4-2020

Essays on empirical asset pricing

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ESSAYS ON EMPIRICAL ASSET PRICING

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A DISSERTATION

In

ECONOMICS

Presented to the Singapore Management University in Partial Fulfilment

of the Requirements for the Degree of Ph.D. in Economics

2020

Supervisor of Dissertation

Ph.D. in Economics, Programme Director

Essays on Empirical Asset Pricing

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Submitted to School of Economics in Partial Fulfillment of

the Requirements for the Degree of Doctor of Philosophy in Economics

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2020

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Abstract

The dissertation consists of four chapters on empirical asset pricing. The first chapter reexamines the existence of time series momentum. Time series momentum (TSM) refers to the predictability of the past 12-month return on the next one-month return. Using the same data set as Moskowitz, Ooi, and Pedersen (2012) (MOP, henceforth), we show that asset-by-asset time series regressions reveal little evidence of TSM, both in- and out-of-sample. While the t-statistic in a pooled regression appears large, it is not statistically reliable as it is less than the critical values of parametric and nonparametric bootstraps. From an investment perspective, the performance of TSM strategy is virtually the same as that of a similar strategy that is based on historical sample mean and does not require predictability. Overall, the evidence on TSM is weak, particularly for the large cross section of assets.

The second chapter focuses on disagreement, which is regarded as the best horse for behavioral finance to obtain as many insights as classic asset pricing theories. Existing disagreement measures are known to predict cross-sectional stock returns but fail to predict market returns. We propose a disagreement index by aggregating information across individual measures using partial least squares (PLS) method. This index significantly predicts market returns both in- and out-of-sample. Consistent with the theory in Atmaz and Basak (2018), the disagreement index asymmetrically predicts market returns with greater power in high sentiment periods, is positively associated with investor expectations of market returns, predicts market returns through a cash flow channel, and can explain the positive volume-volatility relationship.

The third and fourth chapters investigate the impacts of political uncertainty. We focus on one type of political uncertainty, partisan conflict, which is caused by

the dispute or disagreement among party members or policy makers. Chapter 3 finds that partisan conflict positively predicts stock market returns, controlling for economic predictors and proxies for uncertainty, disagreement, geopolitical risk, and political sentiment. A one standard-deviation increase in partisan conflict is associated with a 0.54% increase in next month market return. The forecasting power is symmetric across political cycles and operates via a discount rate channel. Increased partisan conflict is associated with increased fiscal policy and healthcare policy uncertainties, and leads investors to switch their investments from equities to bonds.

Chapter 4 shows that intensified partisan conflict widens corporate credit spreads. A one standard deviation increase in partisan conflict is associated with a 0.91% increase in the next one-month corporate credit spreads after controlling for bond-issue information, firm characteristics, macroeconomic variables, uncertainty measures, and sentiment measures. The result holds when using instrumental variable to resolve endogeneity concerns. I further find that partisan conflict has a greater impact on corporate credit spreads for firms with higher exposure to government policies, including government spending policy and tax policy, and for firms with higher dependence on external finance. Firms that are actively involved in political activities are also more sensitive to changes in political polarization.

Contents

Acknowledgements

When I first started my Ph.D. study, I had a really fuzzy understanding about economics as an academic discipline. Over the past five years, many people have shaped my development for the better. I am truly indebted to my advisor, Professor Dashan Huang, who opens my minds and leads me into the frontier of empirical asset pricing. He teaches me not only the essential knowledge of asset pricing, but also the efficient ways of doing research. There were always ups and downs, and I express my deepest gratitude to his consistent encouragement and patient guidance. I would like to thank my co-chair, Professor Anthony Tay, for providing me with adequate flexibility of exploring research topics. Professor Guofu Zhou gets a special vote of thanks for his guidance on our first joint paper. I am also grateful to his kindness of being my reference and limitless supports on the job market. Meanwhile, I wish to thank the faculty inside and outside SMU, including Tse Yiu Kuen, Hao Liang, Roger Loh, Ekkehart Boehmer, and Quan Wen, who have offered cordial supports and valuable suggestions in related fields. Their diligence and strictness deeply motivate me and I hope I can continue to set for myself the same high standards that they expect of themselves and their students.

My classmates and friends have provided me with warm companionship in Singapore. Various staff members in the School of Economics have provided able administrative supports. The last words of thanks are reserved for my parents, for their upbringing and incredible love. They are always there to celebrate my small accomplishments and most importantly, to cheer me up through many downturns. This thesis is dedicated to my parents.

Chapter 1

Existence of Time Series Momentum[1]

Time series momentum (TSM) refers to the predictability of the past 12-month return on the next one-month return and is the focus of several recent influential studies. This chapter shows that asset-by-asset time series regressions reveal little evidence of TSM, both in- and out-of-sample. While the t-statistic in a pooled regression appears large, it is not statistically reliable as it is less than the critical values of parametric and nonparametric bootstraps. From an investment perspective, the TSM strategy is profitable, but its performance is virtually the same as that of a similar strategy that is based on historical sample mean and does not require predictability. Overall, the evidence on TSM is weak, particularly for the large cross section of assets.

1.1 Introduction

Whether past returns predict future returns is a central topic in finance. Fama (1965), French and Roll (1986), Lo and MacKinlay (1988), and Conrad and Kaul (1988), among others, show that past returns can positively predict future returns at a short horizon, but the magnitude is too small to be exploitable. Moskowitz, Ooi, and Pedersen (MOP, 2012) conclude with a much greater degree of predictability that time series momentum (TSM) is everywhere: The past 12-month return positively

[1]This is a joint work with Dashan Huang, Jiangyuan Li and Guofu Zhou

predicts the next one- to 12-month return for a comprehensive set of approximately 55 assets. In addition, MOP show that a TSM trading strategy, which buys assets if their past 12-month returns are positive and sells them otherwise, earns significant average and risk-adjusted returns. Hurst, Ooi, and Pedersen (2017) and Georgopoulou and Wang (2017) examine the TSM on a broader range of asset classes and longer sample periods, and they find similar results. Koijen, Moskowitz, Pedersen, and Vrugt (2018) use TSM portfolio returns as a risk factor to analyze carry trade. Kim, Tse, and Wald (2016) find that the profits of the TSM strategy are driven by volatility scaling and that its performance without volatility scaling is no better than that of a buy-and-hold strategy. Moreover, in a concurrent study, Goyal and Jegadeesh (2018) show that the traditional cross-sectional momentum strategy is more profitable than the TSM once the leverage ratio is properly adjusted. Despite an improved understanding, whether time series predictability is present at the 12-month frequency remains an open question.[2]

In this chapter, using the same data set as MOP (2012), we reexamine the statistical and economic evidence of TSM. From both time series and cross-sectional analyses, we find that the evidence on TSM is weak. Hence, concluding that TSM exists across the global asset classes appears questionable.

We conduct our study in three stages. In the first stage, we run a time series regression of monthly return for each asset on its past 12-month return. At the 10% significance level, 47 of the 55 assets have a t-statistic of less than 1.65, suggesting that the in-sample evidence of TSM is weak.[3] Since Welch and Goyal (2008), studies on return predictability have shifted the focus to out-of-sample performance. We compute the standard Campbell and Thompson (2008) out-of-sample R^2_{OS} for each asset and find that only three assets deliver significant R^2_{OS} at the 10% level. Univariate time series regressions thus indicate that the evidence on time series predictability is weak among the assets.

[2]In this chapter, the terms "TSM" and "time series predictability" are used interchangeably.

[3]The multiple test framework of Harvey, Liu, and Zhu (2016) could suggest even weaker evidence on TSM.

In the second stage, we follow MOP's approach and run a pooled regression by stacking all asset returns together. Consistent with MOP, we find a t-statistic of 4.34 in the regression of predicting the next one-month return using the past 12-month return. At the conventional critical level of 2, one could interpret this t-statistic of 4.34 as strong evidence against no predictability. We argue that the pooled regression is likely to over-reject the null hypothesis for three reasons. First, if assets have different mean returns, the slope estimate from the pooled regression without controlling for fixed effects tends to be biased upward (Hjalmarsson, 2010). Second, as a predictor, the past 12-month return is persistent and can generate substantial size distortions (Ang and Bekaert, 2007; Boudoukh, Richardson, and Whitelaw, 2008; Campbell and Yogo, 2006; Hodrick, 1992; Li and Yu, 2012; Stambaugh, 1999). Third, because volatility varies dramatically across assets, volatility scaling in the pooled regression without controlling for fixed effects further exacerbates the upward bias.

To assess the degree of over-rejection, we use two bootstrap methods. The first is a parametric wild bootstrap that simulates samples based on the fitted pooled regression residuals, and the second is a nonparametric pairs bootstrap that resamples the predictor and the dependent variable simultaneously. Both methods accommodate conditional heteroskedasticity, but the latter allows for more general data-generating processes. We find that the 5% critical values of the bootstraps are 12.53 and 4.83, respectively. They are larger than 4.34, the t-statistic from the pool regression with real data. This finding is robust to all alternative cases, such as within each asset class, with different sample periods, and without volatility scaling. Hence, a high t-statistic found by MOP is not statistically significant in supporting the existence of TSM.

