Partisanship and Portfolio Choice: Evidence from Mutual Funds*

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Abstract

Political beliefs shape the portfolio choice and asset demand of institutional investors. After Donald Trump's surprise 2016 election, Republican fund managers actively increased the equity share of their portfolios by nearly two percent, primarily through purchases of stocks sensitive to economic fluctuations. We validate that partisan beliefs drive changes in portfolio choice using the reaction of stock analysts to the same shock. Republican analysts increased their near-term earnings forecasts following Trump's election, especially in those same stocks exposed to economic conditions. Further, we show that partisanship has transformed the entirety of the intermediation chain. Republican and Democratic-run funds now receive systematically different inflows due to alignment between fund manager partisanship and fund sustainability ratings. Motivated by our empirical results, we build a Koijen-Yogo style model of partisan asset demand.

JEL Codes: G23, G40, P00

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1 Introduction

The United States is strongly divided by partisan politics – Democrats and Republicans have systematically different beliefs and even actions.[1] For example, during the Covid-19 pandemic, political partisanship was a strong determinant of attitudes towards social distancing and mask wearing (Allcott et al. (2020); Milosh et al. (2020)). That politics shapes people's views and behavior is now well documented. However, it's still an open question whether, how and to what extent partisanship matters for investment decisions.

Recent studies find that political partisanship affects economic expectations and decision making. For example, in Coibion et al. (2020), households surveyed around the 2020 election expect a rosy economic scenario if their favored party wins the election, but a dire one if the other candidate wins. Here and in other studies, Democrats and Republicans respond in opposite directions to the same political event depending on which party wins. In situations that affect corporate borrowing rates, bankers and credit analysts behave more optimistically when their party wins the presidency and more pessimistically when their party loses the presidency (Dagostino et al. (2020); Kempf and Tsoutsoura (2021)). Notably, however, their slight misestimation of default rates is hard to identify in real time. For financial decisions with more immediate and measurable consequences, much of the extant evidence suggests that partisan beliefs play a smaller role. Mian et al. (2017) and Meeuwis et al. (2020) find a relatively small effect of political beliefs on households' consumption-savings decisions and portfolio choice.[2]

In light of this mixed evidence, a natural question is whether political beliefs matter for institutional investors. This question is of direct consequence to not only behavioral finance but also asset pricing as a whole. Recent work has emphasized that households are typically infra-marginal investors while institutional investors are marginal (Koijen et al. (2020a)). According to these theories, the behavior of institutional investors, not households, will be the driving force behind asset price movements.

We show that political beliefs matter for an important class of institutional investor – mutual funds. Political beliefs, measured using self-reported party affiliation from voter registration data, shape active trading. We use Donald Trump's surprise victory in 2016 as a laboratory to study the impact of political beliefs on the actions of mutual fund managers. Immediately after Donald Trump's election, funds with a larger share of Republican man-

[1]See Bertrand and Kamenica (2018) for evidence on systematically different beliefs. For evidence on systematically different actions, Hersh and Goldenberg (2016), Chen (2019) and Kempf and Tsoutsoura (2021) show this for populations of physicians, judges and credit analysts, respectively.

[2]Along with this paper, some contemporaneous work, for instance Kempf et al. (2021) and Engelberg et al. (2022) find larger impacts of partisanship on financial decision making, specifically.

agers actively increase their risk-taking relative to all other funds, buying more equity and tilting their portfolios towards stocks in industries exposed to market fluctuations. We find less sharp, but consistent, results in the opposite direction around the 2012 and 2020 presidential elections, consistent with the outcomes of these elections being partially anticipated.

We conclude that fund managers actively increase their risk taking when a member of the same political party wins the Presidency. These effects are not due to differential benchmarks, price effects, or investment flow effects.

In terms of trading magnitudes, it's not obvious whether institutional investors should respond to their own political beliefs more or less than households. Mutual fund managers are paid to trade on their beliefs, so it is possible that beliefs should matter more for fund managers' trading. At the same time, managers are strongly monetarily incentivized to not underperform their fund-specific benchmarks, in which case fund managers may be unwilling to make directional bets based on their political beliefs.

Presidential elections provide a convenient testing ground to study the heterogeneous pass-through of beliefs to portfolio choice because we can compare the response of mutual fund managers and households to the same event and for the same dependent variable. Meeuwis et al. (2020) show that retail investors in the most Republican zip codes incrementally increase their equity allocation by 0.25% of net assets compared to those in the least Republican zip codes. In our sample of active equity mutual funds, Republican majority teams increase their equity share by nearly 2.0% of net assets relative to majority Democratic teams. The pass-through of beliefs for active mutual fund managers is *an order of magnitude larger* than for households. We further show that these effects persist and grow over the course of Donald Trump's presidency.

The trading differences between fund managers and households appear to be driven by base differences in their trading frequency. The typical active fund turns over 40-50% of its portfolio every year, while only a third of households in Meeuwis et al. (2020) trade *at all* in any given year. When we compare the effects we document with households that do actively trade, fund managers still trade more aggressively than households, but the gap is much smaller.

Our finding that institutional investors actively trade on their political beliefs has important implications for the asset management industry. In particular, the typical investor that entrusts active fund managers with their retirement savings is subject to unexpected principal-agent conflicts. With $13.3 trillion of actively managed US-listed mutual funds and ETFs alone, the welfare costs are likely large.[3]

[3] The $13.3 trillion figure is for all actively managed US-listed funds at the end of 2020. For US-listed equity funds, $7.72 trillion (49.4%) is actively managed at the end of 2020. These values are from Bloomberg.

In addition, we document that Republican- and Democratic-majority funds now systematically receive differential inflows. We attribute this to alignment between fund manager partisanship and fund-level measures of sustainability. Comparing funds within the same benchmark, we show that funds that have a majority Democratic fund team are more likely to have the highest possible Morningstar sustainability rating as well as a Low Carbon designation. We conjecture that the connection between fund manager partisanship and sustainability metrics, combined with the increased importance of sustainability to end-investors lead to large differential inflows to Republican- and Democratic-majority funds in 2020.[4]

To better understand the mechanism driving this behavior, and to rule out alternative explanations, we study the earnings forecasts of stock analysts. We show that stock analysts whose political affiliation is aligned with the current president have earnings forecasts approximately 2% higher than those who are non-aligned. This emphasizes the importance of political partisanship for growth expectations, extending the results of Coibion et al. (2020) from households participating in an unpaid survey to stock analysts with career risks related to these near-term forecasts.

Around Trump's 2016 election, Republican analysts' upward earnings revisions are concentrated in industries exposed to business cycle fluctuations, that is, industries sensitive to economic growth. This suggests political alignment contributes to a rosier lens through which to forecast economic growth. We characterize the term structure of partisan growth rate expectations. We show that politically aligned analysts revise forecasts of horizons of up to two years upwards, while long-term earnings forecasts are unchanged. Partisan alignment is a shock to near-term cashflow expectations. Comparing analysts' earnings forecasts to realized earnings, we also show that politically aligned analysts make larger forecast errors. These results suggest a causal link between partisanship and inaccurate economic forecasts.

To the best of our knowledge, we are the first in the literature to study how political beliefs affect equity analysts' forecasts. These estimates both validate our results and are separately important for the growing literature that studies the impact of beliefs on asset demand.

The rest of the paper is organized as follows. In Section 2, we situate the paper's contribution in the related literature. In Section 3, we describe the data and the key covariates of interest. In Section 4, we document partisan fund manager trading, investigate the mechanism, describe important robustness tests, and document partisanship-related biases in analyst forecasts. Section 5 builds a Koijen-Yogo style model to better interpret our results, as we simultaneously examine asset demand and subjective partisan beliefs. Section 6 concludes.

[4]See Hartzmark and Sussman (2019) for evidence that Morningstar sustainability ratings matter for flows.

2 Related Literature

Our paper's first contribution is to show that political beliefs translate into economic decisions. Historically, the literature has typically found small effects. The first paper in this area, Gerber and Huber (2010), verifies that partisanship affects economic assessments (in survey data) and shows that counties with larger vote shares for winning political candidates experience an increase in taxable sales. Mian et al. (2017) re-examine this issue, finding that political beliefs affect surveyed expectations but not county-level car purchases. In contrast, Gillitzer and Prasad (2018) find that political beliefs have a small effect on consumption using Australian data. More recently, Meeuwis et al. (2020) show that political beliefs appear to shape retail investor trading after Trump's 2016 presidential election, although the effect is heterogeneous and small on average. We study the relation between political beliefs and economic decisions in an ideal environment, as mutual fund managers trade very actively, are financially sophisticated and strongly monetarily incentivized, and receive frequent performance feedback. That even mutual fund managers make economic decisions in line with their political beliefs suggests this channel may be more pervasive than previously believed.

While Meeuwis et al. (2020) show that political beliefs have a small average effect on portfolio choice, our results suggest this is only because most retail investors do not trade regularly. Our measured effect is an order of magnitude larger, although of a similar magnitude to the small proportion of retail traders that do actively trade. These differences strongly echo the findings in Giglio et al. (2021) – they find that the effects of beliefs on portfolio choice are small on average because many investors simply don't trade.

More generally, a robust literature finds that the asset management industry is largely immune from meaningful political biases. For example, Hong and Kostovetsky (2012) find Democratic managers overweight socially responsible stocks relative to Republican managers, but there is no effect on performance. Shu et al. (2012), DeVault and Sias (2017), Brown et al. (2018), and Wintoki and Xi (2020) reach similar conclusions. A key distinction is that this earlier literature studies values as opposed to beliefs. This literature shows Republicans and Democrats have different non-pecuniary benefits from holding sin stocks as opposed to differential beliefs about firm performance.

In contrast, political biases have consequences in many other settings, affecting outcomes for retail investors, physicians, judges, credit analysts, bankers, and corporate executives.[5]

[5]Important partisan performance effects are documented for retail investors in Bonaparte et al. (2017), Cookson et al. (2021), Meeuwis et al. (2020), and Sheng et al. (2021); for physicians in Hersh and Goldenberg (2016); for judges in Chen (2019); for credit analysts in Kempf and Tsoutsoura (2021); for bankers in Dagostino et al. (2020); and for corporate executives in Hutton et al. (2014), Di Giuli and Kostovetsky (2014a), and Bizjak et al. (2021).

Our results show that the asset management industry is *not* actually differentiated, as fund managers similarly trade in line with their political beliefs.

Our paper's second contribution is to corroborate an important mechanism through which political beliefs shape economic decisions. Kempf and Tsoutsoura (2021) and Dagostino et al. (2020) show that credit analysts and bankers appear more optimistic when their party wins the presidency and more pessimistic when their party loses the presidency. Our results are analogous, focusing on mutual fund managers. This mechanism is important because it emphasizes that political beliefs matter. An important competing explanation is that Democrats are more risk-averse than Republicans, as in Pástor and Veronesi (2020). The fact that the relation between increased risk-taking and partisanship flips when the party winning the presidency changes strongly weighs in favor of beliefs. These findings are important not only for political economy, but also for the broader asset pricing literature, where it is often difficult to disentangle the roles of risk aversion and beliefs.

Our paper's third contribution is to study the reaction of partisans to Biden's 2020 presidential election. To the best of our knowledge, we are the first paper to do so. We show that Democratic fund managers increase their risk-taking in the third quarter of 2020, before Biden's election, while Republican managers do not. While this pattern is in line with Kempf and Tsoutsoura (2021) and Dagostino et al. (2020), it is not obvious that Biden's election was as surprising as Trump's 2016 election, which drives the main effect in Meeuwis et al. (2020). Coibion et al. (2020) show that, for a large-scale survey of households, most voters dogmatically believed their party (Democratic or Republican) would win the 2020 election just two weeks prior to the election and were minimally swayed by polling data. Our results are consistent with Democratic fund managers anticipating that Biden would win and altering their positions in advance of his election.

Two related papers attempt to quantify the relation between partisanship and the behavior of institutional investors. First, Kempf et al. (2021) studies the role of partisanship in international capital flows. They find that the ideological alignment of investors and foreign governments affects capital flows both in the syndicated loan market and for equity mutual funds. Our work is differentiated primarily in that we focus on the ideological alignment of institutional investors and the domestic (US) government. Second, Vorsatz (2021) studies the differential performance of partisan and non-partisan mutual fund teams. He finds that partisan fund teams (either Democratic or Republican) have lower fund returns and lower fund flows after the onset of Covid-19, compared to non-partisan teams. Our work is differentiated in that we focus on differences between Democrats and Republicans, not partisans and non-partisans, and that we study the relation between partisanship and performance across multiple political events.

3 Data

Our study is made possible through the construction of 2 unique databases: (1) mutual fund managers matched to fund holdings, fund returns, fund characteristics, and fund manager biographical and political partisanship information; and (2) I/B/E/S equity analysts matched to earnings forecasts as well as biographical and political partisanship information. To construct these databases, we rely on a variety of non-standard data sources and implement a variety of novel merges. These databases provide a large amount of information for better understanding how individual characteristics – whether political partisanship, age, gender, educational background, or residential zip code – shape information processing and decision-making in highly motivated and sophisticated individuals.

Our mutual fund manager database matches Morningstar fund characteristics and fund managers to scraped fund manager demographic information, to political partisanship in voter registration data, and to CRSP fund holdings, fund returns, and fund characteristics. Our final database is quarterly with time-varying fund characteristics and time-varying fund management team partisanship.

Our equity analyst database matches largely anonymized I/B/E/S analysts and their earnings forecasts to fully de-anonymized voter registration records. This merge requires several intermediate steps: matching partial I/B/E/S analyst names to full names and firms in TipRanks, and then to work addresses in BrokerCheck. The intermediate steps allow us to collect additional personal information for verifying the accuracy of the matches.

Additional details on our databases – sources, cleaning, and merging – can be found in the Data Appendix in Appendix Section A.

3.1 Fund Characteristics

Morningstar Direct provides substantial information about individual fund characteristics. The most important of these are fund age, net expense ratio, turnover ratio and total net assets (Pástor et al. (2020)). Following the mutual fund literature, these are included as controls in all regressions where a choice variable of the fund is the dependent variable. In addition, Morningstar provides data on fund holdings above and beyond what can be gleaned from CRSP. In particular, Morningstar accurately classifies securities in the holdings data that lack a *permno*, and Morningstar carefully differentiates between US bonds, non-US bonds, and cash. Thus, Morningstar's measurement of the share of the portfolio invested in foreign and domestic bonds and equity, as well as cash, is more accurate than measures relying on CRSP alone.

Besides information on fund holdings, Morningstar also identifies the fund-specific benchmark that best matches the fund's holdings. An important advantage of Morningstar-designated benchmarks is that they avoid issues of funds strategically choosing their prospectus benchmark (Sensoy (2009)). Instead of using the FTSE/Russell benchmark, we use the Morningstar Category, which is approximately identical with greater coverage. Notably, the Morningstar Category is used to determine peer groups for assigning Morningstar stars – a measure of risk- and style-adjusted past performance that is *the* most important determinant of fund flows (Del Guercio and Tkac (2008); Ben-David et al. (2019)) – and so the Morningstar Category is in fact the most relevant delineation of fund benchmarks. For this study, information on the benchmark is important because fund performance and purchases around Presidential elections and other major political events could be due to managers tracking their benchmark as opposed to actively trading on their own beliefs.

We also collect the CRSP style code from CRSP, which measures the strategy of the fund in a slightly coarser but often similar fashion. In our analysis, the style code is sometimes included as a control for the same reason as the benchmark – to ensure that beliefs, not the fund's time-invariant strategy – are influencing trading around major political events.

3.2 Fund Managers

Morningstar Direct provides full fund manager names – first and last names as well as middle initial. For this reason, our study and many others collect fund manager information from Morningstar Direct, as opposed to CRSP, which contains only the initial of the first name and the last name, making fund manager identification much more challenging. We scrape information about individual fund managers from Morningstar.com, which is entirely separate from the Morningstar Direct database. We collect this demographic data to improve the quality of our matching between fund managers in Morningstar and the voter registration records. Manager name as well as proxies for age, gender and location are imputed from the manager biography listed online.[6] The manager's age is inferred using two methods. First, Morningstar frequently lists educational information, including the manager's undergraduate university and graduation year. Assuming people generally graduate at age twenty-two, an estimate of the manager's age is twenty-two plus the number years that have elapsed since college graduation. Manager biographies also frequently list the duration of industry experience. This also provides a lower bound on age, which is used when college graduation data is unavailable.

We infer the gender of the manager in two ways. First, following Kempf and Tsoutsoura (2021), we infer the gender of managers from their first name using the publicly available API

[6] An example fund manager biography is provided in the Online Appendix.

genderize.io. This name databank infers the likely gender of the manager based on the ratio of males to females observed in a large sample. For example, “Peter” has a 99% probability of being a male while “Erin“ has a 77% probability of being a female. Second, many of the manager biographies include self-reported gender pronouns such as “he” or “she.” We use these whenever they are available.

We approximate the fund manager’s likely geographic location using the location of the fund firm’s headquarters. There can be multiple offices but only one headquarters. As most fund manager teams work together in person, and thus must live in the same area, this strategy ensures that, when we match one manager on a fund team, we have also searched in the correct location for all other co-managers.

3.3 Political Affiliation from Voter Registration Data

Data on the political affiliation of fund managers comes from voter registration data. The five states with the highest number of mutual fund headquarters are New York, Massachusetts, California, Texas and Illinois. We purchase data for New York, Massachusetts, and Texas, as well as for Colorado, Connecticut, Florida, New Jersey, North Carolina, Ohio, and Tennessee. Data for California and Illinois was unavailable for legal reasons.

The content of voter registration data depends on the state in which the voter lives.[7] All voter registration records include the name and some demographic information about the voter, including their address, age, name and gender. For most states, the voter file lists each election that the voter has voted in and their party of registration for that election. Republican or Democratic voters that have populated party of registration fields are called “Registered Republicans” or “Registered Democrats”. A subset of voters declare that they are Independents, and these voters are called “Registered Independents”. For each matched manager, we use the party affiliation as of the most recent election in which that voter voted.

For a subset of states, the party affiliation is not listed. The party affiliation of these voters is inferred by looking at the most recent party primary that the voter voted in. If they most recently voted in a Republican primary, then we count the party affiliation as Republican. Before we observe a primary vote, we count the voter as unaffiliated. This approach is standard in the small literature that has tried to infer party affiliation through registration records.[8]

Morningstar managers are matched to registration records by fuzzy matching on name. For each manager, there are often multiple matches, of which at most one is correct. We

[7] Two anonymized voting records that correspond to actual fund managers are provided in the Online Appendix.

[8] See Fos et al. (2021), for instance.

eliminate incorrect matches by validating these matches using the demographic variables that are present for both the fund manager of interest and the voter registration records. For example, a match would be invalid if our inferred gender from Morningstar is male, but the recorded gender in the registration records is female. By conditioning on name, age, gender, and approximate location, we eliminate the vast majority of non-unique matches.

In total, we uniquely match 4,590 managers, which is approximately 20% of our initial sample. We have non-unique matches for an additional 15% of our manager sample – as of now, we do not use these non-unique matches because we cannot be confident that we've identified the fund manager.[9] At any point in time, approximately 2,000 matched managers are active as fund managers. Managers drop out of our sample because they leave their fund or retire. They also enter our sample if they register to vote or vote for the first time. Detailed party breakdowns over time are available in the Online Appendix. Consistent with evidence from Fos et al. (2021) on corporate managers, we find that the most common group in our sample is Republicans, but there are also significant numbers of Democrats and registered independents. There are a smaller number of managers that belong to other political parties.

To ensure that party affiliation from voter registration data indeed reflects partisanship, we compare the set of fund managers matched to both party affiliation in our study and federal political contributions in Vorsatz (2021). Table 1 compares the partisan classifications from voter registration and political contributions for the managers matched to both data sources. In short, both data sources largely agree, indicating that we accurately identify fund managers as Republicans or Democrats. Panel A, which makes these comparisons for Democrats and Republicans, indicates a very high level of agreement across the two measures. Panel B, which makes these comparisons for unregistered voters and non-partisans who contribute to non-partisan committees, agrees substantially less.

These findings are consistent with prior research that has assessed the accuracy of voter registration records in capturing political affiliation. Igielnik et al. (2018) shows that voter registration records accurately reflect self-reported political identities.

