
Research-Thesis: Political Realignment and Household Portfolio Choice : Evidence from the 2016 U.S. Presidential Election

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POLITICAL REALIGNMENT AND HOUSEHOLD PORTFOLIO CHOICE : EVIDENCE FROM THE 2016 U.S. PRESIDENTIAL ELECTION

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Use of artificial intelligence

In accordance with the ULiège guidelines for the responsible use of generative artificial intelligence in academic work, the author declares that Claude (Anthropic, 2026), a large language model, was used during the preparation of this thesis. The tool was used as a writing assistant to draft and revise text across chapters, improve clarity and academic style, and identify redundant or inconsistent passages across sections. It was also used as a coding assistant to help debug and structure Stata code for the construction of the dataset and the econometric estimations, and to verify consistency between descriptive statistics, regression tables, and the corresponding narrative text. All analytical choices, empirical results, interpretations, and conclusions are the sole responsibility of the author. All AI-assisted content was critically reviewed and revised before inclusion, and several errors identified during this review process, including an incorrect citation and a data coding inconsistency, were corrected by the author.

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1 List of abbreviations

asinh	Inverse hyperbolic sine
CDS	Credit default swap
CEO	Chief executive officer
DC	District of Columbia
DiD	Difference-in-differences
FE	Fixed effects
FEC	Federal Election Commission
FIPS	Federal Information Processing Standards
HS	High school
IRA	Individual retirement account
OECD	Organisation for Economic Co-operation and Development
PSID	Panel Study of Income Dynamics
US	United States
VIX	Chicago Board Options Exchange Volatility Index

2. Introduction

On the night of November 8, 2016, most polls, most financial markets, and most political observers expected a different outcome than the one that unfolded. Donald Trump's victory did not just surprise Washington. It redrew the political map of the country overnight, flipping states that had voted the other way just four years earlier, some of them for the first time in a generation.

A question follows naturally from a moment like this, one that has less to do with politics than with money. If a state's political alignment shifts overnight, something may also shift in how the people living there manage their own savings, not how they vote next time, not what they tell a pollster, but what they actually do with the money already in their accounts.

This is a hard question to answer well, and 2016 makes it harder than most elections would. Agarwal et al. (2022) study a related question using a series of state governor elections spread out over several years, where some states vote in a given year and others do not, a natural comparison group built into the calendar itself. A single national presidential election offers no such comparison. Every state votes on the same day, for the same office, and by the next morning, every household in the country is living under the same new administration, whether their own state's vote moved an inch or swung by twenty points.

The existing evidence on 2016 runs into exactly this wall. Meeuwis et al. (2022), the closest study to this question, look at how American households actually reallocated their money after the election, rather than what they said or how markets moved, but rely on brokerage records that show equity holdings and nothing else, no housing, no retirement accounts, no savings. Their data can say whether investors changed their stock positions. It cannot say whether they moved money somewhere else instead, or nowhere at all. And Agarwal et al.'s (2022) design, built specifically to study political uncertainty at the household level with a genuine control group, depends on elections that unfold in stages, some states voting this year, others the next, exactly what a single national race does not allow.

This thesis is an attempt to close both of these gaps at once, working with the tools a single national election actually leaves available. It follows the same American households across five waves of a long-running national survey, from 2011 to 2019, tracking the full shape of their wealth, stocks, retirement accounts, real estate beyond the family home, other financial assets, checking and savings. And in place of the staggered elections other studies rely on, it uses something 2016 does offer, three kinds of states, those that stayed Democrat, those that stayed Republican, and the six that switched from one to the other, comparing how each group's portfolio moved once the results were in.

What emerges is not a dramatic reallocation across the board. It is something narrower, and for that reason more interesting. Only the stocks share moves, and it moves in one direction, down, in the states that switched sides. The rest of the portfolio holds still. The story this thesis tells is about that one component, why it is the one that responds, and how much confidence the data actually allow in an answer this hard to isolate.

The remainder of this thesis develops that argument in six chapters. Chapter 3 reviews the literature this thesis builds on, from the pricing of political risk in financial markets to the household-level evidence on 2016 specifically. Chapter 4 describes the survey data and how the analysis sample was built. Chapter 5 sets out the empirical strategy. Chapter 6 reports what the data show. Chapter 7 discusses what that finding does, and does not, establish. Chapter 8 concludes.

3 Literature Review

This section reviews the theoretical and empirical literature on political uncertainty and its effects. It surveys how the 2016 U.S. election is documented as a market shock, and examines the exposure channels through which that shock reached different firms, countries, sectors, and ultimately households unevenly. A critical synthesis identifying the gap addressed by the present study concludes the section. A single question runs through the literature reviewed below, whether political uncertainty affects markets uniformly, or whether its effect depends on who is exposed to it and how. Most of the early literature answers the first version of that question. The most recent and most relevant papers answer the second, and it is the second version that motivates this thesis.

3.1 Theoretical Foundations: Political Uncertainty as a Priced Risk

The theoretical starting point is that political change is not neutral for economic agents even before it is enacted. Alesina (1987) shows that when policy is set by parties with different objectives, a political cycle emerges purely from the anticipation of a change in power. This mechanism differs from the older idea of politicians deliberately manipulating the economy to win re-election. Pástor and Veronesi (2013) formalize this for asset prices. In their model, uncertainty about future government policy acts as an extra source of risk that investors must be compensated for, and this risk is more costly when the economy is already weak, because government policy normally acts as a safety net, and that safety net becomes less reliable under political uncertainty. As a result, stock returns become more volatile and move more closely together across firms.

Kelly et al. (2016) find this premium priced directly into option markets around elections and summits worldwide, confirming political uncertainty as a distinct, compensated risk factor rather than a proxy for ordinary macroeconomic uncertainty. Pástor and Veronesi (2017), writing after the fact, add a point that matters directly for this thesis. They note that the Trump presidency combined high policy uncertainty with unusually low realized volatility. Their explanation is that political signals from the new administration were noisy and hard to interpret precisely, and uncertainty only translates into volatility when investors can read the signal clearly. This is an early hint that the reaction to 2016 should not be assumed to be uniform across investors, since some investors may have been able to read the signal better than others.

3.2 The Baseline: Electoral Cycles and Market Reactions in General

This uniform, average effect is nonetheless well established for elections in general, and it forms the baseline against which 2016 needs to be judged. Białkowski et al. (2008) find that country-specific stock market volatility across 27 OECD countries roughly doubles in election weeks, more so with narrow victory margins and a change in government orientation. Goodell and Vähämaa (2013) study how the VIX, the market's measure of expected future volatility, behaves over the course of U.S. presidential campaigns. They find that as it becomes clearer which candidate will actually win, the VIX rises rather than falls, and this holds even after accounting for how uncertain the race itself still is. In other words, it is not the closeness of the race that drives volatility expectations, it is the growing clarity about which policy path is coming next.

Shaikh (2019) applies the same VIX-based approach to compare the 2012 and 2016 campaigns directly, and finds that investor sentiment reacted more strongly in 2016 than in 2012, an early sign that the 2016 race behaved differently from a typical point in the electoral cycle. On the real side, Julio and Yook (2012) show that firms cut investment by 4.8 percent in election years worldwide. By comparison, Jens (2017) uses the asynchronous timing of U.S. gubernatorial elections, the elections

of governors, to separate the effect of political uncertainty from other shocks that might be happening at the same time. He finds a similar pattern to Julio and Yook, firms cut investment by about 5 percent on average during these elections, and by as much as 15 percent for the firms most exposed to political risk. Jens and Page (2024) extend this to firm liquidity, showing that firms build up precautionary cash a year before a prescheduled election and draw it down afterward.

Kim (2018) finds a parallel effect on the financing side, firms pay a higher interest rate premium on syndicated loans when their lender is going through an election, and this premium grows larger the closer and more contested that election is. Chan et al. (2020) show that this general pattern already masks differences underneath it. During a presidential honeymoon, the first period after a new president takes office, the value-based investing strategy earns an average of 3.51 percent per month, compared to just 0.27 percent the rest of the time. This means the honeymoon period does not benefit all investment styles equally, some strategies do far better than others during this window.

Botta and Sakariyahu (2025), studying volatility across multiple asset classes from 1992 to 2024, add that abnormal volatility concentrates specifically in the months before an election and during election week, is amplified during recessions, and is further shaped by polarization, incumbency, and the closeness of the race, three variables that are themselves proxies for how unevenly a given election is expected to be felt. Brogaard et al. (2020) confirm that this pattern is global rather than a U.S. peculiarity, building a broad cross-country measure of political uncertainty and linking it to asset prices worldwide. Taken together, this literature establishes that elections reliably move markets and reduce investment, but even at this aggregate level the size of the effect already depends on firm type, investment style, financing structure, and closeness of the race, which is the exposure logic the rest of the review builds on.

3.3 The 2016 Election as an Unusually Persistent Shock

The 2016 election fits this general pattern of election-driven volatility, but stands out as an unusually large surprise, and the literature specific to it shows that its effects did not settle down quickly or evenly the way earlier elections did. Wolfers and Zitzewitz (2016) look at market movements around the first presidential debate, the moment when Clinton's odds of winning briefly rose, to estimate what investors expected a Trump victory to mean for markets. They find that a Trump win was priced as bad news, expected to lower the level of the S&P 500 by 10 to 15 percent and to sharply raise expected future volatility.

Blau et al. (2019) show that specific, policy-sensitive stock prices had already begun foreshadowing a Trump win before election day, despite prediction markets giving Clinton over a 90 percent chance. Cox and Griffith (2019) compare the 2016 election to earlier U.S. elections that serve as a benchmark. In those earlier elections, uncertainty dropped once the result was known. In 2016, it did not. In the days after the election, two measures of market stress kept rising instead of falling, the cost of trading, and the extra compensation market makers demand when they worry they are trading against better-informed investors. Both of these normally fall once an election is decided, but in 2016 they kept climbing, which the authors take as evidence that the result itself created new uncertainty rather than resolving it.

Pham et al. (2018) confirm a graduated reaction across 47 campaign and post-election milestones, strongest around the Republican nomination and concentrated in sectors such as life insurance. Wielechowski et al. (2026) compare 2016 with the two other Trump-involved elections, 2020 and 2024. In all three cases, they find a significant spike in volatility, but the effect on actual returns is not consistently significant across the three elections.

Diaconășu et al. (2023) look at international markets and find a different pattern, prices first fell after the 2016 result and then swung back upward, more than a clean and efficient repricing would predict, which the authors read as evidence of overreaction rather than accurate pricing. Mlandu et al. (2026) compare the 2016 and 2024 Trump elections directly and find two things. First, the sectors most affected are not the same across the two elections. Second, price and volatility patterns explain the market reaction far better than the tone of campaign-related social media posts does. Together, this suggests that what drove the 2016 reaction was concrete exposure to expected policy, not simply the mood of the news cycle.

What matters for this thesis is not simply that 2016 moved markets, since that much was expected from the baseline literature, but that the shock stayed unresolved for longer than usual and that its earliest price movements were already concentrated in specific, policy-sensitive stocks rather than spread evenly across the market. This suggests that households may have kept adjusting their portfolios for longer than just the days right around the election, and that different households likely reacted differently rather than everyone reacting the same way.

3.4 Exposure Channels: Sectoral and International Evidence

The exposure channel becomes explicit once the literature moves from aggregate reaction to sector and country level detail. Regulatory exposure explains two related results. Bank stocks rose sharply after the election on expectations of lighter regulation ahead, even as the cost of insuring against their default, measured by CDS spreads, widened at the same time (Hachenberg et al. (2017)). Separately, Kundu (2024) finds that the most heavily regulated firms overall earned about 4 percent higher abnormal returns than the least regulated firms in the ten days after the election, consistent with investors pricing in a broad deregulation agenda across the economy, not just in banking.

Climate policy exposure explains a more surprising pattern. Carbon-intensive firms gained after the election, which makes sense given Trump's stance on climate regulation, but climate-responsible firms held by long-term investors gained too. Ramelli et al. (2021) interpret this second result as a bet, long-term investors expected the policy shift to eventually reverse, and that bet paid off when Biden won the following election in 2020.

Diaz-Rainey et al. (2020) look inside the oil and gas sector specifically and find the opposite pattern. Because Trump's policies favored smaller, unlisted domestic producers over larger, listed firms with international operations, listed oil and gas companies did not benefit the way the broader carbon-intensive category did.

Mukanjari and Sterner (2018) find only a moderate market reaction overall, evidence that markets had already priced in much of this policy shift before the election even happened. Morales et al. (2019) extend the same exposure logic to investment style rather than sector, showing that ethical and conventional stock indices moved apart from each other during this period of heightened uncertainty.

Trade exposure follows the same pattern. Silvy (2025) shows it at the firm level, U.S. multinationals with larger operations in Mexico lost thirteen times more value than firms with comparable exposure elsewhere, matching what markets expected based on Trump's trade rhetoric during the campaign. Cunha and Kern (2018) show it at the country level, foreign markets reacted negatively overall, but countries with closer financial ties to U.S. banks were partly shielded from that negative spillover. The JPMorgan Chase Institute (2018) adds a timing dimension to the same story, using trading data from three separate surprise events, Brexit, the U.S. election, and the Swiss National Bank's currency policy

change. Investors reacted very differently to the election than to the Swiss announcement, and the authors trace this back to how much warning each event gave investors to prepare.

Yavas et al. (2022) add an important twist to this exposure story. They find that the way returns and volatility move together between the U.S. and Canada, China, Mexico, and Russia actually changed after the election. This matters because it shows exposure is not just something the shock reveals, a country was already more or less exposed before the shock hit, it is also something the shock itself can change, altering how closely a country's market is tied to the U.S. going forward.

Across all of these examples, the same idea keeps showing up. An average market reaction, taken on its own, hides real differences between firms or countries, and those differences only become visible once you identify what exposure variable is driving them, regulation, climate policy, trade, financial ties, or timing. This is also why the international spillover studies matter mainly as a methodological example for this thesis rather than as direct evidence. They rely on comparing countries with different levels of exposure to the same U.S. shock, and a study of U.S. households responding to a single national election cannot make that same comparison, since there is no country to compare the U.S. against. The lesson still carries over, though. If geography cannot serve as the source of identifying variation for this thesis, exposure has to.

3.5 Individual Investors and the Partisan Divide

The final and closest layer of literature moves the same exposure logic down to the level of individual investors. Agarwal et al. (2022) show, using a difference-in-differences design built on staggered U.S. gubernatorial elections, that households reduce stock market participation and shift into safer assets as political uncertainty rises, a response they trace both to a perceived increase in the riskiness of holding stocks and to a desire to protect against uncertainty about future job income tied to the election outcome.