In the third stage, we examine why the TSM strategy is profitable even though the statistical evidence on time series predictability is weak. In their study, MOP (2012) construct the TSM strategy by buying assets with positive past 12-month return and selling assets with negative past 12-month return. At the same time, MOP assign a portfolio weight equal to 40% divided by the asset volatility, so that for each asset the ex ante annualized volatility is 40%. This volatility scaling can make the attribution of the performance of the TSM strategy complex. To separate the volatility effect, we follow Kim, Tse, and Wald (2016) and Goyal and Jegadeesh (2018) and focus on the TSM strategy with the simple equal weighting without volatility scaling as the benchmark. Volatility scaling is not an issue if all strategies are based on volatility-scaled returns when comparing their performances.

We examine the performance of the TSM strategy in four ways. First, we investigate the performance of an alternative trading strategy that does not require predictability. Based on the observation that a high mean asset is more likely to have a positive past 12-month return and therefore is more likely to be bought by the TSM strategy, we propose a times series history (TSH) strategy that buys assets if their historical mean returns are positive and sells them otherwise, which is theoretically profitable even if asset returns are independent over time, but some have significantly higher means than others. We find that the TSM and TSH strategies perform virtually the same and their differences in average returns, as well as in risk-adjusted ones, are indifferent from zero. Also, we find that the performances of the TSM and TSH strategies mainly stem from their long legs, and their short legs have insignificant average and risk-adjusted returns. This result suggests that both the TSM and TSH tend to long assets with greater mean returns and short those with lower means. Because the TSH strategy is defined without requiring time series predictability, it seems questionable to attribute the performance of the TSM strategy to predictability.

Second, we report the results with alternative portfolio weighting schemes, such as volatility weighting as in MOP (2012), past 12-month return weighting, and equal weighting with a zero-investment constraint as in Goyal and Jegadeesh (2018). The results are similar, and the alpha differential between the TSM and TSH strategies is always indifferent from zero. In short, the profitable performance of the TSM strategy is similar to that of the TSH strategy that requires no predictability, suggesting that the performance of the TSM does not necessarily support predictability at the 12-month frequency across asset classes.

Third, based on the predictive slope of Lewellen (2015), we examine the overall predictability of TSM across assets. The slope measures how realized returns are explained by predicted returns. If the past 12-month return perfectly predicts the next one-month return, the slope should have a value of one. We find that for the TSM forecasts the slope has a value close to zero, suggesting that the TSM forecasts have little predictive power.[4] When regressing the TSM forecasts on the TSH forecasts, the slope is very close to one, irrespective of whether the TSM forecasts use volatility scaling or not. This result indicates little difference in predictability between the two forecasts, suggesting, again, no evidence of TSM across the assets.

Fourth, of interest is examining under what conditions the TSM is a better trading strategy than the TSH. Based on one thousand simulated samples by using pooled regression with varying assumed degrees of time series momentum (i.e., the slope varies from 0.1 to 0.4), we find that the TSM and TSH strategies perform similarly when the slope is 0.1 (with real data, the slope of the pooled regression is 0.08, controlling for fixed effects). When the slope is 0.2, the TSM outperforms the TSH, but the difference is statistically insignificant. This means that if there is genuine time series predictability, the advantage of the TSM strategy is not apparent as long as the slope is small. When the slope is 0.4, the TSM dominates the TSH in the sense that it does better in almost all the simulated samples. Because the two strategies generate similar performance using real data, our simulation indicates that the evidence of TSM is likely weak if it exists. Combined with other results, the TSM is unlikely to be statistically significant for all the assets. In short, a lack of empirical evidence exists to support the hypothesis that the TSM is everywhere.

As a final remark, our results do not claim in any way that there is no predictability in the asset classes, but that the predictability, if it exists, is not as simple as a constant 12-month return rule. The best example could be the stock

[4] Lewellen (2015) shows in his footnote 3 that a slope of less than 0.5 under-performs a naive forecast. Han, He, Rapach, and Zhou (2019) discuss more properties of the slope.

market, with ample evidence that its risk premium can be predicted by a wide range of predictors such as macroeconomic variables and investor sentiment [see, e.g., Jiang, Lee, Martin, and Zhou (2019) for the latest literature]. Cross-sectionally, since Jegadeesh and Titman's (1993) discovery of momentum, there have been hundreds of potential anomalies [see, e.g., Hou, Xue, and Zhang (2019) for the replication]. Recently, Gu, Kelly, and Xiu (2018) and Freyberger, Neuhierl, and Weber (2019), among others, use machine learning tools to find even stronger predictability.[5] However, none of them is related to the TSM.

The rest of this chapter is organized as follows. Section 1.2 introduces data we use. Section 1.3 shows that asset-by-asset regressions suggest that the evidence of TSM, if any, is weak. Section 1.4 finds that the pooled regression overstates the presence of TSM and that bootstrap-corrected t-statistics cannot reject the null hypothesis of no predictability. Section 1.5 shows that the TSM strategy performs the same as an alternative trading strategy that does not require predictability. Section 1.6 concludes.

1.2 Data

We collect futures prices for 24 commodities, nine developed country equity indexes, 13 developed government bonds, and nine currency forwards from the same data sources as MOP (2012). These 55 instruments are the same as those in MOP's Table 1.[6] The sample period is from January 1985 to December 2015. For each day, we calculate the daily excess return of each futures contract with the nearest- or next-nearest-to-delivery contract and compound the daily returns to a cumulative month return index. For brevity, returns in this chapter always refer to excess returns, unless otherwise stated.

[5] Rapach, Strauss, and Zhou (2013) use LASSO, a major machine learning tool, to forecast international equity markets, which is the earliest study that we find in the finance literature applying LASSO to predict stock returns.

[6] MOP use 12 cross-currency pairs in trading strategies but report only nine underlying currencies in their summary statistics table. To maximally replicate the results, we focus on the nine underlying currencies. As a consequence, we examine a total of 55 assets, not 58.

Table 1.1 reports the sample mean (arithmetic), volatility (standard deviation), and first-order autocorrelation of the returns on the 55 futures contracts. The mean and volatility are annualized and represented in percentage. Significant variations exist in mean return and volatility across different contracts. Within the commodity asset class, 24 contracts yield positive, zero, and negative mean returns, from −11.33% for natural gas to 12.31% for unleaded gasoline. The volatility ranges from 13.56% for cattle to 50.79% for natural gas. On average, the mean return and volatility are 2.59% and 28.50%, respectively. The nine equity index futures contracts are more homogenous, with mean return from 3.09% for TOPIX (Tokyo Stock Price Index) to 9.69% for DAX (German Stock Index) and volatility from 15.87% for FTSE 100 (Financial Times Stock Exchange 100 Index) to 23.21% for FTSE/MIB (Italian National Stock Exchange Index). On average, the return and volatility are 7.24% and 19.26%, respectively. Finally, bond futures and currency forwards earn lower mean returns with lower volatilities. Within each asset class, the average mean and volatility are 4.54% and 7.85% for bond futures and 1.22% and 10.90% for currency forwards, respectively. Table 1.1 also highlights a well-known fact that the past one-month return cannot predict the next one-month return, because the first-order correlation is generally close to zero.

1.3 Univariate Time Series Regression

In this section, we run univariate time series regressions to explore the predictability of the past 12-month return for individual assets. These regressions clearly tell which asset return can be predicted by its past 12-mopnth return and which cannot, thereby providing direct evidence on whether the finding in MOP (2012) is common across asset classes.

1.3.1 In-sample performance

Time series regressions are standard for identifying return predictability, but the pooled regression is seldom used in the literature. Ang and Bekaert (2007) and Hjalmarsson (2010) are exceptions, showing a heterogeneous pattern in predictability. For example, Ang and Bekaert (2007, p.663) show that, in contrast to the US, the UK and Japan return predictability disappears when expected returns are constrained to be non-negative and conclude that "none of the [return predictability] patterns in other countries resembles the US pattern."

For each asset, we run the predictive regression

$$r^i_{t+1} = \alpha + \beta r^i_{t-12,t} + \varepsilon^i_{t+1}, \tag{1.1}$$

where r^i_{t+1} is the return of asset i in month $t+1$ and $r^i_{t-12,t}$ is its past 12-month return (i.e., the return between months $t-12$ and t). The predictive power is based on either the regression slope β or the R^2 statistic. If the regression R^2 statistic is significantly larger than zero with a positive β, then asset i displays TSM, i.e., its past 12-month return predicts the next one-month return.

Table 1.2 reports the regression slope, the Newey-West t-statistic, and R^2. We have four observations. First, the presence of TSM is not prevalent. Of the 55 assets, only eight display significant regression slopes at the 10% level, representing 15% of assets (only three significant at the 5% level). Second, the significance is not concentrated but disperse among the four asset classes, including three commodities, two equity indexes, two government bonds, and one currency. Third, although not significant, 17 assets deliver negative slopes, amounting to 31% of the assets. Fourth, the R^2s are small, with an average of 0.39%, and only five assets generate an R^2 larger than 1%. To have an intuitive understanding of the predictive performance, Panel A of Fig. 1.1 plots the R^2 statistic for each asset. Only two assets have R^2s that stand out above 2%.