These differences are important to keep in mind when making comparisons between our results and other studies that use contributions data, for instance Vorsatz (2021). Voter registration data does a better job at capturing the universe of partisans, but is less helpful for identifying the most committed party members. Our study, in contrast to Vorsatz (2021), will not be able to make fine distinctions about whether the members of a fund team are particularly committed to the Republican or Democratic cause, even among Republicans

[9]In the future, we may try to manually differentiate between correct and incorrect matches to expand our matched fund manager sample.

and Democrats. Conversely, our results will be able to detect ideological homogeneity even among teams whose contributions data do not indicate that all members belong to a single party. For instance, it will accurately detect partisans who make strategic contributions and partisans that don't contribute at all. In particular, we suspect that many partisan fund managers do not make political contributions because of the SEC's "Pay-to-Play" rule 206(4)-5, in which case the voter registration data will be particularly helpful.[10]

3.4 Merging Morningstar and CRSP

There are three datasets we use from CRSP. The first is holdings data. This dataset lists the name and quantity of each security that the mutual fund holds as of a particular date. Mutual funds are required to report their holdings at a quarterly frequency. The second dataset from CRSP is the monthly security file. We primarily use this for stock prices. The third dataset includes the fund's monthly net return and additional fund characteristics like the CRSP Style Code.

Merging Morningstar and CRSP is non-trivial. There is no standard mapping table between CRSP and Morningstar. We perform the mapping following Ma and Tang (2019), merging between CRSP and Morningstar by matching CUSIPs and tickers to establish valid fund share class matches ($SecId$ in Morningstar and $crsp\_fundno$ in CRSP). We then ensure that the associated fund-level link (aggregated across share classes, $FundId$ in Morningstar and $crsp\_cl\_group$ in CRSP) is in fact a valid match.

3.5 Main Variables of Interest

Our analysis focuses on four main variables of interest: the Republican and Democratic share of fund managers on the fund team, the total equity share of the fund's portfolio, fund active purchases as a share of net assets, and net investment flows to the fund. We discuss the construction of these variables in this subsection.

Our main partisan variables are the Republican and Democratic shares of matched managers on the fund team. These variables are time-varying and are recalculated quarterly as the share of matched managers on the fund team that are Republican or Democratic. Notably, this variable changes as fund managers retire or are fired, join the fund or are recorded in the registration records for the first time.

[10]The SEC's "Pay-to-Play" rule prohibits investment advisors from providing investment advisory services for compensation to a state or local government entity if the advisor has made a political contribution to certain state or local government officials in the prior 2 years. This is consequential because many investor advisors manage state pensions. Notably, some asset management firms will ask about political contributions during the job application process, which may create future hesitation about even contributing.

To capture active changes by mutual fund managers, we study active net purchases (as a percent of net assets) computed with holdings data. Active net purchases are defined to be net of price effects – that is, so that this variable reflects actual active portfolio changes and not simply valuation effects as the prices of the underlying securities rise and fall. We define active net purchases for fund i of stock s between time $t-1$ and time t as

$$\text{AP}_{i,s,t} = \frac{(\text{Num Shares}_{i,s,t} - \text{Num Shares}_{i,s,t-1}) \times P_{i,t-1}}{\text{Total Net Assets}_{i,t-1}} \tag{3.1}$$

Note that, by including only lagged prices in the numerator, our measure is net of price effects, like in Kacperczyk et al. (2005). For a stock to be included in the numerator, we must have data on the lagged price. Given CRSP data limitations, this means our measure of active net purchases captures only stocks in CRSP, which are US-listed. As a result, we cannot measure active net purchases of foreign-listed stocks, bonds, and any over-the-counter financial instruments. We address this data limitation by focusing on mutual funds that principally (or exclusively) invest in US stocks.

We then aggregate active net purchases for fund j as the sum of active net purchases across all stocks s in fund i's portfolio

$$\text{AAP}_{i,t} = \sum_{s \in i} \text{AP}_{i,s,t} \tag{3.2}$$

This variable, aggregate active (net) purchases (AAP), is our main measure of active trading.

Last, we also look at net fund investment flows. Following Barber et al. (2016), we measure net fund flows as

$$\text{Net Fund Flows}_{i,t} = \text{TNA}_{i,t} - (1 + R_{i,t})\,\text{TNA}_{i,t-1}$$

where $R_{i,t}$ is the net return of fund i from time $t-1$ to t. This measures how much money investors have invested or withdrawn from the fund in total, net of price effects and also net of fund fees. Mechanically, this is important for the ability of the fund to make active purchases. Higher inflows will give fund managers greater scope for additional purchases. If outflows are high enough, the manager will be forced to liquidate existing positions to meet redemptions – to avoid selling at fire-sale prices (Edelen (1999); Coval and Stafford (2007)), most funds try to maintain a cash buffer or else forgo active purchases they otherwise would have made. As a result, controlling for net fund flows is important to understand whether managers are actively trading because of beliefs or because of cash management needs.

3.6 Sample Summary Statistics

We impose a few standard restrictions to the universe of funds that we study. First, we focus on active funds, excluding index funds. Index funds are identified in Morningstar with an index fund flag, and we also consider funds with the word "index" in their name as index funds. Second, we restrict consideration to fund styles that are equity domestic or mixed. These restrictions are made because we want to look at changes in US stock holdings. By definition (and often also by fund mandate), equity domestic funds predominantly hold US stocks. Mixed funds, which include "balanced" funds, have more leeway in their holdings, but generally invest between forty and sixty percent of their portfolio in equities, many of which are US companies.

Mixed funds are included in the analysis specifically because they have laxer mandates. If any funds are likely to strongly change their relative share of equities and bonds after major political events, it is these mixed funds. For example, Comer (2006) emphasizes that these funds *intend* to engage in "sophisticated market timing or tactical asset allocation techniques in an attempt to generate high returns" (772). In addition, we restrict the universe of funds to those that have at least $10 million in assets. This screen is common in the mutual fund literature, for example in Elton et al. (2001). Our motivation for this screen is slightly different than the rest of the literature. Our measure of active purchases can be thrown off by small funds that rapidly increase their holdings, for example when a new fund first accepts inflows. This results in extremely high values of active net purchases, not because the fund is being aggressive in purchasing equity, but simply because it is expanding rapidly and wants to maintain at most modest cash holdings.

Within the equity domestic universe of funds, we exclude funds that are hedged and short. These funds are often motivated by the desire to outperform through stock-picking skill, for example within industry, and so may hedge out more systematic exposures. With a goal of stock selection, they are less likely to trade on political beliefs, and to the extent that they do, it would be exceptionally challenging to tease out this effect. Note, however, that our results are robust to the inclusion of hedged and short funds.

Table 2 reports relevant summary statistics for the funds that survive these restrictions. At the end of 2020, our sample includes 840 funds with $2.0 trillion of total net assets. The average fund in our sample has $2.4 billion of TNA at the end of 2020, although the median fund is meaningfully smaller, with $500 million of TNA.

Reflecting the fact that most funds in our sample are equity domestic funds, the median fund invests 97.7% of net assets in equity. This distribution is left-skewed, as the average fund only holds 86.1% of net assets in equity, in large part reflecting the inclusion of mixed funds. This dynamic is also present in the summary statistics for the share of net assets in bonds.

Table 3 provides manager counts by partisanship-year for managers that appear in our final sample of funds. Across the 2012 to 2020 time period, our sample includes approximately 850 managers per year.[11] Across all years, a total of approximately 1200 funds ever have a single active manager in our matched manager sample. The largest group of managers in our sample are Republicans, followed by Democrats, and last Independents. A meaningful number of matched managers do not have any information about their partisan inclinations. We treat these voters as independents because they have not voted in any party primaries and have not registered their party affiliation (at least for the states that report this field in voter registration data).

3.7 Analyst Expectations

Data on analyst expectations are earnings-per-share (EPS) forecasts taken from I/B/E/S. To match these forecasts with voter-registration data, we proceed in three steps.[12] We begin by pulling a complete record of all analyst forecasts made in I/B/E/S. These forecasts are indexed by a unique analyst identifier ("AMASKCD"), a stock identifier, and the date of the forecast. In addition, I/B/E/S provides the analyst's first initial and last name as well as a masked identifier of the analyst's firm ("ESTIMID").

The information contained in I/B/E/S is insufficient to fuzzy match with voter registration records, as it does not contain a full first name, middle name or location information. We collect this information by merging in additional data from `www.TipRanks.com` and `www.BrokerCheck.com`.

TipRanks is a subscription-based webservice that provides information about analyst forecast accuracy to retail traders. To recover information about the analyst's name and firm, we search TipRanks for all analysts that have rated the stock the analyst is recorded as rating. We then search for analysts that, among the set of analysts that have rated that stock, have a last name and first initial consistent with the "ANALYST" partial name field in I/B/E/S. For each candidate analyst, we also record the firm that TipRanks records the analyst as working for.

[11] Many of the managers in this sample are matched to multiple funds, so a significant number of funds have multiple matched managers. The Online Appendix includes detailed breakdowns of the distribution of matches.

[12] Full details on how we merge analyst partisanship to EPS forecasts is provided in the Section A.4 of the data appendix. Our approach to matching analysts to voter records shares similarities with previous methods, such as in Gibbons et al. (2021). Like these authors, we use the name identifiers (first initial and last name) in I/B/E/S to recover analyst identities. Our approach automates analyst matching (to full names and firm names), and goes beyond such prior work to also collect exact work office address, home address, political party affiliation, and other demographic data.

If there is only a single candidate match between I/B/E/S and TipRanks based on last name and first initial, then we consider that a valid match. Using our set of unique matches, we then constructed a correspondence between firm names and the I/B/E/S mnemonic for each firm. We do this in two steps. First, we sort firm name mnemonic pairs based on the frequency of their coincidence in the set of valid matches constructed in the prior step.

We then hand-audit each potential firm name mnemonic pair and classify each pair as a correct match based on match frequency and semantic similarity. We find that I/B/E/S firm mnemonics ("ESTIMID") are almost always similar to the full firm name. For example "Loop Capital Markets" becomes "LOOPCAMA" and "Morgan Stanley" becomes "MORGAN". We then verify that the mapping is internally consistent with unique one-to-one relations. Using this correspondence between firm names and mnemonics, we are able to disambiguate additional analyst names by requiring that the firm name and firm mnenomic is consistent across I/B/E/S and TipRanks.

As a next step, we then search for each analyst we match in BrokerCheck, using the full name and firm retrieved from TipRanks. BrokerCheck is a free service provided by the Financial Industry Regulatory Authority (FINRA) that provides information about registered financial advisors and analysts. From BrokerCheck, we recover the full, legal name of the analyst, including the middle name, as well as alternate names the analyst may have used. In addition, BrokerCheck provides the exact physical address of the office at which the analyst is registered, and the work history of the analyst, including both years experience and firms the analyst previously worked at. We use the analyst's work office address as a starting point for identifying the analyst's home address (in the voter registration data) and years of experience allows us to calculate a lower bound for the analyst's age (which we compare to date of birth in the voter registration data).

Using this combined information, we match analysts to voter registration records, using an algorithm similar to that used in Section 3.2 and fully described in Section A.4 in the Appendix.

Table 4 provides summary statistics for our sample of yearly EPS forecasts. On the vertical axis, this table displays the fiscal year the forecast was made. The horizontal axis marks the year the forecast concerns. For each pair of years listed, we show the number of forecasts made as well as the mean, median and standard deviation of forecasts. Most forecasts concern the next or year-after fiscal year. A smaller number refer to the fiscal year three years ahead. Relatively few forecasts concern the same fiscal year or the fiscal years four or five years ahead. For this reason, our analysis focuses on the next three fiscal years.

4 Empirical Results

Figure 1 provides a preliminary look at the relation between active net equity purchases and fund manager partisanship, focusing on two mutually exclusive partisanship sorts: funds with a majority Republican team and funds with a majority Democratic team. The results are striking. There is significant visual variation in active net equity purchases by partisanship in only two periods: in the few quarters immediately following the 2016 Presidential election and the quarter prior to the 2020 Presidential election. While there are no standard errors or measures of statistical significance in these plots, regression estimates shown later confirm that the visual evidence from this figure is not spurious.

There are a few other striking features of the time series of active net equity purchases. First, in the appendix, we show that there is no visual evidence of differences in net equity purchases by partisanship in 2012. This finding is consistent with the broader polarization literature, which finds that polarization has worsened over the last decade. Mian et al. (2017) study the differential change in beliefs between households in Republican and Democratic counties. They also find a relatively small difference in the change in surveyed economic expectations after the 2012 election relative to 2016.

Second, and more importantly, there are differences in net purchases by partisanship *both before and after* Presidential elections. The largest effects in 2020 occur in the quarter before the 2020 election. To our knowledge, we are the first to document evidence consistent with anticipatory effects around Presidential elections. These anticipatory effects are in contrast to the behavior of both households and mutual fund managers around the 2016 election. Anticipatory effects are consistent with the job description of fund managers and the nature of the 2020 Presidential Election. Fund managers are paid to trade on news and maximize performance, taking into account major events that may or may not occur. Further, the outcome of the 2020 Presidential election was at least partially anticipated, with some forecasting services assigning a 90% probability to a Biden victory.[13]

4.1 Regression Analysis

We start by regressing Morningstar portfolio share measures of equity and debt on the Republican fund team majority indicator and estimate the following differences-in-differences specification:

$$\text{Share}_{it} \sim \beta \mathbb{I}\left\{\text{Rep Majority}\right\}_i \times \mathbb{I}\left\{\text{Post-2016 Election}\right\}_t + \nu_i + \nu_t \tag{4.1}$$

[13]See https://projects.fivethirtyeight.com/2020-election-forecast/.

The dependent variable is the share of net assets invested in equity or debt, focusing both on total equity or debt. We both include fund (ν_i) and date (ν_t) fixed-effects and cluster by date. We estimate this regression over the sample of funds that have either a Democratic or Republican majority.

Recall that we use the Morningstar variables for this because Morningstar fully classifies all holdings with no omissions, while using CRSP data only works for US stocks. We estimate this regression at a quarterly frequency focusing on Trump's 2016 election. Accordingly, the indicator for the post-election period takes a value of one for 2017 and 2018 and a value of zero for 2015 and 2016. The post-election period begins in 2017 to allow for the effects of active trading in Q4 2016 to show up in the portfolio allocations.

Table 5 presents the results from this regression. Column 1 indicates that a Republican majority team allocates 1.86% more net assets to equity following Trump's election than Democratic majority teams. We show that this result is robust to alternative specifications. In columns two and four, we instead compare all funds where we have matched at least one manager by the percentage of the fund team that is registered as a Republican. The results are quantitatively extremely similar. In the appendix, we show that this result is robust to alternative clustering. Finally, columns 3 and 4 provide suggestive evidence that funds finance these equity purchases in part by decreasing the size of their bond portfolio.

Republican's 1.86% incremental allocation to equity after Trump's election allows us to compare our results to those in Meeuwis et al. (2020). They find that after Trump's election, there is a wedge of about 0.25% in the equity share between retail investors in the most Republican counties and those in the least Republican counties. Our estimated effect is nearly eight times as large.

This implies that the effect of political beliefs is *an order or magnitude* larger for mutual fund managers than for households. This makes sense, as Giglio et al. (2021) show that the relevance of beliefs for trading is highly correlated with how frequently the investors normally trade. The modal mutual fund in our sample turns over almost half of its portfolio each year, while the modal investor in Meeuwis et al. (2020) doesn't trade as all. Conditioning the Meeuwis et al. (2020) results on the investor having traded at least once in the previous year, the pass through is much larger at approximately 1%, which is the same order of magnitude as our result but still meaningfully smaller, being only about half as large.

The magnitude of the effect we document is large not only relative to Meeuwis et al. (2020), but also relative to soft constraints in the mutual fund industry. Mutual funds have fund-specific benchmarks against which their performance is judged, and deviating meaningfully from the benchmark is a high-risk proposition. In particular, Morningstar ranks mutual funds in terms of their risk-adjusted past performance within a peer group of funds that have de-facto identical benchmarks. The resulting Morningstar star rating is the single largest determinant of mutual fund flows (Ben-David et al. (2019)), which in turn matter greatly for fund manager careers and compensation. That fund managers appear to actively trade on their political beliefs despite these career risks is part of what makes our result surprising. Nevertheless, the deviation between Republican and Democratic equity shares is never more than 5%, which we consider to be a reasonable upper bound for a plausible effect size.[14]

4.2 Portfolio Allocations: High and Low Beta Industries

We next investigate the composition of equity purchases by funds. In particular, we study whether when purchasing equity after Trump's election, Republican mutual fund managers purchase stocks more or less exposed to market and business cycle fluctuations. We follow classifications used by institutional investors (whose behavior we are trying to understand) to identify industries with greater or lesser exposure to the business cycle. Morningstar uses three delineations – "Cyclical" or high market beta, "Sensitive" or moderate market beta, and "Defensive" or low market beta – while MSCI uses only two delineations – "Cyclical" and "Defensive."

The results in Table 6 confirm the earlier results from the active trading regressions. After Trump's 2017 election, funds with a higher share of Republican managers increase their exposure to industries more exposed to aggregate market risk.

We carry out a similar exercise in Table 7. Instead of aggregating industries at the level of their market exposure, we instead focus on funds' allocations to these individual industries. The signs for the Republican specifications are uniformly positive on industries more exposed to business cycle fluctuations: basic materials, consumer cyclical, energy and financial services. The estimated coefficients are likewise negative for many of the industries arguably less exposed to business cycle fluctuations: utilities and technology.

[14] Recall that our sample also includes a smaller number of "mixed" funds, which have less stringent benchmarks and more scope to change their equity share over time.

4.3 Transient or Persistent Portfolio Effects?

We next investigate the long-term effects of the behavior we document. To do this, we run a regression from 2015 to 2020 with the following specification:

$$\begin{aligned}\text{Equity Share}_{it} \sim \beta_0 + \sum_{j=2017}^{2020} \beta_{j-2016} \mathbb{I}\left\{\text{Year} = \text{j}\right\} \times \mathbb{I}\left\{\text{Rep Majority}\right\}_{it} \\ + \boldsymbol{\gamma}' \text{Controls}_{it} + \nu_i + \nu_t\end{aligned} \tag{4.2}$$

The coefficients of interest are the interactions between year dummies and and indicator for whether the majority of the fund team is registered Republican as of December 2016. We also include the same set of controls as in equation (4.4) as well as time and fund fixed effects.

Table 8 presents the results. Interestingly, the size of the effect grows over time. For each of the years 2017, 2018, 2019 and 2020, mutual funds with higher Republican shares have a higher share of equity than in 2015 and 2016. This size of this difference grows over time as well, increasing from 2.459% more equity in 2017 to 3.727% more equity in 2020.

A natural question is whether this increase over time reflects continued active purchases by Republican funds over the course of Trump's Presidency, or simply that Republican funds purchased a significant amount of equity at the beginning of Trump's Presidency and then did not rebalance over time. In fact, regression evidence confirms that there was not statistically different net active purchases for either 2018 or 2019. Since the stock market performed well under Trump, it's likely that the increasing share of equity over 2018, 2019 and 2020 reflects a valuation effect.

A second question is the extent to which the fact that managers act on their political beliefs is value enhancing or destructive. Unfortunately, the time span over which we have the partisan affiliation of fund managers is limited. Thus, it is hard to make general statements about the effect of beliefs on the performance of these funds. However, for this single Presidential term, it would appear that political beliefs contribute to the outperformance of Republican funds, who purchased more net equity immediately prior to large stock market gains under Trump.

In a later section, we lean on analyst forecasts to assess whether partisanship results in less-accurate economic expectations. We show that partisan analysts make systematically less accurate forecasts than their non-partisan counterparts. This evidence is at least consistent with portfolio decisions made due to partisan bias

4.4 Active Purchases

Figure 1 provides clear visual evidence for differential net active equity purchases by partisans around elections. Republicans appear to actively purchase equity immediately following Trump's election. There is also visual evidence that Democratic fund managers anticipate Biden's election in 2020. We study this further through the lens of regression analysis to assess whether this is in fact statistically significant and whether this result survives the inclusion of important controls.