Their design has a genuine control group of households in non-election states, unavailable for a single national election such as 2016. Meeuwis et al. (2022) study 2016 directly and find a sharp partisan split, likely Republicans raised their equity share and portfolio beta after the election while likely Democrats moved into safer assets, a divergence they trace to differing beliefs about the economy rather than to income hedging, preferences, or local exposure.

They note this reaction was concentrated in a small share of highly active investors, which is itself an exposure result, exposure to trading activity and attention rather than to a sector or a border. Three further studies show that this same partisan divide extends beyond individual portfolios. Mbanga et al. (2019) find it at the level of trading strategies, certain well-known strategies that exploit stock mispricing perform better when a Republican is in office than when a Democrat is, an advantage strongest specifically around elections and their aftermath.

Selmi and Bouoiyour (2020) find a matching result at the firm level, political uncertainty around the 2016 election made it more expensive for firms to raise financing, the same shift toward safety that Agarwal et al. (2022) find at the household level. Baker et al. (2020) suggest a reason all of these reactions may have been unusually sharp in 2016 specifically, rising political polarization increases economic policy uncertainty on its own, independent of any single election. This same polarization may help explain the belief divide central to Meeuwis et al.'s (2022) result, and Hu et al. (2025) show that this kind of divide was not limited to portfolios, minority CEOs became more pessimistic in their earnings forecasts after 2016, evidence that identity and belief-based reactions to the shock extended well beyond investment decisions.

3.6 Research Gap and Positioning of This Thesis

Two gaps follow directly from this exposure thread. First, the only household-level evidence on 2016 itself, Meeuwis et al. (2022), relies on proprietary brokerage data covering equity holdings. It is not a nationally representative panel, and it does not cover bonds or retirement accounts. As a result, it cannot say whether households reallocated across asset classes, only whether they changed their equity holdings.

Second, the one available quasi-experimental household design for political uncertainty, Agarwal et al. (2022), depends on a staggered election, one where some states vote and others do not in a given year. A single national race like 2016 cannot offer that. An equivalent identification strategy therefore has to replace the missing geographic control group with something else, exposure. State political orientation, income source, and socioeconomic profile can all serve this purpose, following the same logic already used at the firm and country level in the sectoral and international literature reviewed above.

This thesis is built to close exactly this pair of gaps. It uses five biennial PSID waves, from 2011 to 2019, in an event-study difference-in-differences design that covers stocks, bonds, checking accounts, real estate and retirement accounts, and it treats differential household exposure, rather than geography, as the source of identifying variation.

4. Data

4.1 Data Source and Variable Selection

4.1.1 Data Source and Rationale

The data used in this study come from the Panel Study of Income Dynamics (PSID, 2026),¹ the longest running longitudinal household survey in the United States, conducted by the Survey Research Center at the Institute for Social Research, University of Michigan. PSID was chosen for three main reasons.

It follows the same individuals and their descendant families over time, which allows the construction of a genuine panel around the 2016 election. It has included a dedicated wealth module since 1984, collected biennially since 1999, which covers the five asset classes at the center of this thesis, namely stocks, bonds and related financial assets, retirement holdings, checking/savings and real estate. It combines a Family-level file, which holds household level information such as portfolio composition and income, with an Individual-level file, which holds person level information such as age, education, employment status and health. Combining both levels was a requirement since the household is the natural unit for portfolio decisions while several relevant control variables, in particular the socioeconomic profile of the reference person, are only recorded at the individual level.

4.1.2 Sample Structure

Data were extracted for five biennial waves, 2011, 2013, 2015, 2017 and 2019, providing three waves before the election (2011, 2013, 2015) and two waves after (2017, 2019). Both the Family-level and Individual-level PSID files were pulled and combined to allow the construction of household level dependent variables alongside individual level demographic and socioeconomic controls.

4.1.3 Summary of Retained Variables

Table 4.1 summarizes the number of variables retained in each category, across the five waves and both the Family-level and Individual-level files. The initial working list targeted 291 variables. The final extracted dataset from PSID's Data Center contains 299 variables for 34,795 individuals, the additional eight being administrative fields that PSID appends automatically to any extraction like release numbers repeated per wave, the person number and the sample or non sample indicator.

¹ Panel Study of Income Dynamics, *Main Interview User Manual: Release 2017*, Survey Research Center, Institute for Social Research, University of Michigan. <https://psidonline.isr.umich.edu/data/Documentation/UserGuide2017.pdf>

Table 4.1. PSID Variable Selection by Thematic Category

Category	Sub-category	Number of variables
Portfolio and financial assets	Stocks (possession and value, imputed)	10
	Bonds and other financial assets (possession and value, imputed)	10
	IRA and private annuities (possession and value, imputed)	10
	Total net worth, with and without equity (imputed)	10
	Checking and savings accounts (possession and value, imputed)	10
Employer pension plans	Current job pension possession and account value (Head/Spouse)	20
	Benefit calculation method, proxy for Defined Benefit versus Defined Contribution plans	10
Housing and debt	Home equity (imputed)	5
	Mortgage possession and remaining principal	10
	Household debt across five categories, credit card debt, medical debt, legal debt, student loan, family loan	50
	Real estate other than main residence	14
Income	Total family income	5
	Labor income (Head/Spouse)	10
	Transfer income	5
Demographic and individual controls	Age, sex, race, education, marital status, employment, health	51
Household and geographic controls	State of residence, family size, number of children, homeownership	20
Technical identifiers and weights	Interview numbers, 1968 ID, sequence number, relation to head, family and individual weights	31
Wealth transfer control	Inheritance and gift receipt and value	10
Total		291

Note. Variable counts are drawn from the PSID Variable Search tool and reflect systematic selection across thematic blocks. Counts include separate Head and Spouse entries and imputed values where indicated.

4.2 Data Construction and Sample Selection

This section documents the construction of the final analysis dataset from the raw PSID extract, covering variable harmonization, panel reshaping, sample restriction, and construction of the electoral and portfolio share variables used in the empirical analysis. All data management was performed in Stata.

4.2.1 Variable Renaming

All 299 raw PSID variable names were renamed to a descriptive, wave-suffixed naming convention, for example `stocks_value2011`, `stocks_value2013`, to prepare the file for reshaping and to improve legibility. Role-based variable names that changed terminology across waves, reflecting PSID's 2017 transition from "Head" and "Wife" to "Reference Person" and "Spouse", were harmonized into a single consistent naming convention, "head" and "spouse" referring to the same underlying household role across all five waves.

4.2.2 Reshaping to Long Panel Format

The dataset was reshaped from wide format, one row per individual, with each variable repeated five times, to long format, one row per individual per year, using a unique person identifier following standard PSID practice for cross-wave individual identification. The reshape produced 173,975 person-year observations, 34,795 individuals across five waves, and 65 variables.

4.2.3 Sample Restriction to Head and Spouse

The analysis sample was restricted to household heads and their spouses or partners, excluding children and other household members, on the grounds that financial portfolio decisions are made by the head of household and their partner rather than by dependents. The total number of people in the family unit remains available as a separate variable, but the estimation sample itself retains only the head and the spouse or partner.

Gender imbalance in the PSID Head designation

Before implementing this restriction, the composition of the resulting sample was examined for a known limitation of the PSID Head variable. Prior to 2017, PSID's coding convention automatically designated the male partner as "Head" in any heterosexual married couple, irrespective of who managed the household's finances, with a woman coded as Head only in single, divorced, widowed, or separated households. Cross-tabulation of the current sample confirmed this pattern empirically, 23,592 of 23,786 married-couple heads are male. Because this designation reflects a data collection convention rather than a substantive household characteristic, it is noted here as a limitation, and the sample retains both the head and the spouse or partner rather than the head alone, so that both members of a couple contribute to the estimation sample.

Restriction criteria and household-size adjustment

The sample was restricted to individuals coded as household head, legal spouse, or cohabiting partner of 12 or more months, reducing the panel from 173,975 to 71,996 observations. A household size counter (`n_couple`) was constructed as the number of individuals remaining per household after this restriction, which takes the value 1 for a single head with no partner and 2 for a couple, as expected under this restriction criterion for the large majority of cases.

4.2.4 Multi-Couple Household Exclusion

The household-size counter (`n_couple`) is expected to equal 1 for a single head or 2 for a head and one spouse or partner. Cross-tabulation showed that in 790 person-year observations, three or four

individuals were coded as head, spouse, or partner within the same household, which should not happen under a normal one-couple household structure.

Two possible explanations were ruled out before deciding how to treat these cases. First, these were not duplicate records, each individual carried a distinct person identifier. Second, they were not simply unrelated people sharing a home, since all individuals involved traced back to the same PSID family of origin. The most likely explanation is a multi-generational or multi-couple household, for instance a married adult child still living with their parents. PSID records a single shared wealth figure for the whole household in these cases, confirmed by identical wealth values reported for everyone in it, even though more than one couple actually lives there. PSID's head-designation coding is built for a single couple per household, so it does not cleanly identify who counts as head and spouse when more than one couple is present.

Because dividing household wealth by two or three people in these atypical households would not produce a meaningful per-person value, the 790 affected observations were excluded from the sample. This reduced the sample from 71,996 to 71,206 person-year observations.

4.2.5 Balanced Pre- and Post-2016 Restriction

Because the identification strategy relies on within-individual comparison of portfolio composition before and after the November 2016 election, the sample was further restricted to individuals with at least one observation in a pre-election wave (2011, 2013, or 2015) and at least one observation in a post-election wave (2017 or 2019), a necessary condition for individual fixed-effects estimation, since an individual observed only before or only after the election contributes no within-person variation to identify the treatment effect and is dropped automatically by a fixed-effects estimator in any case. This restriction reduced the sample to 60,442 person-year observations covering 13,247 unique individuals, with a roughly even distribution across the five waves, 18 to 21 percent of observations per wave. The cases of individuals having data recorded only after or before 2016 can easily be explained by examples of children becoming the heads of their own family units or by the death of some individuals during those 5 waves.

4.2.6 Per-Person Wealth Adjustment

Once the restrictions and exclusions were applied, all family-level variables were divided by the remaining number of individuals in the household, `n_couple`, equal to 1 or 2, to obtain a per-person measure. Dividing by the total number of people in the family unit was considered and rejected, since financial portfolio decisions are made by the head and partner only, and dividing by full family size would understate per-person wealth differentially across families with different numbers of dependents, introducing variation unrelated to the treatment. Dividing by `n_couple` instead ensures both members of a couple are assigned an equal share of household wealth rather than the couple's combined total being attributed identically to each partner. This adjustment also neutralizes the gender imbalance in PSID's Head designation noted in section 4.2.3, since both partners in a heterosexual couple are assigned an identical divided amount regardless of which one is coded as Head.

This adjustment was applied to the seventeen household-level dollar variables (checking and savings, real estate, stocks, other assets, IRA, the five debt categories, home equity, the two net worth aggregates, total family income, transfer income, gift and inheritance value, and mortgage principal), and was not applied to variables already measured at the individual level (head and spouse labor income, head and spouse pension account balances), to sampling weights, to household size counts, or to categorical and binary indicator variables, for which division by household size would not be meaningful.

4.2.7 Correcting for Household-Level Duplication on Role-Specific Variables

PSID stores some variables once per household per wave, then copies that same value onto both the head's row and the spouse's row. For example, if the head's health status is "Good", both the head's row and the spouse's row show "Good" for that variable, even though only the head actually reported it. This was confirmed by comparing values across the two members of each family for a given year, no discrepancy was found across more than 40,000 family-year pairs, meaning the duplication is systematic rather than a data error.

For most variables this duplication is harmless. Variables like total family income or home equity describe the household as a whole, not one specific person, so it makes sense that both partners show the same value once that value has been divided by `n_couple` (section 4.2.6). Each spouse correctly gets their fair share of the family total.

Health status, labor income, race and Hispanic origin are different, because they describe one specific person, the head or the spouse, not the household as a whole. If used as they are stored, the head's health status or income would incorrectly appear on the spouse's row too, and vice versa, misattributing one person's characteristic to the other. To fix this, four new variables were created, `own_health`, `own_labor_income`, `own_race` and `own_hispanic`, which pick out the value that actually belongs to each person using the `relation_head` indicator, which says whether a given row is the head or the spouse.

Race and Hispanic origin are recorded as two independent questions in PSID, following the U.S. Census convention, so a respondent can report being White and also report a Hispanic origin such as Mexican or Cuban. `Own_race` and `own_hispanic` were therefore combined into a single `race_grp` variable, giving Hispanic origin priority over race whenever both are reported, a standard convention in applied research since most Hispanic respondents in the sample report White or Other on the race question itself.

4.2.8 State-Level 2016 Electoral Variables

Since the treatment in this thesis is the outcome of a national election, exposure has to be captured at the state level rather than through a natural control group. Three variables were constructed for this purpose, based on each person's state of residence.

The state variable itself first had to be checked. PSID uses its own state coding system, different from the standard FIPS codes used elsewhere, so the PSID codes were verified against official documentation to make sure states were correctly identified, an exact match was found across all 51 codes (50 states plus DC).

The first electoral variable, `trump_margin_2016`, is the gap in percentage points between the Republican and the Democratic vote share in each state's certified 2016 result, sourced from the FEC. A positive value means the state voted Republican, a negative value means it voted Democrat, and the size of the number reflects how close or lopsided the result was. Wyoming, for example, sits at plus 46 points, while Michigan and Wisconsin are close to zero, reflecting how tightly contested they were. The second variable, `trump_won_2016`, is just the same information simplified to a yes or no, whether the state was won by the Republican candidate. It was built directly from the sign of the margin variable rather than re-entered separately, to avoid two independent sources of transcription error. 348 person-year observations have a missing state and are therefore missing on both variables.

A third variable, `state_flip_cat`, adds one more layer, whether a state's result actually changed between 2012 and 2016. It sorts states into three groups, stayed Republican in both elections, stayed

Democrat in both elections, or flipped from Democrat to Republican in 2016. Six states flipped, Florida, Iowa, Michigan, Ohio, Pennsylvania, and Wisconsin. This matters because a flip represents a real, unexpected political shift for the people living there, unlike a state that simply confirmed its usual political leaning. Flipped states should therefore give a cleaner signal of the effect of the election than the simple Republican/Democrat split alone.

4.2.9 Portfolio Share Variables

Portfolio composition is expressed in relative rather than absolute terms. Five portfolio share variables were constructed, expressing the value of each asset class (stocks, IRA, real estate other than the primary residence, other assets, and checking and savings) as a proportion of total gross assets.

