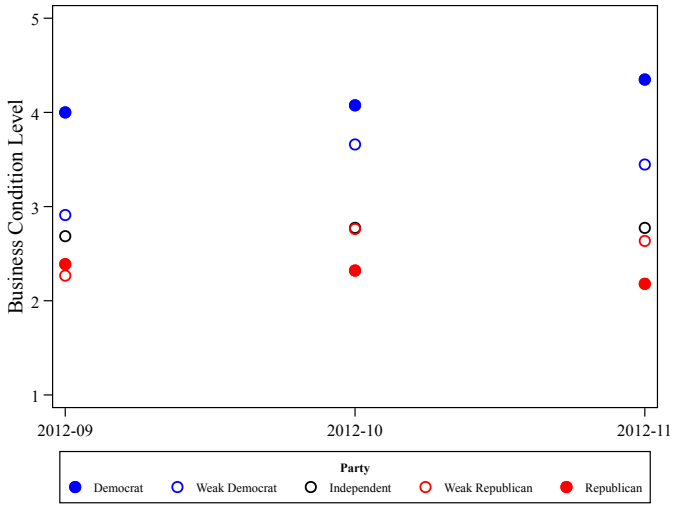
Panel A. Expected Business Conditions in 5 Years



Panel B. Expectations of Unemployment in a Year

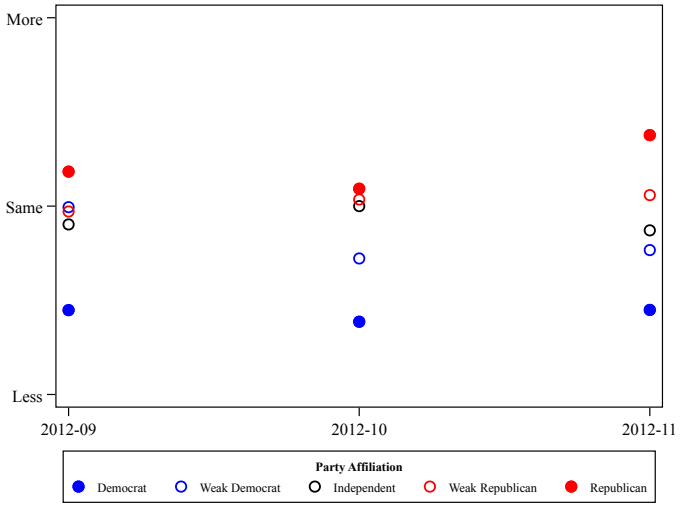


Figure A.20. Survey Expectations on Future State of the Economy in 2012. These graphs plot survey expectations on the future state of the economy by political affiliation around the 2012 election (November 6). The data is from the University of Michigan Survey of Consumers. The upper panel shows the average response to the question "Looking ahead, which would you say is more likely – that in the country as a whole we'll have continuous good times during the next 5 years or so, or that we will have periods of widespread unemployment or depression, or what?" Responses range from 1 (worst) to 5 (best). The lower panel shows expectations of unemployment in a year relative to the current unemployment rate.

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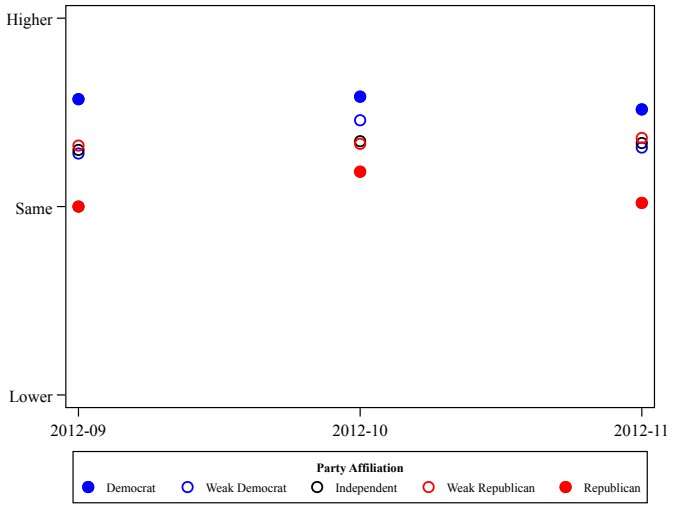
Panel A. Expected Own Income in 1 Year



Panel B. Expected Business Conditions in 1 Year

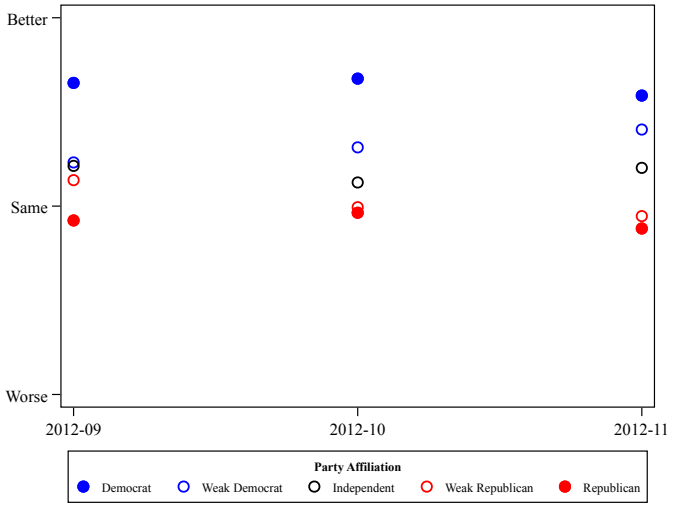


Figure A.21. Survey Expectations on Own Economic Circumstances Versus Overall Conditions in 2012. These graphs plot survey expectations on the future state of own economic circumstances and the overall economy by political affiliation around the 2012 election (November 6). The data is from the University of Michigan Survey of Consumers. The upper panel shows the average response to the question "During the next 12 months, do you expect your income to be higher or lower than during the past year?" The lower panel shows expectations on whether business conditions overall will be better or worse in a year.

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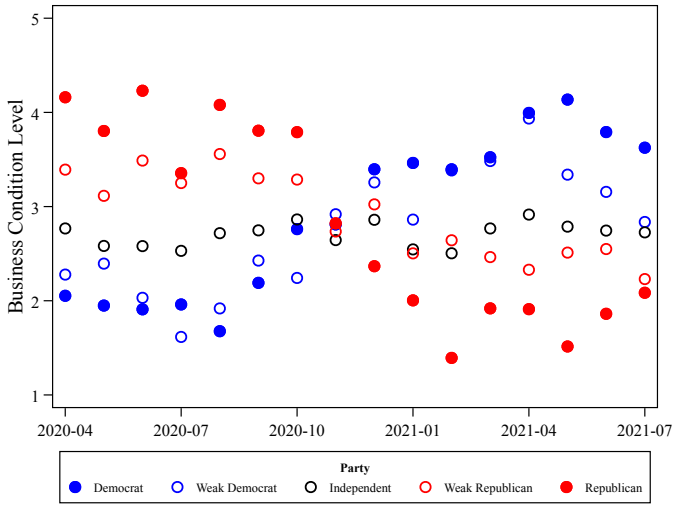
Panel A. Expected Business Conditions in 5 Years



Panel B. Expectations of Unemployment in a Year

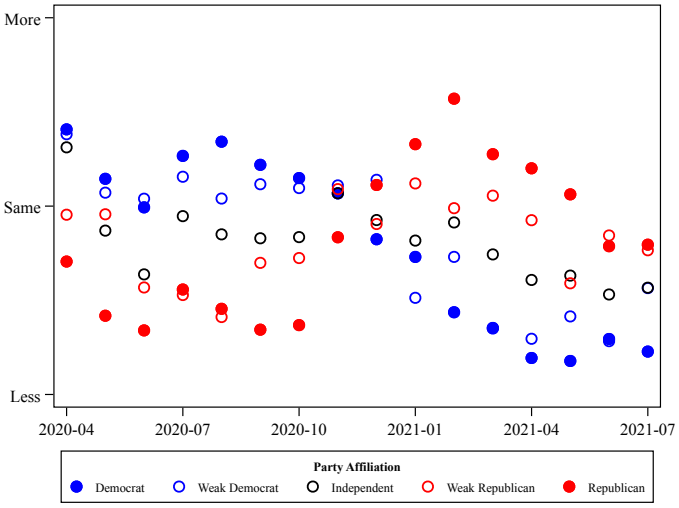


Figure A.22. Survey Expectations on Future State of the Economy in 2020. These graphs plot survey expectations on the future state of the economy by political affiliation around the 2020 election (November 3). The data is from the University of Michigan Survey of Consumers. The upper panel shows the average response to the question "Looking ahead, which would you say is more likely – that in the country as a whole we'll have continuous good times during the next 5 years or so, or that we will have periods of widespread unemployment or depression, or what?" Responses range from 1 (worst) to 5 (best). The lower panel shows expectations of unemployment in a year relative to the current unemployment rate.