We first estimate the following regression:

$$\text{Active Net Purchases}_{it} \sim \beta\mathbb{I}\left\{\text{Post-Trump}\right\}_t \times \mathbb{I}\left\{\text{Rep Majority}\right\}_t + \boldsymbol{\gamma}'\text{Controls}_{it} + \nu_i + \nu_t \quad (4.3)$$

We display the results from this regression in Table 9. The dependent variable in this regression is the active net equity purchases of the i^{th} fund in period t. The independent variables are an indicator for the first two quarters of 2017 and an indicator for whether the fund has a majority Republican fund team. We also include both controls and fund and date fixed effects. In all regressions we control for log lagged total net assets, the lagged net expense ratio, the lagged turnover ratio, and the log of the fund's age. These are standard controls in the mutual fund literature.[15] In some specifications, the interaction between the fund style or benchmark and the election indicator is also included to test whether different benchmarks or fund styles motivate the trading around elections that we document.

We likewise estimate the regression

$$\text{Active Net Purchases}_{it} \sim \beta\mathbb{I}\left\{\text{Pre-Biden}\right\}_t \times \mathbb{I}\left\{\text{Rep Majority}\right\}_t + \boldsymbol{\gamma}'\text{Controls}_{it} + \nu_i + \nu_t \quad (4.4)$$

where the "Pre-Biden" indicator takes the value one in the third quarter of 2020.

Table 9 presents the regression results, studying the relation between fund manager partisanship and active net equity purchases around elections. The main variable of interest is the interaction of the election indicator and the indicator for whether the fund has a Republican majority fund team. The regression evidence is strongly supportive of our results from Morningstar. Republican majority funds *actively* purchased equity after the 2016 election. The increase we see in equity shares taken from Morningstar is not due a valuation effect, where the equity share mechanically increased due to strong stock performance. Instead, managers actively chose to increase their equity share.

In columns two and four, we also include controls interacting the fund benchmark (called the "objective code" by CRSP) with the relevant time dummy. This helps us rule out that

[15] See, for example, Pástor et al. (2020).

our results are driven by partisan sorting. For instance, it's possible that Republican fund managers disproportionately manage funds that primarily hold energy stocks, for instance. Perhaps they increased their purchases of equity because there was a positive shock to these industries. Conceivably our results could be driven by partisan sorting as opposed to partisan beliefs. This control rules out that story, even comparing funds within the same benchmark, Republican managers actively chose to purchase equity relative to Democrats. We find that for both 2020 and 2016 our results are unchanged when including controls for the objective code of the fund.

4.5 The Role of Fund Flows

Finally, we also investigate the role of fund flows. One concern is that differential trading could be driven by net inflows or outflows for funds with a relatively high or low share of Republicans or Democrats. To test this hypothesis, we estimate the following regression over the sample of Democratic and Republican majority funds

$$\text{Fund Flows}_{it} \sim \sum_t \gamma_t \mathbb{I}\left\{\text{Time} = \text{t}\right\} \times \text{Rep Majority}_{it} + \boldsymbol{\beta}'\text{Controls}_{it} + \nu_t + \nu_i \tag{4.5}$$

As in equation (4.4), we include controls for the log fund age, the log lagged fund size, the lagged turnover ratio and the lagged net expense ratio. The specification also includes date and fund fixed effects as well as controls for the fund style.

Figure 2 plots the γ_t coefficients on the interaction term between the partisan share and the time dummies. In most months prior to 2018, there is no systematic difference between the flows to Republican and Democratic majority funds. If anything, it is more common for Republican majority funds to receive greater inflows that Democratic funds. In 2018, we observe a consistent pattern where Democratic funds receive systematically higher net inflows in many months.

This pattern that starts in 2018 is likely due to an increased investor interest in sustainability. As we have already shown, Democratic-run funds tend to have higher sustainability ratings than otherwise similar funds run by Republicans. We observe the largest net inflows to Democratic funds in September 2020. This particularly extreme observation appears to be driven, in part, by the COVID-19 pandemic.

To formally test this, we explicitly control for net fund flows and estimate the following regression:

$$\begin{aligned}\text{Active Net Purchases}_{it} \sim\ & \beta_1 \mathbb{I}\left\{\text{Time Dummy}\right\}_t \times \mathbb{I}\left\{\text{Rep Majority}\right\}_t \\ & + \beta_2 \text{Flows} + \boldsymbol{\beta}_3'\text{Controls}_{it} + \nu_i + \nu_t\end{aligned} \tag{4.6}$$

This regression is identical to 4.4, except that we control for flows.

Table 10 presents the regression results and shows that the key coefficients of interest – the post-2016 election and pre-2020 election dummies interacted with the Republican majority indicator – retain their statistical significance, directional signs, and large magnitudes, even after controlling for net fund flows. That is, we still find that Democrats make smaller active purchases after Trump's 2016 election, while Republicans make smaller active purchases before Biden's 2020 election. This suggests that beliefs, not fund flows, are in fact the driving force behind the changes in the portfolio of these funds.

Although it is outside of the scope of the paper to do a full analysis of why funds with different manager partisan affiliations experienced different flows, we offer one possible explanation. There is a pronounced statistical correlation between measures of the ESG rating of the fund and the partisan makeup of the mutual fund team. We show this in Table 12. We study two key measures of the sustainability of the fund's portfolio. The first is the number of globes that Morningstar assigns to the fund. The second is whether the fund receives a "low carbon designation." Both the sustainability globes and the low carbon designation are salient mutual fund labels that are known to significantly drive fund flows (Hartzmark and Sussman (2019); Ceccarelli et al. (2023)). In Figure 3, we show screenshots from Morningstar.com fund pages to emphasize how salient these labels are to investors.

Table 12 shows that there is a pronounced negative statistical relationship between the number of sustainability globes that a fund has and the partisan makeup of the fund team. A fund with a fully Republican team, on average, has approximately 0.26 fewer sustainability globes. In this regression we include both objective code and date fixed effects. We estimate a logit regression in the second column where the dependent variable is an indicator for whether Morningstar assigned the fund a "low carbon designation." We find that, at most, a fully Republican mutual fund team results in a 15% lower probability of being classified as a low carbon fund. Based on these results, we conjecture that one explanation for different flows into mutual funds of different partisan compositions is that there are differential inflows into ESG versus non-ESG funds.

4.6 Synthesis and Mechanism

We show that Democratic managers increase risk-taking – actively purchasing more net equity and tilting their portfolios towards higher beta industries – around Democratic presidential victories in 2012 and 2020, while Republican managers increase risk-taking the same way around the Republican presidential victory in 2016. These systematic differences in active trading show up in their portfolios as meaningful adjustments to the share of the portfolio allocated to equity and the share of the portfolio allocated to cyclical (high market beta) industries relative to defensive (low market beta) industries. These effects from active trading are persistent over time, that is, managers do not undo their active tilts and modified portfolio allocations following the elections. In addition, the differential partisan risk-taking around elections is driven by neither valuation effects nor differential net fund flows, but rather, by deliberate and voluntary active trading that is highly differentiated by partisanship.

A popular belief in the asset pricing and politics literature is that partisanship matters because Democrats are more risk-averse than Republicans (Pástor and Veronesi (2020)). If heterogeneous risk-aversion was responsible for our results, we would expect a uniform partisan response – Republicans taking more risk before all elections. That the relation between partisanship and risk-taking flips when the political party winning the presidency flips rules out this mechanism.

Instead, the patterns we document appear widely consistent with political beliefs driving active trading. The pattern – Democrats appear more optimistic when a Democrat wins the presidency and Republicans appear more optimistic when a Republican wins the presidency – is consistent with the pattern in Dagostino et al. (2020) and Kempf and Tsoutsoura (2021). This provides new evidence that partisanship affects economic decision-making, and in the most unlikely setting – for fund managers that are sophisticated and well-incentivized and receive frequent performance feedback.

4.7 Results from Analyst Expectations

We next turn to our sample of Democratic and Republican stock analysts to better understand if, and how, partisanship shapes beliefs. We start by investing changes in analyst-specific forecasts in the 12 months before and after Trump's 2016 election. We estimate the following regression:

$$\text{EPS}_{ist} = \beta \mathbb{I}\left\{\text{Post-2016 Election}\right\}_t \times \mathbb{I}\left\{\text{Republican}\right\}_i + \eta_t + \nu_{is} \tag{4.7}$$

This difference-in-difference specification regresses the analyst (i) by stock (s) at time (t) earnings-per-share forecast on the interaction of whether or not the analyst is a Republican with an indicator for the post-2016 election period. We also include analyst-by-stock level fixed effects (ν_{is}) and date fixed effects (ν_t). We estimate this regression over the sample of analysts that we classify as Republican or Democratic using the leading and trailing twelve months around the 2016 Presidential election. We use two years of data due to the relative infrequency with which analysts update their forecasts.

The results in Table 13 indicate that, after Trump's election, Republican analysts substantially revised their stock-level earnings forecasts upwards. This effect was statistically most pronounced over the next two fiscal years (2017 and 2018). In percentage terms, earnings forecasts were approximately 2% higher for Republican as opposed to Democratic analysts in fiscal years 2017 and 2018.

We also investigate the change in earnings expectations at horizons longer than the next two fiscal years. The point estimate of the estimated coefficient for 2019 is nearly the same as for 2017 and 2018. However, the t-stat is less than one. We likewise observe a positive and large in magnitude coefficient for 2020, but which is again insignificant. We are unsure whether statistical insigificance for these coefficients reflects a lack of an underlying economic relation or is due to a lack of power as there are many fewer observations for these years.

To shed light on this, we also study the long-term earnings growth (LTG) forecasts made by the same analysts. We again find no statistically significant relationship and, in fact, the coefficient is negative although very close to zero. In sum, we view our evidence as consistent with partisan beliefs affecting cash flow expectations over the next two years with no effects at longer horizons.

In light of these results, a natural question is whether this effect is specific to Trump's election or more general. To understand the role of expectations outside of the 2016 election, we study the effect of political alignment between analysts and the President. We estimate the following regression:

$$\mathrm{EPS}_{ist} = \beta \mathbb{I}\left\{\mathrm{Aligned}\right\}_{it} + \eta_t + \nu_{is} \tag{4.8}$$

As before, i indexes analyst, s stock and t time. $\mathrm{Aligned}_{it}$ is an indicator that takes the value one if the analyst's party is the same as the current President and zero otherwise. Based on the counts shown in Table 4, we focus on forecasts for the next three fiscal years. We estimate this regression on the sample of analysts that we match to Republican or Democratic voter registration records.

The results in Table 14 are broadly consistent with the results from the diff-in-diff around Trump's election. We again find that the effect of partisanship on EPS forecasts is most pronounced two fiscal years ahead, with a statistically insignificant effect three fiscal years ahead. In the Appendix (Table 20), we interact our "alignment" indicator with the year that the forecast was made. Our interpretation of the results in this table is that these effects are strongest early in a President's term and weaken over time.

In our study of partisan mutual fund managers, we find that, after Trump's 2016 election, Republican fund teams disproportionately increase their holdings in growth-sensitive industries, especially basic materials and consumer cyclicals. Here, we examine whether Republican stock analysts similarly raise their earnings forecasts for companies in these same growth-sensitive industries.

Given the relatively small number of analysts and infrequency with which analysts update their forecasts, it is difficult to investigate heterogeneity by industry. Nevertheless, we conduct some analysis by industry, which we report in the appendix. In Table 22, we find evidence that Republican stock analysts differentially raise their earnings forecasts for industries exposed to economic conditions. Notice that these are the same industries that Republican mutual fund managers increase their equity exposure to. While in the fund manager regressions, we're able to use portfolio allocations for every fund, with stock analyst forecasts, we quite literally need to divide the sample into three parts. As such, these regressions are extremely underpowered. Nevertheless, these results are consistent with the idea that politically aligned individuals are more optimistic about economic growth under their favored party, and therefore also expect that companies most sensitive to economic growth will perform particularly well.

In Table 15, we investigate whether partisanship affects forecast accuracy. To do this, we compare the accuracy of analysts that we successfully match to a party affiliation to analysts who appear in the registration records, but do not belong to either the Democratic or Republican party by estimating the following regression:

$$\text{Forecast Error}_{ist} = \mathbb{I}\left\{\text{Post-Trump}\right\}_t \times \mathbb{I}\left\{\text{Partisan}_i\right\} + \nu_t + \nu_{st} \tag{4.9}$$

We define the forecast error as the absolute value of the deviation of the forecast value from the realized value. Since both alignment and non-alignment plausibly affects the forecasts of partisans, to test forecast accuracy overall, the appropriate test it so compare those most likely to have their beliefs affected by partisan politics versus those least likely, i.e. partisan versus non-partisan.

We show that, after Trump's election, the forecast accuracy of partisan analysts is systematically worse than that of non-partisan analysts, particularly for forecasts made for 2017. Recall that this was exactly the horizon over which we observed that partisanship most strongly affected earnings forecasts.

We also verify that the signed forecast error goes in the "right" direction. Relative to Democracts, Republicans systematically have forecast errors that are too optimistic. These results are displayed in the second set of retuls in Table 15. For these regressions the dependent variable is the difference between the forecast and the realized value. We then regress the forecast error on a post-Trump indicator interacted with a Republican indicator. We estimate this regression on the sample of analysts identified as either Republicans or Democrats. In all specifications we include both time and stock by analyst fixed effects.

Taken together, we find that politically aligned stock analysts have more optimistic earnings forecasts, and that these optimistic earnings forecasts are more inaccurate. This suggests that partisanship contributes to a rosy – but incorrect – lens through which these strongly incentivized finance professionals view the world.

As the accuracy of forecasts is much easier to assess than the "correctness" of active trading, we believe our analyst forecast results provide context for understanding our mutual fund manager trading results. In particular, that biased partisan beliefs contribute to less accuracy for stock analysts suggests that fund managers who trade on their political beliefs are also doing so sub-optimally. Unfortunately, we don't have a sufficiently long time-series to study whether mutual fund managers who act on biased partisan beliefs perform worse. Nevertheless, we think our analyst results are useful for suggesting that partisan beliefs generally result in less accurate return expectations.

5 Model

To interpret our results, we build a model in the spirit of Koijen and Yogo (2019) that incorporates both asset demand and subject partisan beliefs.[16] Following Koijen and Yogo we write asset demand as

$$\theta_{p,n,t} = \begin{cases} \frac{\hat{\theta}_{p,n,t}}{1+\sum_{m=1}^{N} \hat{\theta}_{p,m,t}} & n = 1, \dots, N \\ \frac{1}{1+\sum_{m=1}^{N} \hat{\theta}_{p,m,t}} & n = 0 \end{cases} \tag{5.1}$$

$\theta_{p,n,t}$ denotes the portfolio weight for partisan p in asset n at time t. There are N risky assets. The zeroth asset is the risk-free asset. All assets are in constant supply. Asset demand is a function of subjective expected returns ($\mu_{p,n,t}$) and latent demand $\left(\epsilon^{D}_{p,n,t}\right)$.

$$\hat{\theta}_{p,n,t} = \exp\left[\kappa \mu_{p,n,t} + \epsilon^{D}_{p,n,t}\right], \qquad n = 1, \dots, N \tag{5.2}$$

Both latent demand and subjective expected returns are allowed to vary by partisanship. For simplicity we keep the semi-elasticity of demand with respect to subjective expected returns, κ, constant across partisans.

$$\mu_{p,n,t} = \frac{\tilde{\mathbb{E}}_{p,t}\left[P_{n,t+1} + D_{n,t+1}\right]}{P_{n,t}} - R^{f}_{t} \tag{5.3}$$

[16]Our treatment closely follows Koijen and Yogo (2019) and also Chaudhry (2022). The key difference is that we allow for heterogeneity in wealth and expected returns across partisans. In addition, we use a novel specification of the dividend growth process motivated by our findings in Section 4.7.

$\tilde{\mathbb{E}}_{p,t}$ denotes the expectation under the subjective measure of partisan p given the time t information set. We analyze the change in asset demand and prices before and after the resolution of presidential elections. We denote the instant prior to the election as t^- and following instant as t^+.

Lemma 1 (Linearized Demand Curve)**.** *Denote percentage changes from t^- to t^+ in the quantity of shares demanded by partisan p as $\Delta q_{p,n,t}$, current price as $\Delta p_{n,t}$, expected next period price as $\Delta p^e_{n,t+1}$, expected next period dividends as $\Delta d^e_{p,n,t+1}$ and changes in other demand shocks as $\Delta\epsilon_{p,n,t}$. δ is the average price-dividend ratio. Linearizing portfolio weight demand function around $\left(\Delta p_{n,t}, \Delta p^e_{n,t+1}, \Delta d^e_{p,n,t+1}, \Delta\epsilon_{p,n,t}\right) = (0,0,0,0)$ yields the following demand curve for partisan p and stock n:*[17]

$$\Delta q_{p,n,t} = -\left(1+\kappa\left(1+\delta\right)\right)\Delta p_{n,t} + \kappa\left[\delta\Delta d^e_{p,n,t+1} + \Delta p^e_{n,t+1}\right] + \Delta\epsilon_{p,n,t} \tag{5.4}$$

In this expression, expected changes in prices tomorrow $\left(\Delta p^e_{n,t+1}\right)$ do not vary across partisans. While partisans disagree about future dividend growth, the extent of partisan differences in beliefs are common knowledge. Partisans of both sides understand the equilibrium effect of these beliefs on asset demand and prices and fully comprehend what prices will be tomorrow.

The key point of departure from earlier work follows from the imposition of market clearing. Because asset demand can vary across partisans, the effect of differences in subjective expected returns across partisanship on prices will critically depend on the wealth differences across partisans. We write time t market clearing as

$$W_{R,n,t}\Delta q_{R,n,t} + W_{D,n,t}\Delta q_{D,n,t} = 0 \tag{5.5}$$

Market clearing enforces that if there is net buying demand from Republicans in a stock, then Democrats must be sellers. The total change in shares held across partisans must be zero from each period to the next. $W_{p,n,t}$ is the wealth weight of partisans in a particular stock n. As before, $\Delta q_{p,n,t}$ is the change in quantity demanded in percentage terms for partisans p for stock n.

Lemma 2 (Price Change)**.** *Imposing market clearing (Equation 5.5) yields the following*

[17] There is no new economic content in Lemma 1 relative to earlier work. While we have allowed for heterogeneity across partisans, this has only changed notation. The linearized demand curve, conditional on partisanship, is the same as the aggregate linearized demand from Chaudhry (2022).

equilibrium price change:

$$\Delta p_{n,t} = \frac{\kappa^{-1}}{1+\phi}\Delta p^{e}_{n,t+1} + \frac{\delta}{1+\phi}\left(W_{R,t}\Delta d^{e}_{R,n,t+1} + W_{D,t}\Delta d^{e}_{D,n,t+1}\right) + \frac{1}{1+\phi}\left(W_{R,n,t}\Delta\epsilon_{R,n,t} + W_{D,n,t}\Delta\epsilon_{D,n,t}\right) \tag{5.6}$$

where $\phi \equiv \kappa^{-1} + \delta$.

This expression expresses the change in prices as the sum of three components: the wealth-weighted average shock to expected dividends, the shock to expected prices and the shock to the wealth-weighted average of latent demand. A shock to latent demand, average expected dividend growth or expected next-period price will increase the price today. To operationalize this equation, we provide an alternative formulation that expresses the change in price today purely in terms of changes in expected dividends and latent demand. To this end, we forward iterate Equation 5.6 to derive the following lemma.