An initial specification using PSID's net worth aggregate excluding home equity as the denominator produced extreme outliers, with individual shares as high as 155 times total net worth and kurtosis between roughly 2,200 and 10,800 across the five variables, driven by households whose general debt nearly offset their gross assets, so that dividing by this near-zero denominator produced extreme ratios unrelated to genuine portfolio concentration. The denominator was therefore replaced with the sum of the five gross asset categories, together with a minimum threshold of 100 dollars, which reduced kurtosis to a range of roughly 1.6 to 16, an order-of-magnitude improvement.

Winsorization

As a final step, each of the five share variables was winsorized at the first and ninety-ninth percentile, computed from the sample's own empirical distribution, with values below the first percentile set to the first-percentile value and values above the ninety-ninth percentile set to the ninety-ninth-percentile value, following standard practice in applied household finance research to limit the influence of extreme observations on regression estimates without discarding observations outright. Winsorized variables were retained alongside the original, non-winsorized share variables under a separate suffix, to permit robustness checks against the untrimmed specification. The winsorization procedure was verified to leave the count of missing observations unchanged for all five variables, since Stata treats the missing value code as larger than any real number in numeric comparisons, which would otherwise silently overwrite genuinely missing observations with the ninety-ninth-percentile value during the upper-bound trimming step. This was avoided by explicitly excluding missing values from the upper-bound replacement condition.

4.2.10 Summary of Sample Construction

Table 4.2 summarizes the sequential sample restrictions described above, from the raw PSID extract to the final estimation sample.

Table 4.2. Sample Restriction Steps

Sample restriction step	Person-year observations	Unique individuals
Raw import (all individuals, all relations, 2011-2019)	173,975	34,795
Restricted to Head, legal spouse, or cohabiting partner (12+ months)	71,996	n/a
Multi-couple households excluded ($n_couple \geq 3$)	71,206	n/a
Restricted to individuals with at least one pre-2016 (2011, 2013, 2015) and one post-2016 (2017, 2019) observation	60,442	13,247

Note. n/a, not applicable.

The final analysis dataset contains 60,442 person-year observations across 13,247 unique individuals and 77 variables, combining the harmonized demographic, income, and wealth variables described above with the state-level electoral variables and portfolio share measures constructed for the empirical analysis.

4.3 Profile and Moderator Variables

Seven dimensions were selected to explore heterogeneity in portfolio composition, summarized in Table 4.3.

Table 4.3. Profile and Moderator Variables

Dimension	Variable	Categories
Age	<i>age_grp</i>	15-29, 30-44, 45-59, 60-74, 75-89, 90-104
Sex	<i>sex</i>	Male, Female
Education	<i>educ_grp</i>	Less than 12 years (no HS diploma), 12 years (HS diploma), 13 to 15 years (some college), 16 to 17 years (bachelor and above)
Health status	<i>health_grp</i>	Excellent or very good, Good, Fair or poor
State political orientation	<i>state_flip_cat</i>	Stayed Democrat, Stayed Republican, Flipped to Trump between 2012 and 2016
Race and ethnicity	<i>race_grp</i>	White, Black, Hispanic, Other
Household composition	<i>n_couple</i>	Single head, Couple

Age is grouped into six fifteen year bands spanning the full range of the sample, from 15 to 104 years, chosen for consistency with the panel's age range rather than an arbitrary cutoff. Education, measured in completed years of schooling, follows standard thresholds used in the United States schooling

system, with the top category capped at 17 years, the maximum value recorded in the data. Health status uses the `own_health` variable described in section 4.2.7, collapsed from its original five point scale into three broader categories, since the original five categories produced very small cell counts once split by year. Race and ethnicity uses the `race_grp` variable described in section 4.2.7, which gives Hispanic origin priority over the race response whenever both are reported. Household composition distinguishes single heads from couples using `n_couple`, the household-size counter introduced in section 4.2.3.

4.4 Descriptive Statistics

4.4.1 Portfolio Share Variables Used in Descriptive Analysis

The five outcome variables, the shares of stocks, IRA, real estate other than the principal residence, checking and savings, and other assets, are each expressed as a proportion of gross assets, as built in section 4.2.9.

Households with gross assets below the 100 dollar floor, around 26 percent of the sample, are left out of the share calculations rather than counted as zero. The reasoning is simple, a household with no assets at all has nothing to allocate, so there is no portfolio choice to observe. Counting it as a zero stock share would mix up two different questions, whether a household holds any wealth in the first place, and how a household that does hold wealth chooses to split it across asset classes. This thesis is only interested in the second question, so households with essentially no wealth are excluded rather than treated as if they chose to hold 0 percent in stocks. This share of near-zero-wealth households falls over time, from around 30 percent in 2011-2015 to around 20-22 percent from 2017 onward.

All descriptive tables and graphs in this thesis use the winsorized versions of the five share variables, as described in section 4.2.9.

4.4.2 Descriptive Results

Table 4.4 reports pooled sample means for the main background variables across the five waves, alongside the five portfolio shares themselves. It offers useful context before turning to each asset class individually. Average household income and wealth rise steadily in nominal terms across the panel, while the stocks and IRA shares fall and the checking and savings share rises, a shift toward liquidity that shows up consistently in the profile breakdowns discussed below. All dollar amounts are reported in nominal terms, without adjustment for inflation, so part of the rise in income and wealth reflects general price growth rather than a real increase, whereas the portfolio shares themselves are largely unaffected since both the numerator and denominator are expressed in the same year's dollars. The background variables and the portfolio shares are computed on two different samples, as reflected in the two N rows at the bottom of the table, the background variables use the full sample, while the shares are restricted to households with gross assets of at least 100 dollars, as described in section 4.4.1.

Table 4.4. Descriptive Statistics by Wave

Variable	2011	2013	2015	2017	2019
Health status (1=excellent...5=poor)	2.456	2.485	2.518	2.586	2.627
Labor income, own (\$)	31,660	33,215	34,263	36,415	39,438
Transfer income (\$)	3,240	3,233	3,287	3,599	3,941
Family income (\$)	41,669	43,711	45,293	48,917	52,684
Credit card debt (\$)	1,872	1,553	1,525	1,585	1,780
Age (years)	44.1	44.7	45.4	47.4	49.0
Home equity (\$)	37,332	37,844	42,703	51,104	60,943
Wealth, no home equity (\$)	95,160	93,746	107,816	120,844	132,665
Education (years)	13.56	13.63	13.67	13.73	13.76
Female (share)	0.564	0.559	0.554	0.553	0.559
Republican state (share)	0.637	0.640	0.643	0.648	0.650
Gross assets (\$)	73,270	74,279	81,611	93,851	101,338
Stocks share	7.3%	7.3%	7.0%	6.2%	5.6%
IRA share	18.6%	17.2%	16.5%	14.9%	17.3%
Real estate share	9.0%	8.8%	8.7%	7.8%	7.6%
Other assets share	6.5%	6.3%	6.9%	5.1%	5.0%
Checking share	58.5%	60.4%	60.9%	66.1%	64.5%
N (full sample)	10,939	11,862	12,834	12,916	11,891
N (gross assets ≥ \$100, used for shares)	7,672	8,291	8,947	10,329	9,301

Note. Statistics are sample means by wave, except Female and Republican state, which are proportions. Portfolio shares are computed among individuals with gross assets of \$100 or more, with the corresponding N reported in the final row.

The full set of pooled descriptive tables by profile dimension, for all five portfolio shares, is reported in Appendix B.

Stocks

The stocks share falls steadily across the panel, from around 7.3 percent in 2011 to 5.6 percent in 2019.

The clearest gradients are by age, education and race. Older respondents and those with a bachelor's degree hold a substantially larger stocks share throughout, up to roughly 15 percent for the 75-89 age group and around 10-12 percent for bachelor's graduates, compared to just 1 to 5 percent for the youngest respondents and those without a high school diploma. White and Other households hold a stocks share of 8.7 and 8.0 percent, close to three times the 2.1 and 2.6 percent held by Black and Hispanic households. Men hold slightly more than women, and respondents in better health hold noticeably more than those in fair or poor health. Couples hold a higher stocks share than single heads, 7.2 against 4.9 percent. By state, stocks shares decline everywhere after 2016, but the decline is markedly steeper in states that flipped to Trump than in states that stayed Democrat or stayed Republican.

Overall, stock ownership is clearly stratified by age, education and health, and the state comparison offers the clearest preliminary sign in the data that the 2016 election coincided with a sharper portfolio shift specifically in flipped states.

Individual Retirement Accounts (IRA)

The IRA share declines from around 18.6 percent in 2011 to 14.9 percent in 2017, before an unexplained rebound to 17.3 percent in 2019.

As with stocks, IRA holdings rise with education and are highest among respondents nearing retirement age (60 to 74), and higher among healthier respondents. The same racial gradient as for stocks appears here too, White households hold the largest IRA share at 20.8 percent, followed by Other households at 16.8 percent, while Black and Hispanic households hold substantially less, at 8.7 and 7.2 percent. Couples hold a larger IRA share than single heads, 18.7 against 10.7 percent, in line with the pattern already seen for stocks. By state, stayed-Democrat and flipped states follow almost the same path throughout the panel, both consistently above stayed-Republican states, with no visible widening of the gap around 2016.

IRA allocation is clearly stratified by age and education, but unlike stocks, the state comparison shows no distinct additional shift in flipped states, the gap between Republican and non-Republican states looks like a stable, pre-existing difference rather than a 2016 effect.

Checking and Savings

The checking and savings share rises across the panel, from around 58.5 percent in 2011 to a peak of 66.1 percent in 2017, settling at 64.5 percent in 2019.

This share moves in the opposite direction to stocks and IRA on nearly every dimension. It is highest among the youngest respondents, those without a high school diploma, and those in fair or poor health, and lowest among older, more educated, healthier respondents, essentially a mirror image of the stocks gradient. Black and Hispanic households hold the largest checking share, at 75.9 and 76.0 percent, against 55.9 percent for White households and 61.4 percent for Other households, again the reverse of the stocks and IRA pattern. Single heads hold a larger checking share than couples, 73.0 against 59.0 percent, the opposite direction from stocks and IRA. By state, stayed-Republican households hold consistently the largest checking share, with flipped states in between and stayed-Democrat states lowest, though all three groups rise together after 2016.

The checking share confirms the same profile groups identified for stocks, just from the opposite side. The rise in liquidity after 2016 is broad-based across states rather than concentrated in a specific profile.

Real Estate Other Than the Principal Residence

The real estate share declines only mildly across the panel, from around 9.0 percent in 2011 to 7.6 percent in 2019.

Profile gradients are noticeably weaker here than for the financial asset shares. Real estate holdings rise moderately with age up to the 60 to 74 group before flattening, and the education gradient is fairly flat, unlike the sharp gradient seen for stocks. Race shows a milder version of the same pattern, White households hold the largest share at 9.1 percent, Other and Hispanic households hold 8.3 and 8.1 percent, and Black households hold the least at 6.2 percent. Household composition shows the widest gap of any profile dimension for this share, couples hold nearly twice the real estate share of single heads, 9.4 against 4.9 percent. By state, flipped states hold a consistently lower real estate share than both other groups throughout the entire panel, a gap already present well before 2016.

Real estate shows the weakest profile stratification of the five asset classes. Its main notable feature is a persistent, pre-existing gap for flipped states rather than a shift coinciding with the election.

Other Assets

The other assets share, a proxy combining bonds, life insurance cash value, and valuable collections, declines from around 6.5 percent in 2011 to 5.0 percent in 2019, with most of the drop occurring after 2016.

Profile differences are modest and inconsistent in direction across dimensions. Respondents without a high school diploma and those in fair or poor health hold slightly more of this asset class than their counterparts, a mild reversal of the pattern seen for stocks. Black households hold a slightly larger share, at 7.0 percent against 5.5 to 6.1 percent for the other three groups, and single heads hold marginally more than couples, 6.5 against 5.7 percent. By state, the three groups follow broadly similar downward paths, without a distinctly sharper decline in flipped states.

Other assets is the least stratified and least politically differentiated of the five shares, consistent with its role as a residual, less actively managed category.

4.4.3 Closer Look at the Explanatory and Main Variables

The pooled comparisons in section 4.4.2 describe average differences across profile groups over the whole 2011-2019 panel, but they do not show how each group's portfolio share moved over time, which is the relevant comparison given the thesis's focus on the 2016 election as a shock. Figures 4.1 to 4.5 address this directly for the treatment variable, `state_flip_cat`, and Table 4.5 reports the exact coefficients behind them, from a regression of each share on the full interaction of wave and `state_flip_cat`, individual fixed effects absorbed, standard errors clustered by `state2015`. Each coefficient is the gap between a given group's mean share and the mean share of states that stayed Democrat in 2015, the omitted reference cell, so that a coefficient of zero would indicate no difference from the reference group in that year.

Table 4.5. Event-Study Coefficients by Share and Group, Relative to 2015

Share	Group	2011	2013	2017	2019
Stocks	Stayed Republican	0.0023 (0.0080)	0.0039 (0.0064)	0.0059 (0.0062)	0.0076 (0.0057)
	Flipped to Trump	0.0037 (0.0073)	-0.0009 (0.0084)	-0.0066 (0.0068)	-0.0113 (0.0073)
IRA	Stayed Republican	-0.0071 (0.0097)	-0.0040 (0.0111)	-0.0052 (0.0111)	-0.0036 (0.0115)
	Flipped to Trump	0.0005 (0.0159)	0.0072 (0.0114)	0.0107 (0.0119)	0.0097 (0.0162)
Real estate	Stayed Republican	0.0175** (0.0076)	0.0221*** (0.0059)	0.0066 (0.0054)	0.0048 (0.0070)
	Flipped to Trump	-0.0015 (0.0076)	-0.0029 (0.0047)	-0.0051 (0.0053)	-0.0054 (0.0065)
Other assets	Stayed Republican	-0.0114 (0.0079)	-0.0192** (0.0090)	-0.0075 (0.0063)	-0.0173** (0.0077)
	Flipped to Trump	-0.0189** (0.0087)	-0.0106 (0.0101)	0.0021 (0.0053)	-0.0149** (0.0073)
Checking	Stayed Republican	-0.0011 (0.0126)	-0.0028 (0.0120)	0.0006 (0.0146)	0.0088 (0.0152)
	Flipped to Trump	0.0147 (0.0174)	0.0059 (0.0109)	-0.0023 (0.0142)	0.0207 (0.0188)

*Note. Coefficient (clustered standard error). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The reference cell is 2015, Stayed Democrat. Individual fixed effects absorbed, standard errors clustered by state2015 for all five regressions*

Stocks

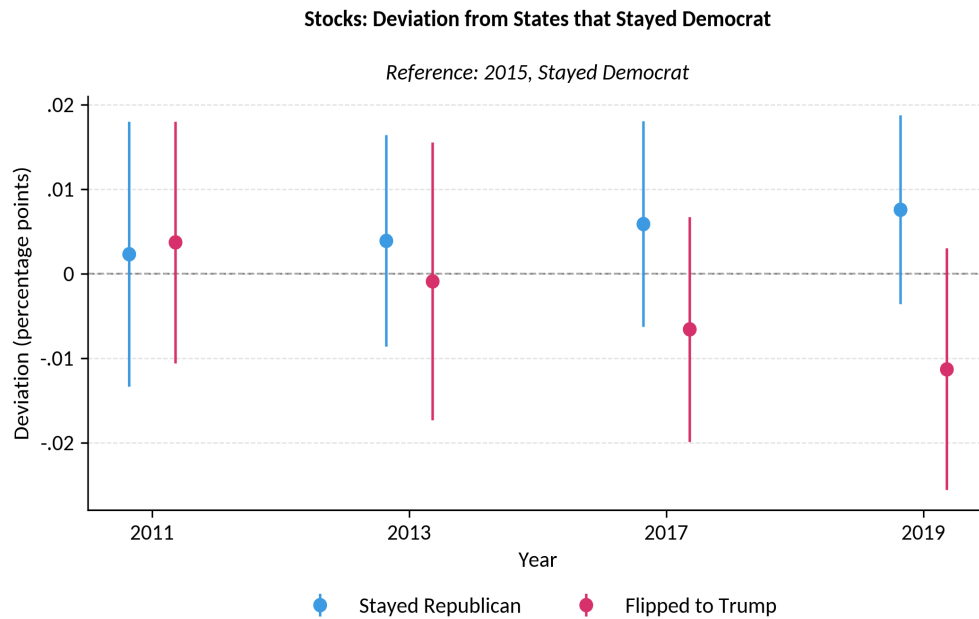


Figure 4.1. Event study, stocks share, deviation from Stayed Democrat, relative to 2015. Contemporaneous state of residence. Own calculations.