1.3.2 Out-of-sample performance

Due to concerns of data mining and structural breaks, studies on return predictability have shifted the focus to out-of-sample performance since Welch and Goyal (2008). To investigate the out-of-sample performance of TSM, we use the Campbell and Thompson (2008) out-of-sample R^2_{OS} statistic as the assessment criterion, which is defined as

$$R^2_{OS} = 1 - \frac{\sum_{t=K}^{T-1}(r^i_{t+1} - \hat{r}^i_{t+1})^2}{\sum_{t=K}^{T-1}(r^i_{t+1} - \bar{r}^i_{t+1})^2}, \tag{1.2}$$

where K is the initial sample size for parameters training, $\hat{r}^i_{t+1}$ is the expected return estimated with information up to month t and calculated as $\hat{r}^i_{t+1} = \hat{\alpha}_t + \hat{\beta}_t r^i_{t-12,t}$, $\hat{\alpha}_t$ and $\hat{\beta}_t$ are the coefficients of the time series regression Eq. (1.1), and $\bar{r}^i_{t+1}$ is the sample mean of asset i with data up to month t. The choice of K is ad hoc in the literature, which depends on the nature of the possible model instability and the timing of the possible breaks. Hansen and Timmermann (2012) theoretically show that a large K is preferable if the data-generating process is stationary, but it comes at the cost of low power as there are fewer observations for out-of-sample evaluation. A small K can give the out-of-sample test more desirable size properties, but it perhaps does not provide precise estimation. For these reasons, we select the first 15 years of data for in-sample training and the remaining 16 years of data for out-of-sample evaluation. That is, the full sample period is from January 1985 to December 2015 and the out-of-sample period is from January 2000 to December 2015.

Welch and Goyal (2008) show that the sample mean is a very stringent out-of-sample benchmark. If $R^2_{OS} > 0$, the forecast $\hat{r}^i_{t+1}$ outperforms the sample mean in terms of mean squared forecast error (MSFE). Empirically, they show that the in-sample forecasting abilities of a variety of return predictors generally do not hold in out-of-sample tests. To ascertain whether a forecast delivers a statistically significant improvement in MSFE relative to the sample mean, we use the Clark and West (2007) statistic to test the null hypothesis that the MSFE of the sample mean forecast is less than or equal to the MSFE of the forecasted expected return, corresponding to H_0: $R^2_{OS} \leq 0$ against H_A: $R^2_{OS} > 0$.

Although no strict relation exists between the in-sample and out-of-sample performance (Inoue and Kilian, 2005), the last column of Table 1.2 and Panel B of Fig. 1.1 show that the R^2_{OS} is smaller than the in-sample R^2 on average. Of the 55 assets, 45 have negative R^2_{OS}, indicating no out-of-sample predictability. Of the remaining ten assets with positive R^2_{OS}, only three are significant at the 10% level, which are the two-year European bond, the two-year US bond, and the JPY/USD (Japanese yen/US dollar) forward. As a result, the average R^2_{OS} across the 55 assets is -0.67%, suggesting that there is no TSM out of sample.

To further explore the robustness, Fig. 1.2 plots the R^2s and R^2_{OS}s of TSM by regressing the next one-month return on the past one-, three-, and six-month return, respectively. The results are similar to the case with the past 12-month return in Fig. 1.1. In addition, we consider volatility scaling when running the asset-by-asset time series regressions. The results are still quantitatively the same: Only the same three assets have significant R^2_{OS}s. In sum, based on the typical univariate time series regression, the evidence of TSM across all the assets is very weak.

1.4 Pooled Regression

In this section, we first replicate the results in MOP (2012) and then show that the pooled regression tends to overstate the presence of TSM.

1.4.1 The t-statistic

By stacking all futures contracts' returns and dates, MOP (2012) run a pooled predictive regression of monthly returns scaled by volatility on the scaled returns lagged h months,

$$r^i_{t+1}/\sigma^i_t = \alpha + \beta r^i_{t-h+1}/\sigma^i_{t-h} + \varepsilon^i_{t+1}, \tag{1.3}$$

where r^i_{t+1} is asset i's return in month $t+1$ and σ^i_t is the ex ante annualized volatility estimated by its exponentially weighted lagged squared daily returns:

$$(\sigma^i_t)^2 = 261 \sum_{j=0}^{\infty} (1-\delta)\delta^j (r^i_{t-1-j} - \bar{r}^i_t)^2, \tag{1.4}$$

where $\bar{r}^i_t$ is the exponentially weighted average return and δ is chosen so that $\sum_{j=0}^{\infty}(1-\delta)\delta^j = 60$ days.

MOP (2012) also use an alternative specification with the sign of lagged returns as the regressor to examine the robustness of TSM,

$$r^i_{t+1}/\sigma^i_t = \alpha + \beta sign(r^i_{t-h+1}) + \varepsilon^i_{t+1}, \tag{1.5}$$

where *sign* is the sign function that equals $+1$ when $r^i_{t-h+1} \geq 0$ and -1 when $r^i_{t-h+1} < 0$.

As in MOP (2012), we calculate the t-statistics by clustering the standard errors by time (month) and plot the t-statistics of the pooled regression slopes with lagged returns from one month to 60 months in Fig. 1.3.[7] Qualitatively, we confirm MOP (2012) that the past 12-month return of each asset is a positive predictor of its future returns for one month to 12 months in the pooled regression. After 12 months, the forecasting sign changes and the forecasting power decays. In Fig. 1.3, Panel A shows the results with Eq. (1.3) over all asset classes, and Panel B replaces the lagged return with its sign as Eq. (1.5). Both have a sizable t-statistic of about 4 at the 12-month horizon.

Panels C to F of Fig. 1.3 plot the t-statistics of Eq. (1.5) within each asset class and exhibit a similar pattern. The t-statistics appear to show a strong return continuation for the first 12 months and weak reversal for the following 48 months. Overall, Fig. 1.3 appears to provide strong evidence on TSM. However, the t-statistics at the conventional level tend to overstate the predictability of the past

[7]The t-statistics that double-cluster the standard errors by time and asset are quantitatively similar.

12-month return.

1.4.2 Estimation bias

In Eq. (1.3), MOP (2012) make an implicit assumption that the mean returns of all assets are the same by imposing a common intercept. From Table 1.1, the sample means of individual assets vary dramatically across asset classes. In the literature, Jorion and Goetzmann (1999) show strong evidence that the equity premium varies across countries. Ang and Bekaert (2007) investigate return predictability with pooled regression but explicitly consider the variation in average returns. Menzly, Santos, and Veronesi (2004) analyze cross-sectional differences in time series return predictability.

To highlight fixed effects, a possible specification is

$$r^i_{t+1}/\sigma^i_t = \alpha + \beta r^i_{t-h+1}/\sigma^i_{t-h} + \mu_i/\sigma_i + \varepsilon^i_{t+1}, \tag{1.6}$$

where μ_i and σ_i are the unconditional mean and volatility of asset i. Hence, the estimate of β from Eq. (1.3) should be

$$\hat{\beta} = \beta + \frac{\mathrm{Cov}(r^i_{t-h+1}/\sigma^i_{t-h}, \mu_i/\sigma_i)}{\mathrm{Var}(r^i_{t-h+1}/\sigma^i_{t-h})}. \tag{1.7}$$

If all assets have the same Sharpe ratio (or mean, if volatilities are the same), the second term is zero. Otherwise, it would be significantly positive when the number of assets is large, as the correlation between realized returns and their means is mechanically positive. As a result, the slope estimate of Eq. (1.3) is biased upward.

The question then is whether the 55 assets have the same mean or Sharpe ratio. We perform four tests for this hypothesis. The first is analysis of variance (ANOVA), which was proposed by Ronald A. Fisher in 1918 (Fisher, 1918) with two assumptions: normality and homoskedasticity. The second is B.L. Welch's ANOVA (Welch, 1951), which allows the variance to be Heteroskedastic. The third is the Kruskal-Wallis test, which relaxes both the normality and homoskedasticity assumptions (Kruskal and Wallis, 1952), and the fourth is a bootstrap test. When applying these four tests to real data, Table 1.3 shows that the null hypothesis that all 55 assets have the same mean is strongly rejected. In addition, we reject the null that they have the same Sharpe ratio. Therefore, the evidence of TSM shown in MOP (2012) is at least partially driven by the fixed effects.

The fixed effects are not easily corrected in the predictive regression framework. Statistically, Hjalmarsson (2010) shows that when different assets have different average returns, the pooled regression, after controlling for fixed effects, suffers from a looking-forward bias because the time series demeaning of the data requires information after month t, which induces a correlation between the lagged value of the demeaned regressor and the error term in the predictive regression.

In addition to the fixed effects, two more reasons can lead to overstating the evidence on time series predictability. First, as a predictor, the past 12-month cumulative return is persistent and can generate substantial size distortions (Ang and Bekaert, 2007; Boudoukh, Richardson, and Whitelaw, 2008; Campbell and Yogo, 2006; Hodrick, 1992; Li and Yu, 2012; Stambaugh, 1999; Valkanov, 2003). For example, Ang and Bekaert (2007) show substantial size distortions with the Newey-West t-statistic when predicting stock returns with persistent variables. Second, because volatility varies dramatically across assets, volatility scaling in the pooled regression without controlling for fixed effects can further exacerbate the upward bias. For example, in Eq. (1.6), even when all assets have the same mean, volatility scaling generates the fixed effects as σ_i varies dramatically across assets. MOP (2012) also explore the pooled regression in Eq. (1.5) by using the sign of the past 12-month return as the predictor, which can distort and change the true statistical significance as the sign of the past 12-month return is highly skewed.