Lemma 3 (Belief Shock Effect on Prices)**.** *let* $\Delta d^{e}_{n,t+s}$ *represent the percentange change between* t^{-} *and* t^{+} *in the expected dividend in period* $t+s$ *and let* $\Delta\epsilon^{e}_{n,t+s}$ *represent the change in the expected residual demand shock in* $t+s$*. We have this expression for price change today*

$$\Delta p_{n,t} = \delta\sum_{s=1}^{\infty}\left(\frac{1}{1+\phi}\right)^{s}\left(W_{R,t+s-1}\Delta d^{e}_{R,n,t+s} + W_{D,t+s-1}\Delta d^{e}_{D,n,t+s}\right) + \frac{1}{\kappa}\sum_{s=0}^{\infty}\left(\frac{1}{1+\phi}\right)^{s+1}\left(W_{R,t+s-1}\Delta\epsilon^{e}_{R,n,t+s} + W_{D,t+s-1}\Delta\epsilon^{e}_{D,n,t+s}\right) \tag{5.7}$$

Motivated by the empirical results in 4.7, we use the following expression to model belief differences between aligned and non-aligned mutual fund managers. In Table 13 we showed that Republican analysts shifted increased their earnings expectations over a two-year horizon. The specification below captures this by allowing investor subjective expectations of dividend payouts to likewise differ over the next two years.

$$\tilde{\mathbb{E}}_{p,t}\left[D_{n,t+s} \mid \mathbb{I}\left\{\text{Aligned}\right\}_{p,t+s}\right] = \tilde{\mathbb{E}}_{p,t}\left[D_{n,t+s} \mid \mathbb{I}\left\{\text{Non-Aligned}\right\}_{p,t+s}\right] + \chi_{n},, \quad s \in \{1,2\} \tag{5.8}$$

χ_n is the stock-specific "partisan belief wedge", as it is the wedge in beliefs about earnings between aligned and non-aligned partisans. χ_n captures that aligned partisans have more optimistic beliefs about near-term cash flows. The magnitude of χ_n encodes the extent of this over optimism. To operationalize our model, we derive an expression for the change in asset prices given a surprise shift in electoral control.

Proposition 1 (Partisan Shock)**.** *The change in prices given a surpise shift in political alignment is approximated by*

$$\Delta p_{n,t} \approx \Phi \left(W_{R,n,t} - W_{D,n,t}\right) \chi_n + e_{n,t} \tag{5.9}$$

where

$$\Phi \equiv \delta \sum_{s=1}^{3} \left(\frac{1}{1+\phi}\right)^{s} \text{ and } e_{n,t} \equiv \frac{1}{\kappa} \sum_{s=0}^{\infty} \left(\frac{1}{1+\phi}\right)^{s+1} \left(W_{R,t+s-1}\Delta\epsilon^{e}_{R,n,t+s} + W_{D,t+s-1}\Delta\epsilon^{e}_{D,n,t+s}\right)$$

This expression is intuitive. The effect on prices depends on the three components: the partisan belief distortion, the difference in the wealth of Republicans and Democrats, a scaling factor and changes in latent demand.

6 Conclusion

We show that the political beliefs of mutual fund managers matter for high stakes investment decisions. Mutual fund teams respond in different ways to the same political event depending on whether the fund team has more Republicans or Democrats. During the 2012 and 2020 elections when a Democratic presidential candidate won, Democratic teams purchased more equity than Republican teams. This pattern reversed during the 2016 election when a Republican presidential candidate won, as Republicans purchased more equity than Democrats. These effects not only persist but also grow over time.

These findings point to a large and heretofore unrecognized role for mutual fund managers' political beliefs. Our results are inconsistent with time-varying and heterogeneous risk aversion driving differences in Republican and Democratic-led mutual funds, which has traditionally been a focus of the research at the intersection of asset pricing and political economy. Instead, we view our results as strongly consistent with political beliefs driving the actions of fund managers. Even apart from politics, we are among the first to show that beliefs matter at all for the actions of institutional investors.

Evidence from this and other work indicate that politics plays a pervasive role in the expectations formation and actions of economic agents. As the United States is forecast to become even more politically polarized, it is likely that the effects we document will only become more important. Importantly, this research and other recent studies suggest that these effects are not limited to households. Corporate boards and mutual fund teams, among other sophisticated economic agents, are strongly influenced by their political beliefs. We anticipate that further research will document far-reaching effects as these major institutions take actions based on the political beliefs of their managers and boards.

From the perspective of investors, this raises important questions about the role of politics in intermediation. Managers' beliefs affect the trading and performance of the fund. Investors may want to exercise caution if their own beliefs are different from those of the fund team. This principal-agent problem is compounded by the lack of information available to investors, particularly retail investors. Households cannot account for factors about which they have no information. As it stands, households have no reasonable means to investigate the partisanship of fund managers.

Insofar as households do differentially invest in Republican- or Democratic-led mutual funds, this is likely due to alignment between fund manager partisanship and fund-level measures of sustainability. Our results suggest that sustainability may have given rise to clientele effects whereby Republican households disproportionately invest in Republican-led mutual funds and Democratic households do likewise. The existence and aggregate implications of any such clientele effects is fertile ground for future research.

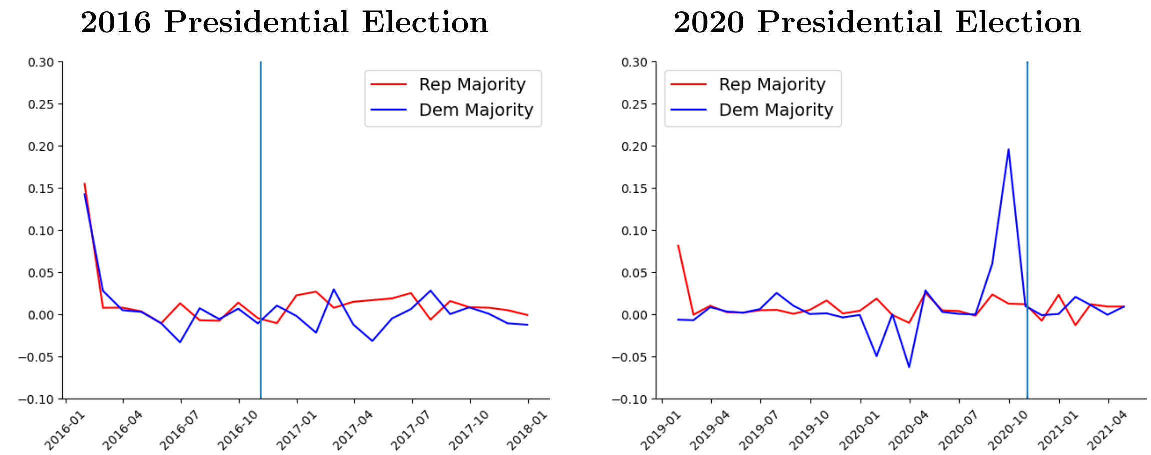


Figure 1. Active Net Purchases by Partisanship of Fund Team. This figure plots monthly average active net equity purchases as a percent of total assets for funds with majority Republican and majority Democratic fund teams. They vertical gray lines denote the 2016 and 2020 Presidential elections, respectively. The same figure for the 2012 election is provided in the appendix.

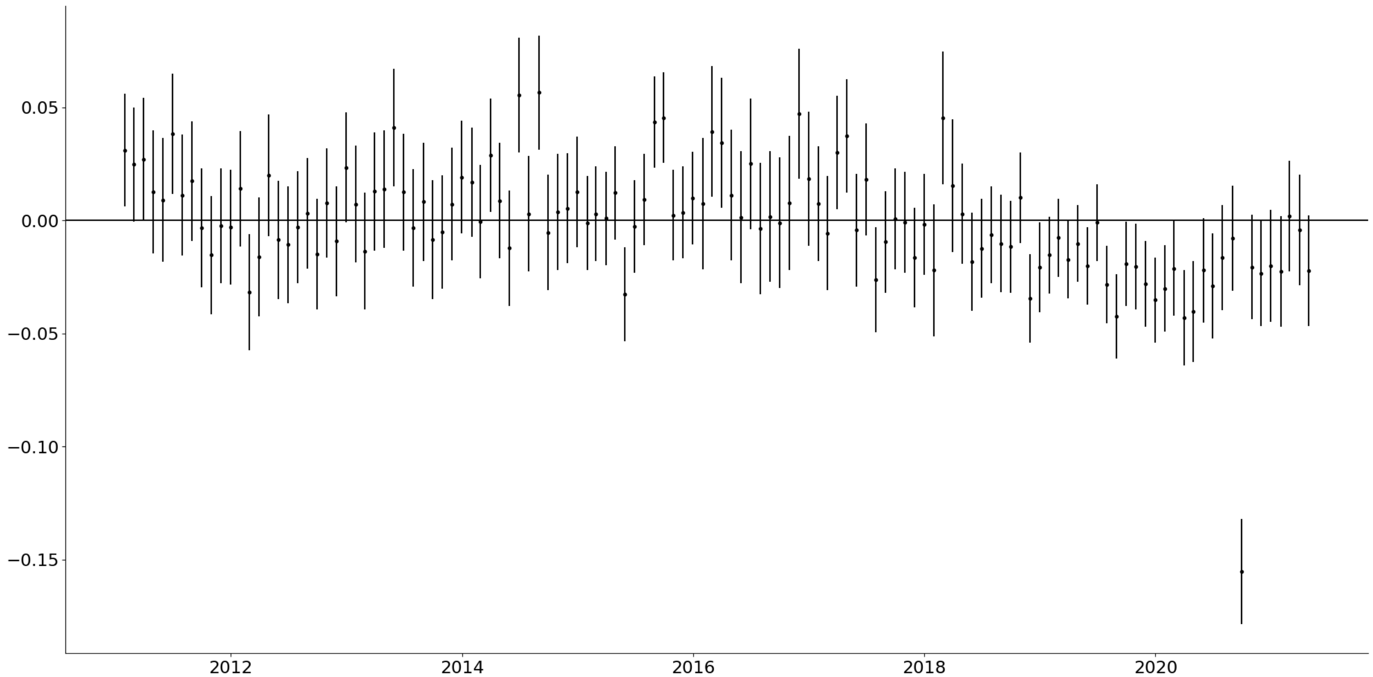
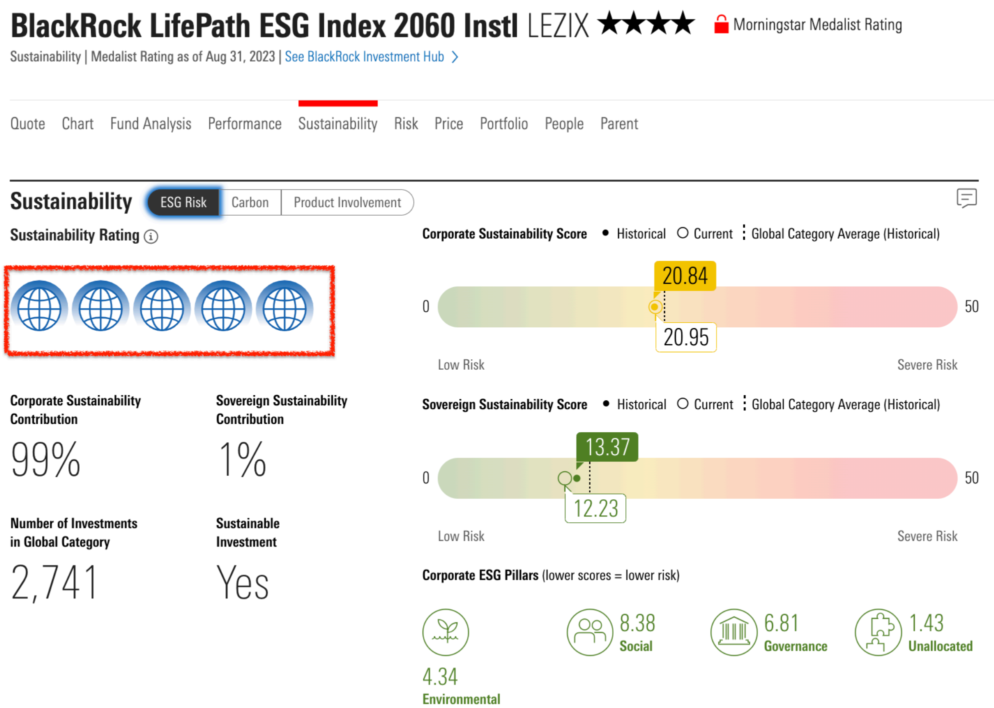


Figure 2. Incremental Fund Flows by Partisanship. This figure plots the monthly fund flows for entirely Republican teams versus entirely Democratic teams. Mechanically, we estimate this by regressing net fund flows on the interaction between monthly time dummies and an indicator for a majority Republican fund team, also including date fixed effects, fund fixed effects, and controls for the lagged log size of the fund, the log fund age, the lagged turnover ratio, and the lagged net expense ratio. Note that fund flows are measured as a percent of net assets, with the y-axis units in decimal form, i which 0.04 corresponds to 4% of net assets. The sample consists of active US equity mutual funds with at least $10 million of net assets and at least one matched manager. The shaded bands denote 95% confidence intervals, with standard errors clustered by date.



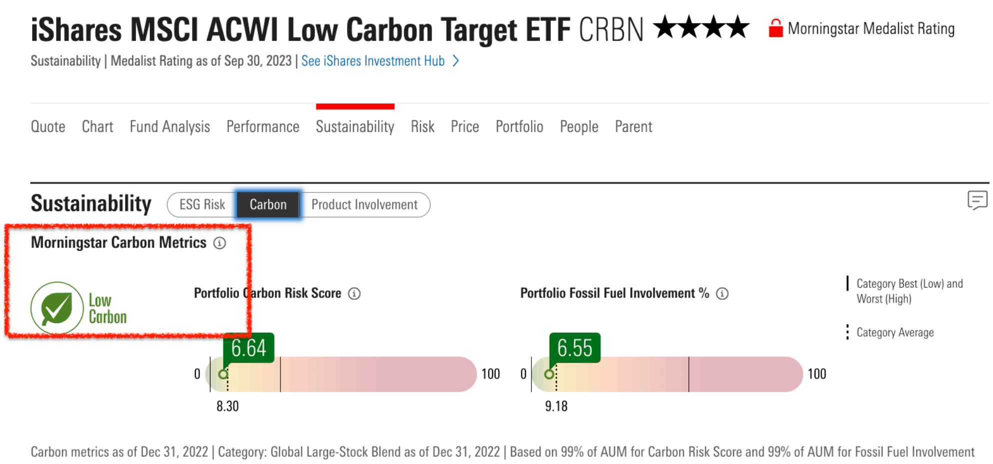


Figure 3. Morningstar's Salient Sustainability Labels. This figure shows images of Morningstar's salient sustainability labels, namely the sustainability globe rating in the top panel and the low carbon designation in the bottom panel. Given the prominence with which these ratings are displayed on Morningstar's fund pages, these strong sustainability indicators are highly coveted by mutual funds. These pages can be viewed at https://www.morningstar.com/funds/xnas/lezix/sustainability and https://www.morningstar.com/etfs/arcx/crbn/sustainability, although given the dynamic nature of these ratings, it is hard to predict whether these scores will persist as shown here.

Table 1
Comparing Voter Registration Data and Political Contributions

This table compares fund manager partisanship between voter registration data (self-reported party affiliation) and the Federal Election Commission (FEC) database (federal political contributions). Panel A compares classifications of partisans – Democrats and Republicans – between the two sources, and Panel B compares classifications of non-partisans – those that appear to be neither Democrats nor Republicans – between the two sources. For Democrats and Republicans, the contributions correctly (incorrectly) identify the manager's partisanship if the registered party is the same (not the same) as the party receiving the majority of political contributions. Notably, 24 registered Republicans and 12 registered Democrats with only contributions to non-partisan committees (which give less than two-thirds of contributions to candidates from a single political party) are omitted from consideration as the registered party affiliation is neither correct nor incorrect. For other political parties and unregistered voters, we say they are in fact non-partisan if the share they contribute to both Democrats and Republicans (instead of non-partisan committees) is less than 75%. Managers are considered "big" contributors if their total contributions are at least $1000.

	Panel A. Partisans		
	Correct	Incorrect	% Correct
All Republicans	50	17	75
Big Republicans	38	13	75
All Democrats	38	8	83
Big Democrats	24	8	75
	Panel B. Non-Partisans		
	Non-Partisan	Partisan	% Non-Partisan
All Other	8	11	42
Big Other	6	8	43
All Unregistered	32	27	54
Big Unregistered	26	19	58

Table 2
Portfolio Summary Statistics by Year

This table reports summary statistics, by year, for the funds in our matched manager sample. The main variables of interest are the number of unique funds, the net equity share of net assets (in percentage points), the net cash share of net assets (in percentage points), the net bond share of net assets (in percentage points), and the fund's total net assets (in tens of millions of dollars). For the equity, cash, bond, and total net assets variables, we also provide cross-sectional means and medians, and for the total net assets, we also provide the cross-sectional sum. All values are as of Q4, that is, December 31st of each year.

		Equity Net		Cash Net		Bond Net		Total Net Assets		
	Count	Mean	Median	Mean	Median	Mean	Median	Mean	50%	Sum
2010	14	77.96	96.44	2.83	2.86	4.63	0.0	292.15	69.05	4090.09
2011	184	84.14	97.38	2.49	1.6	4.2	0.0	125.04	44.42	23006.74
2012	254	87.69	97.42	2.5	1.74	4.68	0.0	126.81	44.56	32209.08
2013	284	89.12	97.2	3.34	2.0	4.41	0.0	174.92	65.47	49678.32
2014	251	84.52	96.16	3.17	2.33	5.33	0.0	224.23	79.27	56282.09
2015	200	85.26	96.79	3.17	2.11	5.13	0.0	213.75	58.84	42750.9
2016	220	90.87	96.92	3.39	2.14	4.62	0.0	224.74	62.39	49442.61
2017	178	90.24	97.68	2.95	1.59	6.29	0.0	202.53	49.42	36049.84
2018	315	85.5	97.45	2.63	1.64	9.04	0.0	161.07	37.16	50737.04
2019	428	86.78	97.36	3.18	1.98	8.35	0.0	182.79	45.99	78236.22
2020	342	88.69	98.06	2.67	1.59	6.15	0.0	212.57	61.56	72699.78

Table 3
Manager Counts by Year

This table reports the number of matched managers each year by their partisanship. The five classifications are registered Republican, registered Democrat, no party information available, registered independent, and other political party aside from Republican, Democrat, or independent. For the matched managers without available party information, we can tell that the individual has voted, but a party is not listed and we cannot infer the manager's partisan affiliation.

	2012	2013	2014	2015	2016	2017	2018	2019	2020
Republican	408	387	400	389	399	380	377	358	315
Democrat	181	177	189	185	196	198	200	190	179
No Party Information	195	184	209	199	214	216	211	217	215
Registered Independent	44	41	47	41	41	44	38	37	33
Other	31	34	34	34	40	34	35	37	33
Total	859	823	879	848	890	872	861	839	775

Table 4
Earnings-Per-Share (EPS) Summary Stats

This table displays summary statistics for Earnings-Per-Share forecasts made by analysts in I/B/E/S. The vertical axis corresponds to the fiscal year the forecast was made, while the horizontal axis refers to the fiscal year the forecast concerns. All forecasts are annual-level forecasts.

Year	Forecast Year: Statistic	2010	2011	2012	2013	2014	2015	2016	2017	2018	2019	2020	2021	2022
2010	Count	784	10259	7433	2350	852	408	0	0	0	0	0	0	0
	Mean	1.74	1.72	2.4	3.08	2.96	3.24							
	Standard Dev.	1.88	12.1	12.66	12.02	1.87	2.09							
	Median	1.29	1.01	1.64	2.6	2.66	2.9							
2012	Count	0	0	1187	14110	10371	3639	1133	605	0	0	0	0	0
	Mean			2.84	2.77	4.42	4.26	4.54	5.51					
	Standard Dev.			8.26	14.74	29.43	15.8	8.16	14.52					
	Median			1.67	1.41	2.05	3.07	3.56	3.4					
2014	Count	0	0	0	0	1226	16035	11773	4335	1405	660	0	0	0
	Mean					3.07	3.32	4.42	5.62	5.98	7.09			
	Standard Dev.					5.11	16.84	20.45	17.37	9.09	12.31			
	Median					2.21	1.68	2.47	3.74	4.12	4.46			
2016	Count	0	0	0	0	0	0	1600	18360	13495	4785	1101	594	0
	Mean							2.83	2.48	4.03	5.31	5.51	7.0	
	Standard Dev.							4.95	5.62	11.79	14.09	5.75	7.05	
	Median							1.95	1.7	2.44	3.75	4.19	4.82	
2018	Count	0	0	0	0	0	0	0	0	1511	18603	13957	5071	1260
	Mean									3.99	3.7	4.72	6.4	6.73
	Standard Dev.									6.92	8.14	9.41	8.09	6.25
	Median									2.93	2.27	3.04	4.85	5.12
2020	Count	0	0	0	0	0	0	0	0	0	0	1292	14646	10408
	Mean											3.26	2.97	4.32
	Standard Dev.											4.45	6.83	9.09
	Median											2.34	1.62	2.66
2022	Count	0	0	0	0	0	0	0	0	0	0	0	0	87
	Mean													2.71
	Standard Dev.													2.82
	Median													1.69

Table 5
Portfolio Allocations to Equity and Debt

This table reports slope coefficients estimated from regressions of stock and bond portfolio allocation shares on fund characteristics and controls. The dependent variables are the share of net assets in equity and bonds. For the first and third specification, we compare the reaction of funds with a Republican and Democratic majority on the fund team. For the remaining specifications, we study the reaction of all funds by the share of the fund team identified as Republican. All specifications include fund and date fixed effects.