Stayed-Republican states are small and stable across all four waves, showing no clear movement from wave to wave. Flipped states are close to zero before 2016, then turn negative afterward. Stocks has the clearest visual pattern of the five shares, a flat pre-trend for both groups and a level shift confined to the flipped group after 2016. As this pattern suggests, households in states that flipped to Trump held a noticeably smaller share of their portfolio in stocks after the 2016 election than before, compared to households in states that stayed Democrat. This is examined formally in the following chapter.

Individual Retirement Accounts (IRA)

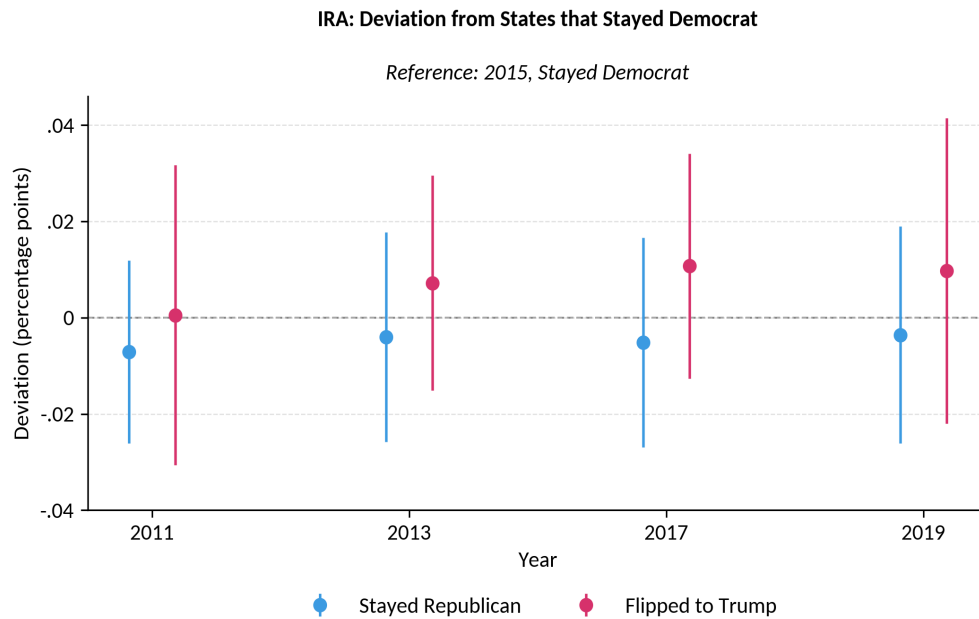


Figure 4.2. IRA share, deviation from Stayed-Democrat 2015 baseline, Contemporaneous state of residence, by year and state category.

Stayed-Republican states are flat and small throughout, showing no clear movement from wave to wave. Flipped states start near zero in 2011 and settle at a noticeably higher level from 2013 onward, a shift that happens before 2016 and then holds steady through 2017 and 2019, rather than moving again around the election. IRA allocation is clearly stratified by age and education in the pooled comparisons of section 4.4.2, but households do not appear to have changed how much they held in IRAs because of the 2016 election itself, for either group. This is examined formally in the following chapter.

Checking and Savings

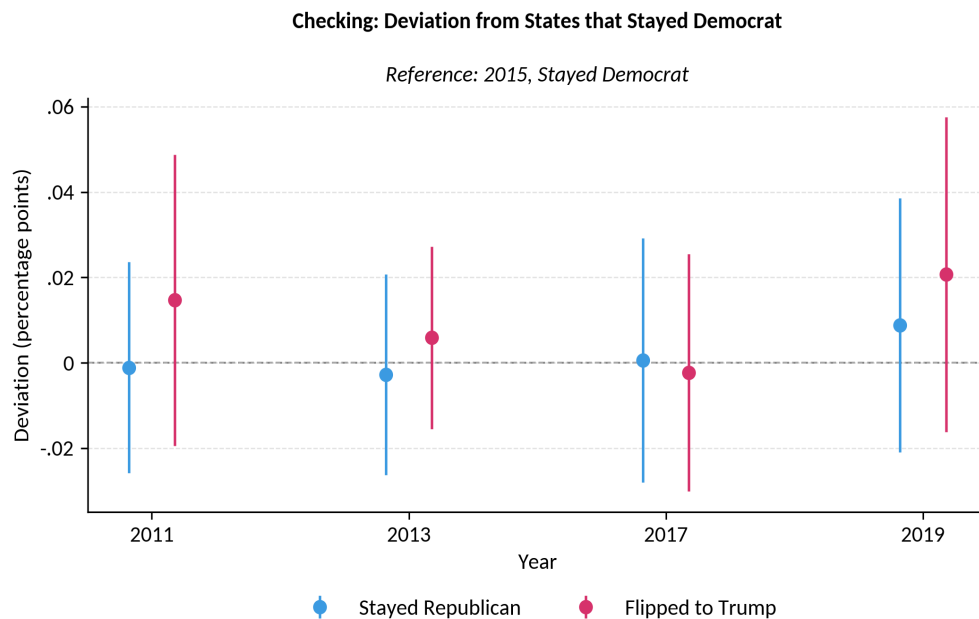


Figure 4.3. Checking share, deviation from Stayed-Democrat 2015 baseline, Contemporaneous state of residence, by year and state category.

Stayed-Republican states drift mildly upward after 2016, but the movement stays small. Flipped states swing more, starting relatively high in 2011, dropping through 2013 and 2017, before rising again in 2019, the largest single movement of the series, occurring after the election rather than at it. Because the largest movement falls between 2017 and 2019 rather than at 2016 itself, this series does not point to a clean, isolated shift tied to the election. This is examined formally in the following chapter.

Real Estate Other Than the Principal Residence

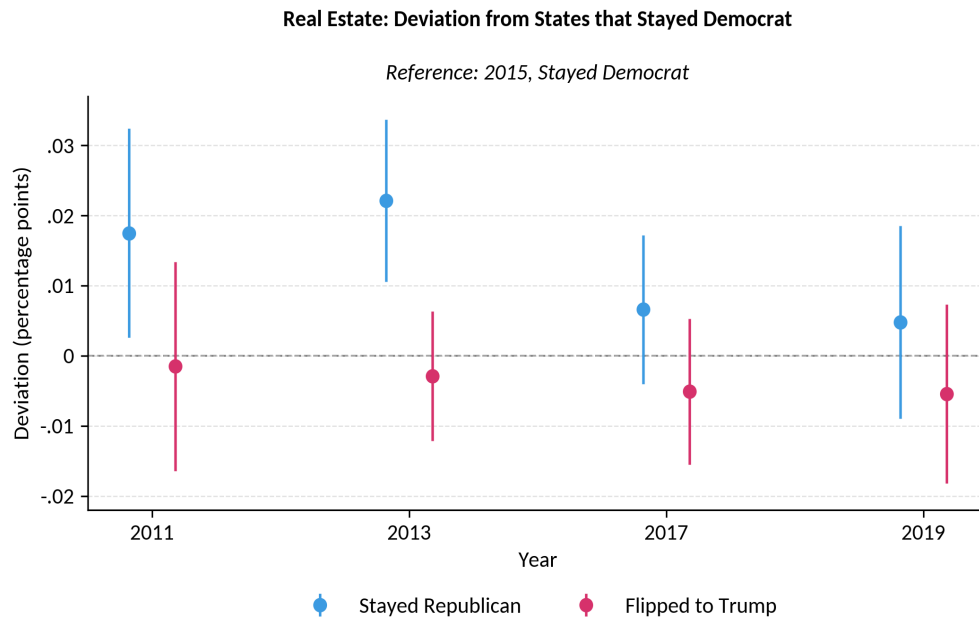


Figure 4.4. Real estate share, deviation from Stayed-Democrat 2015 baseline, Contemporaneous state of residence, by year and state category.

Flipped states stay small and flat across all four waves, showing little movement either before or after 2016. Stayed-Republican states tell a different story, they hold a clear premium relative to Stayed-Democrat states before 2016, which then fades away afterward. This shift concerns the Republican group rather than the flipped group this thesis is built around, but it is worth reporting explicitly rather than folding into a single flat description of real estate. For flipped states specifically, the flat pattern here is consistent with the lower real estate share identified in section 4.4.2 predating the election rather than resulting from it. This is examined formally in the following chapter.

Other Assets

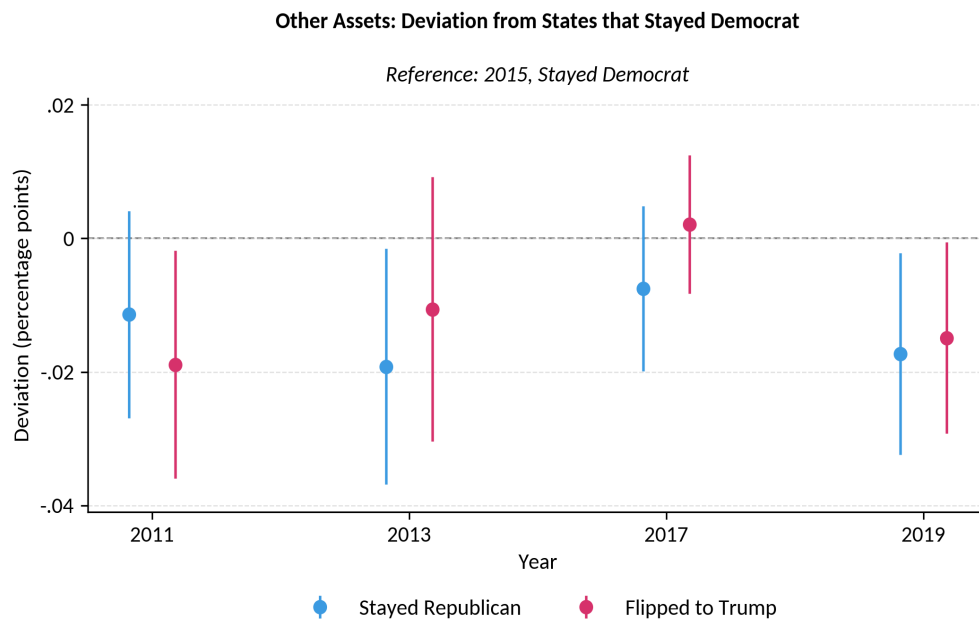


Figure 4.5. Other assets share, deviation from Stayed-Democrat 2015 baseline, Contemporaneous state of residence, by year and state category.

Other assets stands out from the other four shares by how much both series move around, without settling into a stable level before 2016 and shifting once at the election. Flipped states dip, rise, and dip again across the panel. Stayed-Republican states follow a similarly uneven path. Neither series holds still long enough to describe a clean before-and-after picture for this asset class, a trajectory that runs through the treatment date rather than breaking at it. This is examined formally in the following chapter, where other assets turns out to be the share with the least orderly pattern of the five.

5 Methodology

Building on the data described in the preceding section, this chapter presents the empirical strategy used to estimate the effect of the 2016 election on portfolio composition, details the regression specifications, and discusses the identifying assumptions required for a causal interpretation of the estimates.

5.1 Empirical strategy

This study cannot rely on a conventional difference-in-differences design, for a specific reason tied to the nature of the shock under study. A standard DiD design relies on a group of units untouched by the treatment to serve as a benchmark. The 2016 presidential election offers no such benchmark, since every state in the sample was exposed to the same national election on the same day, with the same winner subsequently governing the entire country. No investor in the sample can be classified as unexposed to the shock itself.

The identifying variation instead comes from the differential political trajectory of each state between the 2012 and 2016 elections. States are classified into three groups based on how their electoral outcome evolved over this period, using the `state_flip_cat` variable constructed in section 4.2.8. States that voted for the Democratic candidate in both elections are classified as Stayed Democrat, states that voted Republican in both elections as Stayed Republican, and the six states that voted Democrat in 2012 but Republican in 2016, namely Florida, Iowa, Michigan, Ohio, Pennsylvania and Wisconsin, as Flipped to Trump. The state of residence used for this classification is fixed at the 2015 wave, the last observation preceding the election. This choice avoids contaminating the treatment definition with post-election residential mobility, since a move triggered by the political or economic climate following the election would itself be a potential outcome of the shock rather than a pre-determined characteristic of the investor.

This positions the present study within the family of generalized difference-in-differences designs that exploit differential exposure intensity, a distinction already introduced in section 3.6 in contrast to Agarwal et al. (2022), whose staggered exposure to sequential gubernatorial elections provides genuine within-period control units that the present setting lacks. The coefficient of interest should accordingly be read as a differential effect across categories of political exposure, not as the effect of the election itself.

Stayed Democrat states serve as the reference category throughout, as the only group whose local political alignment did not change and that remains unaligned with the incoming administration. A complementary contrast between Flipped and Stayed Republican states, both aligned with the incoming administration but differing in whether that alignment is new, is examined directly in the results chapter and discussed further in the discussion chapter.