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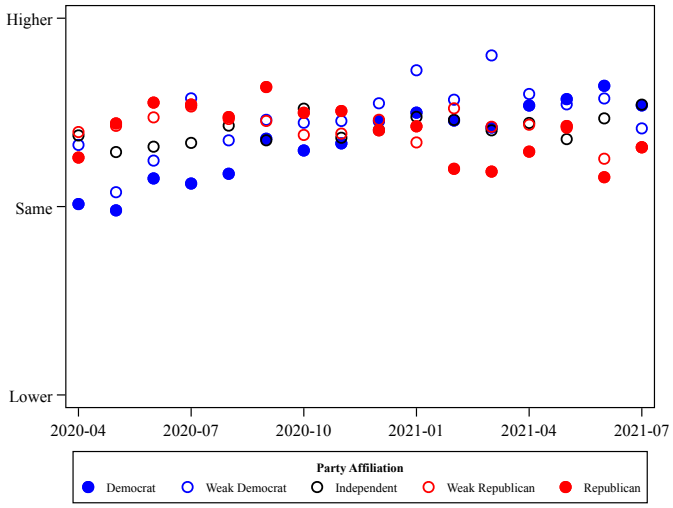
Panel A. Expected Own Income in 1 Year



Panel B. Expected Business Conditions in 1 Year

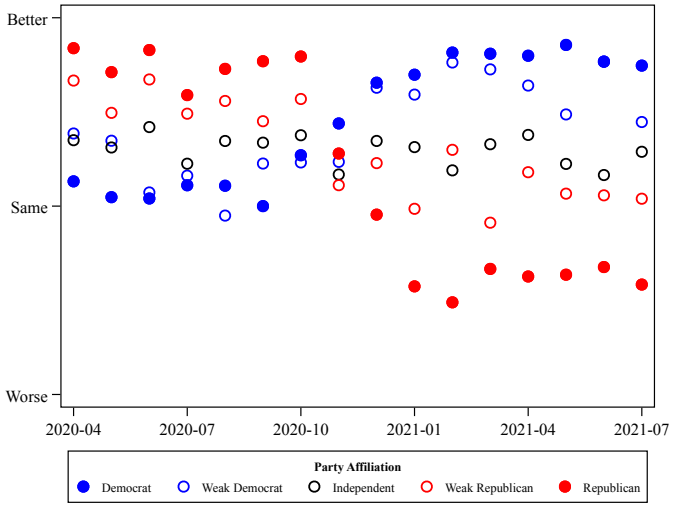


Figure A.23. Survey Expectations on Own Economic Circumstances Versus Overall Conditions in 2020. These graphs plot survey expectations on the future state of own economic circumstances and the overall economy by political affiliation around the 2020 election (November 3). The data is from the University of Michigan Survey of Consumers. The upper panel shows the average response to the question "During the next 12 months, do you expect your income to be higher or lower than during the past year?" The lower panel shows expectations on whether business conditions overall will be better or worse in a year.

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Table A.I
Party Committees

This table lists all 32 campaign committees with at least $20 million in contributions during the 2015–2016 election cycle from individuals with a valid zip code on record. To construct our Republican contribution share measure for the 2016 presidential election at the zip code level, we include the subset of 21 committees that support a party or presidential nominee and exclude committees that are related to a senator or losing presidential primary candidate.

A. Included committees

Name	Amount (in USD)
HILLARY VICTORY FUND	418,127,519
HILLARY FOR AMERICA	281,412,789
PRIORITIES USA ACTION	151,702,351
TRUMP VICTORY	106,907,122
NEXTGEN CLIMATE ACTION COMMITTEE	90,834,927
REPUBLICAN NATIONAL COMMITTEE	89,493,374
DSCC	74,197,205
SENATE LEADERSHIP FUND	74,165,450
DCCC	73,561,758
TRUMP MAKE AMERICA GREAT AGAIN COMMITTEE	68,604,341
SENATE MAJORITY PAC	58,688,399
HILLARY ACTION FUND	45,522,557
NRSC	44,563,979
CONGRESSIONAL LEADERSHIP FUND	44,138,600
DONALD J. TRUMP FOR PRESIDENT, INC.	43,918,500
DNC SERVICES CORP./DEM. NAT'L COMMITTEE	41,855,861
HOUSE MAJORITY PAC	36,078,425
FUTURE45	24,555,649
REBUILDING AMERICA NOW	23,071,271
NRCC	22,773,247
MAKE AMERICA NUMBER 1	20,126,000

B. Excluded committees

Name	Amount (in USD)
RIGHT TO RISE USA	91,047,726
BERNIE 2016	73,961,700
TEAM RYAN	53,432,005
CRUZ FOR PRESIDENT	47,481,222
CONSERVATIVE SOLUTIONS PAC	46,066,194
JEB 2016, INC.	31,080,894
MARCO RUBIO FOR PRESIDENT	30,833,321
VAN HOLLEN FOR SENATE	25,652,235
CARSON AMERICA	24,901,494
INDEPENDENCE USA PAC	21,665,124
UNINTIMIDATED PAC INC	20,717,593

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Table A.II
Regressions of Quarterly Equity Share Changes on Likely Political Affiliation

This table presents regression coefficients of quarterly changes in household portfolio equity shares on the zip code Republican contribution share, interacted by quarterly dummies, for various sets of controls. The baseline controls are the lagged equity share, age, gender, marital status, log lagged financial wealth, log labor income in 2015, the lagged winning and losing sectors shares of equity, and zip code house price growth (2010–2015), interacted by quarterly indicators. In specifications (3)–(8) we consider additional sets of controls (interacted by a full set of quarterly dummies) that include ex post income growth (2016–2017) and house price growth (2015–2017), employer industry indicators (3-digit NAICS), employer indicators, state indicators, county indicators, and employer × county indicators. The sample is our full set of RI households between October 2015 and October 2017, for which we observe the complete set of controls (27.7% of all RIs). Standard errors are clustered at the zip code level.

	Three-month difference in equity share (in %), all households							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Zip code Republican contribution share	-0.145	-0.074	-0.076	-0.011	-0.002	-0.089	0.024	-0.114
× Pre 3 quarters	(0.026)	(0.026)	(0.026)	(0.025)	(0.025)	(0.029)	(0.035)	(0.030)
Zip code Republican contribution share	0.056	0.144	0.144	0.162	0.037	0.055	-0.055	0.163
× Pre 2 quarters	(0.026)	(0.026)	(0.026)	(0.025)	(0.023)	(0.028)	(0.033)	(0.029)
Zip code Republican contribution share	-0.161	-0.100	-0.098	-0.134	-0.033	-0.001	-0.040	-0.078
× Pre 1 quarter	(0.026)	(0.027)	(0.027)	(0.024)	(0.023)	(0.036)	(0.033)	(0.028)
Zip code Republican contribution share	-0.116	-0.050	-0.065	-0.015	-0.062	-0.075	-0.124	-0.089
× Pre election	(0.021)	(0.021)	(0.021)	(0.021)	(0.022)	(0.024)	(0.030)	(0.031)
Zip code Republican contribution share	0.361	0.400	0.395	0.388	0.378	0.393	0.332	0.290
× Post 1 quarter	(0.030)	(0.029)	(0.030)	(0.029)	(0.028)	(0.034)	(0.038)	(0.034)
Zip code Republican contribution share	0.189	0.254	0.251	0.222	0.252	0.232	0.237	0.266
× Post 2 quarters	(0.028)	(0.028)	(0.028)	(0.027)	(0.023)	(0.032)	(0.032)	(0.032)
Zip code Republican contribution share	-0.090	0.020	0.028	0.091	0.086	0.015	0.091	-0.016
× Post 3 quarters	(0.023)	(0.022)	(0.022)	(0.022)	(0.021)	(0.026)	(0.030)	(0.029)
Zip code Republican contribution share	-0.063	0.125	0.138	0.171	0.118	0.056	0.071	0.182
× Post 4 quarters	(0.027)	(0.027)	(0.027)	(0.026)	(0.023)	(0.030)	(0.034)	(0.031)
Controls by quarter								
Baseline		Y	Y	Y	Y	Y	Y	Y
Labor income growth 2016-17			Y					
Zip code house price growth 2015-17			Y					
Employer industry				Y				
Employer					Y			
State						Y		
County							Y	
Employer × County								Y
R^2	0.003	0.032	0.032	0.036	0.059	0.033	0.034	0.103