Dependent Variables:	Equity Net		Bond Net	
Model:	(1)	(2)	(3)	(4)
Variables				
Post 2016 Election × Rep. Majority	1.860***		-0.0028	
	(4.071)		(-0.0235)	
Post 2016 Election × Rep. Share		1.781***		-0.9656**
		(4.434)		(-2.247)
Fixed-Effects				
Date	Yes	Yes	Yes	Yes
Fund ID	Yes	Yes	Yes	Yes
Fit Statistics				
Observations	3,244	16,021	3,244	16,021
R^2	0.89176	0.92723	0.96665	0.90080
Within R^2	0.00814	0.00279	0.00164	0.00882

Clustered (Date) co-variance matrix, t-stats in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 6
Portfolio Allocations to Industries by Exposure to Business Cycle

This table reports slope coefficients estimated from regressions of industry portfolio allocation shares on fund characteristics and controls. The dependent variables are the share of net assets in each of the following categories: cyclical (most exposed) industries, defensive (least exposed) industries, and sensitive (moderate exposure) industries. We consider classifications according to both Morningstar and MSCI. Morningstar defines cyclical industries as basic materials, consumer cyclical, financial services, and real estate; defensive industries as consumer defensive, healthcare, and utilities; and sensitive industries as communication services, energy, industrials, and technology. MSCI defines cyclical industries as consumer cyclical, financial services, real estate, industrials, technology, basic materials, and communication services; and defensive industries as consumer defensive, energy, healthcare, and utilities. Note that MSCI does not have a sensitive industry categorization, and that Morningstar's cyclical and defensive industry classifications are strict subsets of the MSCI versions.

Classification	Morningstar			MSCI	
Dependent Variables:	Cyclical	Defensive	Sensitive	Cylical	Defensive
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
Post 2016 Election × Rep. Majority	1.412***	0.2298	-0.0770	1.116**	0.4491**
	(3.035)	(1.001)	(-0.2090)	(2.155)	(2.167)
Fixed-Effects					
Date	Yes	Yes	Yes	Yes	Yes
Fund ID	Yes	Yes	Yes	Yes	Yes
Fit Statistics					
Observations	3,244	3,244	3,244	3,244	3,244
R^2	0.90409	0.95616	0.92336	0.91167	0.95396
Within R^2	0.00846	0.02142	0.00926	0.00235	0.01188

Clustered (Date) co-variance matrix, t-stats in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 7
Portfolio Allocations to Individual Industries

This table reports slope coefficients estimated from regressions of industry portfolio allocation shares on fund characteristics and controls. The dependent variables are the share of net assets in each of the following industries: basic materials, communications, consumer cyclical, consumer defensive, healthcare, industrials, real estate, technology, energy, financial services, and utilities.

Industry:	*Basic Materials*	*Communications*	*Consumer Cyclical*	*Consumer Defensive*	*Healthcare*	*Real Estate*	*Technology*	*Energy*	*Financial Services*	*Utilities*
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Variables										
Post 2016 Election × Rep. Majority	1.157***	0.2712***	0.3720	0.1909	0.3882*	-0.3441***	-1.257***	0.2194*	0.2270	-0.3493***
	(4.974)	(3.444)	(1.324)	(0.9299)	(1.825)	(-3.197)	(-5.346)	(2.055)	(1.139)	(-4.105)
Fixed-Effects										
Date	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fund ID	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit Statistics										
Observations	3,244	3,244	3,244	3,244	3,244	3,244	3,244	3,244	3,244	3,244
R^2	0.81626	0.89020	0.85486	0.90893	0.97291	0.92655	0.93873	0.94632	0.95637	0.98213
Within R^2	0.00617	0.01227	0.02029	0.02636	0.00699	0.00742	0.01314	0.01823	0.02804	0.01382

Clustered (Date) co-variance matrix, t-stats in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 8
Long Term Effects

This table reports slope coefficients estimated from the regression of the fund's share of net assets in stock on fund characteristics and controls. Note that the dependent variable is from Morningstar and so includes accurate classifications of all holdings, both US and non-US stocks. There are two other points of note. First, the time horizon is quarterly for the 2015 to 2020 time period. Second, the sample consists of active US equity mutual funds with at least $10 million of net assets and that we identify as either being majority Republican or Democratic.

Dependent Variable:	Equity Net
Model:	(1)
Rep. Majority × $\mathbb{I}\{2017\}_t$	1.678^{***}
	(3.786)
Rep. Majority × $\mathbb{I}\{2018\}_t$	2.459^{***}
	(6.045)
Rep. Majority × $\mathbb{I}\{2019\}_t$	3.961^{***}
	(7.375)
Rep. Majority × $\mathbb{I}\{2020\}_t$	3.727^{***}
	(3.910)
Fixed-Effects	
Date	Yes
Fund ID	Yes
Fit Statistics	
Observations	4,901
R^2	0.86023
Within R^2	0.01400

Clustered (Date) co-variance matrix, t-stats in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 9
Active Net Equity Purchases

This table reports slope coefficients estimated from regressions of active net equity purchases on fund characteristics and controls. Active net equity purchases, as a percent of assets, is reported in decimal form, where 0.02 means 2%. Post-2016 is an indicator variable that takes the value one over the first two quarters of 2017. Pre-2020 is an indicator that takes the value one in the third quarter of 2020. All specifications include fund and date fixed effects. The second and fourth specifications include dummies for the fund's objective code interacted with the relevant time dummy.

Dependent Variable:	Active Net Equity Purchases			
Model:	(1)	(2)	(3)	(4)
Variables				
Rep. Majority × Post-2016	0.0401**	0.0401**		
	(2.588)	(2.393)		
Rep. Majority × Pre-2020			-0.1267***	-0.1107***
			(-18.25)	(-11.36)
Controls				
Obj. Code x Time Dummy	N	Y	N	Y
Fixed-Effects				
Fund ID	Yes	Yes	Yes	Yes
Date	Yes	Yes	Yes	Yes
Fit Statistics				
Observations	1,719	1,719	1,751	1,751
R^2	0.34851	0.35306	0.22112	0.23575
Within R^2	0.04139	0.04809	0.00983	0.02843

Clustered (Date) co-variance matrix, t-stats in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 10
Active Net Equity Purchases Accounting for Flows

This table reports slope coefficients estimated from regressions of active net equity purchases on fund characteristics and controls. Active net equity purchases, as a percent of assets, is reported in decimal form, where 0.02 means 2%. Post-2016 is an indicator variable that takes the value one over the first two quarters of 2017. Pre-2020 is an indicator that takes the value one in the third quarter of 2020. All specifications include fund and date fixed effects. The second and fourth specifications include flows into the the fund as an additional control.

Dependent Variable:	Active Net Equity Purchases			
Model:	(1)	(2)	(3)	(4)
Variables				
Rep. Majority × $\mathbb{I}\{\text{Post-2016}\}_t$	0.0401**	0.0436*		
	(2.589)	(2.319)		
Rep. Majority × $\mathbb{I}\{\text{Pre-2020}\}_t$			-0.1268***	-0.0485***
			(-18.25)	(-3.940)
Flows		0.8109***		0.7475***
		(5.183)		(4.578)
Fixed-Effects				
Fund ID	Yes	Yes	Yes	Yes
Date	Yes	Yes	Yes	Yes
Fit Statistics				
Observations	1,719	1,520	1,751	1,543
R^2	0.34850	0.38762	0.22109	0.61464
Within R^2	0.04138	0.11187	0.00982	0.47227

Clustered (Date) co-variance matrix, t-stats in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 11
Average Team Partisanship by Morningstar Rating

This table shows the average Republican share of mutual fund teams, sorted by the number of Morningstar sustainability globes assigned to the fund. A score of five is the highest sustainability rating and a score of one the lowest.

Sust Globes #	Avg R Share
1	0.624937
2	0.639567
3	0.588903
4	0.563938
5	0.551688

Table 12
Partisanship and ESG Ratings

This table reports regression results from regressing measures of the sustainability of a mutual fund's portfolio on the share of Republicans on the mutual fund team. "Sustainability Globes" refers to the number of globes assigned by Morningstar. The highest rating is five and the lowest is one. The dependent variable for the second column is an indicator for Morningstar's low carbon designation.

Dependent Variables:	Sustainability Globes	Low Carbon Designation
Model:	(1)	(2)
	OLS	Logit
Variables		
Republican Share	-0.2632***	-0.4060**
	(-3.826)	(-2.020)
Fixed-Effects		
CRSP Objective Code	Yes	Yes
Date	Yes	Yes
Fit Statistics		
Observations	53,492	136,208
Squared Correlation	0.05325	0.13261
Pseudo R^2	0.01848	0.10473
BIC	156,293.1	170,571.8

Clustered (Fund ID) co-variance matrix, t-stats in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 13
Trump Election Differences-in-Differences Specification – Analysts

This table displays results from a diff-in-diff around Trump's election in 2016. The dependent variable is the stock level earnings-per-share forecast issued by a particular analyst. The independent variables are a Post-2016 election dummy and an indicator for whether the analyst is a Republican. We also include date and stock by analyst fixed effects.

$$\text{EPS}_{ist} = \mathbb{I}\left\{\text{Post-2016 Election}\right\}_t \times \mathbb{I}\left\{\text{Republican}\right\}_i + \nu_{is} + \gamma_t$$

The sample consists of forecasts in the prior and trailing twelve months after the 2016 election. All regressions include date and analyst-by-stock fixed effects.

Earnings-Per-Share Forecast	Year				
Dependent Variables:	2017	2018	2019	2020	LTG
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
$\mathbb{I}\left\{\text{Post-2016 Election}_t\right\} \times \mathbb{I}\left\{\text{Rep}_t\right\}$	0.0820**	0.0794**	0.0809	0.2697	-0.0016
	(2.572)	(2.221)	(0.9865)	(1.208)	(-0.0447)
Fixed-Effects					
Date	Yes	Yes	Yes	Yes	Yes
Stock x Analyst	Yes	Yes	Yes	Yes	Yes
Fit Statistics					
Observations	9,620	8,033	3,708	1,204	1,478
R^2	0.99310	0.99157	0.99493	0.99270	0.44720
Within R^2	0.00101	0.00068	0.00047	0.00243	3.91×10^{-7}

Clustered (Date) co-variance matrix, t-stats in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 14
Analyst Political Alignment Impact on EPS Forecasts

This table reports regressions of the form

$$\text{EPS}_{ist} = \text{Aligned}_{it} + \eta_t + \nu_{is}$$

where i indexes analyst, s stock and t time. The dependent variable is a stock-level (s) EPS forecast made by analyst i at time t. Aligned_{it} is a dummy variable that takes the value 1 if a President of the same party as the analyst is currently in office. η_t are date fixed-effects and ν_{is} is a stock-by-analyst fixed effect.

Dependent Variable:	Earnings-Per-Share (EPS) Forecast		
Horizon (Years Ahead)	Zero	One	Two
Model:	(1)	(2)	(3)
Variables			
Aligned	0.2125***	0.1463***	0.0631
	(3.217)	(2.835)	(1.009)
Fixed-Effects			
Date (Daily)	Yes	Yes	Yes
Stock x Analyst	Yes	Yes	Yes
Fit Statistics			
Observations	71,000	80,626	45,368
R^2	0.49431	0.65916	0.79166
Within R^2	2.59×10^{-5}	1.43×10^{-5}	9.26×10^{-6}

Clustered (Date (Daily)) co-variance matrix, t-stats in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 15
Forecast Errors

The dependent variable for all eight regressions is the forecast error. For the first four regressions this is calculated as the absolute value of the difference between the forecasted and actual value. For these regressions we use the entire sample matched to voter registration records and an indicator ($\mathbb{I}\{\text{Partisan}_i\}$) for whether the analyst is registered as either a Republican or a Democrat. For the second set of regressions, we compare the signed forecast errors between Republicans and Democrats by regressing the signed error on an indicator for Republican political affiliation for the sample of registered Republicans and Democrats.

Error Construction	Unsigned Error				Signed Error			
Dependent Variables:	2017	2018	2019	2020	2017	2018	2019	2020
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Variables								
$\mathbb{I}\{\text{Post-2016 Election}\}_t \times \mathbb{I}\{\text{Partisan}\}_i$	0.8197***	0.1040	0.0370	0.0495				
	(4.787)	(1.486)	(0.3331)	(0.2967)				
$\mathbb{I}\{\text{Post-2016 Election}\}_t \times \mathbb{I}\{\text{Rep}\}_t$					0.0848***	0.0794**	0.0809	0.2697
					(2.649)	(2.221)	(0.9865)	(1.208)
Fixed-Effects								
Date	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Stock x Analyst	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit Statistics								
Observations	16,027	13,052	5,784	1,820	9,588	8,033	3,708	1,204
R^2	0.57001	0.86416	0.99072	0.95854	0.86928	0.88360	0.97661	0.97635
Within R^2	0.00283	0.00012	1.22×10^{-5}	9.7×10^{-5}	0.00108	0.00068	0.00047	0.00243
Sample								
Only Partisans	N	N	N	N	Y	Y	Y	Y

Clustered (Date) co-variance matrix, t-stats in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

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APPENDIX

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A Data Appendix

In the data appendix, we provide additional details about the construction of our database. To construct our very granular database, we rely on an unusually broad set of sources and conduct a variety of non-standard merges. At a high-level, we leverage 2 unique databases: (1) mutual fund managers matched to fund holdings, fund returns, fund characteristics, and fund manager biographical and political partisanship information; and (2) IBES analysts matched to earnings forecasts as well as biographical and political partisanship information.

To join together a variety of databases, we perform several unusual steps:

1. Collecting mutual fund manager demographic information from Morningstar.com to help establish unique matches when matching managers to other public databases

2. Matching mutual fund managers to voter registration data to identify their political party affiliation

3. Merging Morningstar mutual fund data to CRSP mutual fund data to exploit the pooled collection of fund characteristics as well as CRSP holdings data

4. Matching largely anonymized stock analysts from IBES to additional biographical information in TipRanks and BrokerCheck before identifying their political party affiliation in voter registration data

This data appendix describes are steps in greater detail, with an eye towards helping other researchers to implement these merges. We believe our unique databases offer an unprecedented amount of information for better understanding how individual characteristics – whether political partisanship, age, gender, educational background, or residential zip code – shape information processing in highly sophisticated individuals. Moreover, with both stock analysts and mutual fund managers, we can study financially incentivized changes in forecasts and stock investment.

A.1 Mutual Fund Manger Demographic Information

We collect self-reported fund manager biographies from Morningstar.com, which is notably separate from the Morningstar Direct database. On Morningstar.com, after searching for the fund ticker, one can find manager biographies on the "People" tab. We show an example of this in Figure 1 for Nationwide Small Company Growth A (Ticker NWSAX): `https://www.morningstar.com/funds/xnas/nwsax/people`. Notably, the only component of the URL that varies by fund share class is the ticker portion, which makes it easy to algorithmically extract this information. In the figure, we see information on the current managers as well as the manager history. Clicking on the manager names in the "Manager Timeline" launches a pop-up with manager biographical information, which we collect.

We show an example of the manager biographical information in Figure 2. Given the sensitivity of the information, we anonymize the example. Important information that we collect includes the educational degrees received (BA and MBA), the year of college graduation (1991), years of industry experience (21), the gendered pronoun (he), and any additional certifications (CFA).

A.2 Mutual Fund Manager Match to Voter Registration Data

The voter registration data includes full names (first, last, and middle initial), date of birth, sex, complete home address (ie: street number and name, apartment unit number if applicable, city, state, and zip code), phone number, and a measure of political partisanship.[18]

In the Morningstar manager data, we have full names (first, last, and middle initial), a measure of approximate age, gender, and the zip code of the fund firm's headquarters.

To merge Morningstar mutual fund managers to the voter registration data, we use the following algorithm:

1. **Name match:** First, we fuzzy match on first name and last name, and rule out potential matches with conflicting middle initials.

2. **Rule out incorrect matches with age:** Next, we use inferred age (from Morningstar) and exact age (from the voter registration data) to rule out incorrect matches. We require that the inferred age (if available) from Morningstar is between: (A) 5 years less than the voter registration exact age; and (B) 10 years more than the voter registration exact age. We drop any potential match that does not satisfy this restriction. We use wide age bounds given the inherent estimation error in inferring age from college graduation year or industry tenure.

3. **Stop here if match unique:** If there is only a single match between the voting data and the Morningstar data use that match.

4. **Continue if needed:** If there are multiple matches, continue:

    (a) For observations with both age and location data, use the observation that minimizes both, if such an observation exists. If such an observation does not exist,

[18] The voter registration data is unusually personal and detailed precisely because it is used by political campaigns to contact individuals in the mail and/or by phone.

then if only one observation fits the age requirements and has a distance less than fifty miles, use that observation.

(b) For all other observations: require that the voter registration state is either the same state or a neighboring state to the location of the firm headquarters. We consider neighboring states to account for commuters, for example a manager that lives in Connecticut or New Jersey but works in New York. If after this step there is only a single observation left, use that observation.

(c) If the age difference variable is available, filter observations so that that voter_age$-$ estim_age $\in [-2, 5]$. If the age difference variable is unavailable, require that the age from the voter file is between 34 and 69. If only a single match remains, use that match.

(d) Successively decrease the amount of allowed distance from a firm headquarters. Delete observations that have distances from headquarters ¿150 miles, 100 miles and 50 miles. If after any of these cuts only a single observation is left, use that observation.

(e) Require that the age from the voter file is no more than five years greater than the lower bound on age inferred from industry experience. If only a single match left, use that match.

(f) If there are still multiple matches, try requiring no more than one year difference between the lower bound on age and the age from the voter file. If only a single match left, use that match.

A.3 CRSP Morningstar Merge

We merge CRSP mutual fund data (fund characteristics, holdings, and returns) with Morningstar mutual fund data (primarily fund manager names and information but also star ratings, sustainability globes, and other fund characteristics). This merge is done at the share-class level ($SecId$ in Morningstar, $crsp\_fundno$ in CRSP) and aggregated to the fund level ($FundId$ in Morningstar, $crsp_cl_group$ in CRSP). There is no standard mapping table. We accomplish this merge by matching on common share class identifiers, in order of reliability, and using the share class match to construct a fund level match. We proceed in matching share classes in the following order, stopping the process for a fund when the most reliable method is successful:

1. Match on both CUSIP and ticker

2. Match only on CUSIP

3. Match only on ticker

4. Match only on fund name

Most matches occur in the first step, and so are highly reliable, but subsequent levels add some additional matches. We validate all share-class matches by comparing net assets and monthly returns in Morningstar and CRSP. This gives us confidence that all share class matches, regardless of which method is employed, are in fact correct, leading to valid fund level matches between the two databases.

A.4 Analyst Match to Voter Registration Data

A key contribution of our paper is beginning with largely anonymized stock analysts from IBES – each analyst is identified only by their first initial, last name, stock coverage, and an identifier for their firm – and finish with fully de-anonymized stock analyst – each analyst is identified by their full name, work history, home address, and political partisanship. We are not the first paper to de-anonymize the IBES stock analysts, but our procedure leads to much deeper personal identification information than previous analyses. Moreover, our procedure is perhaps easiest to replicate, and can be truncated in varying places while still providing substantial information.

At a high-level, we proceed as follows: (1) collect anonymized stock analysts and their stock coverage from IBES; (2) match partial names and stock coverages to full names, firms, and stock coverages in TipRanks; (3) match full names and firm names to FINRA's BrokerCheck to get work history and office address; and (4) match full names and office addresses to the voter registration data to get home addresses, age, gender, and political partisanship.