5.2 Regression specification

The baseline regression is estimated on the full unbalanced panel described in section 4.2, with observations from all five waves retained. The main estimating equation, applied separately to each of the five winsorized portfolio shares, is

$$\text{share}_{it}^k = \alpha_i + \lambda_t + \beta_1 (\text{Republican}_i \times \text{Post}_t) + \beta_2 (\text{Flipped}_i \times \text{Post}_t) + X'_{i,2015} \lambda_t + \varepsilon_{it}$$

where share_{it}^k is portfolio share k for individual i in year t . α_i denotes individual fixed effects, which absorb all time-invariant characteristics of the investor, including the political classification of their state of residence itself, their sex, and any other permanent trait. λ_t denotes year fixed effects, absorbing common shocks affecting all investors in a given wave, such as aggregate market movements. Post_t is a binary indicator equal to one for the 2017 and 2019 waves. The coefficients of interest, β_1 and β_2 , capture the average differential change in portfolio share after 2016 for investors residing in Stayed Republican and Flipped states respectively, relative to investors in Stayed Democrat states. Republican_i and Flipped_i do not appear as separate regressors, since both are absorbed by the individual fixed effects.

$X'_{i,2015}$ is a vector of six profile characteristics, namely age group, sex, education group, health group, race and ethnicity group, and household composition, measured at their 2015 value and interacted with year. Fixing these controls before the treatment avoids the risk that a contemporaneous control could itself be affected by the election, a concern raised during supervision. Age requires particular care in this respect, since a contemporaneous, linear age term would be exactly collinear with the individual and year fixed effects already in the model, age being by construction the difference between current year and year of birth. Using the fixed 2015 age group avoids this issue.

The five portfolio shares are treated symmetrically throughout, without designating any single share as the primary outcome, consistent with the equally weighted treatment of the five asset classes established in the data section. Because the five shares are estimated in five separate regressions rather than jointly, a Bonferroni correction is additionally reported in the results chapter to account for the possibility of a spurious finding arising from multiple testing across outcomes.

Standard errors are clustered at the state level throughout, using the 2015 state of residence. Given that treatment intensity varies at the state level, and that only six states fall into the Flipped category, section 5.3 discusses why this level of clustering also motivates a complementary inference procedure.

5.3 Identification assumptions

The causal interpretation of β_1 and β_2 rests on three assumptions, namely parallel trends, no anticipation, and the stable unit treatment value assumption, alongside the structural limitation already noted in section 5.1 regarding the absence of a genuinely unexposed group.

The central identifying assumption is that, absent the 2016 election, Flipped states would have followed the same trend in portfolio shares as Stayed Democrat states. This assumption cannot be tested directly, since the counterfactual trajectory of Flipped states is unobserved by definition, but it can be assessed empirically using pre-treatment data. An event study specification, estimated on the full 2011 to 2019 panel excluding 2015 as the reference year, tests whether the pre-treatment coefficients for 2011 and 2013 are jointly equal to zero. Across all five portfolio shares, this joint test finds no evidence against parallel pre-trends.

A second assumption is that the treatment did not affect outcomes before it occurred, that is, that portfolio behavior in Flipped states did not begin adjusting in anticipation of the 2016 result. This assumption is plausible in the present context since state-level electoral outcomes only become known on election night, leaving no meaningful window for anticipatory portfolio adjustment, and is further supported by the pre-treatment event study coefficients discussed above showing no evidence of divergence before 2016.

The stable unit treatment value assumption requires that the treatment status of one state does not affect outcomes in another. This assumption could be violated if investors relocated across state lines in response to the political or economic climate following the election, or if behavioral spillovers occurred between neighboring states with different political trajectories. The robustness check restricting the sample to non-movers, reported in section 6, addresses the first concern directly. Cross-state behavioral spillovers cannot be ruled out with the available data and are noted as a limitation in the discussion.

Finally, as discussed in section 5.1, the absence of a genuinely unexposed state means that β_1 and β_2 capture a differential effect across categories of political exposure, not the effect of the election against a true counterfactual of no election. This limitation cannot be resolved, only addressed empirically. Section 6 reports two robustness checks in this spirit. The first uses region-by-year fixed effects to rule out region-specific shocks unrelated to the political shock itself. The second replaces the three-group classification with a continuous measure of the 2016 Trump vote margin. If the estimated relationship is genuine, it should scale with the size of the margin, larger in states where the political shift was more pronounced. Finding the same pattern with this continuous measure would rule out the possibility that the three-group result merely reflects an arbitrary choice of thresholds.

With only six treated states, standard clustered standard errors become unreliable. This chapter therefore also reports results from a wild cluster bootstrap, following Cameron et al. (2008), alongside the conventional estimates.

6 Results

Building on the empirical strategy set out in Chapter 5, this chapter reports the estimated relationship between political exposure and portfolio composition. Results are organized around the question each section answers. Section 6.1 reports the main difference-in-differences estimates, first without and then with the 2015 profile controls. Section 6.2 reports a more direct contrast between Flipped and Stayed Republican states, motivated by the asymmetric relationship each of the three groups holds with the national election outcome. Section 6.3 asks whether the design itself can be trusted, reporting the formal test of pre-treatment parallel trends. Section 6.4 asks whether the reported p-values can be trusted, given the small number of treated states, and reports inference under the wild cluster bootstrap. Section 6.5 reports four additional robustness checks. Section 6.6 reports a correction for multiple testing across the five portfolio shares and a verification of the main result using the dollar value of stock holdings. Section 6.7 summarizes the findings.

Throughout this chapter, coefficients are reported in percentage points of portfolio share unless otherwise noted, and t statistics are reported in parentheses beneath each coefficient, following the notation introduced in Section 5.2.

6.1 Main estimates: three-group difference-in-differences

Table 6.1. Difference-in-differences, three groups, no controls

	Stocks	IRA	Real estate	Other assets	Checking
Stayed Republican x Post-2016	0.28 (0.74)	-0.17 (-0.24)	-0.58 (-1.30)	-0.28 (-0.54)	0.78 (0.80)
Flipped to Trump x Post-2016	-1.07 (-1.80)	0.77 (0.93)	-0.20 (-0.43)	0.24 (0.47)	0.18 (0.16)
Constant	6.84*** (67.20)	17.2*** (97.05)	8.52*** (77.77)	5.87*** (47.77)	61.6*** (268.49)
Observations	43,102	43,102	43,102	43,102	43,102
R-sq within	0.0004	0.0001	0.0001	0.0000	0.0000

*Coefficients in percentage points of portfolio share. t statistics in parentheses. Individual and year fixed effects absorbed. No controls. Standard errors clustered by 2015 state of residence. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.*

Table 6.1 reports the central specification of this study, following the equation set out in Section 5.2, estimated separately for each portfolio share. Two coefficients are of interest. Stayed Republican x Post-2016 captures the differential change in portfolio share, after 2016, for investors residing in states that remained Republican in both elections, relative to investors in states that remained Democrat. Flipped to Trump x Post-2016 captures the same differential change for investors residing in the six states that shifted from Democrat to Republican between 2012 and 2016.

Without controls, the coefficient on Flipped to Trump x Post-2016 is negative for stocks, at -1.07 percentage points ($t = -1.80$), and for real estate, at -0.20 percentage points ($t = -0.43$), and positive for IRA, other assets, and checking, ranging from 0.18 to 0.77 percentage points. None of the five coefficients reaches conventional significance at the 5 percent level, though the coefficient for stocks is the largest in magnitude and the closest to significance.

Table 6.2. Difference-in-differences, three groups, with 2015 controls

	Stocks	IRA	Real estate	Other assets	Checking
Stayed Republican x Post-2016	0.06 (0.15)	0.10 (0.13)	-0.42 (-0.96)	-0.00 (-0.00)	0.27 (0.29)
Flipped to Trump x Post-2016	-1.11 (-1.91)	0.93 (1.12)	-0.06 (-0.14)	0.21 (0.42)	-0.01 (-0.01)
Observations	42,441	42,441	42,441	42,441	42,441
R-sq within	0.0029	0.0046	0.0042	0.0033	0.0079

*Coefficients in percentage points of portfolio share. t statistics in parentheses. Individual and year fixed effects absorbed. Controls (age, education, health, race and ethnicity, household composition, fixed at 2015) interacted with year. Standard errors clustered by 2015 state of residence. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.*

Table 6.2 repeats this specification with the six profile controls, fixed at 2015 and interacted with year, added to the model. The coefficient on Flipped to Trump x Post-2016 for stocks strengthens slightly to -1.11 percentage points ($t = -1.91$), while the coefficients for the other four shares remain small and statistically indistinguishable from zero. The coefficient on Stayed Republican x Post-2016 is close to zero across all five shares once controls are included, none of them significant.

The within R-squared increases modestly once controls are added, from 0.04 percent to 0.29 percent for stocks, and similarly for the other four shares, consistent with the controls absorbing a small but non-negligible share of the residual variation. Taken together, these two specifications point to a single robust pattern. There is a negative, sizeable coefficient on stocks for the Flipped group, close to but not reaching conventional significance, and no consistent pattern across the remaining four shares.

6.2 A more direct contrast: Flipped versus Stayed Republican

As discussed in Section 5.1, the three groups differ not only in their local political trajectory but also in their relationship to the incoming administration. Stayed Democrat states experienced no local change but ended up unaligned with the winner of the 2016 election. Stayed Republican states experienced no local change either, but remained aligned with the winner throughout. Flipped states experienced a local change and became aligned with the winner as a result. Comparing Flipped states to Stayed Democrat states, as in Section 6.1, therefore mixes two things at once, the effect of the local political shift, and the effect of now being aligned with the party in power nationally.

Comparing Flipped states to Stayed Republican states isolates the first of these more directly. Both groups are aligned with the incoming administration after 2016, so the comparison holds that dimension roughly constant, leaving the recency of the local shift as the main difference between them. Table 6.3 reports this contrast, computed as the linear combination of Flipped to Trump x Post-2016 minus Stayed Republican x Post-2016.

Table 6.3. Direct contrast, Flipped to Trump minus Stayed Republican

Share	Diff., no controls	P-value	Diff., with controls	P-value
Stocks	-1.35	0.021*	-1.17	0.032*
IRA	0.94	0.234	0.83	0.329
Real estate	0.38	0.356	0.36	0.408
Other assets	0.52	0.326	0.21	0.673
Checking	-0.60	0.614	-0.28	0.811

*Coefficient = flip_post - rep_post (lincom), in percentage points of portfolio share. Individual and year fixed effects absorbed. Standard errors clustered by 2015 state of residence. * p<0.05.*

Without controls, the estimated difference for stocks is -1.35 percentage points, significant at the 5 percent level ($p = 0.021$). With controls, the estimate is -1.17 percentage points, also significant ($p = 0.032$). None of the other four shares reaches significance in either specification. This is the sharpest result in this chapter under conventional inference, larger in magnitude than the -1.07 and -1.11 percentage points reported against Stayed Democrat states in Section 6.1, consistent with the idea that holding national alignment constant makes the effect of the local political shift easier to detect.

6.3 Can the design be trusted: parallel trends

The credibility of the difference-in-differences design rests on the assumption that Flipped states would have followed the same trend in portfolio shares as Stayed Democrat states, absent the 2016 election. Table 6.4 reports the joint Wald test of the two pre-treatment event-study coefficients, for 2011 and 2013, relative to the 2015 reference year, described in Section 5.3.

Table 6.4. Joint test of pre-treatment coefficients (2011, 2013), relative to 2015

Share	F-statistic	P-value
Stocks	0.151	0.861
IRA	1.611	0.210
Real estate	0.088	0.916
Other assets	2.112	0.132
Checking	0.587	0.560

Joint Wald test that the Flipped-to-Trump interaction coefficients for 2011 and 2013 are equal to zero, relative to the 2015 reference year. A p-value above 0.05 supports the parallel trends assumption. Standard errors clustered by 2015 state of residence.

Across all five portfolio shares, this test finds no evidence that the pre-treatment trends of the Flipped and Stayed Democrat states differ from each other at the 5 percent level. P-values range from 0.13 for other assets to 0.92 for real estate. This does not reject the parallel trends assumption underlying the estimates reported in Section 6.1, though the margin is markedly thinner for other assets than for the other four shares. The full coefficient table and the corresponding event-study figures for all five shares are reported in Appendix A.

The coefficients behind this test are closely related to, but not identical to, the event-study figures reported in Section 4.4.3. Section 4.4.3 uses each individual's state of residence as observed in the current wave, appropriate for a descriptive account of the sample as it stands. The test above instead uses the state fixed at 2015, for the reasons set out in Section 5.1, to avoid contaminating the

treatment definition with post-election residential mobility. The two versions agree closely, including on every qualitative pattern described in Section 4.4.3, such as the comparatively unstable path of other assets, the one share for which neither the Flipped nor the Stayed Republican series settled at a stable level before drifting further after 2016. This is consistent with the thinner margin found for other assets in the formal test above.

The same broad pattern is visible for stocks in Figure 4.1, in Section 4.4.3. Before 2016, in 2011 and 2013, both the Flipped and Stayed Republican series sit close to zero relative to Stayed Democrat states, consistent with the formal joint test reported above. After 2016, the Stayed Republican series remains close to zero, while the Flipped series moves below zero in 2017 and stays below zero in 2019, mirroring the negative coefficient on Flipped to Trump x Post-2016 reported in Section 6.1.

6.4 Can the p-values be trusted: wild cluster bootstrap

Only six states fall into the Flipped category, which limits the reliability of standard cluster-robust inference at the state level. Following Cameron et al. (2008), this section reports a wild cluster bootstrap with Webb weights and 9,999 replications, alongside the conventional cluster-robust estimates, for both the three-group comparison and the direct Flipped versus Stayed Republican contrast.

Table 6.5 reports the bootstrap results for the three-group specification without controls. For stocks, the bootstrap p-value is 0.134, compared to 0.078 under conventional inference. Table 6.6 reports the same specification with controls. The bootstrap p-value for stocks is 0.105, compared to 0.061 under conventional inference. In both cases, the bootstrap p-value is higher than the conventional p-value, and neither reaches the 5 percent threshold, though both remain below 15 percent.

Table 6.5. Wild cluster bootstrap, Flipped to Trump x Post-2016, no controls

Share	Coefficient	P classical	P bootstrap
Stocks	-1.07	0.078	0.134
IRA	0.77	0.357	0.409
Real estate	-0.20	0.670	0.682
Other assets	0.24	0.642	0.693
Checking	0.18	0.870	0.883

Coefficient on Flipped to Trump x Post-2016, in percentage points. Individual and year fixed effects absorbed. No controls. Wild cluster bootstrap: 9,999 replications, Webb weights, clustered by 2015 state of residence.