1.4.3 Bootstrap tests

Due to the concerns discussed above, the t-statistic from the pooled regression is questionable. To correctly evaluate the statistic significance, we use bootstrap to simulate the distribution of the t-statistic and define its 97.5% quantile as the simulated t-statistic for significance at the 5% level. If the t-statistic from the real data is larger than 1.96 but smaller than the simulated t-statistic, we can conclude that the pooled regression tends to overreject the null hypothesis and no significant evidence supports TSM.

We use two standard bootstrap approaches. The first is a more restrictive parametric wild bootstrap that samples data based on the pooled regression residuals, and the second is a more general nonparametric pairs bootstrap that resamples the predictor and the dependent variable simultaneously. Both approaches accommodate conditional heteroskedasticity, but the second allows for a wider range of data-generating processes. Pairs bootstrap is considered the most general and applicable method of bootstrapping.

Wild bootstrap Suppose the true data-generating process is as Eq. (1.3). Let $\hat{\alpha}$ and $\hat{\beta}$ be the estimates from the full sample of real data. Then, the residuals are

$$\hat{\varepsilon}^i_{t+1} = r^i_{t+1}/\sigma^i_t - \hat{\alpha} - \hat{\beta} r^i_{t-h+1}/\sigma^i_{t-h}. \tag{1.8}$$

We simulate a pseudo sample path with T observations as

$$r^{i*}_{t+1}/\sigma^{i*}_t = \hat{\alpha} + \hat{\beta} r^i_{t-h+1}/\sigma^i_{t-h} + \hat{\varepsilon}^i_{t+1} v^i_{t+1}, \tag{1.9}$$

where $*$ indicates that the value is a bootstrapped observation and v^i_t is a random draw from a two-point Rademacher distribution with mean 0 and variance 1:

$$v^i_t = \begin{cases} 1 & \text{with probability } 1/2, \\ -1 & \text{with probability } 1/2. \end{cases} \tag{1.10}$$

This distribution has an appealing property that the error-in-rejection probability is minimal when the sample size is small, and it is robust to other distributions such as the Mammen distribution and standard normal distribution (Davidson and Flachaire, 2008). After constructing a pseudo sample path, we run pooled regression Eq. (1.3). We repeat this procedure one thousand times to calculate the simulated t-statistic of $\hat{\beta}$.

Pairs bootstrap Pairs bootstrap resamples T pairs of $(r^i_{t+1}/\sigma^i_t, r^i_{t-h+1}/\sigma^i_{t-h})$ with replacement from the real data and uses these pairs to run the pooled regression

$$r^{i*}_{t+1}/\sigma^{i*}_t = \alpha_h + \beta_h r^{i*}_{t-h+1}/\sigma^{i*}_{t-h} + \varepsilon^i_{t+1}. \tag{1.11}$$

Hence, the simulated t-statistic of $\hat{\beta}$ can be calculated after repeating the procedure one thousand times. This bootstrap allows not only more general data-generating processes, but also potential model misspecification [e.g., $\mathrm{E}(r)$ is not a linear function of r_{t-h+1}].

Table 1.4 reports the t-statistics with real data and the bootstrapped t-statistics. Consistent with Ang and Bekaert (2007), the t-statistics that cluster by time tend to overreject the null hypothesis. For example, when forecasting the next one-month return with the past one-month return, the t-statistic from the real data is 3.11, suggesting strong evidence of TSM. However, this is not the case because the bootstrapped t-statistics are 9.26 and 3.63, respectively. Similarly, when forecasting the next one-month return with the past 12-month return, the t-statistic from the real data is 4.34, and the simulated t-statistics are 12.53 and 4.83, respectively, suggesting that the evidence is weak in support of TSM. Moreover, forecasting with the sign of lagged returns does not support TSM either.

Table 1.5 presents the simulated t-statistics within each asset class. For brevity, we report the results of predicting the next one-month return with the past one-, three-, six-, and 12-month return, respectively. Consistent with Tables 1.2 and 1.4, the results reveal that TSM is unlikely to be present in any of the four asset classes.

Does volatility scaling play a role in estimating the regression slopes and detecting the existence of TSM because MOP (2012) run Eqs. (1.3) and (1.5) with volatility scaling while volatility varies across assets and is predictable by its lagged values (Paye, 2012)? Table 1.6 reports the t-statistics with real data and boostrapped t-statistics from Eqs.(1.3) and (1.5) without volatility scaling. The results display two empirical facts. First, the t-statistics without volatility scaling are much smaller than those with volatility scaling. For example, when predicting the next one-month return with the past one- and 12-month return without volatility scaling, the t-statistics of the regression slope are 1.80 and 1.68, respectively, which are much smaller than the values with volatility scaling in Table 1.4 (3.11 and 4.34), lending little support to TSM. Second, when predicting the next one-month return with the signs of the past one- and 12-month return, the t-statistics are 2.20 and 3.72, respectively, and they are smaller than that with volatility scaling. Therefore, volatility scaling plays a role. In fact, it seems at least partially responsible for the performance of the TSM trading strategy.

This chapter extends the sample ending period of MOP (2012) from 2009 to 2015 and raises a possibility that TSM exists before 2009 and disappears thereafter. Table 1.7 reports the results for the 1985 to 2009 sample period and rules out the possibility. The t-statistics from real data are still smaller than the simulated t-statistics, regardless of which bootstrap approach is employed. For example, when forecasting the next one-month return with the past 12-month return, the t-statistic is 4.48 with real data, but it is 12.76 and 4.96 with the two bootstrap approaches, respectively. The TSM is also insignificant for each asset class. The results are reported in the Online Appendix.

1.4.4 Controlling for fixed effects

Earlier evidence shows that the assets do not have the same mean, implying that fixed effects should be controlled in the pooled regression. In so doing, one can run the pooled regression by removing the asset means. The bootstrap procedures can also make a similar modification. The question is whether controlling for fixed effects can alter substantially the evidence on TSM.

Following Gonçalves and Kaffo (2015), we now compute the t-statistic from the pooled regression

$$r^i_{t+1}/\sigma^i_t - \overline{r^i/\sigma^i} = \beta(r^i_{t-h+1}/\sigma^i_{t-h} - \overline{r^i_{-h+1}/\sigma^i_{-h}}) + \varepsilon^i_{t+1}, \tag{1.12}$$

where $\overline{r^i/\sigma^i}$ and $\overline{r^i_{-h+1}/\sigma^i_{-h}}$ denote the time series averages of r^i_{t+1}/σ^i_t and $r^i_{t-h+1}/\sigma^i_{t-h}$, respectively. Suppose the estimate of β is $\hat{\beta}_{FE}$, then the residual $\hat{\varepsilon}^i_{t+1}$ can be calculated as

$$\hat{\varepsilon}^i_{t+1} = r^i_{t+1}/\sigma^i_t - \overline{r^i/\sigma^i} - \hat{\beta}_{FE}(r^i_{t-h+1}/\sigma^i_{t-h} - \overline{r^i_{-h+1}/\sigma^i_{-h}}). \tag{1.13}$$

Then, we simulate a pseudo sample path with T observations as

$$(r^i_{t+1}/\sigma^i_t - \overline{r^i/\sigma^i})^* = \hat{\beta}_{FE}(r^i_{t-h+1}/\sigma^i_{t-h} - \overline{r^i_{-h+1}/\sigma^i_{-h}}) + \hat{\varepsilon}^i_{t+1}v^i_{t+1}, \tag{1.14}$$

where v^i_{t+1} follows the Rademacher distribution. We can then estimate the model with the simulated samples and repeat the procedure one thousand times, to obtain the critical value of the wild bootstrap t-statistic.

Regarding the pairs bootstrap, we resample T pairs of the predictor and the dependent variable in Eq. (1.12) with replacement from real data after de-meaning and then use these pairs to rerun the pooled regression. The pairs bootstrap t-statistic is naturally obtained after repeating the procedure one thousand times. Both the wild and pairs bootstraps do not suffer from the incidental parameter bias emphasized in Gonçalves and Kaffo (2015), because the sample size is relatively large here.

Table 1.8 reports the t-statistics from the pooled regression and the bootstrapped t-statistics. Compared with Table 1.4, after controlling for the fixed effects, the t-statistic is smaller than that without controlling for fixed effects. For example, when predicting the next one-month return with the past 12-month return, the t-statistic is 4.34 in Table 1.4 and 3.37 in Table 1.8. The most important result is that the t-statistic of 3.37 when controlling for fixed effects does not affect the conclusion that insufficient evidence exists in support of TSM. In the Online Appendix, we show that this finding is robust to the cases within each asset class and without volatility scaling.

1.4.5 Out-of-sample performance

A further implicit assumption of a pooled regression is that all 55 futures contracts are homogenous with the same slope in Eqs. (1.3) and (1.5). If the individual slopes are all identical, the pooled estimate converges to the common slope, and pooling data leads to a more precise estimate than the individual time series regression estimate. Whether the slopes of all individual assets are identical or not, there is no guarantee that pooling the data will help. Nevertheless, whether pooling the data improves out-of-sample performance is of interest to examine empirically.