Notably, the full collection of analyst level information is so granular as to allow for a variety of further checks for internal consistency. For example, FINRA's BrokerCheck includes information of all certifications the registered financial professional has obtained. FINRA requires research analysts that publish reports to have the Series 86 & 87 certifications, which we generally expect our analysts to have.[19] In addition, given both BrokerCheck's complete work history and the person's age from the voter registration data, we can further verify that our matches are realistic. In general, the linkage is quite exact between IBES and TipRanks (given the stock coverages and firm name), which usually leads to an exact match in BrokerCheck, which then quite often leads to an approximately unique match in the voter

[19] Notably, FINRA allows exceptions. We primarily use these requirements in the unusual case we need to disambiguate between 2 analysts with the same name that work at the same firm. FINRA rules on this can be viewed at https://www.finra.org/rules-guidance/key-topics/research-analyst-rules.

registration data (in terms of full names matching, a generally short distance between work and home, and a plausible age given the work history).

A.4.1 Recovering Full Analyst Names

We begin with IBES data. As shown in Figure 3, the primary identifiers are the covered stocks (identified by ticker or CUSIP), the analyst's first initial and last name ("ANALYST"), the analyst's quasi-coded firm name ("ESTIMID"), and a unique identifying code for the analyst ("AMASKCD"). We will illustrate the de-anonymizing process, and for the sake of privacy, will do our best to keep the example analyst's personal information confidential. We focus on "A UERK****", an analyst at firm "FAHN" who covers Apple (ticker AAPL). While we could use IBES to track this analyst over time and in the cross-section using the AMASKCD, we would be unable to collect an additional identifying information, and would likely also fail to de-anonymize the analyst's firm (but could uniquely identify the firm with ESTIMID).

We then check TipRanks for an analyst with last name "UERK****" and first initial "A" who also covers Apple. If there are multiple matches, then we can compare the rest of the coverage. We ultimately de-anonymize the firm identifiers in IBES ("ESTIMID") which can be used for the few cases in which an analyst with the same first initial and last name covers the same stock. We show the TipRanks information in Figure 4 to illustrate how clean this mapping process is. With the TipRanks information, we determine that the analyst's first name is "Andrew," he works at "Jefferies" (hence the ESTIMID "FAHN" maps to "Jefferies"), and we confirm that he covers Apple. Notably, we also have dates for his ratings, as well as the ratings themselves, which can help us further ensure our match is correct in the event of any uncertainty or duplication. By checking TipRanks for identifying information, we're able to link the analyst codes ("AMASKCD") to full names and firm names, with a very high degree of accuracy.

In a small number of cases multiple analysts with the same first name and last name were mapped to the unique firm name. We checked each of these matches manually. First, using Brokercheck, we eliminated the analysts if they did not have a FINRA series 87 exam which is a prerequisite for inclusion in Tipranks. We also ruled out inactive analysts since they are most likely excluded from Tipranks. In addition, we compared photos on TipRanks to LinkedIn and then compared the work history on BrokerCheck to LinkedIn for consistency. We also eliminated analysts who did not work in the USA or were legally barred from making recommendations. Finally, we also compared the Tipranks suggestion history with the Linkedin work history.

A.4.2 Recovering Additional Analyst Background Information

We next search FINRA's BrokerCheck to get additional background information on the analysts. Continuing with our example, we search for "Andrew Uerk****" and get the results shown below in Figure 5. While there are 4 possible matches, the full name and firm name provide a unique match. We click on the "More Details" tab to view the additional analyst background information we wish to collect.

As shown below in Figure 6, BrokerCheck provides us with substantial additional information. First, we collect the office address. This is particularly useful because it is *not* the headquarter's address, but rather, the *actual location* that Andrew works.[20] This exact work location is what enables us to so precisely match analysts to the voter registration data, which uses home addresses. Second, we collect the years of experience, which is 13 for Andrew. This can be useful for sanity checking our matches into the voter registration data (where we can compare years of experience with age). Third, we get Andrew's work history – we see that he has been at Jefferies since 2021, and before that, he was at Oppenheimer. This information can be useful for further verifying the accuracy of our matches. Last, we get the exams that Andrew has passed. Scrolling down on the page (ie: not shown in the image), we see that Andrew received his Series 7 on Feb 16, 2010; his Series 63 on Mar 1, 2010; his Series 86 on Apr 22, 2010; his Series 87 on Mar 19, 2010; and his SIE on Oct 1, 2018. We generally expect to see the Series 86 & 87 for stock analysts, as described previously, although there are a variety of methods for being excepted from at least one of these. Note that, for privacy issues, we do not continue our example with Andrew for matching to home address and party affiliation.

A.4.3 Matching Analysts to Voter Registration Data

The matching process for analysts is slightly different than that of managers, reflective of the more granular information available through FINRA relative to Morningstar. As for analysts, we are able to calculate an estimated age based on the analyst's experience. However, FINRA also provides every single alternate name used by the analyst (for example, "Mike" if someone named "Michael" goes by a diminutive) and the address of the analyst's workplace. In Morningstar, we do not observe alternate names that the analyst may use or the exact location of the analysts' workplace, only all the office locations of the manager's firm. Apart from this change, we implement the same algorithm as described in Section A.2.

To assess the credibility of this match, we hand-audited 50 individual matches. In partic-

[20] To see this is the case, if you search BrokerCheck for firm CRD 2347 (Jefferies) and a given name (eg: James), you'll see many different states/addresses listed for the exact same firm name/identifier.

ular, we hand audited observations with the largest distances between the workplace address and the location listed in the voter registration records. This match was meant to isolate cases where our matching algorithm was most likely to have failed. In the hand-audit, we found a LinkedIn account or other similar documentation online for each analyst. We then verified that this was the correct analyst by cross-checking the work history listed on LinkedIn to the work history listed on FINRA. We then searched for additional information in the work or educational history of the analyst that was consistent with the location from the voter registration records. *We found that in 100% of the cases we hand audited, we were able to find corroborating evidence that the analyst had lived in the state indicated by the voter-registration records.* Most commonly this was due to either attending college or previous employment in the state of the matched voter registration record.

A.5 Figures

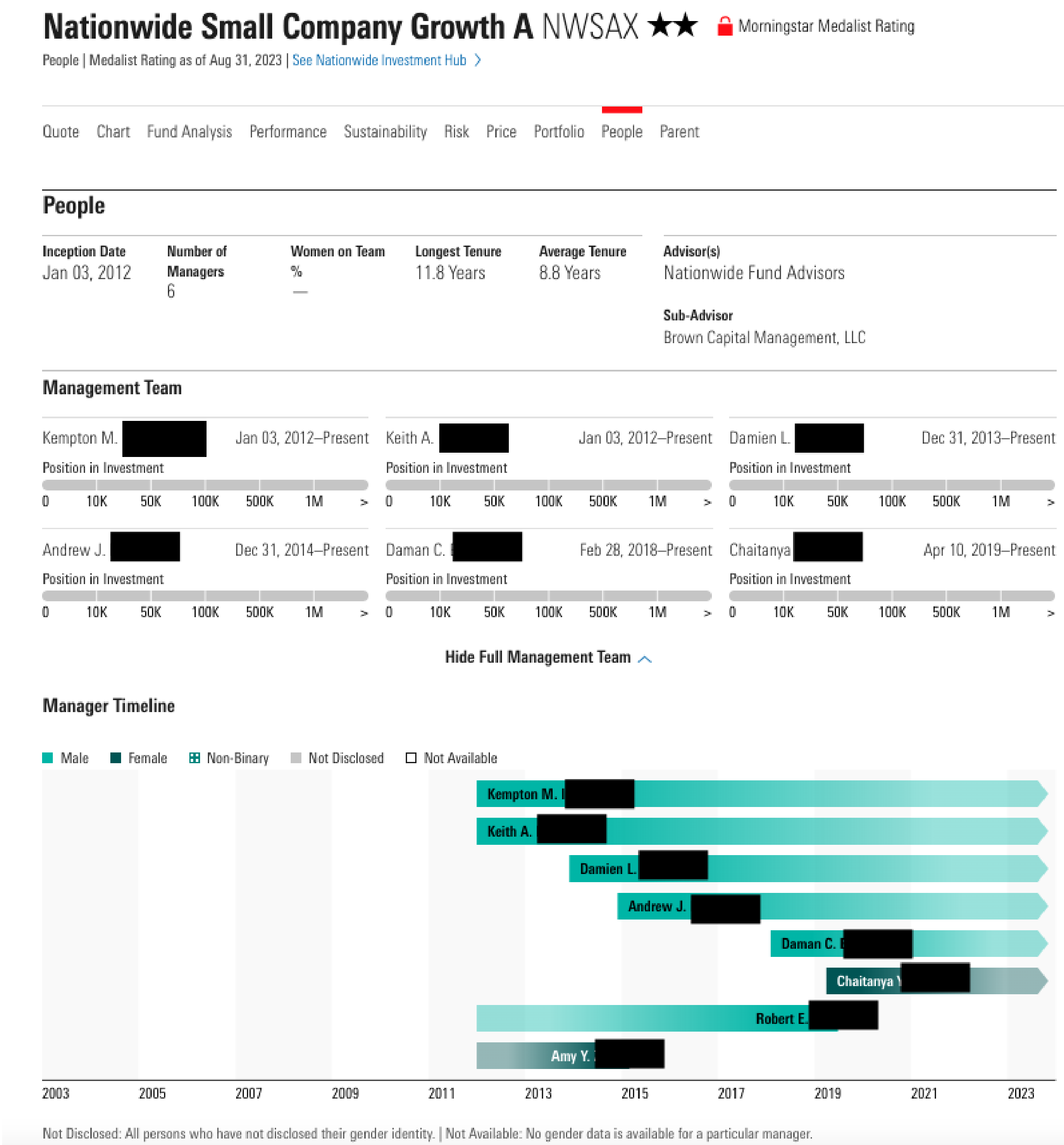
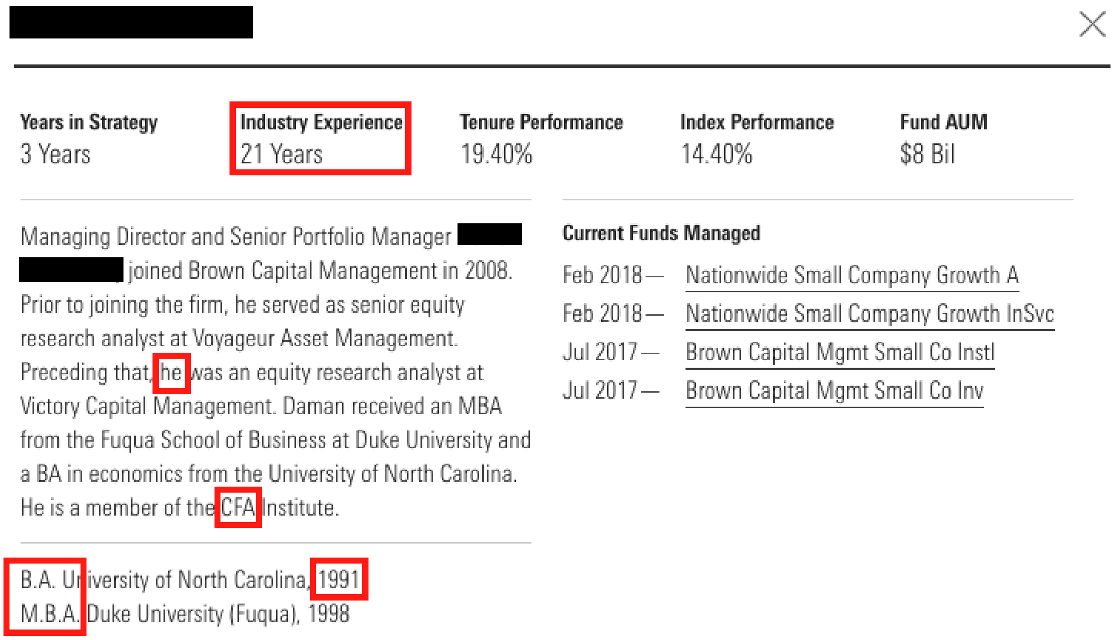


Figure A1. Morningstar Example Fund Managers Page. This figure shows the Morningstar "People" page for Nationwide Small Company Growth A (Ticker NWSAX). The page is available at https://www.morningstar.com/funds/xnas/nwsax/people.


[redacted]

Years in Strategy	Industry Experience	Tenure Performance	Index Performance	Fund AUM
3 Years	21 Years	19.40%	14.40%	$8 Bil

Managing Director and Senior Portfolio Manager [redacted] joined Brown Capital Management in 2008. Prior to joining the firm, he served as senior equity research analyst at Voyageur Asset Management. Preceding that, he was an equity research analyst at Victory Capital Management. Daman received an MBA from the Fuqua School of Business at Duke University and a BA in economics from the University of North Carolina. He is a member of the CFA Institute.

B.A. University of North Carolina, 1991
M.B.A. Duke University (Fuqua), 1998

Current Funds Managed

Feb 2018— Nationwide Small Company Growth A
Feb 2018— Nationwide Small Company Growth InSvc
Jul 2017— Brown Capital Mgmt Small Co Instl
Jul 2017— Brown Capital Mgmt Small Co Inv

Figure A2. Morningstar Example Manager Biography. This figure shows the Morningstar manager biography page for one of the fund managers of Nationwide Small Company Growth A (Ticker NWSAX). This pop-up can be accessed by clicking on the fund manager names in the "Manager Timeline" from Figure A1.

TICKER	CUSIP	CNAME	OFTIC	ACTDATS	ESTIMID	ANALYST	ITEXT	EMASKCD	AMASKCD	U
AAPL	3783310	APPLE	AAPL	20200301	FAHN	UERK[redacted] A	STRONG BU	836	136753	
AAPL	3783310	APPLE	AAPL	20200313	WHEAT	RAKERS A	BUY	2446	108607	
AAPL	3783310	APPLE	AAPL	20200324	LAWRENCE	ONG J	BUY	1382	192858	
AAPL	3783310	APPLE	AAPL	20200416	TOPAZ	RESEARCH DEPA	BUY	41714	194510	
AAPL	3783310	APPLE	AAPL	20200417	GOLDMAN	HALL R	UNDERPERF	1020	77028	
AAPL	3783310	APPLE	AAPL	20200422	DAIWAUS	MISCIOSCIA L	BUY	39729	52370	

Figure A3. IBES Example of Analyst Identifying Information. This figure shows the partially anonymized stock analyst data from IBES. We focus on the first analyst (highlighted in yellow) for our examples. He is uniquely identified by the AMASKCD identifier, but ultimately, we seek to decipher his full name ("ANALYST") as well as his firm ("ESTIMID").

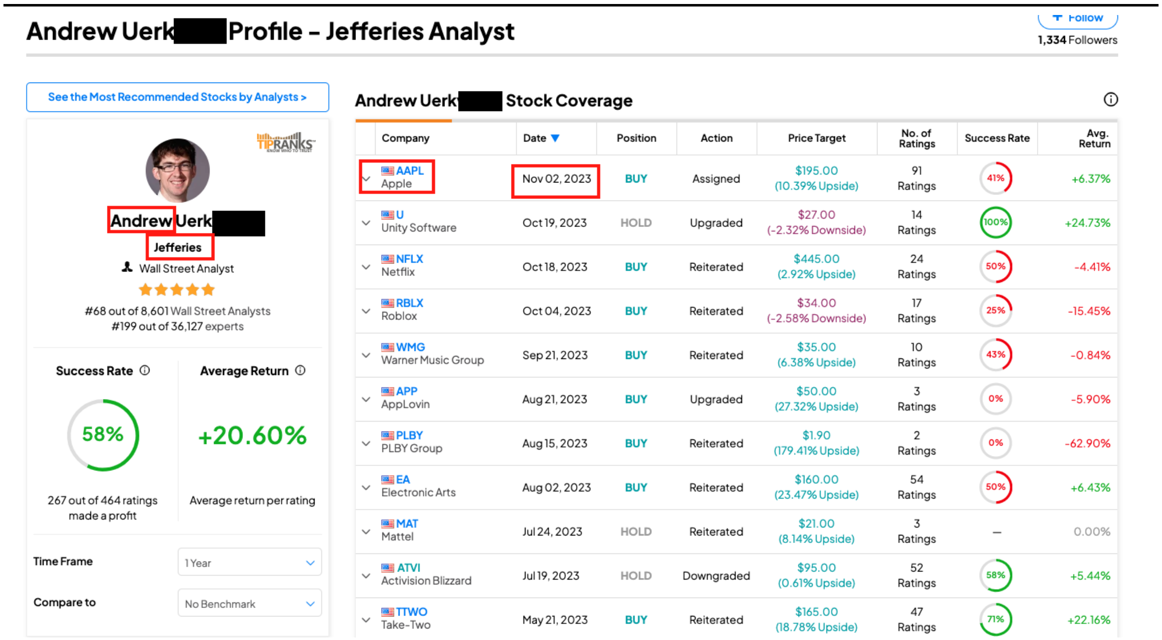


Figure A4. TipRanks Example of Analyst Identifying Information. This figure shows the TipRanks page for the IBES analyst (from Figure A3) that we are trying to de-anonymize. Here, we see his full name, his firm name, and his stock coverage.

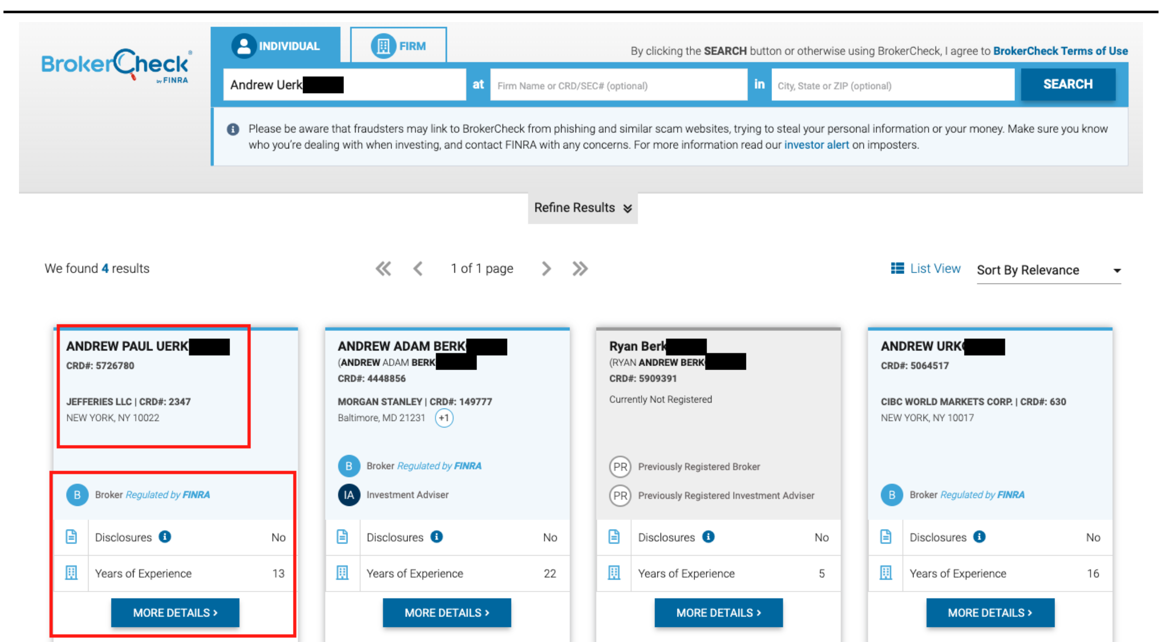


Figure A5. BrokerCheck Example of Finding Analyst. This figure shows the search results on BrokerCheck for our IBES analyst of interest. Boxed in red is the correct match.