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Table A.III
Regressions of Quarterly Price-Constant Equity Shares on Likely Political Affiliation

This table presents regression coefficients of quarterly changes in price-constant household portfolio equity shares on the zip code Republican contribution share, interacted by quarterly dummies, for various sets of controls. The price-constant equity share is calculated for a hypothetical portfolio that is insensitive to passive appreciations and is driven by trading only. The baseline controls are the lagged equity share, age, gender, marital status, log lagged financial wealth, log labor income in 2015, the lagged winning and losing sectors shares of equity, and zip code house price growth (2010–2015), interacted by quarterly indicators. In specifications (3)–(8) we consider additional sets of controls (interacted by a full set of quarterly dummies) that include ex post income growth (2016–2017) and house price growth (2015–2017), employer industry indicators (3-digit NAICS), employer indicators, state indicators, county indicators, and employer × county indicators. The sample is our full set of RI households between October 2015 and October 2017, for which we observe the complete set of controls (27.7% of all RIs). Standard errors are clustered at the zip code level.

	Three-month difference in price-constant equity share (in %), all households							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Zip code Republican contribution share	-0.141	-0.074	-0.080	-0.008	0.024	-0.014	0.047	-0.072
× Pre 3 quarters	(0.020)	(0.021)	(0.021)	(0.021)	(0.022)	(0.023)	(0.031)	(0.026)
Zip code Republican contribution share	0.007	0.010	0.011	0.045	0.015	0.014	-0.051	0.059
× Pre 2 quarters	(0.022)	(0.022)	(0.022)	(0.021)	(0.022)	(0.024)	(0.030)	(0.026)
Zip code Republican contribution share	-0.068	-0.037	-0.038	-0.018	-0.006	-0.023	-0.072	0.011
× Pre 1 quarter	(0.021)	(0.021)	(0.021)	(0.020)	(0.022)	(0.023)	(0.030)	(0.025)
Zip code Republican contribution share	-0.139	-0.110	-0.111	-0.084	-0.074	-0.105	-0.126	-0.112
× Pre election	(0.020)	(0.020)	(0.020)	(0.020)	(0.022)	(0.023)	(0.030)	(0.029)
Zip code Republican contribution share	0.349	0.375	0.373	0.422	0.407	0.349	0.286	0.321
× Post 1 quarter	(0.024)	(0.025)	(0.025)	(0.025)	(0.027)	(0.029)	(0.036)	(0.030)
Zip code Republican contribution share	0.273	0.324	0.316	0.286	0.281	0.291	0.282	0.295
× Post 2 quarters	(0.022)	(0.022)	(0.022)	(0.022)	(0.024)	(0.025)	(0.031)	(0.028)
Zip code Republican contribution share	0.039	0.087	0.088	0.085	0.095	0.085	0.103	0.015
× Post 3 quarters	(0.020)	(0.020)	(0.020)	(0.020)	(0.022)	(0.023)	(0.029)	(0.026)
Zip code Republican contribution share	0.067	0.138	0.140	0.144	0.143	0.134	0.147	0.159
× Post 4 quarters	(0.021)	(0.021)	(0.022)	(0.022)	(0.024)	(0.025)	(0.032)	(0.029)
Controls by quarter								
Baseline		Y	Y	Y	Y	Y	Y	Y
Labor income growth 2016-17			Y					
Zip code house price growth 2015-17			Y					
Employer industry				Y				
Employer					Y			
State						Y		
County							Y	
Employer × County								Y
R^2	0.000	0.023	0.023	0.023	0.033	0.023	0.024	0.090

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Table A.IV
Regressions of Equity Share Changes on Likely Political Affiliation

This table presents regression coefficients of annual changes in household portfolio equity shares on the zip code Republican contribution share, before and after the election, for various sets of controls. The baseline controls are the lagged equity share, age, gender, marital status, log lagged financial wealth, log labor income in 2015, the lagged winning and losing sectors shares of equity, and zip code house price growth (2010–2015), interacted by annual indicators. In specifications (3)–(8) we consider additional sets of controls (interacted by yearly dummies) that include ex post income growth (2016–2017) and house price growth (2015–2017), employer industry indicators (3-digit NAICS), employer indicators, state indicators, county indicators, and employer × county indicators. The sample is our full set of RI households between October 2015 and October 2017, for which we observe the complete set of controls (27.7% of all RIs). Standard errors are clustered at the zip code level.

	One-year difference in equity share (in %), all households							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Zip code Republican contribution share	-0.367	-0.104	-0.119	-0.041	-0.093	-0.129	-0.212	-0.166
	(0.045)	(0.044)	(0.044)	(0.041)	(0.039)	(0.051)	(0.058)	(0.061)
Zip code Republican contribution share × Post	0.764	0.803	0.829	0.802	0.824	0.736	0.868	0.795
	(0.077)	(0.066)	(0.066)	(0.063)	(0.060)	(0.078)	(0.089)	(0.091)
Lagged equity share		-12.025	-12.025	-12.123	-12.313	-12.040	-12.080	-12.304
		(0.065)	(0.065)	(0.065)	(0.065)	(0.065)	(0.065)	(0.072)
Lagged equity share × Post		-4.800	-4.803	-4.751	-4.682	-4.795	-4.720	-4.693
		(0.085)	(0.085)	(0.085)	(0.084)	(0.084)	(0.084)	(0.092)
Age		-0.147	-0.147	-0.146	-0.143	-0.147	-0.147	-0.143
		(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Age × Post		-0.039	-0.038	-0.040	-0.044	-0.040	-0.039	-0.045
		(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Female		0.148	0.150	0.113	0.093	0.141	0.139	0.096
		(0.013)	(0.013)	(0.014)	(0.014)	(0.013)	(0.013)	(0.016)
Female × Post		-0.373	-0.366	-0.421	-0.375	-0.367	-0.383	-0.368
		(0.020)	(0.020)	(0.021)	(0.021)	(0.020)	(0.020)	(0.024)
Married		0.055	0.055	0.049	0.055	0.062	0.057	0.049
		(0.014)	(0.014)	(0.014)	(0.015)	(0.014)	(0.014)	(0.016)
Married × Post		0.091	0.092	0.089	0.089	0.078	0.078	0.093
		(0.022)	(0.022)	(0.022)	(0.023)	(0.022)	(0.022)	(0.025)
Log wealth		-0.074	-0.076	-0.083	-0.110	-0.077	-0.077	-0.113
		(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)
Log wealth × Post		0.554	0.555	0.558	0.602	0.554	0.551	0.601
		(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.013)
Log labor income 2015		0.088	0.083	0.107	0.132	0.107	0.117	0.133
		(0.015)	(0.015)	(0.014)	(0.015)	(0.015)	(0.014)	(0.017)
Log labor income 2015 × Post		0.266	0.279	0.172	0.087	0.267	0.231	0.050
		(0.023)	(0.023)	(0.022)	(0.023)	(0.023)	(0.022)	(0.027)
Controls by year								
Baseline		Y	Y	Y	Y	Y	Y	Y
Labor income growth 2016-17			Y					
Zip code house price growth 2015-17			Y					
Employer industry				Y				
Employer					Y			
State						Y		
County							Y	
Employer × County								Y
R^2	0.012	0.099	0.100	0.101	0.121	0.100	0.101	0.227

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Table A.V
Regressions of Price-Constant Equity Share Changes on Likely Political Affiliation for Alternative Political Affiliation Measures

This table presents regression coefficients of annual changes in price-constant household portfolio equity shares on the zip code or county Republican share, before and after the election, for various measures of party affiliations: the zip code share of contributions in numbers (1) and in dollars (2), the corresponding contribution measures at the county level (3–4), and the county shares of votes in 2016, 2012, or the difference between 2016 and 2012 (5–8). The price-constant equity share is calculated for a hypothetical portfolio that is insensitive to passive appreciations and is driven by trading only. The baseline controls are the lagged equity share, age, gender, marital status, log lagged financial wealth, log labor income in 2015, the lagged winning and losing sectors shares of equity, and zip code house price growth (2010–2015), interacted by annual indicators. In addition, we control for employer × state × period fixed effects. The sample is our full set of RI households between October 2015 and October 2017, for which we observe the complete set of controls (27.7% of all RIs). Standard errors are clustered at the zip code level.