Fig. 1.4 plots the out-of-sample R^2_{OS} for each individual asset. In Panel A, we present the results with volatility scaling when running the pooled regression. To predict returns in month $t+1$ at the end of month t, we run pooled regression Eq. (1.3) with returns up to month t as MOP (2012). Let $\hat{\alpha}_t$ and $\hat{\beta}_t$ be the estimated intercept and slope. We calculate the expected return of asset i for month $t+1$ as

$$\mathrm{E}_t(r^i_{t+1}) = \hat{\alpha}_t \sigma^i_t + \hat{\beta}_t \frac{r^i_{t-12,t}}{\sigma^i_{t-1}} \sigma^i_t, \tag{1.15}$$

which can be plugged into Eq. (1.2) to calculate the R^2_{OS} accordingly.

In comparison with earlier univariate regressions (Table 1.2), the pooled regression does improve the out-of-sample forecasting performance in some of the markets. The R^2_{OS} is significantly positive for three commodity futures contracts: cocoa, copper, and gold. Of the nine international equity markets, six are significant at the 10% level, with the remaining three positive but not significant. The average R^2_{OS} in the equity markets is 2.08%, indicating potential economic significance as well (Campbell and Thompson, 2008). The R^2_{OS}s in the bond and currency markets are generally negative or slightly positive. The two exceptions are the two-year

European bond and two-year US bond. Overall, if there is any TSM, it appears to show up in the international equity markets only, not present in the entire cross section of assets.

Panel B of Fig. 1.4 plots the R^2_{OS} for each asset without volatility scaling when running regression Eq. (1.3). The out-of-sample performance does not change significantly. Untabulated results show that the value of R^2_{OS} with volatility scaling is generally similar to that without volatility scaling. Two exceptions are the two-year European bond and two-year US bond, which have extreme positive R^2_{OS} in the case with volatility scaling (15.18% and 3.48%) but extreme negative R^2_{OS} in the case without volatility scaling (-19.54% and -16.71%). As such, the average R^2_{OS} using volatility scaling is -0.06%, which is larger than -0.35% without volatility scaling.

Overall, to a certain extent, a pooled regression can improve the out-of-sample forecasting power relative to the asset-by-asset time series regression, but such improvement is restricted to some specific assets. For the entire cross section of assets, it does little to improve their out-of-sample forecasting performances, and it cannot provide significant support for TSM either.

1.5 Trading Strategy

In this section, we examine the source of profitability of the TSM strategy proposed by MOP (2012). We show in various ways that its performance does not necessarily indicate that TSM exists across assets.

1.5.1 TSM versus TSH at asset level

The early univariate regressions show that time series predictability is not a common feature across assets, which suggests that the performance of the TSM strategy perhaps is not attributed to predictability, at least not entirely. Furthermore, it raises the possibility that some strategies that do not require predictability can perform as well as the TSM strategy. As it turns out, this is the case.

Suppose the return of asset i follows an independent and identically distributed normal distribution with mean μ^i and volatility σ^i. Then, the probability of the past 12-month return being positive is

$$\Pr(r^i_{t-12,t} > 0) = 1 - \Pr\left(\frac{r^i_{t-12,t} - 12\mu^i}{\sqrt{12}\sigma^i} \leq -\sqrt{12}\frac{\mu^i}{\sigma^i}\right) = \Phi(\sqrt{12}\mu^i/\sigma^i), \quad (1.16)$$

where $\Phi(\cdot)$ is the $N(0,1)$ cumulative distribution function. Hence, without time series predictability, the TSM strategy tends to buy an asset with high mean return (i.e., Sharpe ratio). Based on this observation, we consider an alternative strategy based on the time series history of asset i's return

$$r^{\mathrm{TSH},i}_{t+1} = sign(r^i_{1,t})r^i_{t+1}, \quad (1.17)$$

where $r^i_{1,t}$ is the accumulative return of asset i from month 1 to month t or the historical sample mean multiplied by t.

Volatility scaling on the TSH strategy is unnecessary because we compare the TSM and TSH strategies at the asset level in this section. Without volatility scaling, the corresponding return of the TSM strategy in month $t+1$ for asset i is

$$r^{\mathrm{TSM},i}_{t+1} = sign(r^i_{t-12,t})r^i_{t+1}. \quad (1.18)$$

Comparing Eqs. (1.17) and (1.18), the TSM strategy attempts to exploit possible predictability of the past 12-month return, and the TSH does not rely on any predictability at all. Our goal here is to examine their performances across assets.

Table 1.9 reports the average returns and Sharpe ratios, and their differences, of the TSM and TSH strategies based on Eqs. (1.18) and (1.17), respectively. The results show that the TSM strategy generally performs the same as the TSH strategy. Of the 55 assets, only five show that the TSM strategy generates a higher average return than the TSH strategy. When we use the Sharpe ratio as the performance measure, the results remain unchanged. Thus, the TSM strategy does not significantly outperform at the asset level the TSH strategy that does not require predictability.

1.5.2 TSM versus TSH at portfolio level

Even though no time series predictability exists, the TSM strategy could still be profitable. Conrad and Kaul (1988), Jegadeesh (1990), and Jegadeesh and Titman (1993) note that if there are differences in mean returns, a strategy that buys high-mean assets using the proceeds from selling low-mean assets has a natural tilt toward high-mean assets. To see this, consider just two assets. If the mean return of the first asset far exceeds that of the second, buying the first and shorting the second is profitable. Because the past 12-month return can be viewed as an estimate of the mean return, the TSM strategy could profit from the differences in mean returns. If this is the case, it will unlikely outperform the TSH strategy at the portfolio level.

To make the two strategies comparable, we consider the following equal-weighting scheme for the TSM and TSH:

$$r_{t+1}^{\mathrm{TSM}} = \frac{1}{N_t} \sum_{i=1}^{N_t} sign(r_{t-12,t}^{i}) r_{t+1}^{i} \tag{1.19}$$

and

$$r_{t+1}^{\mathrm{TSH}} = \frac{1}{N_t} \sum_{i=1}^{N_t} sign(r_{1,t}^{i}) r_{t+1}^{i}, \tag{1.20}$$

where N_t is the number of assets investable at time t.[8] By doing so, the two strategies differ only in how the past information is used to select the assets. So, differences in performance stem from the differences in asset selection, not from differences in scaling the portfolio weights. Because our goal here is not to improve the

[8]TSH and TSM have similar expressions here, in parallel with the asset-level comparison. At the portfolio level, we also explore two alternative TSH strategies that buy (sell) half or one-third of the assets with high (low) historical sample means and find that their performances are quantitatively similar.

performance, the concern that the TSM strategy tilts weighting toward low volatility assets is not an issue, because the TSH strategy has the same tilts. Nevertheless, we will examine volatility weighting.

Panel A of Table 1.10 presents the average and risk-adjusted returns of the two strategies, the second of which is computed from two benchmark asset pricing models as in MOP (2012). The first model is the Fama-French four-factor model that uses the MSCI World Index as the market factor, and the second is the Asness, Moskowitz, and Pedersen (2013) three-factor model with the MSCI World Index, the value everywhere factor, and the momentum everywhere factor. Over the 1986 to 2015 investment period, the average return differential between the two strategies is as small as 0.14%, not significant with a p-value of 0.19. The results suggest that the TSM strategy generates virtually the same average portfolio return as the TSH strategy, consistent with earlier comparison at the asset level.

When turning to the risk-adjusted returns, the TSM and TSH alphas are 0.15% (t-statistic = 1.94) and 0.05% (t-statistic = 0.80) with the Fama-French four-factor model and 0.07% (t-statistic = 1.01) and 0.09% (t-statistic = 1.14) with the Asness, Moskowitz, and Pedersen (2013) three-factor model, respectively. As for the case with average return, the two strategies' alpha differentials with the two models are 0.10% (p-value = 0.29) and −0.02% (p-value = 0.84) and, therefore, are not statistically significant from zero. Thus, the TSM and TSH strategies do not generate sizable abnormal returns. Moreover, their difference in alpha is even smaller than the difference in average return and is also insignificant.

Also reported in Panel A of Table 1.10 are the average and risk-adjusted returns of the long- and short-leg portfolios of the TSM and TSH strategies. The results show that the performance of the two strategies mainly stems from the long legs and that the performance of their short legs is always indifferent from zero. This new finding, not shown by MOP (2012) or Goyal and Jegadeesh (2018), is consistent with our argument that the strong TSM performance is due to the difference in mean returns.

For robustness, we also consider three alternative portfolio weighting schemes: volatility weighting as in MOP (2012), past-12-month-return weighting, and equal weighting with a zero-investment constraint as in Goyal and Jegadeesh (2018). The results do not change qualitatively, and the alpha differential between the TSM and TSH strategies is always indifferent from zero. Table A1 in Online Appendix considers the case of constructing the TSM with the past six-month return, instead of the past 12-month return, and the results remain unchanged. In sum, the performance of the TSM strategy in MOP (2012) seems mainly stemming from the difference in mean returns, not from the times series predictability of the past 12-month return.

1.5.3 TSM and TSH forecast comparison: predictive slope

Lewellen (2015) proposes an interesting predictive slope that assesses the degree of predictability of cross-sectional forecasts in an elegant way, in which one simply runs a cross-sectional regression of the realized returns on the forecasts. If the forecasts are perfect, the slope should be one. Generally, a value less than 0.5 indicates no predictability as the forecasts under-perform naive forecasts.