Table 6.6. Wild cluster bootstrap, Flipped to Trump x Post-2016, with 2015 controls

Share	Coefficient	P classical	P bootstrap
Stocks	-1.11	0.061	0.105
IRA	0.93	0.269	0.323
Real estate	-0.06	0.891	0.890
Other assets	0.21	0.679	0.729
Checking	-0.01	0.995	0.994

Coefficient on Flipped to Trump x Post-2016, in percentage points. Individual and year fixed effects absorbed. Controls (age, education, health, race and ethnicity, household composition, fixed at 2015) interacted with year. Wild cluster bootstrap: 9,999 replications, Webb weights, clustered by 2015 state of residence.

Table 6.7 reports the bootstrap results for the direct Flipped versus Stayed Republican contrast. Without controls, the bootstrap p-value for stocks is 0.061, compared to 0.021 under conventional inference. With controls, the bootstrap p-value is 0.068, compared to 0.032. The result that reached conventional significance in Section 6.2 falls just short of the 5 percent threshold under the bootstrap, while remaining below the 10 percent threshold in both specifications.

Table 6.7. Wild cluster bootstrap, direct contrast Flipped minus Stayed Republican

Share	Diff., no controls	P bootstrap	Diff., with controls	P bootstrap
Stocks	-1.35	0.061	-1.17	0.068
IRA	0.94	0.288	0.83	0.370
Real estate	0.38	0.368	0.36	0.408
Other assets	0.52	0.370	0.21	0.694
Checking	-0.60	0.642	-0.28	0.825

Coefficient = flip_post - rep_post (lincom), in percentage points. Wild cluster bootstrap: 9,999 replications, Webb weights, clustered by 2015 state of residence.

Figure 6.2 illustrates this result graphically for the direct Flipped versus Stayed Republican contrast on stocks, without controls. The horizontal axis represents every value the true coefficient could plausibly take, not only the value actually estimated. The vertical axis shows the p-value that would result if each of these values were the true one. The curve peaks at 1 exactly at the estimated coefficient, -1.35 percentage points, since testing the estimate against itself trivially yields the highest possible p-value, and declines as the tested value moves further away in either direction. The dashed horizontal line marks the conventional 5 percent threshold, and the two points where the curve crosses it define the 95 percent confidence interval. Zero lies just inside this interval, barely, consistent with the bootstrap p-value of 0.061, just above the 5 percent threshold, reported in Table 6.7.

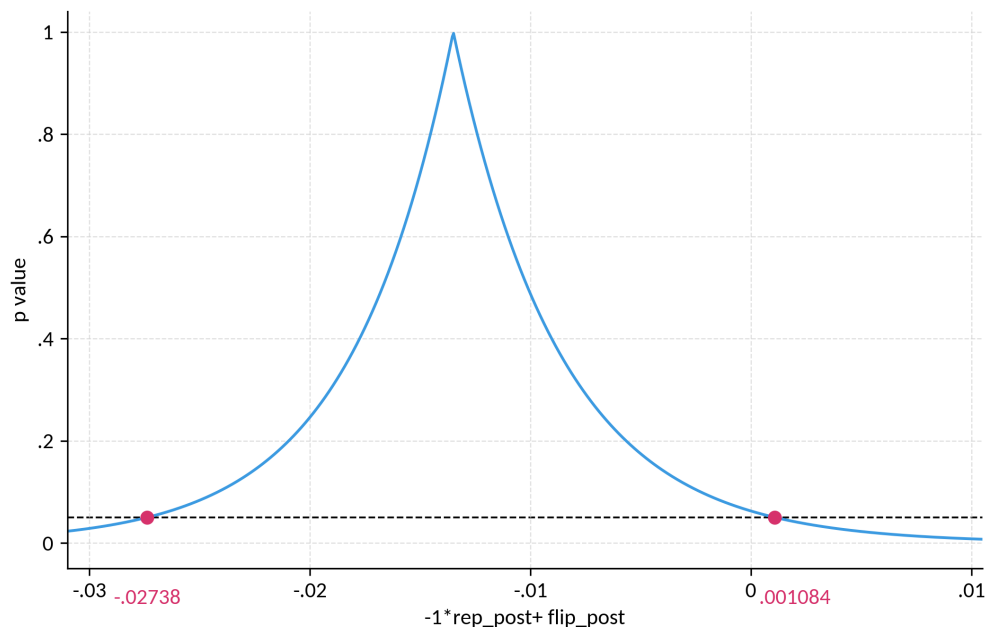


Figure 6.2. Wild cluster bootstrap p-value curve, stocks, Flipped vs Stayed Republican, no controls. Own calculations.

Across every specification, the bootstrap p-value is higher than the conventional one. The ranking of results is unchanged, stocks remains the sharpest contrast throughout. This confirms the concern raised in Section 5.3. With only six treated states, conventional inference may be too optimistic. For

the direct contrast without controls, the p-value moves from 0.021 under conventional inference to 0.061 under the bootstrap, no longer below the 5 percent threshold.

6.5 Robustness checks

Table 6.8 reports four additional checks on the three-group specification, all reporting the coefficient on Flipped to Trump for each portfolio share.

The first check restricts the sample to individuals whose state of residence never changed. The coefficient for stocks stays close to its baseline value, at -1.25 percentage points.

The second check restricts the comparison to the 2015 and 2017 waves only, instead of pooling 2017 and 2019. The coefficient for stocks is -0.71 percentage points.

The third check tests whether the effect is stable over time, by comparing the 2017 and 2019 coefficients directly. For stocks, the two years do not differ significantly ($p = 0.769$), and the same holds for IRA and real estate. Checking differs at the 10 percent level ($p = 0.079$) but not at the conventional 5 percent level. Other assets is the clear exception, where the effect changes significantly between 2017 and 2019 ($p = 0.001$), suggesting its effect is not stable over the post-treatment period.

The fourth check reweights the sample using the PSID longitudinal individual weight. The coefficient for stocks falls to -0.15 percentage points, smaller than the unweighted estimate.

Table 6.8. Robustness checks on the three-group specification

Share	Non-mover	2015 to 2017	Persistence (p)	Weighted
Stocks	-1.25	-0.71	0.769	-0.15
IRA	0.60	1.01	0.970	0.40
Real estate	0.01	-0.58	0.927	-0.01
Other assets	0.44	0.99	0.001**	0.64
Checking	0.13	-0.80	0.079	-1.02

*Columns 1, 2, and 4 report the coefficient on Flipped to Trump, in percentage points. Non-mover: sample restricted to individuals whose state of residence never changed. 2015 to 2017: coefficient from a first-difference regression using only the 2015 and 2017 waves. Persistence: p-value of the test that the 2017 and 2019 coefficients are equal. Weighted: reweighted using the PSID longitudinal individual weight. Standard errors clustered by 2015 state of residence. ** $p < 0.01$.*

Table 6.9 reports two further checks, addressing the limitation discussed in Section 5.3 that no state in the sample is genuinely unexposed to the election.

The first check replaces the simple year fixed effects with region-by-year fixed effects, based on the four Census regions, Northeast, Midwest, South, and West. This absorbs any economic shock common to all states within the same region in a given year, rather than to the country as a whole. It should remove any regional shock that happens to coincide with the political shock, rather than being caused by it. Under this specification, the coefficient for stocks falls to -0.72 percentage points ($p = 0.171$), no longer close to conventional significance.

The second check replaces the three-group classification with a continuous measure of the 2016 Trump vote margin, the exact size of Trump's win or loss in each state, rather than a simple category. If the stocks effect reflects a real, graded response to political realignment, it should be stronger in

states where Trump won by more, and weaker where the margin was thin. It is not. The coefficient is 0.00007 per percentage point of margin ($p = 0.471$), close to zero, and points in the opposite direction from the main result.

Neither this check nor the previous one reproduces the pattern found in Section 6.1 for stocks. Chapter 7 returns to what this means for how the main result should be read.

Table 6.9. Region-by-year fixed effects and continuous political exposure

Share	Flip_post (region FE)	P-value	Margin_post (continuous)	P-value
Stocks	-0.72	0.171	0.00007	0.471
IRA	0.54	0.548	-0.00003	0.879
Real estate	-0.64	0.323	-0.00005	0.679
Other assets	-0.12	0.854	0.00004	0.723
Checking	0.85	0.406	-0.00005	0.829

Column 1-2: coefficient on Flipped to Trump x Post-2016, individual and region-by-year fixed effects, in percentage points. Column 3-4: coefficient on the continuous 2016 Trump vote margin (state fixed at 2015) interacted with Post-2016, per percentage point of margin, individual and year fixed effects. Standard errors clustered by 2015 state of residence.

6.6 Multiple testing and verification in levels

Because the five portfolio shares are estimated in five separate regressions, testing five hypotheses at once raises the risk that a significant result appears by chance alone. This concern applies specifically to the direct Flipped versus Stayed Republican contrast in Section 6.2, the only specification in this chapter where a coefficient, for stocks, reaches conventional significance. Table 6.10a reports a Bonferroni correction applied to the five p-values from that contrast. Multiplying the raw p-value for stocks, 0.021, by five yields a corrected p-value of 0.106, above the conventional 5 percent threshold. A Benjamini-Hochberg procedure, which ranks the five p-values and compares each to a threshold proportional to its rank, reaches the same conclusion, since the smallest p-value, for stocks, does not fall below its corresponding rank-one threshold of 0.01.

Table 6.10a. Bonferroni correction, direct contrast Flipped vs Stayed Republican, no controls

Share	Raw p-value	Bonferroni-corrected
Stocks	0.021	0.106
IRA	0.234	1.000
Real estate	0.356	1.000
Other assets	0.326	1.000
Checking	0.614	1.000

Bonferroni-corrected p-value = raw p-value x 5, capped at 1. A Benjamini-Hochberg procedure at the 5 percent false discovery rate reaches the same conclusion.

Table 6.10b offers one further check on the stocks result, this time using the dollar value of stock holdings instead of the portfolio share.

The concern this addresses is mechanical. A falling stocks share does not necessarily mean investors sold stocks. It could equally arise if the value of other assets, such as real estate, rose while stock

holdings stayed flat, mechanically pulling the share down without any real change in stock investment. Measuring stock holdings directly in dollars sidesteps this concern entirely.

Dollar values are transformed using the inverse hyperbolic sine rather than a standard logarithm, since many households hold no stocks at all and a logarithm is undefined at zero. For large values, the two transformations are nearly identical.

On this dollar-based measure, the direct Flipped versus Stayed Republican contrast is -0.21, still significant at the 5 percent level ($p = 0.025$). The result therefore holds up when measured in dollars, not only as a share, which rules out the mechanical explanation above.

Table 6.10b. Verification in levels, dollar value of stock holdings

Diff. (asinh of stock value)	P-value
-0.21	0.025*

*asinh(x) = log(x + sqrt(x^2+1)), equivalent to a logarithm for large values but defined at zero. Coefficient = flip_post - rep_post (lincom) on the asinh-transformed dollar value of stock holdings. Individual and year fixed effects absorbed. Standard errors clustered by 2015 state of residence. * $p < 0.05$.*

6.7 Summary

Across every specification considered in this chapter, the stocks share is the only outcome for which the estimated effect of political realignment approaches conventional statistical significance, and it does so consistently, with a negative sign of roughly one percentage point, across the three-group comparison, the direct Flipped versus Stayed Republican contrast, the wild cluster bootstrap, the non-mover subsample, the 2015 to 2017 comparison, and the dollar-value verification.

The direct contrast with Stayed Republican states, under conventional inference, is the only result in this chapter that reaches the 5 percent significance threshold, at $p = 0.021$ without controls and $p = 0.032$ with controls, and it remains significant when the outcome is measured in dollars rather than as a share ($p = 0.025$). This result does not survive a Bonferroni correction for testing five outcomes, and falls just short of the 5 percent threshold under the wild cluster bootstrap, remaining below the 10 percent threshold throughout. It also weakens considerably under region-by-year fixed effects and does not hold when political exposure is measured as a continuous margin rather than a categorical classification, two points taken up in the discussion chapter.

The remaining four portfolio shares, IRA, real estate, other assets, and checking, show no consistent pattern across specifications, with coefficients that vary in sign and remain far from conventional significance throughout. One exception is noted for other assets, where the effect of political realignment differs significantly between the 2017 and 2019 waves ($p = 0.001$), suggesting a shift in the effect's magnitude over the post-treatment period rather than a stable pattern.

7 Discussion

7.1 Interpreting the main finding

The stocks result is not a single number that happened to cross a threshold. It is a pattern that keeps reappearing under conditions that had no particular reason to agree with each other. A measurement choice, dollars instead of shares, does not erase it. Removing people who moved states does not erase it. Narrowing the comparison to the two waves immediately around the election does not erase it. Each of these checks could, in principle, have pulled the result apart. None of them did.

This consistency, not the fact that one version crosses the 5 percent threshold, is what makes the finding worth taking seriously. A result that only shows up under one specific setup is easy to dismiss, it might just be an artifact of that particular choice. This one does not depend on any single choice. Change the measurement, change the sample, change the time window, the same story comes back each time.

At the same time, this result should not be overstated. As Section 6.7 detailed, it does not survive a Bonferroni correction, and it weakens or disappears under two further checks, taken up in the next section, that speak directly to how much weight the finding can bear. What limits it is not consistency but power. With only six treated states, no test available here can deliver full statistical certainty. The finding is credible, without being definitively proven.

The remaining four portfolio shares, IRA, real estate, other assets, and checking, offer no comparable evidence of a political effect, with one exception. The effect on other assets differs significantly between 2017 and 2019, suggesting that whatever is happening in this asset class is not a stable post-election shift but something that changes shape over the following waves. Without a clear economic story to explain this instability, the safest interpretation is that this single result reflects noise in a highly heterogeneous asset category, rather than a genuine second channel of political influence on portfolios.

7.2 Why stocks, and why the effect is fragile

A negative effect concentrated in stocks, rather than spread evenly across asset classes, is consistent with the equity-risk channel documented by Agarwal et al. (2022), in which political sentiment shapes risk taking primarily through the perceived riskiness of equity markets. Housing and liquid savings plausibly respond more slowly, since both are more costly to adjust, a household that becomes more pessimistic has a much easier time trimming a stock portfolio than selling a home or closing a checking account, though this specific comparison is not directly established in the literature reviewed in Chapter 3.

The fragility of the result, however, deserves equal attention. Three features of the data plausibly limit statistical power in this setting. First, the treatment is defined at the state level, but only six states fall into the Flipped category, which is the source of the inference problems documented throughout Chapter 6. Second, the outcome itself, the stocks share of gross assets, is a component of a five part portfolio, so a change in one component partly reflects changes in the others, adding measurement noise that a strict share based test cannot fully absorb, an issue explored in Section 6.6. Third, and more fundamentally, the design compares differentially exposed states rather than a treated group against a genuinely untreated one, a limitation already discussed in Section 5.1 and revisited in Section 7.4.