A. Donation measures	One-year difference in price-constant equity share (in %)			
	Zip donations (nbr)	Zip donations (amt)	County donations (nbr)	County donations (amt)
	(1)	(2)	(3)	(4)
Republican share	-0.073	-0.072	0.054	-0.020
	(0.043)	(0.032)	(0.061)	(0.048)
Republican share × Post	0.857	0.374	0.707	0.424
	(0.068)	(0.049)	(0.096)	(0.076)
Controls by year				
Baseline	Y	Y	Y	Y
Employer × State	Y	Y	Y	Y
R^2	0.108	0.108	0.108	0.108

B. Voting measures	One-year difference in price-constant equity share (in %)			
	(5)	(6)	(7)	(8)
County Republican vote share 2016	0.059			
	(0.057)			
County Republican vote share 2016 × Post	0.776			
	(0.091)			
County Republican vote share 2012		0.063		0.058
		(0.065)		(0.069)
County Republican vote share 2012 × Post		0.912		0.933
		(0.103)		(0.109)
County Republican vote share 2016-2012			0.116	0.066
			(0.219)	(0.232)
County Republican vote share 2016-2012 × Post			0.539	-0.279
			(0.346)	(0.365)
Controls by year				
Baseline	Y	Y	Y	Y
Employer × State	Y	Y	Y	Y
R^2	0.108	0.108	0.108	0.108

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Table A.VI
Regressions of Equity Share Changes on Likely Political Affiliation for 2012 Election Cycle

This table presents regression coefficients of annual changes in household portfolio equity shares around the 2012 election on the zip code Republican contribution share, before and after the election, for various sets of controls. The baseline controls are the lagged equity share, age, gender, marital status, log lagged financial wealth, log labor income in 2011, the lagged winning and losing sectors shares of equity, and zip code house price growth (2006–2011), interacted by annual indicators. In specifications (3)–(8) we consider additional sets of controls (interacted by yearly dummies) that include ex post income growth (2012–2013) and house price growth (2011–2013), employer industry indicators (3-digit NAICS), employer indicators, state indicators, county indicators, and employer × county indicators. The sample is the full set of RI households between October 2011 and October 2013, constructed using our same procedure four years earlier, for which we observe the complete set of controls (26.6% of all RIs). Standard errors are clustered at the zip code level.

	One-year difference in equity share (in %), 2012 election, all households							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Zip code Republican contribution share	0.041	0.237	0.223	0.278	0.062	0.029	0.086	0.099
	(0.051)	(0.052)	(0.052)	(0.052)	(0.042)	(0.054)	(0.058)	(0.061)
Zip code Republican contribution share × Post	-0.603	-0.436	-0.381	-0.264	0.084	-0.208	-0.084	0.014
	(0.085)	(0.080)	(0.080)	(0.078)	(0.062)	(0.079)	(0.089)	(0.090)
Lagged equity share		-11.537	-11.545	-11.679	-12.105	-11.558	-11.582	-12.165
		(0.063)	(0.063)	(0.062)	(0.062)	(0.062)	(0.061)	(0.069)
Lagged equity share × Post		-2.966	-2.967	-2.878	-2.741	-2.942	-2.960	-2.787
		(0.084)	(0.084)	(0.084)	(0.086)	(0.083)	(0.083)	(0.097)
Age		-0.102	-0.102	-0.098	-0.098	-0.103	-0.103	-0.098
		(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Age × Post		-0.019	-0.018	-0.024	-0.025	-0.018	-0.017	-0.026
		(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Female		-0.056	-0.053	0.044	-0.003	-0.045	-0.047	0.001
		(0.019)	(0.019)	(0.017)	(0.017)	(0.018)	(0.017)	(0.019)
Female × Post		-0.292	-0.298	-0.312	-0.247	-0.299	-0.333	-0.276
		(0.028)	(0.028)	(0.026)	(0.026)	(0.028)	(0.025)	(0.029)
Married		0.108	0.101	0.073	0.054	0.119	0.118	0.035
		(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.020)
Married × Post		-0.112	-0.107	-0.043	-0.010	-0.130	-0.128	0.004
		(0.028)	(0.028)	(0.028)	(0.028)	(0.028)	(0.028)	(0.031)
Log wealth		0.208	0.207	0.168	0.106	0.205	0.203	0.092
		(0.010)	(0.010)	(0.010)	(0.009)	(0.009)	(0.009)	(0.010)
Log wealth × Post		0.159	0.158	0.210	0.301	0.155	0.149	0.296
		(0.017)	(0.017)	(0.016)	(0.014)	(0.016)	(0.014)	(0.016)
Log labor income 2011		0.134	0.154	0.107	0.107	0.159	0.135	0.108
		(0.020)	(0.020)	(0.020)	(0.019)	(0.019)	(0.018)	(0.022)
Log labor income 2011 × Post		0.293	0.257	0.437	0.374	0.250	0.209	0.323
		(0.033)	(0.033)	(0.033)	(0.030)	(0.031)	(0.028)	(0.034)
Controls by year								
Baseline		Y	Y	Y	Y	Y	Y	Y
Labor income growth 2012-13			Y					
Zip code house price growth 2011-13			Y					
Employer industry				Y				
Employer					Y			
State						Y		
County							Y	
Employer × County								Y
R^2	0.021	0.100	0.101	0.103	0.131	0.101	0.104	0.229

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Table A.VII
Regressions of Price-Constant Equity Share Changes on Likely Political Affiliation for 2012 Election Cycle

This table presents regression coefficients of annual changes in price-constant household portfolio equity shares around the 2012 election on the zip code Republican contribution share, before and after the election, for various sets of controls. The price-constant equity share is calculated for a hypothetical portfolio that is insensitive to passive appreciations and is driven by trading only. The baseline controls are the lagged equity share, age, gender, marital status, log lagged financial wealth, log labor income in 2011, the lagged winning and losing sectors shares of equity, and zip code house price growth (2006–2011), interacted by annual indicators. In specifications (3)–(8) we consider additional sets of controls (interacted by yearly dummies) that include ex post income growth (2012–2013) and house price growth (2011–2013), employer industry indicators (3-digit NAICS), employer indicators, state indicators, county indicators, and employer × county indicators. The sample is the full set of RI households between October 2011 and October 2013, constructed using our same procedure four years earlier, for which we observe the complete set of controls (26.6% of all RIs). Standard errors are clustered at the zip code level.