At the end of month t, we calculate the expected return of asset i as $\hat{r}_{t+1}^{\mathrm{TSM},i}$, which is estimated by pooled regression Eq. (1.3) with data up to month t, and then we regress month $t+1$ return r_{t+1}^{i} on $\hat{r}_{t+1}^{\mathrm{TSM},i}$. The first three columns of Table 1.11 report the results. Consistent with the earlier no predictability results, the regression slope is close to zero and its t-statistic is less than one standard deviation, suggesting that the in-sample performance with pooled regression Eq. (1.3) is not reliable and that the TSM estimates do not line up with the true expected returns out of sample.

We also explore whether the TSM and TSH forecasts have the same mean. The last three columns of Table 1.11 report the results of regressing the TSM forecasts of expected returns on the TSH forecasts. Because these two estimates are potentially unbiased, we do not include an intercept to improve the estimate efficiency. Consistent with earlier results indicating little difference between the two strategies, the slope is close to one and the t-statistic is much larger than two standard deviations. For robustness, we also perform the tests for each asset class, and the results are the same as the overall case. In short, the TSM strategy has little predictive power and behaves in a very similar manner to the TSH strategy.

1.5.4 When does the TSM outperform the TSH?

In the previous sections, we have shown that the predictability of the past 12-month return is weak with real data and that the TSM strategy performs similarly as the TSH strategy that does not require any predictability. This section attempts to answer another question: If the predictability is strong, to what extent does the TSM strategy outperform the TSH strategy?[9]

Consistent with the pooled regression of MOP, we assume the following data-generating process:

$$r^i_{t+1} = \alpha^i + \beta \frac{r^i_{t-12,t}}{12} + \varepsilon^i_{t+1}, \tag{1.21}$$

where $\beta = 0.1,\ 0.2$, and 0.4 (we divide the past 12-month return $r^i_{t-12,t}$ by 12 to make β easy to interpret; β is 0.08 for the real data). There are two ways to draw the residuals. The first is to ignore the off-diagonal elements and draw the residuals asset by asset, thereby assuming no cross-asset predictability, and the second is to keep the covariance matrix structure and draw the residuals across assets. For a given specification and beta, we simulate a path for the 55 assets with $T = 372$ observations and construct the TSM and TSH strategies accordingly. We repeat this procedure one thousand times to test whether these two strategies generate the same mean returns. Empirically, we find that the two specifications generate almost the same results and therefore focus on the first specification.

Fig. 1.5 reports the results. When the slope is 0.1, the two strategies perform almost the same. When the slope is 0.2, the TSM outperforms the TSH, but the

[9] We thank the anonymous referee for this intriguing research question.

difference is not significant. Thus, if there is genuine time series predictability, the advantage of the TSM strategy is not apparent as long as the slope is small. When the slope is 0.4, the TSM dominates the TSH in the sense that it does better in almost all the simulated data sets. Because the two strategies generate similar performance using the real data, our simulation indicates that the evidence of time series predictability is weak if it exists. Combined with other results, the TSM is unlikely to be statistically significant for all the assets. In short, a lack of empirical evidence exists to support that the TSM is everywhere.

Because the TSM and TSH use overlapping data at the beginning of the investment period, one could expect that this explains the statistically indistinguishable difference even when the slope is 0.2. In fact, it is not the case. To see this, we simulate a path of $T + 240$ observations for the 55 assets, construct the TSM and TSH strategies starting from the 241th observation (i.e., the TSM is based on the past 12-month return and the TSH is based on the historical sample mean), and calculate their mean returns. We repeat this procedure one thousand times to test whether these two strategies generate the same mean returns. Fig. A1 of the Online Appendix summarizes the results. Generally, the basic patterns are similar to Fig. 1.5; that is, the TSM strategy performs similarly as the TSH when the data exhibit weak or intermediate time series predictability, and it outperforms the TSH when the data exhibit strong time series predictability. One observation is that with longer historical data in calculating the historical mean, the TSH generates 1% more annualized return relative to the case of Fig. 1.5, which is simply due to the fact that observations over a longer time horizon generate a better estimate of the sample mean (Merton, 1980).

1.6 Conclusion

In their influential study, MOP (2012) assert a surprising time series momentum that the past 12-month return can positively predict the next one-month to 12-

month return everywhere. They also show that a trading strategy based on TSM generates significant average and risk-adjusted returns. As TSM is in stark contrast to the previous literature and strongly challenges the weak form market efficiency hypothesis, we revisit TSM in this chapter. Employing the same data as MOP but extending the sample period to 2015, we show that, statistically, the evidence for TSM is weak in asset-by-asset time series regressions and a pooled regression accounting for size distortions. Economically, we show that the performance of the TSM strategy is likely driven by differences in mean returns, not predictability. A predictive slope analysis following the approach of Lewellen (2015) further confirms weak evidence on TSM.

A number of topics are of interest for future research. First, while the TSM strategy focuses on the 12-month return predictability, examining such a strategy at other time horizons and an optimal combination of all would be worthwhile. Second, the predictability horizon can be time-varying and could be different across assets (instead of the same horizon), and developing a test and a new trading strategy for this possibility would be important. Finally, considering the conditions under which time series predictability can exist in an equilibrium model and developing an empirical test for the implications would be desirable.

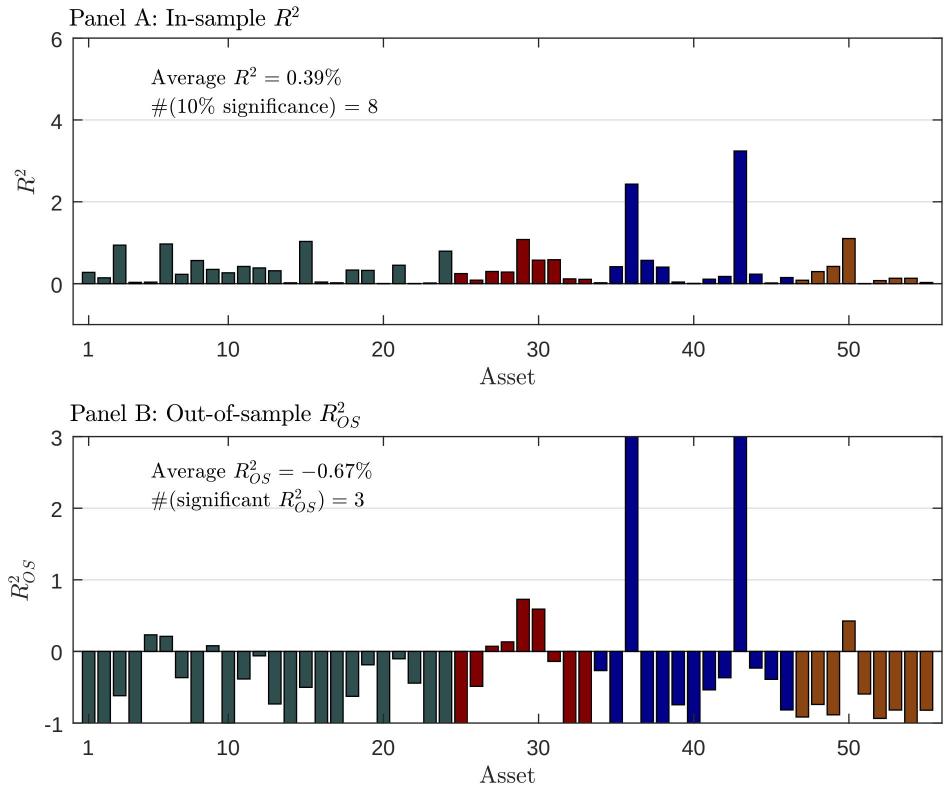

Fig. 1.1 Time series momentum (TSM) with asset-by-asset regression. This figure plots the in- and out-of-sample R^2s of forecasting a futures contract return with time series regression as

$$r^i_{t+1} = \alpha_i + \beta_i r^i_{t-12,t} + \varepsilon^i_{t+1},$$

where $r^i_{t-12,t}$ is asset i's past 12-month return. The in- and out-of-sample periods are 1985:01–2015:12 and 2000:01–2015:12, respectively.