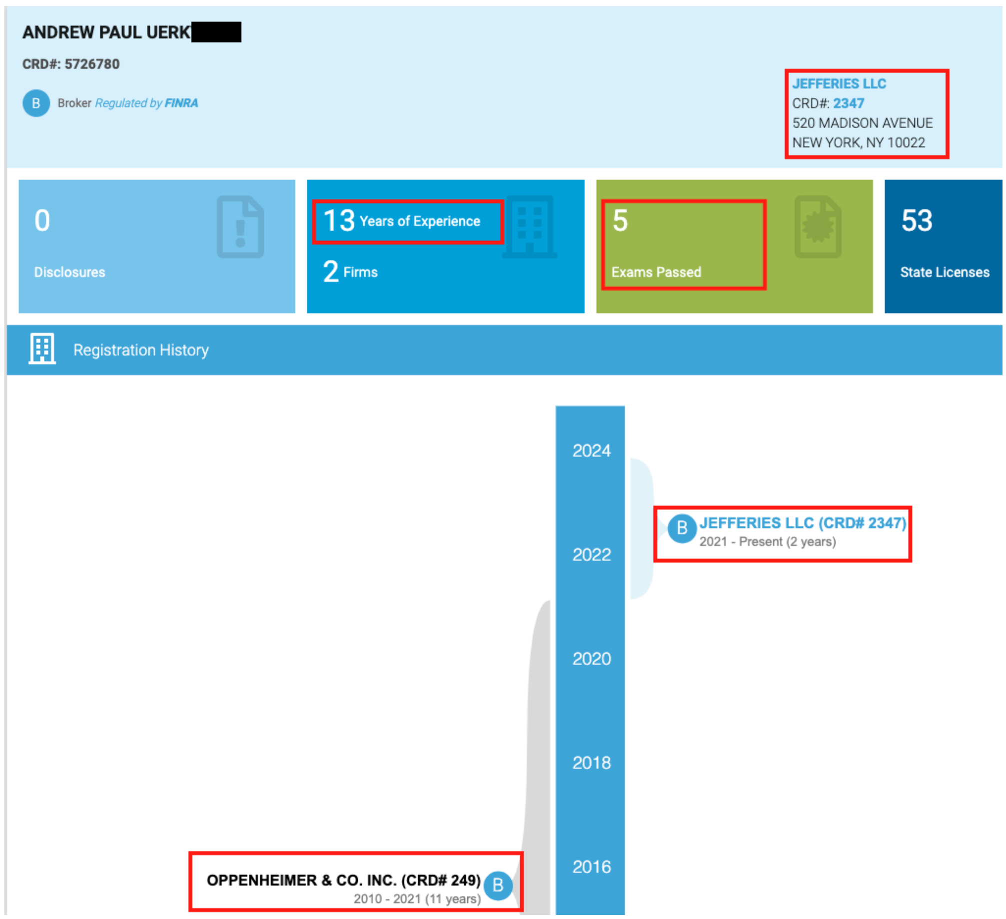


Figure A6. BrokerCheck Example of Additional Analyst Information. This figure shows additional information on BrokerCheck about our IBES analyst of interest. Boxed in red are important pieces of information that we collect, in addition to several more that are further down the webpage but not shown here.

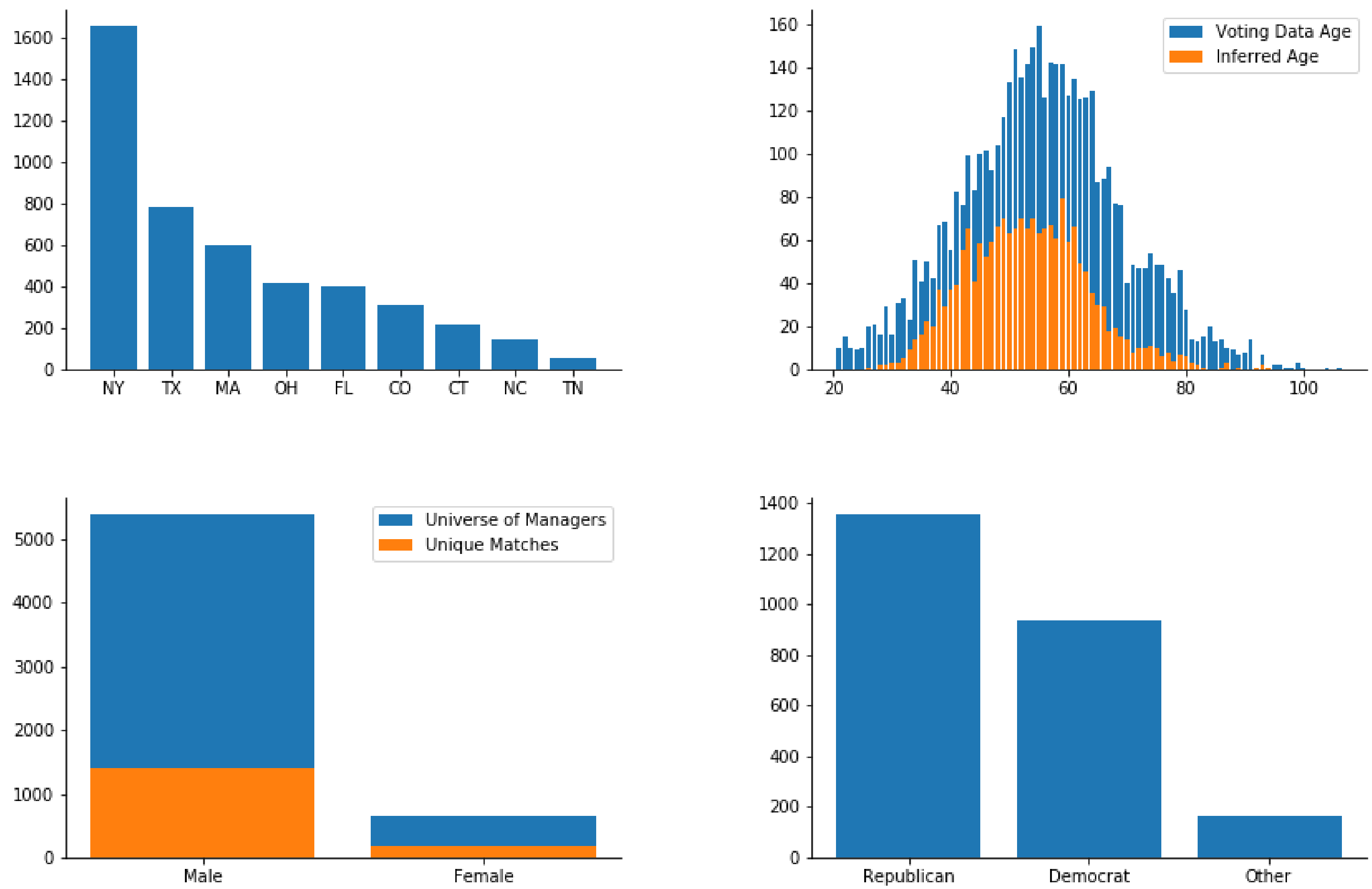


Figure A7. Aggregate Manager Summary Statistics. This figure shows summary statistics for our matched fund managers' states of residence (top left), for the distribution of matched manager ages in the voting data and our inferred ages (top right), for the gender of all fund managers relative to the fund managers we match (bottom left0, and for the distribution of matched managers by partisanship (bottom right).

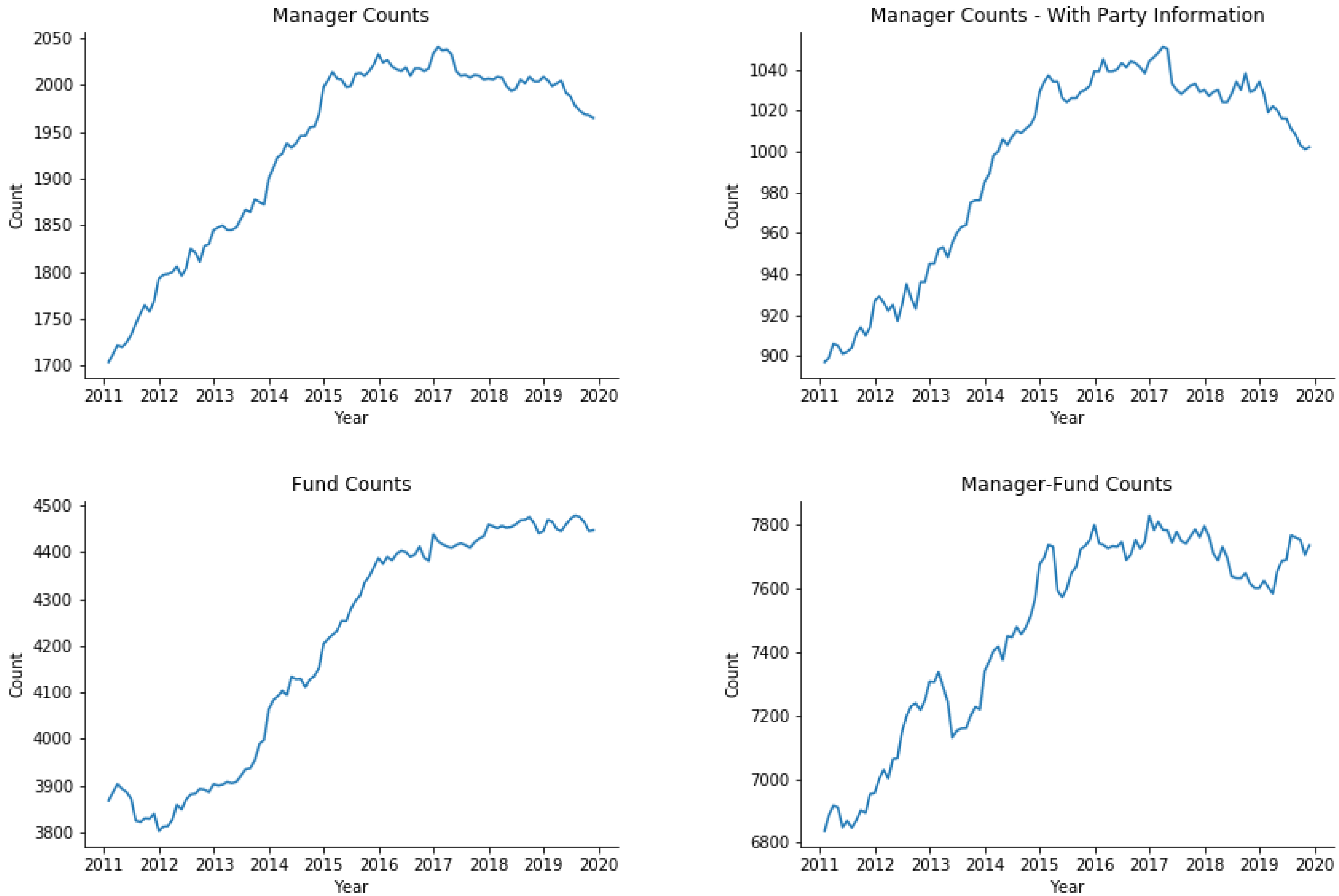


Figure A8. Manager Summary Statistics over Time. This figure shows summary statistics over time to better contextualize our matches. We show, through time, the number of managers in our sample (top left), the number of matched managers with party information (top right), the number of funds in our sample (bottom left), and the number of manager-funds in our sample (bottom right).

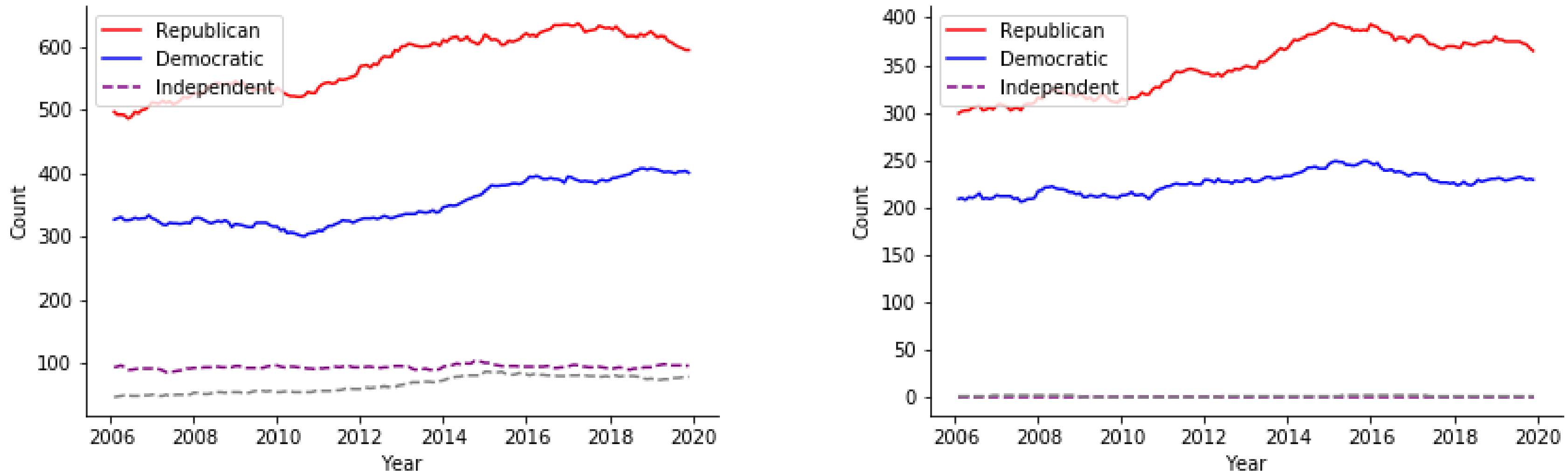


Figure A9. Fund Manager Party Counts over Time. This figure shows partisan counts over time. On the left, we have counts for fund managers, and on the right, we have counts for stock analysts.

A.6 Tables

Table 16
Example with Imputed Party

This table displays an example registration record for a state (Texas) where the party is not explicitly recorded. In these cases we infer the party of the respondent based on voting history in Republican or Democratic primaries. In this case, we would record the manager as a Republican after March 2008, when the manager first voted in a Republican primary.

Election Held At	Election Party	Election Type	State	Name	Party
2000-11-07		General	TX	First MI Last	
2002-11-05		General	TX	First MI Last	
2004-11-02		General	TX	First MI Last	
2008-03-04	republican	Primary	TX	First MI Last	
2008-11-04		General	TX	First MI Last	
2010-11-02		General	TX	First MI Last	
2012-11-06		General	TX	First MI Last	
2014-11-04		General	TX	First MI Last	
2016-03-01	republican	Presidential Primary	TX	First MI Last	
2016-05-24	republican	Unknown	TX	First MI Last	
2016-11-08		General	TX	First MI Last	
2018-11-06		General	TX	First MI Last	

Table 17
Example where Party Recorded

This table displays a registration record for a state (Connecticut) where the party of registration is explicitly recorded. We would count the manager as a Republican from November 2006 onwards.

Election Held At	Election Party	Election Type	State	Name	Party
2006-11-07		General	CT	First MI Last	R
2008-11-04		General	CT	First MI Last	R
2010-11-02		General	CT	First MI Last	R
2012-11-06		General	CT	First MI Last	R
2014-11-04		General	CT	First MI Last	R
2016-04-26		Primary	CT	First MI Last	R
2016-11-08		General	CT	First MI Last	R
2018-11-06		General	CT	First MI Last	R

B Mathematical Appendix

This section presents all relevant proofs and derivations. As is true of our model, our treatment closely follows Koijen and Yogo (2019) and Chaudhry (2022). The key deviation is allowing for heterogeneity in expected dividends, and thus subjective expected returns, across partisans.

In the proofs, we introduce some additional notation. $Q^D_{p,n,t}$ is the quantity demanded by partisan p of stock n at time t. $P_{n,t}$ is the price of stock n at time t and $\theta_{p,n,t}$ is the portfolio weight of stock n in partisan p's portfolio at time t. Note that, for brevity, we also sometimes use the following expression for wealth-weighted latent demand:

$$\epsilon_{n,t} = W_{R,n,t} + \epsilon_{R,n,t} + W_{D,n,t}\epsilon_{D,n,t}$$

with the same expression differenced or with the e superscript to denote expectations.

Lemma 1 (Linearized Demand Curve)**.** *Denote percentage changes from t^- to t^+ in the quantity of shares demanded by partisan p as $\Delta q_{p,n,t}$, current price as $\Delta p_{n,t}$, expected next period price as $\Delta p^e_{n,t+1}$, expected next period dividends as $\Delta d^e_{p,n,t+1}$ and changes in other demand shocks as $\Delta\epsilon_{p,n,t}$. δ is the average price-dividend ratio. Linearizing portfolio weight demand function around $\left(\Delta p_{n,t}, \Delta p^e_{n,t+1}, \Delta d^e_{p,n,t+1}, \Delta\epsilon_{p,n,t}\right) = (0,0,0,0)$ yields the following demand curve for partisan p and stock n:*[21]

$$\Delta q_{p,n,t} = -\left(1+\kappa\left(1+\delta\right)\right)\Delta p_{n,t} + \kappa\left[\delta\Delta d^e_{p,n,t+1} + \Delta p^e_{n,t+1}\right] + \Delta\epsilon_{p,n,t} \tag{5.4}$$

Proof. Starting from the definition of the percentage change in quantity demanded, we have

$$\begin{aligned}
\Delta q^D_{p,n,t} &= \frac{Q^D_{p,n,t+}}{Q^D_{p,n,t-}} - 1 \\
&= \frac{W_{p,t+}}{W_{p,t-}}\frac{P_{n,t-}}{P_{n,t+}}\frac{\theta_{p,n,t+}}{\theta_{p,n,t-}} - 1 \\
&= \frac{W_{p,t+}}{W_{p,t-}}\frac{P_{p,n,t-}}{P_{p,n,t+}}\exp\left[\kappa\Delta\mu_{p,n,t} + \Delta\epsilon_{p,n,t}\right] - 1 \\
&= \frac{1+\Delta w_{p,t}}{1+\Delta p_{n,t}}\exp\left[\kappa\Delta\mu_{p,n,t} + \Delta\epsilon_{p,n,t}\right] - 1
\end{aligned}$$

Linearizing the final expression, we have

$$\Delta q^D_{p,n,t} \approx \Delta w_{p,t} - \Delta p_{n,t} + \kappa\Delta\mu_{p,n,t} + \Delta\epsilon_{p,n,t}$$

So now we have an expression for the percentage change in the quantity demanded as a function of the change in wealth, change in prices, change in expected returns and change in latent demand.

[21] There is no new economic content in Lemma 1 relative to earlier work. While we have allowed for heterogeneity across partisans, this has only changed notation. The linearized demand curve, conditional on partisanship, is the same as the aggregate linearized demand from Chaudhry (2022).