	One-year difference in price-constant equity share (in %), 2012 election, all households							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Zip code Republican contribution share	-0.390	0.002	-0.005	0.065	0.055	-0.017	0.061	0.109
	(0.044)	(0.045)	(0.044)	(0.043)	(0.042)	(0.047)	(0.057)	(0.061)
Zip code Republican contribution share × Post	0.005	0.013	0.032	0.110	0.123	0.043	0.058	0.010
	(0.067)	(0.061)	(0.061)	(0.061)	(0.063)	(0.066)	(0.085)	(0.092)
Lagged equity share		-10.507	-10.511	-10.605	-10.962	-10.526	-10.569	-11.032
		(0.061)	(0.061)	(0.060)	(0.061)	(0.061)	(0.060)	(0.068)
Lagged equity share × Post		-2.672	-2.684	-2.710	-2.681	-2.672	-2.682	-2.692
		(0.068)	(0.068)	(0.068)	(0.071)	(0.068)	(0.068)	(0.078)
Age		-0.090	-0.099	-0.098	-0.098	-0.099	-0.099	-0.098
		(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Age × Post		-0.016	-0.015	-0.015	-0.016	-0.016	-0.015	-0.016
		(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Female		0.080	0.082	0.043	0.012	0.078	0.062	0.018
		(0.016)	(0.016)	(0.017)	(0.017)	(0.016)	(0.016)	(0.019)
Female × Post		-0.342	-0.342	-0.332	-0.332	-0.344	-0.362	-0.357
		(0.024)	(0.024)	(0.026)	(0.027)	(0.024)	(0.025)	(0.030)
Married		0.031	0.028	0.022	0.016	0.040	0.037	0.004
		(0.017)	(0.017)	(0.017)	(0.018)	(0.017)	(0.017)	(0.020)
Married × Post		0.042	0.037	0.036	0.037	0.034	0.034	0.040
		(0.027)	(0.027)	(0.027)	(0.028)	(0.027)	(0.027)	(0.031)
Log wealth		-0.155	-0.156	-0.167	-0.200	-0.160	-0.165	-0.205
		(0.008)	(0.008)	(0.008)	(0.009)	(0.008)	(0.008)	(0.010)
Log wealth × Post		0.252	0.249	0.258	0.272	0.248	0.246	0.255
		(0.014)	(0.014)	(0.014)	(0.015)	(0.014)	(0.014)	(0.016)
Log labor income 2011		0.159	0.169	0.156	0.173	0.183	0.159	0.156
		(0.018)	(0.018)	(0.018)	(0.020)	(0.018)	(0.018)	(0.022)
Log labor income 2011 × Post		0.394	0.395	0.397	0.392	0.375	0.347	0.356
		(0.028)	(0.028)	(0.028)	(0.031)	(0.028)	(0.028)	(0.036)
Controls by year								
Baseline		Y	Y	Y	Y	Y	Y	Y
Labor income growth 2012-13			Y					
Zip code house price growth 2011-13			Y					
Employer industry				Y				
Employer					Y			
State						Y		
County							Y	
Employer × County								Y
R^2	0.001	0.069	0.069	0.070	0.081	0.069	0.070	0.183

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Table A.VIII
Regressions of Price-Constant Equity Share Changes on Likely Political Affiliation for Active Investors (Alternative Definitions)

This table presents regression coefficients of annual changes in price-constant household portfolio equity shares on the zip code Republican contribution share, before and after the election, in various subsamples of the population: households with less than 50% of assets in target date funds (TDFs, column 2), households with prior-year contributions that are not invested fully in either a TDF or a fixed-income fund (column 3), households with trading only in preceding years (columns 4–6), households with trading only in employer-linked accounts in preceding years (columns 7–9), and households with active portfolio reallocations in preceding years (columns 10–12). The price-constant equity share is calculated for a hypothetical portfolio that is insensitive to passive appreciations and is driven by trading only. The baseline controls are the lagged equity share, age, gender, marital status, log lagged financial wealth, log labor income in 2015, the lagged winning and losing sectors shares of equity, and zip code house price growth (2010–2015), interacted by annual indicators. In addition, we control for employer × county × period fixed effects. The sample period is October 2015 to October 2017, and we include RIs for which we observe the complete set of controls (27.7% of all RIs). Standard errors are clustered at the zip code level.

	One-year difference in price-constant equity share (in %)					
	All	TDF share $< 50\%$	Contribution not default	Trade any past 5 years	Trade past year	Trade all past 3 years
	(1)	(2)	(3)	(4)	(5)	(6)
Zip code Republican contribution share	-0.160	-0.228	-0.170	-0.356	-0.350	-0.356
	(0.059)	(0.103)	(0.093)	(0.134)	(0.191)	(0.320)
Zip code Republican contribution share × Post	0.859	1.366	1.173	1.500	1.617	1.404
	(0.091)	(0.160)	(0.146)	(0.202)	(0.279)	(0.489)
Controls by year						
Baseline	Y	Y	Y	Y	Y	Y
Employer × County	Y	Y	Y	Y	Y	Y
R^2	0.189	0.233	0.220	0.272	0.322	0.386
Share of observations	100.0%	52.4%	55.3%	37.4%	23.8%	10.3%

	One-year difference in price-constant equity share (in %)					
	Trade in empl. acc. any past 5 years	Trade in empl. acc. past year	Trade in empl. acc. all past 3 years	Portfolio change any past 5 years	Portfolio change past year	Portfolio change all past 3 years
	(7)	(8)	(9)	(10)	(11)	(12)
Zip code Republican contribution share	-0.258	-0.299	-0.407	-0.554	-0.533	-1.356
	(0.167)	(0.266)	(0.506)	(0.200)	(0.443)	(1.848)
Zip code Republican contribution share × Post	1.432	1.627	1.850	1.765	1.850	4.046
	(0.247)	(0.382)	(0.761)	(0.295)	(0.638)	(2.783)
Controls by year						
Baseline	Y	Y	Y	Y	Y	Y
Employer × County	Y	Y	Y	Y	Y	Y
R^2	0.294	0.367	0.469	0.313	0.417	0.589
Share of observations	28.3%	15.3%	5.1%	24.2%	9.5%	1.7%

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Table A.IX
Regressions of Price-Constant Equity Share Changes on Likely Political Affiliation Conditional on Active Rebalancing during the Year

This table presents regression coefficients of annual changes in price-constant household portfolio equity shares on the zip code Republican contribution share, before and after the election, for various sets of controls. The price-constant equity share is calculated for a hypothetical portfolio that is insensitive to passive appreciations and is driven by trading only. The baseline controls are the lagged equity share, age, gender, marital status, log lagged financial wealth, log labor income in 2015, the lagged winning and losing sectors shares of equity, and zip code house price growth (2010–2015), interacted by annual indicators. In specifications (3)–(8) we consider additional sets of controls (interacted by yearly dummies) that include ex post income growth (2016–2017) and house price growth (2015–2017), employer industry indicators (3-digit NAICS), employer indicators, state indicators, county indicators, and employer × county indicators. The sample is the subset of households with an active portfolio reallocation during the year, covering 9.3% of the full regression sample. The sample period is October 2015 to October 2017, and we include RIs for which we observe the complete set of controls. Standard errors are clustered at the zip code level.

	One-year difference in price-constant equity share (in %), households with active allocation change during year							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Zip code Republican contribution share	-3.370	-2.025	-2.003	-1.182	-1.241	-1.921	-1.954	-1.614
	(0.365)	(0.331)	(0.331)	(0.329)	(0.344)	(0.384)	(0.480)	(0.638)
Zip code Republican contribution share × Post	10.268	6.123	6.085	5.161	4.927	5.505	5.096	4.615
	(0.573)	(0.426)	(0.428)	(0.433)	(0.461)	(0.500)	(0.638)	(0.851)
Lagged equity share		-60.358	-60.359	-60.579	-61.168	-60.433	-60.587	-60.591
		(0.273)	(0.273)	(0.274)	(0.280)	(0.272)	(0.272)	(0.367)
Lagged equity share × Post		-10.024	-10.000	-9.948	-9.388	-9.965	-9.859	-9.062
		(0.322)	(0.322)	(0.323)	(0.333)	(0.323)	(0.324)	(0.436)
Age		-0.486	-0.485	-0.487	-0.486	-0.487	-0.487	-0.474
		(0.007)	(0.007)	(0.007)	(0.008)	(0.007)	(0.007)	(0.011)
Age × Post		0.011	0.014	0.009	0.007	0.011	0.012	-0.013
		(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.013)
Female		2.077	2.086	1.309	1.061	2.002	1.909	1.058
		(0.118)	(0.118)	(0.122)	(0.126)	(0.118)	(0.119)	(0.169)
Female × Post		-2.692	-2.689	-1.927	-1.624	-2.622	-2.502	-1.404
		(0.162)	(0.162)	(0.167)	(0.173)	(0.162)	(0.163)	(0.232)
Married		-0.381	-0.380	-0.335	-0.185	-0.338	-0.334	-0.094
		(0.131)	(0.131)	(0.132)	(0.137)	(0.130)	(0.131)	(0.181)
Married × Post		0.889	0.884	0.788	0.621	0.841	0.817	0.447
		(0.181)	(0.181)	(0.182)	(0.187)	(0.181)	(0.181)	(0.246)
Log wealth		-0.939	-0.938	-0.819	-0.726	-0.921	-0.898	-0.755
		(0.059)	(0.059)	(0.059)	(0.062)	(0.059)	(0.059)	(0.086)
Log wealth × Post		-0.531	-0.528	-0.656	-0.828	-0.532	-0.563	-0.693
		(0.084)	(0.084)	(0.084)	(0.088)	(0.084)	(0.084)	(0.121)
Log labor income 2015		0.625	0.640	0.554	0.649	0.667	0.666	0.763
		(0.104)	(0.105)	(0.105)	(0.116)	(0.104)	(0.104)	(0.163)
Log labor income 2015 × Post		0.173	0.148	0.339	0.424	0.137	0.164	0.327
		(0.141)	(0.141)	(0.144)	(0.160)	(0.141)	(0.144)	(0.220)
Controls by year								
Baseline		Y	Y	Y	Y	Y	Y	Y
Labor income growth 2016-17			Y					
Zip code house price growth 2015-17			Y					
Employer industry				Y				
Employer					Y			
State						Y		
County							Y	
Employer × County								Y
R^2	0.023	0.346	0.347	0.349	0.391	0.347	0.353	0.563