Two checks complicate a causal reading of this result more than any other.

The first concerns the size of the political shift, not just its direction. If the stocks effect reflected a genuine, graded response to political realignment, it should grow larger in states where Trump won by a wider margin. It does not. Replacing the three-group classification with a continuous measure of the vote margin drives the coefficient close to zero. This is hard to reconcile with a real dose-response relationship between political exposure and portfolio behavior, and instead suggests the categorical result may be picking up something specific to the six Flipped states as a group, rather than a general pattern that scales with political intensity.

The second concerns geography. The region-by-year fixed effects check in Section 6.5 weakens the stocks coefficient more than any other check in this chapter, the clearest sign among all the robustness checks that a regional shock may be contributing to the pattern found in the main specification.

7.3 Relation to the existing literature

The two gaps this thesis addresses were set out in Section 3.6. What follows compares the estimates against the two studies that motivated them.

On scope, the results speak directly to what Meeuwis et al. (2022) could not observe. Their equity-only data cannot say whether households reallocated across asset classes or simply adjusted their stock holdings. Tracking five components of wealth here shows it is the latter, only stocks move, the other four do not.

On identification, the modest and fragile effect estimated here is not inconsistent with the broader literature. Meeuwis et al. (2022) find sharper effects, but their identification comes from self-reported political affiliation, a more direct measure than the state-level exposure used here. Agarwal et al. (2022) find effects of comparable magnitude in a design built around staggered elections, a source of identification unavailable for a single national race. Read against both, the effect estimated here fits as a lower-bound version of the same underlying phenomenon, identified through geographic exposure rather than self-reported partisanship. It is detectable, but harder to pin down precisely once the comparison relies on where an investor lives rather than what they say about their own political views.

7.4 Limitations

Four limitations, already flagged at various points in this thesis, are worth restating together here.

The most fundamental is the absence of a genuinely unexposed comparison group, discussed in Section 5.1. Every coefficient reported in Chapter 6 should be read as a differential effect across categories of political exposure, not as the effect of the 2016 election relative to a counterfactual in which no election occurred. This is an unavoidable feature of studying a uniform national shock, not a flaw specific to this design, but it limits how far the results can be generalized beyond the specific comparison being made.

Related to this is the asymmetric relationship each of the three state groups holds with the incoming administration, discussed in Sections 5.1 and 6.2. None of the three groups serves as a neutral benchmark in the way a randomized control group would. The practical consequence shows up directly in the results. The contrast against Stayed Republican states, which holds national alignment roughly constant, is consistently sharper than the contrast against Stayed Democrat states, which does

not. This gap between the two contrasts is itself evidence that the choice of reference group matters, not just a robustness formality.

A third limitation concerns the stable unit treatment value assumption, discussed in Section 5.3. This assumption has two parts, and only one can be tested with the data available. The first part is migration. If people moved to a different state because of the political climate, the comparison between states would be distorted. The non-mover check in Section 6.5 tests this directly, and finds a similar result once movers are excluded. The second part is spillovers between neighboring states. Investors in a Stayed Democrat state next to a Flipped state might behave differently than investors further away, simply through proximity. The PSID contains no data on cross-state information flows or social networks, so this possibility cannot be tested, and remains an open limitation.

Finally, the sample itself carries the limitations already documented in Chapter 4, including the exclusion of households with near-zero gross assets, the winsorization of extreme portfolio shares, and the restriction to individuals observed both before and after 2016, which introduces an unbalanced panel structure. Though there is no specific reason to expect these choices to drive the results reported here, they are inherent to any study built on a long-running household panel survey, and their combined effect on the estimates has not been separately tested.

7.5 Contribution

Despite these limitations, this thesis contributes a geographically identified estimate of how the 2016 election affected portfolio composition, a design not previously applied to this specific question in the literature reviewed in Chapter 3.

This sensitivity, concentrated specifically in stocks and consistent with the equity-risk channel discussed in Section 7.2, is the empirical core of that contribution.

The fragility of this result is itself part of the contribution. Reporting it honestly, rather than presenting only the most favorable specification, makes clear how far evidence like this can be trusted. It also points to where future research is needed. A design with access to a genuine control group, such as staggered subnational elections, would be better positioned to isolate the effect with more precision.

8 Conclusion

Section 3.6 identified two gaps in the existing literature. Meeuwis et al. (2022), the only household-level evidence on the 2016 election itself, cover equity holdings alone. They cannot say whether households reallocated across asset classes, only whether they changed their stock holdings. Agarwal et al. (2022), the one available quasi-experimental household design, rely on staggered elections to build a geographic control group. A single national race like 2016 offers no such group. This thesis was built to close both gaps at once. It tracks five portfolio components rather than one, using five biennial PSID waves, and it replaces the missing staggered design with differential state-level political exposure.

The results speak most clearly to the first gap. Household portfolios did not adjust uniformly after 2016. Of the five components examined, stocks, IRA, real estate, other assets, and checking and savings, only the stocks share moved in step with a state's political trajectory. It fell by roughly one to one and a half percentage points more in states that flipped to Trump than in comparison states. This is exactly the question the equity-only data used by Meeuwis et al. (2022) could not answer, namely whether households reallocated across asset classes or simply adjusted one. Here, they adjusted one, and only one.

This regression finding does not stand alone. It largely confirms the pattern already visible in the descriptive statistics in Section 4.4. There, stocks showed the clearest preliminary sign of a state-specific shift after 2016, IRA showed a stable gap between Republican states and the other two groups, present well before 2016 and unchanged after, and checking showed a broad-based rise in liquidity unrelated to state political trajectory. The regression results match all three of these preliminary readings closely.

Two shares break from this pattern. The break is informative, not a weakness.

Real estate is the first. Section 4.4 showed a persistent gap before 2016, but for Stayed Republican states, not the Flipped group this thesis is built around. Flipped states were already flat, both before and after 2016. The formal parallel-trends test in Section 6.3, which specifically checks the Flipped group, finds no divergence, consistent with this already-flat descriptive pattern. The pre-existing gap visible in Section 4.4 belongs to a different comparison entirely, and does not threaten the identification strategy used here.

Other assets is the second, and it runs the other way. Section 4.4 described it as the least politically differentiated of the five shares, with no state-level pattern to speak of. Yet it is the only share where the regression detects a significant change in the size of the effect between 2017 and 2019. Chapter 7 treats this result with caution, since no clear economic mechanism explains it. Together, these two shares make the same point from opposite sides. Descriptive statistics and regression do not always agree, and when they disagree, the regression is the one built to control for composition, not the raw comparison.

The second gap comes at a cost. The stocks effect is consistent across every check reported in Chapter 6, and fragile under exactly the checks that speak to the missing control group, discussed at length in Chapter 7. This is not a flaw in execution. It is the direct cost of estimating a state-level effect without a state-level control group, the exact constraint that led to the exposure-based design in the first place.

This thesis does not settle the question of whether the 2016 election moved American household portfolios. What it shows, carefully and honestly, is that the question deserves to be asked, and that the answer, at least for now, is neither a clean yes nor a clean no. Investors in states that flipped toward Trump pulled back from stocks by a small but consistent margin, a pattern that survives multiple ways of measuring it and disappears under others. That mixed verdict is not a failure of the analysis. It is what an honest answer looks like when the question is this hard to isolate, and it is offered here as a starting point for the research that can eventually give a sharper one.

9 Appendices

This appendix reports supplementary material referenced in Chapters 4 and 6. Appendix A reports the full coefficient table and the corresponding event-study figures underlying the joint pre-trend test summarized in Table 6.4, using the state fixed at 2015. Appendix B reports the pooled descriptive tables underlying the discussion in Section 4.4, for every profile dimension cited with specific figures across the five portfolio shares.

Appendix A. Full event-study coefficients and figures underlying Table 6.4

	Stocks	IRA	Real estate	Other assets	Checking
2011 x Stayed Republican	0.0063 (0.0082)	-0.0075 (0.0098)	0.0180*** (0.0064)	-0.0110 (0.0076)	-0.0053 (0.0118)
2011 x Flipped to Trump	0.0041 (0.0076)	-0.0040 (0.0156)	-0.0028 (0.0076)	-0.0171** (0.0084)	0.0192 (0.0177)
2013 x Stayed Republican	0.0037 (0.0063)	-0.0036 (0.0111)	0.0220*** (0.0062)	-0.0183** (0.0090)	-0.0037 (0.0127)
2013 x Flipped to Trump	-0.0011 (0.0087)	0.0127 (0.0099)	-0.0009 (0.0044)	-0.0092 (0.0100)	-0.0027 (0.0095)
2017 x Stayed Republican	0.0051 (0.0059)	-0.0050 (0.0105)	0.0071 (0.0054)	-0.0080 (0.0064)	0.0013 (0.0143)
2017 x Flipped to Trump	-0.0087 (0.0067)	0.0105 (0.0110)	-0.0030 (0.0053)	0.0026 (0.0052)	-0.0027 (0.0132)
2019 x Stayed Republican	0.0068 (0.0056)	-0.0054 (0.0116)	0.0065 (0.0070)	-0.0168** (0.0077)	0.0092 (0.0156)
2019 x Flipped to Trump	-0.0112 (0.0086)	0.0109 (0.0168)	-0.0032 (0.0051)	-0.0150** (0.0071)	0.0172 (0.0180)
N	43,102	43,102	43,102	43,102	43,102

*Coefficients in percentage points of portfolio share, standard errors in parentheses. Individual and year fixed effects controlled for, year included via explicit interaction, equivalent to absorption by the Frisch-Waugh-Lovell theorem. Standard errors clustered by 2015 state of residence. The reference category is 2015, Stayed Democrat. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.*

Table A.1 reports the complete set of year-by-group event-study coefficients, using the state fixed at 2015, underlying the joint pre-trend test summarized in Table 6.4. It is distinct from Table 4.5, which uses each individual's contemporaneous state of residence, for the reasons set out in Section 6.3.

Figures A.1 to A.5 plot the coefficients reported in Table A.1, one per portfolio share. They are visually similar to Figures 4.1 to 4.5 in Section 4.4.3, since the underlying pattern is broadly the same whether the state is measured contemporaneously or fixed at 2015, but they are not identical, and are reported here for completeness rather than in the main text to avoid duplicating near-identical figures.

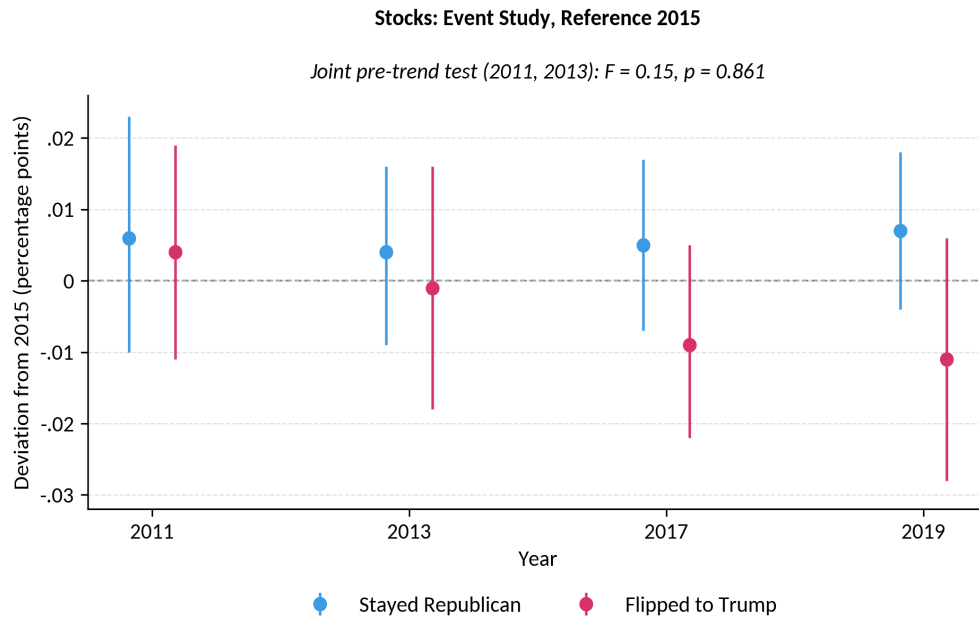


Figure A.1. Event study, stocks share, relative to 2015, state fixed at 2015. Own calculations.

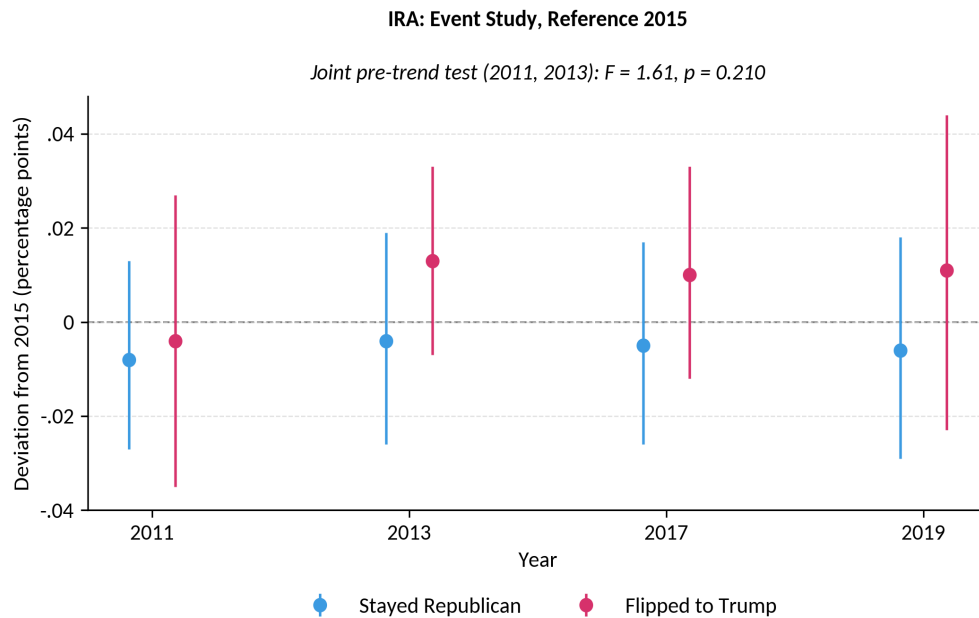


Figure A.2. Event study, IRA share, relative to 2015, state fixed at 2015. Own calculations.