Now, the dollar change in wealth is

$$W_{p,t+} - W_{p,t-} = \left(P_{n,t+} - P_{n,t-}\right) Q^D_{p,n,t-}$$

We then can write the percentage change in wealth as

$$\Delta w_{p,t} = \frac{W_{p,t+} - W_{p,t-}}{W_{p,t-}} = \frac{\left(P_{n,t+} - P_{n,t-}\right) Q^D_{p,n,t-}}{W_{p,t-}} = \frac{P_{n,t+} - P_{n,t-}}{W_{p,t-}} \frac{\theta_{p,n,t-} W_{p,t-}}{P_{n,t-}} = \theta_{p,n,t-} \Delta p_{n,t}$$

We then plug this into the expression for the change in demand. So we have that

$$\Delta q^D_{p,n,t} \approx \theta_{p,n,t-} \Delta p_{n,t} - \Delta p_{n,t} + \kappa \Delta \mu_{p,n,t} + \Delta \epsilon_{p,n,t} \tag{B.1}$$

Starting from the definition of subjective expected returns (Equation 5.3) we can write[22]

$$\begin{aligned} R^f_t + \mu_{n,t-} + \Delta \mu_{n,t} &\approx \frac{\tilde{\mathbb{E}}_{t-}\left[P_{n,t+1}\right]}{P_{n,t-}} \left(1 + \Delta p^e_{n,t+1} - \Delta p_{n,t}\right) + \frac{\tilde{\mathbb{E}}_{t-}\left[D_{n,t+1}\right]}{D_{n,t}} \frac{D_{n,t}}{P_{n,t-}} \left(1 + \Delta d^e_{n,t+1} - \Delta p_{n,t}\right) \\ &= \left(1 + \bar{x}\right) \left(1 + \Delta p^e_{n,t+1} - \Delta p_{n,t}\right) + \left(1 + \bar{x}\right) \delta \left(1 + \Delta d^e_{n,t+1} - \Delta p_{n,t}\right) \end{aligned}$$

We can then rewrite the expression above as

$$\begin{aligned} R^f_t + \mu_{n,t-} + \Delta \mu_{n,t} &= \left(1 + \bar{x}\right)\left(1 + \delta\right) + \left(1 + \bar{x}\right) \left[\Delta p^e_{n,t+1} - \Delta p_{n,t} + \delta \left(d^e_{n,t+1} - \Delta p_{n,t}\right)\right] \\ &= \left(1 + \bar{x}\right)\left(1 + \delta\right) - \left(1 + \bar{x}\right)\left(1 + \delta\right) \Delta p_{n,t} + \left(1 + \bar{x}\right) \left[\Delta p^e_{n,t+1} + \delta \Delta d^e_{n,t+1}\right] \\ \Rightarrow \Delta \mu_{n,t} &\approx -\left(1 + \delta\right)\left(1 + \bar{x}\right) \Delta p_{n,t} + \delta \left(1 + \bar{x}\right) \Delta d^e_{n,t+1} + \left(1 + \bar{x}\right) \Delta p^e_{n,t+1} \end{aligned}$$

Combining the final line with Equation B.1 and imposing diversified portfolios ($\theta_{p,n,t}$ small):

$$\begin{aligned} \Delta q^D_{n,t} &= -\Delta p_{n,t} + \kappa \Delta \mu_{n,t} + \Delta \epsilon_{n,t} \\ &= -\Delta p_{n,t} + \kappa \left(-\left(1 + \delta\right)\left(1 + \bar{x}\right) \Delta p_{n,t} + \delta \left(1 + \bar{x}\right) \Delta d^e_{n,t+1} + \left(1 + \bar{x}\right) \Delta p^e_{n,t+1}\right) + \Delta \epsilon_{n,t} \\ &= -\Delta p_{n,t} - \kappa \left(1 + \bar{x}\right) \Delta p_{n,t} - \kappa \delta \left(1 + \bar{x}\right) \Delta p_{n,t} + \delta \kappa \left(1 + \bar{x}\right) \Delta d^e_{n,t+1} + \kappa \left(1 + \bar{x}\right) \Delta p^e_{n,t+1} + \Delta \epsilon_{n,t} \end{aligned}$$

Then imposing small growth rates we have

$$\begin{aligned} \Delta q^D_{n,t} &= -\Delta p_{n,t} - \kappa \left(1 + \bar{x}\right) \Delta p_{n,t} - \kappa \delta \left(1 + \bar{x}\right) \Delta p_{n,t} + \delta \kappa \left(1 + \bar{x}\right) \Delta d^e_{n,t+1} + \kappa \left(1 + \bar{x}\right) \Delta p^e_{n,t+1} + \Delta \epsilon_{n,t} \\ &= -\Delta p_{n,t} - \kappa \Delta p_{n,t} - \kappa \delta \Delta p_{n,t} + \delta \kappa \Delta d^e_{n,t+1} + \kappa \Delta p^e_{n,t+1} + \Delta \epsilon_{n,t} \\ &= -\left(1 + \kappa \left(1 + \delta\right)\right) \Delta p_{n,t} + \kappa \left(\delta \Delta d^e_{n,t+1} + \Delta p^e_{n,t+1}\right) + \Delta \epsilon_{n,t} \end{aligned}$$

where the final line is as desired.

[22] As in Chaudhry (2022) and Koijen and Yogo (2019) we use the approximation $\tilde{\mathbb{E}}_{t-}\left[P_{n,t+1}\right] / P_{n,t-} = 1 + \bar{x}$. We emphasize that this is an approximation and will not hold exactly if, for instance, discount rates are not constant.

$\square$

Lemma 2 (Price Change)**.** *Imposing market clearing (Equation 5.5) yields the following equilibrium price change:*

$$\begin{aligned}\Delta p_{n,t} = & \frac{\kappa^{-1}}{1+\phi}\Delta p^e_{n,t+1} + \frac{\delta}{1+\phi}\left(W_{R,t}\Delta d^e_{R,n,t+1} + W_{D,t}\Delta d^e_{D,n,t+1}\right) \\ & + \frac{1}{1+\phi}\left(W_{R,n,t}\Delta\epsilon_{R,n,t} + W_{D,n,t}\Delta\epsilon_{D,n,t}\right)\end{aligned} \tag{5.6}$$

where $\phi \equiv \kappa^{-1} + \delta$.

Proof. Combining the market clearing condition Equation 5.5) and Equation 5.4 we have that

$$\begin{aligned}& W_R\Delta q^D_{R,n,t} + W_D\Delta q^D_{D,n,t} \\ & \quad = W_R\left(-\left(1-\theta_{n,t-} + \kappa\left(1+\delta\right)\left(1+\bar{x}\right)\right)\Delta p_{n,t} + \kappa\left(1+\bar{x}\right)\left[\delta\Delta d^e_{n,t+1} + \Delta p^e_{n,t+1}\right] + \Delta\epsilon_{n,t}\right) \\ & \qquad + W_D\left(-\left(1-\theta_{n,t-} + \kappa\left(1+\delta\right)\left(1+\bar{x}\right)\right)\Delta p_{n,t} + \kappa\left(1+\bar{x}\right)\left[\delta\Delta d^e_{n,t+1} + \Delta p^e_{n,t+1}\right] + \Delta\epsilon_{n,t}\right)\end{aligned}$$

Re-arranging, we have that

$$\begin{aligned}\Delta p_{n,t} = & \frac{1}{W_R\left(1-\theta_{R,n,t-} + \kappa\left(1+\delta\right)\left(1+\bar{x}\right)\right) + W_D\left(1-\theta_{D,n,t-} + \kappa\left(1+\delta\right)\left(1+\bar{x}\right)\right)} \\ & \times\left(W_R\Delta\epsilon_{R,n,t} + W_D\Delta\epsilon_{D,n,t}\right)\end{aligned}$$

Now, imposing that $\theta_{P,n,t} \approx 0$ (well-diversified portfolio), $\bar{x} \approx 0$ (small growth rates) and consistent higher order beliefs $p^e_{D,n,t+1} = p^e_{R,n,t+1}$

$$\begin{aligned}\Delta p_{n,t} & = \frac{\kappa\left(1+\bar{x}\right)\delta}{1+\kappa\left(1+\delta\right)}\left(W_R\Delta d^e_{R,n,t+1} + W_D\Delta d^e_{D,n,t+1}\right) + \frac{\kappa\left(1+\bar{x}\right)}{1+\kappa\left(1+\delta\right)}\Delta p^e_{R,n,t+1} + \frac{1}{1+\kappa\left(1+\delta\right)}\Delta\epsilon_{n,t} \\ & = \frac{\delta}{1+\phi}\left(W_R\Delta d^e_{R,n,t+1} + W_D\Delta d^e_{D,n,t+1}\right) + \frac{\kappa^{-1}}{1+\phi}\Delta p^e_{R,n,t+1} + \frac{1}{1+\phi}\Delta\epsilon_{n,t}\end{aligned} \tag{B.2}$$

The final line is Equation 5.6 as desired.

$\square$

Lemma 3 (Belief Shock Effect on Prices)**.** *let $\Delta d^e_{n,t+s}$ represent the percentange change between t^- and t^+ in the expected dividend in period $t+s$ and let $\Delta\epsilon^e_{n,t+s}$ represent the change in the expected residual demand shock in $t+s$. We have this expression for price change today*

$$\begin{aligned}\Delta p_{n,t} = & \delta\sum_{s=1}^{\infty}\left(\frac{1}{1+\phi}\right)^s\left(W_{R,t+s-1}\Delta d^e_{R,n,t+s} + W_{D,t+s-1}\Delta d^e_{D,n,t+s}\right) \\ & + \frac{1}{\kappa}\sum_{s=0}^{\infty}\left(\frac{1}{1+\phi}\right)^{s+1}\left(W_{R,t+s-1}\Delta\epsilon^e_{R,n,t+s} + W_{D,t+s-1}\Delta\epsilon^e_{D,n,t+s}\right)\end{aligned} \tag{5.7}$$

Proof. Iterating forward Equation 5.6 yields:

$$\Delta p_{n,t} = \delta \sum_{s=1}^{\infty} \left(\frac{1}{1+\phi}\right)^{s} \left(W_{R,t+s-1}\Delta d^{e}_{R,n,t+s} + W_{D,t+s-1}\Delta d^{e}_{D,n,t+s}\right) + \frac{1}{\kappa}\sum_{s=0}^{\infty} \left(\frac{1}{1+\phi}\right)^{s+1} \Delta \epsilon^{e}_{n,t+s}$$

This is exactly Equation 5.7 as desired. □

Proposition 1 (Partisan Shock)**.** *The change in prices given a surpise shift in political alignment is approximated by*

$$\Delta p_{n,t} \approx \Phi \left(W_{R,n,t} - W_{D,n,t}\right) \chi_n + e_{n,t} \tag{5.9}$$

where

$$\Phi \equiv \delta \sum_{s=1}^{3} \left(\frac{1}{1+\phi}\right)^{s} \text{ and } e_{n,t} \equiv \frac{1}{\kappa}\sum_{s=0}^{\infty} \left(\frac{1}{1+\phi}\right)^{s+1} \left(W_{R,t+s-1}\Delta \epsilon^{e}_{R,n,t+s} + W_{D,t+s-1}\Delta e^{e}_{D,n,t+s}\right)$$

Proof. This follows from combining Equation 5.6 and the specification for the partisan belief distortion, Equation 5.8. This equation is approximate because I approximate $W_{R,n,t+1}$ as $W_{R,n,t}$. □

C Additional Results

This section provides additional figures and tables.

Figure 1
Obama's Election

This table displays the time-series of net active purchases for Republican and Democratic majority funds around the 2012 Presidential election.

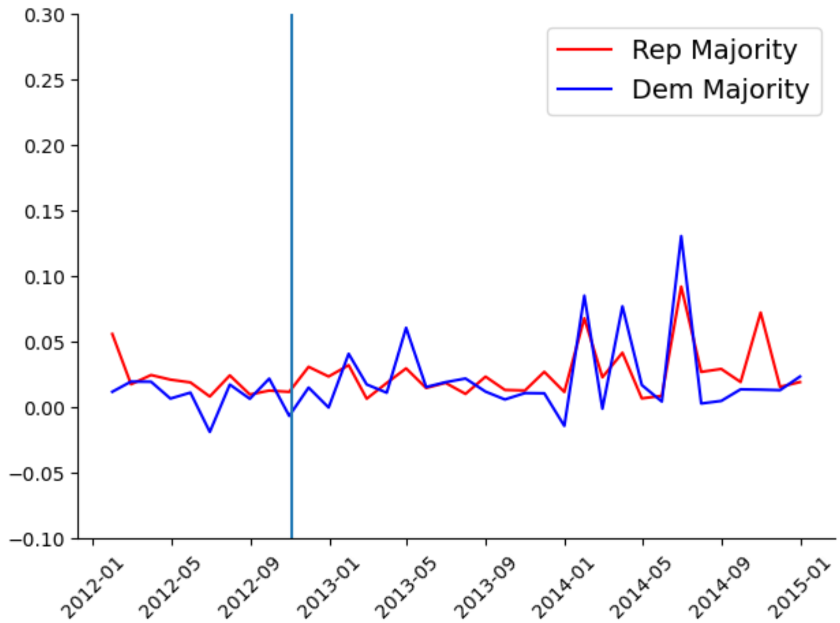


Table 18
Portfolio Allocations to Equity and Debt

This table reports slope coefficients estimated from regressions of stock and bond portfolio allocation shares on fund characteristics and controls. The dependent variables are the share of net assets in equity and bonds. For the first and third specification, we compare the reaction of funds with a Republican and Democratic majority on the fund team. For the remaining specifications, we study the reaction of all funds by the share of the fund team identified as Republican. All specifications include fund and date fixed effects. Standard errors are clustered by fund ID.

Dependent Variables:	Equity Net		Bond Net	
Model:	(1)	(2)	(3)	(4)
Variables				
Post 2016 Election × Rep. Majority	1.860*		-0.0028	
	(1.735)		(-0.0117)	
Post 2016 Election × Rep. Share		1.781**		-0.9656*
		(1.964)		(-1.960)
Fixed-Effects				
Date	Yes	Yes	Yes	Yes
Fund ID	Yes	Yes	Yes	Yes
Fit Statistics				
Observations	3,244	16,021	3,244	16,021
R^2	0.89176	0.92723	0.96665	0.90080
Within R^2	0.00814	0.00279	0.00164	0.00882

Clustered (Fund ID) co-variance matrix, t-stats in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 19
Analyst Political Alignment Impact on EPS Forecasts Including Covid

This table reports regressions of the form

$$\mathrm{EPS}_{ist} = \mathrm{Aligned}_{it} + \eta_t + \nu_{is}$$

where i indexes analyst, s stock and t time. The dependent variable is a stock-level (s) EPS forecast made by analyst i at time t. $\mathrm{Aligned}_{it}$ is a dummy variable that takes the value 1 if a President of the same party as the analyst is currently in office. η_t are date fixed-effects and ν_{is} is a stock-by-analyst fixed effect.

Dependent Variable:	Earnings-Per-Share (EPS) Forecast				
Horizon (Years Ahead)	One	Two	Three	Four	Five
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
Aligned	0.0586	0.1811***	0.1258**	-0.0474	0.3144
	(1.465)	(3.655)	(1.994)	(-0.6062)	(1.576)
Fixed-Effects					
Date (Month)	Yes	Yes	Yes	Yes	Yes
Stock x Analyst	Yes	Yes	Yes	Yes	Yes
Fit Statistics					
Observations	51,548	45,040	18,612	5,185	2,501
R^2	0.81023	0.85037	0.92423	0.92989	0.95109
Within R^2	6.91×10^{-5}	0.00059	0.00055	0.00011	0.00225

Clustered (Date (Month)) co-variance matrix, t-stats in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 20
Analyst Political Alignment Impact on EPS Forecasts by Year

This table reports regressions of the form

$$\text{EPS}_{ist} = \sum_{k \in T} \text{Aligned}_{it} \times \mathbb{I}\left[\text{Year}_k\right] + \eta_t + +\nu_{is}$$

where i indexes analyst, s stock and t time. The dependent variable is a stock-level (s) EPS forecast made by analyst i at time t. Aligned_{it} is a dummy variable that takes the value 1 if a President of the same party as the analyst is currently in office. η_t are a date fixed-effects and ν_{is} is a stock-by-analyst fixed effect. $\mathbb{I}\left[\text{Year}_k\right]$ is a indicator that takes the value 1 in year k.

Dependent Variable:	Earnings-Per-Share (EPS) Forecast				
Horizon (Years Ahead)	One	Two	Three	Four	Five
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
Aligned × I[2010]	0.4785***	0.4270***	0.6545***	0.4773	2.263***
	(3.962)	(3.244)	(2.929)	(1.412)	(4.290)
Aligned × I[2011]	0.3953***	0.6193***	0.1933	-0.1151	1.010**
	(3.226)	(4.339)	(1.467)	(-0.4222)	(2.437)
Aligned × I[2012]	0.3072***	0.4182***	0.2462	0.0001	0.5428*
	(2.950)	(3.105)	(1.146)	(0.0004)	(1.678)
Aligned × I[2013]	0.1088	0.1458	-0.1338	-0.4691*	-0.3016
	(1.164)	(1.411)	(-1.086)	(-1.807)	(-0.5589)
Aligned × I[2014]	-0.0643	0.0162	-0.0861	0.1539	-0.3465
	(-0.7203)	(0.1468)	(-0.5949)	(0.5553)	(-0.8356)
Aligned × I[2015]	-0.0455	0.0228	0.0189	0.1023	-0.5718
	(-0.5175)	(0.2280)	(0.1625)	(0.4266)	(-1.177)
Aligned × I[2016]	-0.0034	0.0602	0.1918**	0.0088	-0.4285
	(-0.0461)	(0.8034)	(2.444)	(0.0355)	(-1.398)
Aligned × I[2017]	0.0954	0.1269	0.1602	-0.3449	0.9377*
	(1.223)	(1.424)	(1.231)	(-1.378)	(1.960)
Aligned × I[2018]	0.2157*	0.3520***	0.3469**	0.2028	
	(1.738)	(3.577)	(2.495)	(0.7448)	
Aligned × I[2019]	0.1709	0.1108	-0.1059		
	(1.057)	(0.9458)	(-0.8625)		
Aligned × I[2020]	0.0138	0.2245			
	(0.0997)	(0.9251)			
Aligned × I[2021]	-0.7894***				
	(-3.332)				
Fixed-Effects					
Date (Month)	Yes	Yes	Yes	Yes	Yes
Stock x Analyst	Yes	Yes	Yes	Yes	Yes
Fit Statistics					
Observations	51,548	45,040	18,612	5,185	2,501
R^2	0.81046	0.85047	0.92438	0.93033	0.95239
Within R^2	0.00130	0.00131	0.00259	0.00633	0.02862

Clustered (Date (Month)) co-variance matrix, t-stats in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 21
Trump Election Diff-in-Diff by Industry

This table displays results from a diff-in-diff around Trump's election in 2016. Notably, each column estimates a different model fit only to stocks within a specific industry. For example, column (1) focuses on stocks within the consumer discretionary industry. The dependent variable is the stock-level fiscal year 2018 earnings-per-share forecast issued by a particular analyst. The independent variables are a Post-2016 election dummy and an indicator for whether the analyst is a Republican. We also include date and stock by analyst fixed effects.

$$\text{EPS}_{ist} = \mathbb{I}\left\{\text{Post-2016 Election}_t\right\} \times \mathbb{I}\left\{\text{Republican}_i\right\} + \nu_{is} + \gamma_t$$

The sample consists of forecasts in the prior and trailing twelve months after the 2016 election. All regressions include date and analyst-by-stock fixed effects.

Industry	Cons Disc	Health Care	IT	Financials	Industrials	Cons Staples	Utilities	Materials	Energy
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Variables									
$\mathbb{I}\{\text{Rep}_t\} \times \mathbb{I}\{\text{Post-2016 Election}_t\}$	0.1026*	-0.2094	0.0211	-0.1083	-0.2793	0.0024	-1.409	-0.5881*	0.1095
	(1.843)	(-1.384)	(0.3840)	(-1.077)	(-1.260)	(0.0542)	(-0.0003)	(-1.906)	(0.2490)
Fixed-Effects									
Date	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Stock x Analyst	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit Statistics									
Observations	2,752	933	948	917	735	948	177	267	111
R^2	0.99791	0.99360	0.99690	0.99797	0.98414	0.99287	0.99900	0.99268	0.99862
Within R^2	0.00186	0.00313	0.00031	0.00239	0.00658	4.43×10^{-6}	1.06×10^{-9}	0.06008	0.00155

Clustered (Date) co-variance matrix, t-stats in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 22
Analyst Forecasts by MSCI Industry Aggregation

This table displays results from a difference-in-differences specification comparing the change in analyst forecasts before and after Trump's election using different forecast horizons. We estimate these regressions over aggregated industries according to Morningstar's sectoral industry classifications using the sample of analysts identified as either Republican or Democratic.

Industry Classification	Cyclical			Sensitive			Defensive		
Dependent Variables:	2017	2018	2019	2017	2018	2019	2017	2018	2019
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Variables									
$\mathbb{I}\{\text{Rep}_t\} \times \mathbb{I}\{\text{Post-2016 Election}_t\}$	0.0523	0.0738	0.1540	-0.1349*	-0.0741	-0.1304	-0.0211	-0.0964	-0.0435
	(0.7410)	(1.011)	(0.9554)	(-1.756)	(-0.8040)	(-0.2922)	(-0.1777)	(-0.7562)	(-0.1103)
Fixed-Effects									
Stock x Analyst	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
anndats:gicdesc	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit Statistics									
Observations	4,453	3,947	1,767	2,708	1,985	753	2,409	2,058	1,152
R^2	0.99801	0.99789	0.99767	0.99212	0.99342	0.99536	0.99152	0.99365	0.99524
Within R^2	0.00052	0.00098	0.00122	0.00575	0.00110	0.00040	7.19×10^{-5}	0.00128	0.00010

Clustered (Stock x Analyst) co-variance matrix, t-stats in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*