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Table A.X
Regressions of Quarterly Equity Share on Likely Political Affiliation

This table presents regression coefficients of quarterly household portfolio equity shares on the zip code Republican contribution share, interacted by quarterly dummies, for various sets of controls. We report the full set of results for the three quarters prior to the election and the four quarters following the election, relative to allocations just before the election. The baseline controls are the initial equity share, age, gender, marital status, log initial financial wealth, log labor income in 2015, the initial winning and losing sectors shares of equity, and zip code house price growth (2010–2015), interacted by quarterly indicators. In specifications (3)–(8) we consider additional sets of controls (interacted by a full set of quarterly dummies) that include ex post income growth (2016–2017) and house price growth (2015–2017), employer industry indicators (3-digit NAICS), employer indicators, state indicators, county indicators, and employer × county indicators. The sample is our full set of RI households between October 2015 and October 2017, for which we observe the complete set of controls (27.7% of all RIs). Standard errors are clustered at the zip code level.

	Equity share (in %), all households							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Zip code Republican contribution share	0.221	0.031	0.043	0.030	0.091	0.041	0.236	0.209
× Pre 3 quarters	(0.042)	(0.041)	(0.041)	(0.038)	(0.038)	(0.050)	(0.055)	(0.057)
Zip code Republican contribution share	0.277	0.166	0.179	0.176	0.113	0.087	0.171	0.183
× Pre 2 quarters	(0.034)	(0.034)	(0.034)	(0.031)	(0.032)	(0.043)	(0.045)	(0.046)
Zip code Republican contribution share	0.116	0.056	0.071	0.026	0.071	0.080	0.127	0.124
× Pre 1 quarter	(0.023)	(0.023)	(0.023)	(0.023)	(0.023)	(0.026)	(0.032)	(0.035)
Zip code Republican contribution share	0.361	0.397	0.392	0.365	0.358	0.384	0.334	0.297
× Post 1 quarter	(0.032)	(0.032)	(0.032)	(0.031)	(0.030)	(0.037)	(0.041)	(0.043)
Zip code Republican contribution share	0.550	0.619	0.612	0.546	0.575	0.587	0.554	0.501
× Post 2 quarters	(0.042)	(0.040)	(0.041)	(0.039)	(0.036)	(0.046)	(0.050)	(0.052)
Zip code Republican contribution share	0.460	0.602	0.603	0.594	0.621	0.570	0.623	0.540
× Post 3 quarters	(0.049)	(0.045)	(0.045)	(0.044)	(0.040)	(0.053)	(0.057)	(0.058)
Zip code Republican contribution share	0.397	0.695	0.709	0.725	0.702	0.599	0.671	0.622
× Post 4 quarters	(0.055)	(0.049)	(0.049)	(0.048)	(0.045)	(0.059)	(0.065)	(0.065)
Household fixed effects	Y	Y	Y	Y	Y	Y	Y	Y
Controls by quarter								
Baseline		Y	Y	Y	Y	Y	Y	Y
Labor income growth 2016-17			Y					
Zip code house price growth 2015-17			Y					
Employer industry				Y				
Employer					Y			
State						Y		
County							Y	
Employer × County								Y
R^2	0.924	0.928	0.980	0.928	0.930	0.928	0.928	0.938

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Table A.XI
Regressions of Quarterly Price-Constant Equity Share on Likely Political Affiliation

This table presents regression coefficients of quarterly price-constant household portfolio equity shares on the zip code Republican contribution share, interacted by quarterly dummies, for various sets of controls. The price-constant equity share is calculated for a hypothetical portfolio that is insensitive to passive appreciations and is driven by trading only. We report the full set of results for the three quarters prior to the election and the four quarters following the election, relative to allocations just before the election. The baseline controls are the initial equity share, age, gender, marital status, log initial financial wealth, log labor income in 2015, the initial winning and losing sectors shares of equity, and zip code house price growth (2010–2015), interacted by quarterly indicators. In specifications (3)–(8) we consider additional sets of controls (interacted by a full set of quarterly dummies) that include ex post income growth (2016–2017) and house price growth (2015–2017), employer industry indicators (3-digit NAICS), employer indicators, state indicators, county indicators, and employer × county indicators. The sample is our full set of RI households between October 2015 and October 2017, for which we observe the complete set of controls (27.7% of all RIs). Standard errors are clustered at the zip code level.

	Price-constant equity share (in %), all households							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Zip code Republican contribution share	0.203	0.151	0.153	0.090	0.095	0.133	0.272	0.220
× Pre 3 quarters	(0.042)	(0.037)	(0.037)	(0.036)	(0.037)	(0.040)	(0.052)	(0.055)
Zip code Republican contribution share	0.209	0.155	0.158	0.122	0.097	0.138	0.208	0.190
× Pre 2 quarters	(0.031)	(0.029)	(0.030)	(0.029)	(0.031)	(0.033)	(0.042)	(0.046)
Zip code Republican contribution share	0.137	0.112	0.113	0.091	0.079	0.106	0.129	0.136
× Pre 1 quarter	(0.021)	(0.021)	(0.021)	(0.021)	(0.023)	(0.024)	(0.031)	(0.034)
Zip code Republican contribution share	0.348	0.375	0.374	0.404	0.389	0.340	0.284	0.302
× Post 1 quarter	(0.026)	(0.026)	(0.027)	(0.027)	(0.029)	(0.030)	(0.038)	(0.042)
Zip code Republican contribution share	0.622	0.681	0.672	0.658	0.639	0.612	0.553	0.520
× Post 2 quarters	(0.033)	(0.033)	(0.033)	(0.033)	(0.036)	(0.037)	(0.047)	(0.052)
Zip code Republican contribution share	0.661	0.742	0.735	0.707	0.700	0.671	0.638	0.569
× Post 3 quarters	(0.038)	(0.037)	(0.037)	(0.037)	(0.040)	(0.042)	(0.055)	(0.058)
Zip code Republican contribution share	0.728	0.855	0.850	0.816	0.808	0.779	0.764	0.693
× Post 4 quarters	(0.044)	(0.042)	(0.042)	(0.043)	(0.045)	(0.048)	(0.062)	(0.066)
Household fixed effects	Y	Y	Y	Y	Y	Y	Y	Y
Controls by quarter								
Baseline		Y	Y	Y	Y	Y	Y	Y
Labor income growth 2016-17			Y					
Zip code house price growth 2015-17			Y					
Employer industry				Y				
Employer					Y			
State						Y		
County							Y	
Employer × County								Y
R^2	0.927	0.929	0.929	0.930	0.930	0.929	0.930	0.939

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Table A.XII
Regressions of Price-Constant Equity Share Changes on Likely Political Affiliation Across Samples

This table presents regression coefficients of annual changes in price-constant household portfolio equity shares on the zip code Republican contribution share, before and after the election, with a basic set of controls and for various subsamples of a non-selected and unbalanced sample. This new sample is constructed by drawing a random sample of one million households that are between the ages of 25 and 84 and have positive asset holdings on October 31, 2015. We report coefficients for the full sample, as well as subsamples restricted to age below 65, the RI sample, and the RI sample with observations of gender and marital status, employer, industry, income over 2015–2017, and all controls, respectively. The price-constant equity share is calculated for a hypothetical portfolio that is insensitive to passive appreciations and is driven by trading only. The sample period is October 2015 to October 2017. Standard errors are clustered at the zip code level.