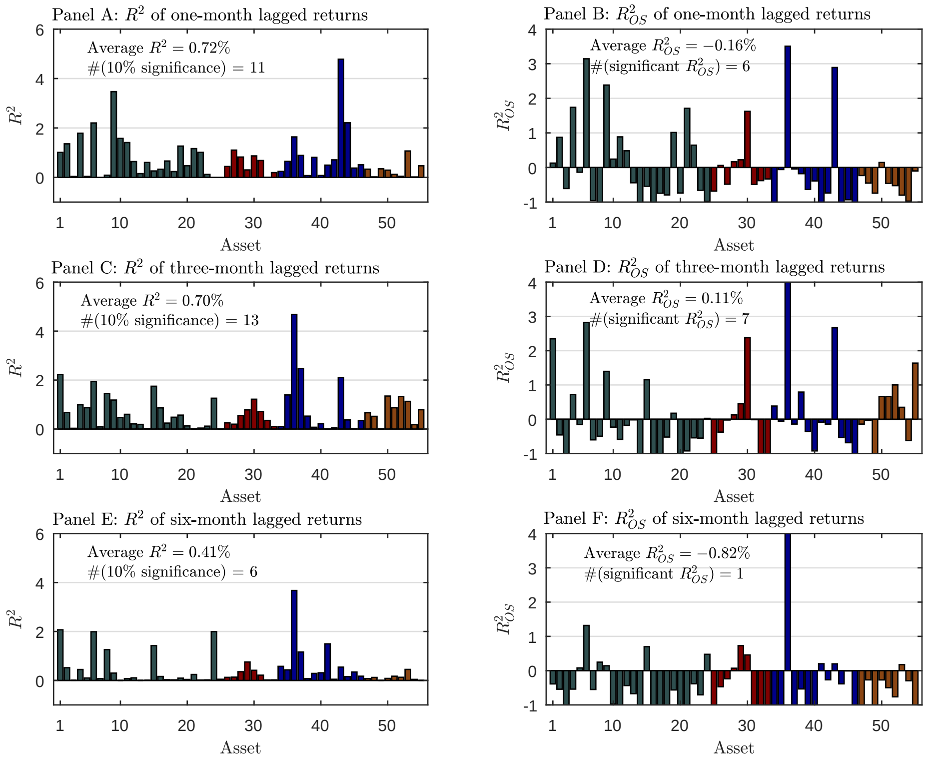

Fig. 1.2 Time series momentum (TSM) with asset-by-asset regression based on different lags. This figure plots the in- and out-of-sample R^2s of forecasting a futures contract return with time series regression as

$$r^i_{t+1} = \alpha_i + \beta_i r^i_{t-h,t} + \varepsilon^i_{t+1},$$

where $r^i_{t-h,t}$ is asset i's past h-month return ($h = 1$, 3, and 6). The in- and out-of-sample periods are 1985:01–2015:12 and 2000:01–2015:12, respectively.

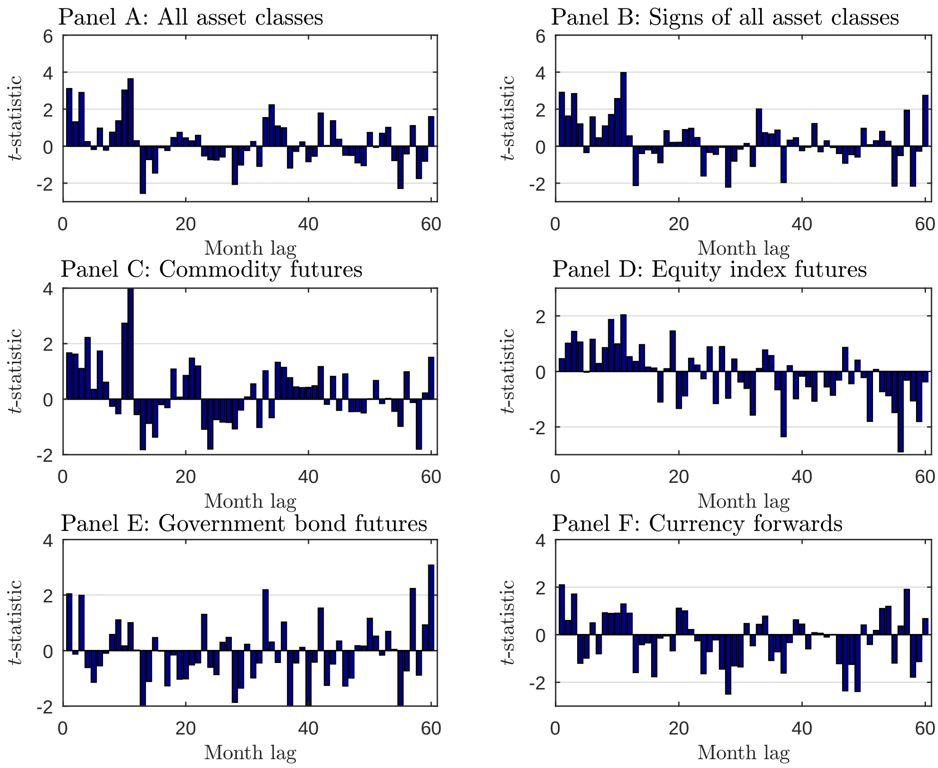

Fig. 1.3 Time series momentum (TSM) with pooled regression: in-sample performance. This figure plots the t-statistics of the pooled regression slopes that regress month t returns on month $t-h$ returns as

$$r^i_{t+1}/\sigma^i_t = \alpha_h + \beta_h r^i_{t-h+1}/\sigma^i_{t-h} + \varepsilon^i_{t+1},$$

for Panel A and

$$r^i_{t+1}/\sigma^i_t = \alpha_h + \beta_h sign(r^i_{t-h+1}) + \varepsilon^i_{t+1}$$

for Panels B to F, where r^i_{t-h+1} is asset i's return in month $t-h+1$ for $h = 1, 2, \cdots, 60$. The t-statistics are clustered by time (month). The sample period is 1985:01–2015:12.

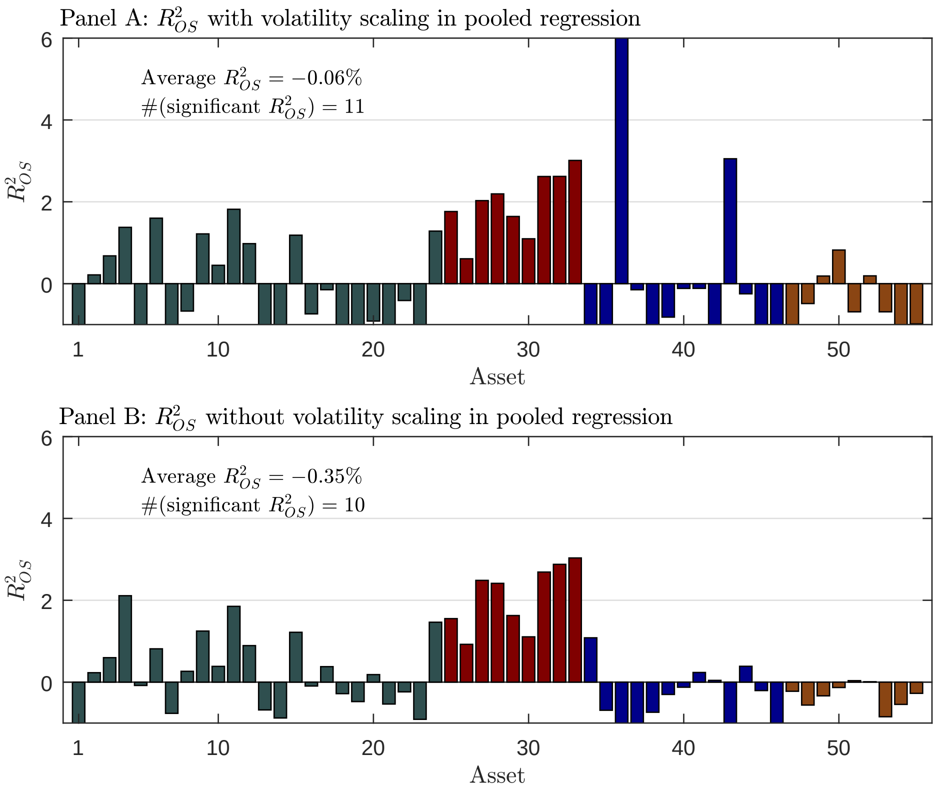

Fig. 1.4 Time series momentum (TSM) with pooled regression: out-of-sample performance. This figure plots the out-of-sample R^2_{OS} of forecasting a futures contract return with pooled regression

$$r^i_{t+1}/\sigma^i_t = \alpha + \beta r^i_{t-12,t}/\sigma^i_{t-1} + \varepsilon^i_{t+1},$$

for Panel A and

$$r^i_{t+1} = \alpha + \beta r^i_{t-12,t} + \varepsilon^i_{t+1}$$

for Panel B, where $r^i_{t-12,t}$ is asset i's past 12-month return. We calculate asset i's out-of-sample R^2_{OS} by applying the same $\hat{\alpha}_t$ and $\hat{\beta}_t$ to all assets in estimating the expected return as $\mathrm{E}_t(r^i_{t+1}) = \hat{\alpha}_t \sigma^i_t + \hat{\beta}_t \frac{r^i_{t-12,t}}{\sigma^i_{t-1}} \sigma^i_t$ in Panel A and $\mathrm{E}_t(r^i_{t+1}) = \hat{\alpha}_t + \hat{\beta}_t r^i_{t-12,t}$ in Panel B, where $\hat{\alpha}_t$ and $\hat{\beta}_t$ are the pooled regression estimates with data up to month t. The out-of-sample period is 2000:01–2015:12.