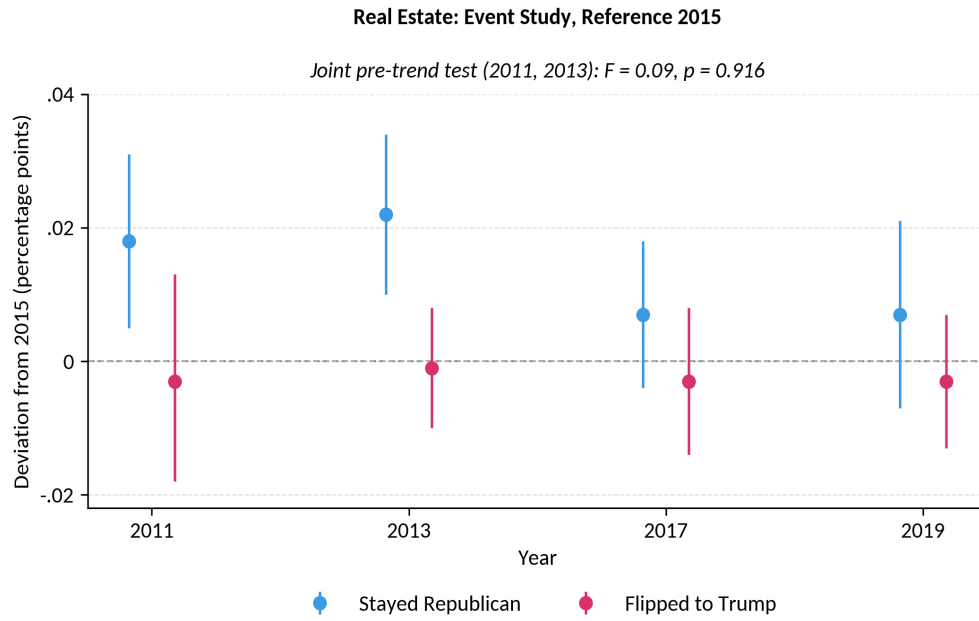


Figure A.3. Event study, real estate share, relative to 2015, state fixed at 2015. Own calculations.

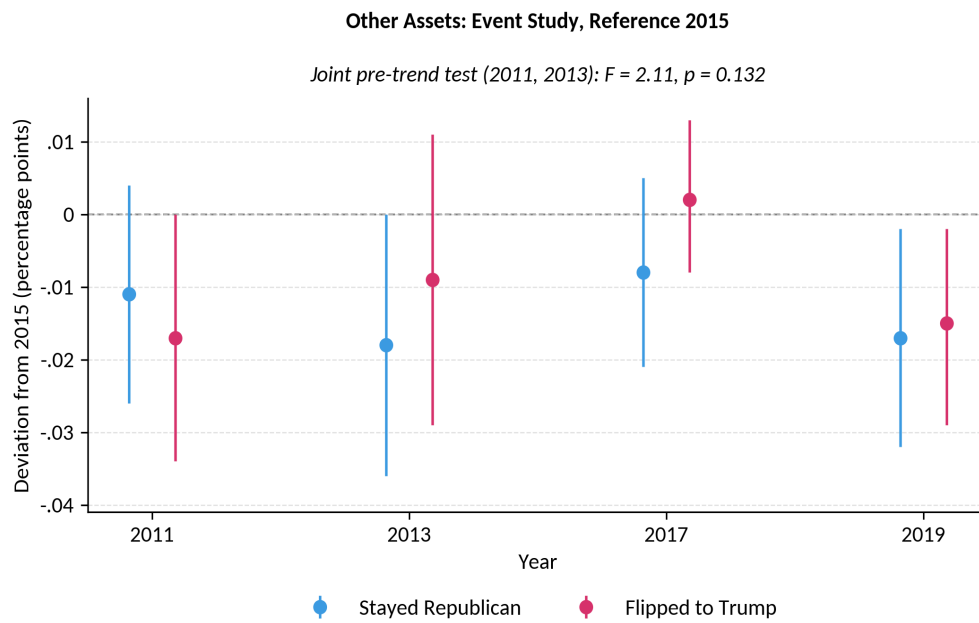


Figure A.4. Event study, other assets share, relative to 2015, state fixed at 2015. Own calculations.

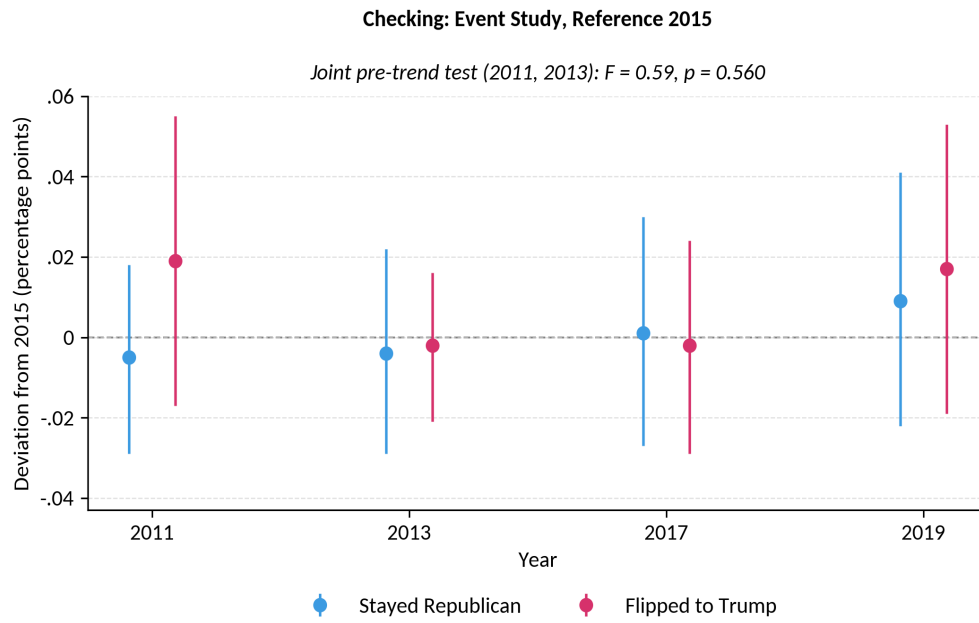


Figure A.5. Event study, checking share, relative to 2015, state fixed at 2015. Own calculations.

Appendix B. Descriptive statistics, by profile dimension

Table B.1 report the pooled 2011-2019 mean stocks share for each of the seven profile dimensions introduced in Table 4.3, underlying the figures cited in Section 4.4.

Table B.1. Stocks share by profile dimension, 2011-2019 pooled

Dimension	Category	Mean	N
Age group	15-29	0.041	5,705
	30-44	0.049	15,487
	45-59	0.060	12,135
	60-74	0.102	8,933
	75-89	0.146	2,119
	90-104	0.135	161
Sex	Male	0.069	20,506
	Female	0.064	24,034
Education level	<12 (no HS)	0.014	1,995
	12 (HS)	0.040	12,018
	13-15 (college)	0.043	12,412
	16-17 (bachelor+)	0.108	17,354
Health status	Excellent/Very good	0.076	24,682
	Good	0.057	13,798
	Fair/Poor	0.047	5,795
Race and ethnicity	White	0.087	29,119
	Black	0.021	10,363
	Hispanic	0.026	3,305
	Other	0.080	1,294
State political category	Stayed Democrat	0.080	16,733
	Stayed Republican	0.052	18,255
	Flipped to Trump	0.069	9,276
Household composition	Single head	0.049	10,578
	Couple	0.072	33,962

Note. Stocks share is winsorized at the 1st and 99th percentiles. For the state political category dimension, state of residence is measured contemporaneously in each wave, consistent with the descriptive account in Section 4.4, unlike the fixed 2015 state used in the main regressions. Own calculations.

Table B.2 report the pooled 2011-2019 mean IRA share for each of the seven profile dimensions introduced in Table 4.3, underlying the figures cited in Section 4.4.

Table B.2. IRA share by profile dimension, 2011-2019 pooled

Dimension	Category	Mean	N
Age group	15-29	0.064	5,705
	30-44	0.136	15,487
	45-59	0.193	12,135
	60-74	0.259	8,933
	75-89	0.157	2,119
	90-104	0.068	161
Sex	Male	0.173	20,506
	Female	0.163	24,034
Education level	<12 (no HS)	0.054	1,995
	12 (HS)	0.118	12,018
	13-15 (college)	0.139	12,412
	16-17 (bachelor+)	0.237	17,354
Health status	Excellent/Very good	0.187	24,682
	Good	0.155	13,798
	Fair/Poor	0.114	5,795
Race and ethnicity	White	0.208	29,119
	Black	0.087	10,363
	Hispanic	0.072	3,305
	Other	0.168	1,294
State political category	Stayed Democrat	0.185	16,733
	Stayed Republican	0.143	18,255
	Flipped to Trump	0.186	9,276
Household composition	Single head	0.107	10,578
	Couple	0.187	33,962

Note. IRA share is winsorized at the 1st and 99th percentiles. For the state political category dimension, state of residence is measured contemporaneously in each wave, consistent with the descriptive account in Section 4.4, unlike the fixed 2015 state used in the main regressions. Own calculations.

Table B.3 report the pooled 2011-2019 mean Checking share for each of the seven profile dimensions introduced in Table 4.3, underlying the figures cited in Section 4.4.

Table B.3. Checking share by profile dimension, 2011-2019 pooled

Dimension	Category	Mean	N
Age group	15-29	0.815	5,705
	30-44	0.695	15,487
	45-59	0.586	12,135
	60-74	0.459	8,933
	75-89	0.496	2,119
	90-104	0.614	161
Sex	Male	0.615	20,506
	Female	0.631	24,034
Education level	<12 (no HS)	0.782	1,995
	12 (HS)	0.701	12,018
	13-15 (college)	0.679	12,412
	16-17 (bachelor+)	0.510	17,354
Health status	Excellent/Very good	0.596	24,682
	Good	0.647	13,798
	Fair/Poor	0.685	5,795
Race and ethnicity	White	0.559	29,119
	Black	0.759	10,363
	Hispanic	0.760	3,305
	Other	0.614	1,294
State political category	Stayed Democrat	0.593	16,733
	Stayed Republican	0.658	18,255
	Flipped to Trump	0.612	9,276
Household composition	Single head	0.730	10,578
	Couple	0.590	33,962

Note. Checking share is winsorized at the 1st and 99th percentiles. For the state political category dimension, state of residence is measured contemporaneously in each wave, consistent with the descriptive account in Section 4.4, unlike the fixed 2015 state used in the main regressions. Own calculations.

Table B.4 report the pooled 2011-2019 mean Real estate share for each of the seven profile dimensions introduced in Table 4.3, underlying the figures cited in Section 4.4.

Table B.4. Real estate share by profile dimension, 2011-2019 pooled

Dimension	Category	Mean	N
Age group	15-29	0.030	5,705
	30-44	0.067	15,487
	45-59	0.100	12,135
	60-74	0.115	8,933
	75-89	0.116	2,119
	90-104	0.108	161
Sex	Male	0.086	20,506
	Female	0.081	24,034
Education level	<12 (no HS)	0.076	1,995
	12 (HS)	0.077	12,018
	13-15 (college)	0.079	12,412
	16-17 (bachelor+)	0.092	17,354
Health status	Excellent/Very good	0.084	24,682
	Good	0.083	13,798
	Fair/Poor	0.079	5,795
Race and ethnicity	White	0.091	29,119
	Black	0.062	10,363
	Hispanic	0.081	3,305
	Other	0.083	1,294
State political category	Stayed Democrat	0.081	16,733
	Stayed Republican	0.089	18,255
	Flipped to Trump	0.073	9,276
Household composition	Single head	0.049	10,578
	Couple	0.094	33,962

Note. Real estate share is winsorized at the 1st and 99th percentiles. For the state political category dimension, state of residence is measured contemporaneously in each wave, consistent with the descriptive account in Section 4.4, unlike the fixed 2015 state used in the main regressions. Own calculations.

Table B.5 report the pooled 2011-2019 mean Other assets share for each of the seven profile dimensions introduced in Table 4.3, underlying the figures cited in Section 4.4.

Table B.5. Other assets share by profile dimension, 2011-2019 pooled

Dimension	Category	Mean	N
Age group	15-29	0.050	5,705
	30-44	0.054	15,487
	45-59	0.062	12,135
	60-74	0.063	8,933
	75-89	0.083	2,119
	90-104	0.073	161
Sex	Male	0.057	20,506
	Female	0.060	24,034
Education level	<12 (no HS)	0.073	1,995
	12 (HS)	0.064	12,018
	13-15 (college)	0.061	12,412
	16-17 (bachelor+)	0.053	17,354
Health status	Excellent/Very good	0.057	24,682
	Good	0.057	13,798
	Fair/Poor	0.074	5,795
Race and ethnicity	White	0.055	29,119
	Black	0.070	10,363
	Hispanic	0.061	3,305
	Other	0.056	1,294
State political category	Stayed Democrat	0.060	16,733
	Stayed Republican	0.057	18,255
	Flipped to Trump	0.060	9,276
Household composition	Single head	0.065	10,578
	Couple	0.057	33,962

Note. Other assets share is winsorized at the 1st and 99th percentiles. For the state political category dimension, state of residence is measured contemporaneously in each wave, consistent with the descriptive account in Section 4.4, unlike the fixed 2015 state used in the main regressions. Own calculations.

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12. EXECUTIVE SUMMARY

The 2016 US presidential election was unexpected and reshuffled the political alignment of several states. This thesis asks whether that shift affected how American households allocated their portfolio, and whether the effect depended on a state's political trajectory.

Existing evidence on this question is limited in two ways. First, the only household-level study of the 2016 election relies on brokerage data covering equity holdings alone. Second, the main quasi-experimental design in this literature exploits staggered elections, using unexposed states as a control group.

This thesis addresses both limitations. Using five biennial waves of the Panel Study of Income Dynamics (2011-2019), it tracks five components of household wealth, stocks, IRA, real estate, other assets, and checking and savings. States are classified by their political trajectory between 2012 and 2016, and a DiD model with individual and year fixed effects estimates how portfolio shares changed around the election. With only six states in the group of interest, inference relies on a wild cluster bootstrap alongside conventional standard errors, complemented by an extensive set of robustness checks.

Households in states that flipped to Trump reduced the stocks share of their portfolio by roughly one to one and a half percentage points more than comparable households elsewhere, a pattern that holds when stock holdings are measured in dollars, when movers are excluded, and across several specifications. The result reaches conventional significance in its sharpest form but does not survive a correction for multiple testing, and weakens under some robustness checks. No comparable pattern emerges for the other four portfolio components.

This thesis offers the first geographically identified estimate of how the 2016 election affected the full composition of household portfolios, and an honest account of how much a single national election, without a genuine control group, can establish on its own.

KEYWORDS: Political realignment – political uncertainty – household portfolio choice – difference-in-differences – fixed effects – wild cluster bootstrap – 2016 US presidential election – PSID

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