	One-year difference in equity share (in %)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Zip code Republican contribution share	-0.078	0.041	-0.055	-0.040	-0.098	-0.082	-0.062	0.075
	(0.067)	(0.072)	(0.080)	(0.091)	(0.086)	(0.087)	(0.104)	(0.124)
Zip code Republican contribution share × Post	1.030	0.981	1.025	1.104	0.891	0.868	0.796	0.606
	(0.102)	(0.108)	(0.119)	(0.134)	(0.131)	(0.131)	(0.161)	(0.193)
Lagged equity share	-7.563	-7.929	-8.979	-9.199	-10.114	-10.148	-10.263	-10.330
	(0.066)	(0.074)	(0.093)	(0.102)	(0.134)	(0.135)	(0.167)	(0.198)
Lagged equity share × Post	-1.766	-1.873	-1.975	-2.043	-2.965	-2.845	-3.436	-3.555
	(0.091)	(0.101)	(0.128)	(0.140)	(0.196)	(0.197)	(0.247)	(0.292)
Age	-0.059	-0.069	-0.073	-0.075	-0.095	-0.096	-0.101	-0.104
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)
Age × Post	-0.018	-0.017	-0.018	-0.019	-0.018	-0.016	-0.024	-0.022
	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.005)
Log wealth	0.046	0.080	0.020	0.041	-0.123	-0.121	-0.216	-0.242
	(0.007)	(0.008)	(0.014)	(0.016)	(0.015)	(0.015)	(0.018)	(0.021)
Log wealth × Post	0.155	0.176	0.180	0.174	0.351	0.348	0.350	0.370
	(0.011)	(0.012)	(0.021)	(0.024)	(0.024)	(0.024)	(0.029)	(0.034)
Lagged winning sectors share of equity	-0.138	-0.332	-0.652	-0.663	-0.289	-0.303	0.030	-0.020
	(0.089)	(0.099)	(0.128)	(0.137)	(0.163)	(0.163)	(0.196)	(0.218)
Lagged winning sectors share of equity × Post	0.076	0.021	0.084	0.182	-0.308	-0.323	-0.341	-0.248
	(0.136)	(0.153)	(0.193)	(0.206)	(0.240)	(0.241)	(0.289)	(0.325)
Lagged losing sectors share of equity	-0.768	-0.986	-1.890	-1.775	-1.445	-1.401	-0.650	-0.657
	(0.101)	(0.112)	(0.161)	(0.174)	(0.218)	(0.220)	(0.296)	(0.344)
Lagged losing sectors share of equity × Post	1.079	0.847	1.454	1.424	1.014	0.959	0.003	-0.097
	(0.157)	(0.174)	(0.249)	(0.266)	(0.354)	(0.356)	(0.462)	(0.523)
Sample	All	Age < 65	RI sample	RI sample, observed gender & marital status	RI sample, observed employer	RI sample, observed industry	RI sample, observed income 2015-17	RI sample, observed complete controls
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
R^2	0.042	0.044	0.049	0.050	0.057	0.056	0.066	0.066
Share of observations	100.0%	88.3%	69.5%	57.3%	46.7%	45.9%	29.1%	21.3%

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Table A.XIII
Zip-Code-Level Regressions of Price-Constant Equity Share Changes on Demographics

This table presents regression coefficients of the average annual post-election change in price-constant household portfolio equity shares by zip code on the zip code Republican contribution share, for various sets of controls. Variables observed at the individual level are averaged over RI households at the zip code level. We weight zip codes by the number of individuals in our sample and estimate the regression on post-election annual changes. The price-constant equity share is calculated for a hypothetical portfolio that is insensitive to passive appreciations and is driven by trading only. We report standard errors that are robust to heteroskedasticity.

	One-year difference in average price-constant equity share (in %), post election, by zip code							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Zip code Republican contribution share	0.724		0.574					0.718
	(0.041)		(0.041)					(0.046)
Lagged average equity share		-6.253	-5.529	-7.830	-5.653	-6.181	-6.432	-8.797
		(0.289)	(0.296)	(0.344)	(0.283)	(0.285)	(0.287)	(0.351)
Average age				-0.038				-0.101
				(0.004)				(0.006)
Share female					-0.740			-0.139
					(0.079)			(0.081)
Share married					0.464			0.855
					(0.090)			(0.109)
Average log wealth						0.078		0.162
						(0.021)		(0.049)
Average log labor income 2015							0.202	0.104
							(0.031)	(0.061)
R^2	0.035	0.056	0.078	0.066	0.075	0.058	0.062	0.129

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Table A.XIV
Regressions of Price-Constant Equity Share Changes on Likely Political Affiliation with Zip-Level Education Controls

This table presents regression coefficients of annual changes in price-constant household portfolio equity shares on the zip code Republican contribution share, before and after the election, for various sets of controls. The price-constant equity share is calculated for a hypothetical portfolio that is insensitive to passive appreciations and is driven by trading only. The baseline controls are the lagged equity share, age, gender, marital status, log lagged financial wealth, log labor income in 2015, the lagged winning and losing sectors shares of equity, and zip code house price growth (2010–2015), interacted by annual indicators. Here, we also control for educational attainment by zip code: we set the base to having a high school diploma and control for the share of people in the zip code without a high school diploma and the share of people with a college degree from the American Community Survey. In specifications (3)–(8) we consider additional sets of controls (interacted by yearly dummies) that include ex post income growth (2016–2017) and house price growth (2015–2017), employer industry indicators (3-digit NAICS), employer indicators, state indicators, county indicators, and employer × county indicators. The sample is our full set of RI households between October 2015 and October 2017, for which we observe the complete set of controls (27.7% of all RIs). Standard errors are clustered at the zip code level.

	One-year difference in price-constant equity share (in %), all households							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Zip code Republican contribution share	-0.225	-0.290	-0.298	-0.192	-0.175	-0.256	-0.307	-0.247
	(0.039)	(0.043)	(0.044)	(0.041)	(0.041)	(0.048)	(0.056)	(0.060)
Zip code Republican contribution share × Post	1.061	1.046	1.062	0.958	0.935	0.925	0.940	0.853
	(0.057)	(0.062)	(0.063)	(0.062)	(0.064)	(0.070)	(0.087)	(0.094)
Zip code share no high school diploma		-0.955	-0.949	-1.072	-0.946	-1.077	-0.918	-0.768
		(0.146)	(0.146)	(0.135)	(0.135)	(0.156)	(0.172)	(0.177)
Zip code share no high school diploma × Post		-0.816	-0.808	-0.564	-0.670	-0.928	-0.802	-0.627
		(0.210)	(0.210)	(0.206)	(0.211)	(0.227)	(0.262)	(0.281)
Zip code share college degree		-0.223	-0.239	-0.333	-0.409	-0.384	-0.367	-0.496
		(0.080)	(0.083)	(0.075)	(0.071)	(0.083)	(0.094)	(0.093)
Zip code share college degree × Post		-0.031	-0.003	0.170	0.263	0.048	0.103	0.337
		(0.113)	(0.116)	(0.111)	(0.111)	(0.120)	(0.141)	(0.145)
Controls by year								
Baseline	Y	Y	Y	Y	Y	Y	Y	Y
Labor income growth 2016-17			Y					
Zip code house price growth 2015-17			Y					
Employer industry				Y				
Employer					Y			
State						Y		
County							Y	
Employer × County								Y
R^2	0.067	0.067	0.067	0.068	0.080	0.067	0.068	0.189

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Table A.XV
Regressions of Price-Constant Equity Share Changes on Likely Political Affiliation Instrumented by Zip Code Demographic Composition

This table presents regression coefficients of annual changes in price-constant household portfolio equity shares on the zip code Republican contribution share, before and after the election, for various sets of controls. The zip code Republican contribution share is instrumented by the zip code population shares of white, black, and Asian individuals and the share of the population of Hispanic or Latino origin, from the American Community Survey. The price-constant equity share is calculated for a hypothetical portfolio that is insensitive to passive appreciations and is driven by trading only. The baseline controls are the lagged equity share, age, gender, marital status, log lagged financial wealth, log labor income in 2015, the lagged winning and losing sectors shares of equity, and zip code house price growth (2010–2015), interacted by annual indicators. Here, we also control for educational attainment by zip code: we set the base to having a high school diploma and control for the share of people in the zip code without a high school diploma and the share of people with a college degree from the American Community Survey. In specifications (3)–(8) we consider additional sets of controls (interacted by yearly dummies) that include ex post income growth (2016–2017) and house price growth (2015–2017), employer industry indicators (3-digit NAICS), employer indicators, state indicators, county indicators, and employer × county indicators. The sample is our full set of RI households between October 2015 and October 2017, for which we observe the complete set of controls (27.7% of all RIs). Standard errors are clustered at the zip code level.

	One-year difference in price-constant equity share (in %), all households						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Zip code Republican contribution share	0.025	0.042	0.350	0.165	-0.124	-0.334	-0.290
	(0.137)	(0.148)	(0.127)	(0.114)	(0.095)	(0.127)	(0.138)
Zip code Republican contribution share × Post	0.815	0.760	0.645	1.020	1.277	1.690	1.606
	(0.191)	(0.204)	(0.190)	(0.177)	(0.143)	(0.196)	(0.215)
Zip code share no high school diploma	-0.593	-0.580	-0.450	-0.575	-0.933	-0.941	-0.804
	(0.202)	(0.206)	(0.188)	(0.176)	(0.183)	(0.193)	(0.201)
Zip code share no high school diploma × Post	-1.082	-1.137	-0.923	-0.578	-0.544	-0.174	-0.006
	(0.287)	(0.290)	(0.288)	(0.277)	(0.267)	(0.301)	(0.324)
Zip code share college degree	0.003	0.021	0.034	-0.202	-0.301	-0.378	-0.513
	(0.114)	(0.127)	(0.105)	(0.093)	(0.094)	(0.102)	(0.102)
Zip code share college degree × Post	-0.197	-0.235	-0.042	0.315	0.269	0.402	0.622
	(0.162)	(0.178)	(0.159)	(0.146)	(0.140)	(0.156)	(0.161)
Political affiliation instrumented	Y	Y	Y	Y	Y	Y	Y
Controls by year							
Baseline	Y	Y	Y	Y	Y	Y	Y
Labor income growth 2016-17		Y					
Zip code house price growth 2015-17		Y					
Employer industry			Y				
Employer				Y			
State					Y		
County						Y	
Employer × County							Y
R^2	0.067	0.067	0.068	0.080	0.067	0.068	0.189

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Table A.XVI
Regressions of Price-Constant Log Equity Share Changes on Likely Political Affiliation

This table presents regression coefficients of annual changes in the logarithm of price-constant household portfolio equity shares (conditional on positive) on the zip code Republican contribution share, before and after the election, for the full sample and for the subsample of households with an active portfolio reallocation in the prior year, and for various sets of controls. The price-constant equity share is calculated for a hypothetical portfolio that is insensitive to passive appreciations and is driven by trading only. The baseline controls are the lagged equity share, age, gender, marital status, log lagged financial wealth, log labor income in 2015, the lagged winning and losing sectors shares of equity, and zip code house price growth (2010–2015), interacted by annual indicators. In specifications (2)–(8) we consider additional sets of controls (interacted by yearly dummies) that include employer indicators, county indicators, and employer × county indicators. The sample period is October 2015 to October 2017, and we include RIs for which we observe the complete set of controls (27.7% of all RIs), 97.3% of which have strictly positive equity shares. Standard errors are clustered at the zip code level.

	One-year difference in price-constant log equity share (in %)							
	All	Active	All	Active	All	Active	All	Active
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Zip code Republican contribution share	-0.362	-0.984	0.032	-0.169	-0.110	-0.993	-0.020	-0.389
	(0.113)	(0.664)	(0.112)	(0.713)	(0.155)	(0.968)	(0.167)	(1.334)
Zip code Republican contribution share × Post	2.034	6.613	1.731	5.177	1.403	5.744	1.356	4.803
	(0.141)	(0.856)	(0.151)	(0.934)	(0.209)	(1.283)	(0.231)	(1.777)
Controls by year								
Baseline	Y	Y	Y	Y	Y	Y	Y	Y
Employer			Y	Y				
County					Y	Y		
Employer × County							Y	Y
R^2	0.124	0.261	0.136	0.304	0.126	0.268	0.238	0.489

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Table A.XVII
Regressions of Price-Constant Equity Share Changes on Likely Political Affiliation for Likely Republicans or Democrats

This table presents regression coefficients of annual changes in price-constant household portfolio equity shares on the zip code Republican contribution share, before and after the election, for various sets of controls. The sample consists of households that live in zip codes that have a Republican contribution share below 35% or above 65%. The price-constant equity share is calculated for a hypothetical portfolio that is insensitive to passive appreciations and is driven by trading only. The baseline controls are the lagged equity share, age, gender, marital status, log lagged financial wealth, log labor income in 2015, the lagged winning and losing sectors shares of equity, and zip code house price growth (2010–2015), interacted by annual indicators. In specifications (3)–(8) we consider additional sets of controls (interacted by yearly dummies) that include ex post income growth (2016–2017) and house price growth (2015–2017), employer industry indicators (3-digit NAICS), employer indicators, state indicators, county indicators, and employer × county indicators. The sample period is October 2015 to October 2017, and we include RIs for which we observe the complete set of controls (27.7% of all RIs), 50.7% of which live in zip codes in the tails of the measured political affiliation distribution. Standard errors are clustered at the zip code level.

	One-year difference in price-constant equity share (in %), zip codes in tails of political affiliation measure							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Zip code Republican contribution share	-0.300	-0.247	-0.263	-0.134	-0.144	-0.281	-0.401	-0.341
	(0.047)	(0.046)	(0.047)	(0.045)	(0.048)	(0.057)	(0.086)	(0.094)
Zip code Republican contribution share × Post	1.074	1.045	1.056	0.947	0.982	1.084	1.266	1.133
	(0.074)	(0.067)	(0.068)	(0.069)	(0.075)	(0.083)	(0.125)	(0.140)
Lagged equity share		-10.346	-10.347	-10.455	-10.879	-10.363	-10.414	-10.856
		(0.086)	(0.086)	(0.085)	(0.088)	(0.086)	(0.086)	(0.100)
Lagged equity share × Post		-3.616	-3.618	-3.586	-3.550	-3.611	-3.597	-3.512
		(0.111)	(0.111)	(0.111)	(0.113)	(0.111)	(0.110)	(0.127)
Age		-0.099	-0.099	-0.100	-0.102	-0.099	-0.100	-0.101
		(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Age × Post		-0.022	-0.021	-0.022	-0.023	-0.023	-0.022	-0.023
		(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Female		0.309	0.309	0.198	0.169	0.302	0.293	0.168
		(0.017)	(0.017)	(0.018)	(0.019)	(0.017)	(0.017)	(0.022)
Female × Post		-0.482	-0.476	-0.415	-0.394	-0.474	-0.464	-0.375
		(0.027)	(0.027)	(0.029)	(0.030)	(0.027)	(0.027)	(0.035)
Married		0.035	0.035	0.043	0.044	0.040	0.042	0.055
		(0.019)	(0.019)	(0.020)	(0.020)	(0.019)	(0.019)	(0.023)
Married × Post		0.093	0.094	0.064	0.061	0.078	0.066	0.037
		(0.030)	(0.030)	(0.031)	(0.032)	(0.030)	(0.030)	(0.036)
Log wealth		-0.232	-0.233	-0.223	-0.212	-0.232	-0.227	-0.209
		(0.009)	(0.009)	(0.009)	(0.010)	(0.009)	(0.009)	(0.011)
Log wealth × Post		0.236	0.236	0.224	0.217	0.236	0.227	0.212
		(0.015)	(0.015)	(0.015)	(0.016)	(0.015)	(0.015)	(0.018)
Log labor income 2015		0.077	0.071	0.104	0.131	0.084	0.097	0.133
		(0.018)	(0.018)	(0.019)	(0.020)	(0.018)	(0.018)	(0.024)
Log labor income 2015 × Post		0.224	0.231	0.185	0.128	0.221	0.203	0.093
		(0.028)	(0.029)	(0.030)	(0.033)	(0.028)	(0.029)	(0.038)
Controls by year								
Baseline		Y	Y	Y	Y	Y	Y	Y
Labor income growth 2016-17			Y					
Zip code house price growth 2015-17			Y					
Employer industry				Y				
Employer					Y			
State						Y		
County							Y	
Employer × County								Y
R^2	0.001	0.065	0.066	0.066	0.084	0.066	0.067	0.217

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