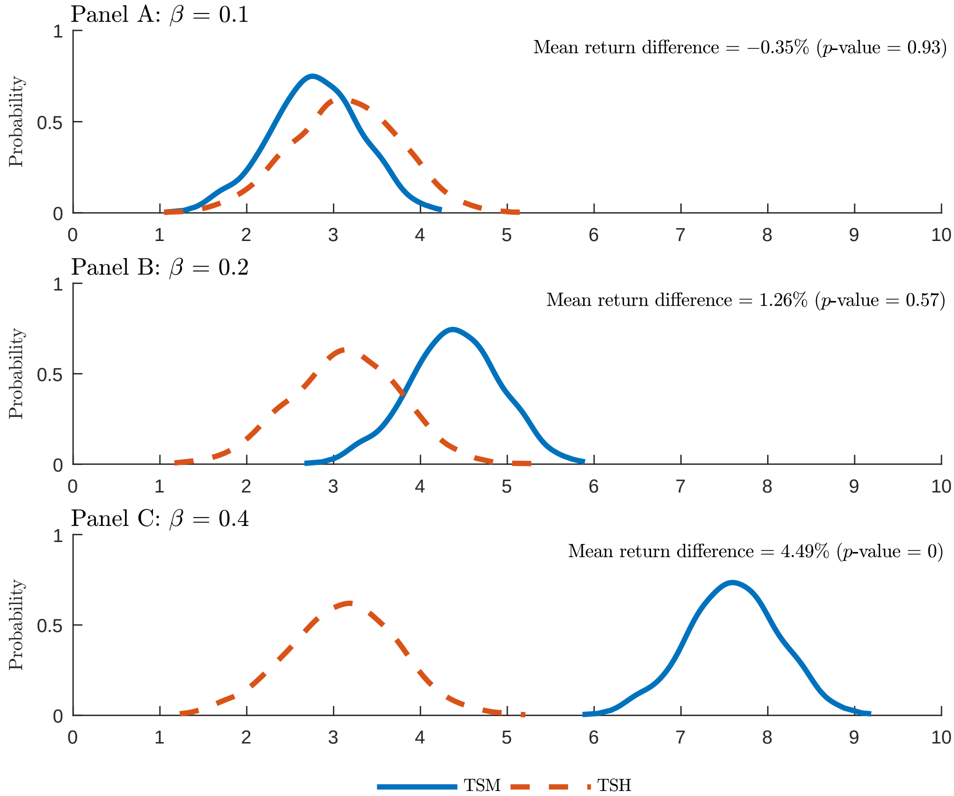

Fig. 1.5 Annualized mean return difference between time series momentum (TSM) and time series history (TSH). This figure plots the distributions of simulated annualized mean returns of the TSM and TSH strategies, where each asset is assumed to follow

$$r_{t+1}^{i} = \alpha^{i} + \beta \frac{r_{t-12,t}^{i}}{12} + \varepsilon_{t+1}^{i},$$

where β equals 0.1, 0.2, and 0.4. For each asset, we assume it has the same mean and variance as that in Table 1.1. Then, given the common slope β, α^{i} is estimated with asset i's real returns. We simulate a path of $T = 372$ observations, construct the TSM and TSH strategies, and calculate their mean returns. We repeat this procedure one thousand times to test whether these two strategies generate the same mean returns.

Table 1.1 Summary statistics of 55 assets across four asset classes

This table reports the mean return, volatility (standard deviation), and first-order autocorrelation ($\rho(1)$), where the mean and volatility are annualized and represented in percentage. "Average" refers to the average value within asset class. The sample period is 1985:01–2015:12.

	Mean	Volatility	$\rho(1)$		Mean	Volatility	$\rho(1)$
Panel A: Commodity futures				Panel C: Government bond futures			
Aluminum	−2.09	19.92	0.10	3-year AUS	3.34	4.58	−0.05
Brentoil	7.77	30.62	0.12	10-year AUS	5.57	6.90	0.08
Cattle	1.64	13.56	0.02	2-year EUR	1.47	3.41	0.13
Cocoa	−2.54	28.11	−0.13	5-year EUR	1.83	4.26	0.09
Coffee	−2.06	37.95	−0.02	10-year EUR	4.16	9.66	0.03
Copper	11.48	26.80	0.15	30-year EUR	7.56	10.39	0.09
Corn	−4.91	26.65	0.00	10-year CAN	6.44	10.79	−0.03
Cotton	1.36	25.83	0.03	10-year JP	3.66	13.78	0.07
Crude	6.37	34.98	0.19	10-year UK	4.14	8.28	0.08
Gasoil	8.21	33.23	0.12	2-year US	1.49	1.67	0.22
Gold	1.54	15.65	−0.12	5-year US	2.84	4.30	0.15
Heatoil	6.41	32.60	0.08	10-year US	3.64	7.60	0.06
Hogs	−3.70	24.23	−0.04	30-year US	12.59	16.44	0.07
Natgas	−11.33	50.79	0.08	*Average*	4.54	7.85	0.08
Nickel	7.06	34.37	0.05				
Platnum	6.36	20.76	0.06	Panel D: Currency forwards			
Silver	2.02	27.73	−0.08	AUD/USD	1.10	12.03	0.06
Soybeans	4.01	23.17	−0.05	EUR/USD	2.06	11.02	0.01
Soymeal	6.23	28.75	−0.11	CAD/USD	0.43	7.44	−0.06
Soyoil	4.33	26.12	−0.07	JPY/USD	1.72	11.12	0.05
Sugar	6.24	34.75	0.11	NOK/USD	0.51	11.01	0.04
Unleaded	12.31	35.71	0.10	NZD/USD	2.13	12.31	−0.02
Wheat	−4.33	26.68	−0.03	SEK/USD	0.39	11.25	0.10
Zinc	−0.33	25.08	0.00	CHF/USD	2.92	11.77	−0.01
Average	2.59	28.50	0.02	GBP/USD	−0.28	10.14	0.07
				Average	1.22	10.90	0.03
Panel B: Equity index futures							
SPI 200	7.41	16.11	0.00				
DAX	9.69	21.70	0.07				
IBEX 35	9.27	22.66	0.10				
CAC 40	6.73	19.69	0.09				
FTSE/MIT	6.29	23.21	0.05				
TOPIX	3.09	19.70	0.09				
AEX	6.96	19.16	0.08				
FTSE 100	6.51	15.87	−0.01				
S&P 500	9.21	15.20	0.04				
Average	7.24	19.26	0.06				

Table 1.2 In- and out-of-sample performance of TSM with time-series regression

This table reports the slope, t-statistic, in-sample R^2, and out-of-sample R^2_{OS} of $r^i_{t+1} = \alpha_i + \beta_i r^i_{t-12,t} + \varepsilon^i_{t+1}$. "Average" refers to the average value within each asset class. #(10% significance) refers to the number of significant in-sample regression slopes or significant R^2_{OS}s at the 10% level or stronger. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. The in- and out-of-sample periods are 1985:01–2015:12 and 2000:01–2015:12, respectively.

	β_i	t-stat	R^2	R^2_{OS}
Panel A: Commodity futures				
Aluminum	0.30	0.88	0.28	−1.42
Brentoil	0.34	0.69	0.14	−1.29
Cattle	0.38**	2.23	0.94	−0.62
Cocoa	−0.14	−0.28	0.03	−1.51
Coffee	0.21	0.40	0.04	0.23
Copper	0.77*	1.69	0.97	0.21
Corn	−0.37	−0.94	0.23	−0.37
Cotton	0.57	1.23	0.56	−1.00
Crude	0.60	1.34	0.35	0.08
Gasoil	0.49	1.11	0.26	−1.00
Gold	0.29	1.43	0.42	−0.38
Heatoil	0.59	1.39	0.38	−0.06
Hogs	0.39	1.25	0.31	−0.73
Natgas	−0.21	−0.30	0.02	−4.51
Nickel	1.01*	1.83	1.03	−0.50
Platnum	−0.12	−0.31	0.04	−1.83
Silver	−0.11	−0.25	0.02	−2.31
Soybeans	−0.39	−1.05	0.33	−0.63
Soymeal	−0.47	−1.06	0.32	−0.19
Soyoil	0.04	0.09	0.00	−1.93
Sugar	−0.65	−1.33	0.45	−0.10
Unleaded	0.05	0.12	0.00	−0.44
Wheat	−0.10	−0.26	0.02	−1.21
Zinc	0.65	1.24	0.79	−2.29
Average			0.33	−0.99
Panel B: Equity index futures				
SPI 200	−0.23	−0.49	0.25	−3.99
DAX	0.18	0.54	0.08	−0.49
IBEX 35	0.36	1.20	0.30	0.07
CAC 40	0.30	0.98	0.28	0.13
FTSE/MIT	0.70*	1.92	1.08	0.73
TOPIX	0.44*	1.69	0.57	0.59
AEX	0.43	1.22	0.58	−0.14
FTSE 100	−0.16	−0.48	0.12	−5.24
S&P 500	0.14	0.45	0.10	−1.78
Average			0.37	−1.12
Panel C: Government bond futures				
3-year AUS	0.01	0.29	0.02	−0.27
10-year AUS	0.13	1.42	0.41	−1.74
2-year EUR	0.15*	1.84	2.43	8.02**
5-year EUR	0.09	1.11	0.57	−1.03
10-year EUR	0.17	1.25	0.40	−1.03
30-year EUR	−0.06	−0.31	0.04	−0.74
10-year CAN	−0.02	−0.17	0.01	−1.32
10-year JP	0.11	0.47	0.11	−0.54
10-year UK	0.10	1.06	0.18	−0.37
2-year US	0.08***	3.57	3.24	4.26***
5-year US	0.06	1.16	0.23	−0.23
10-year US	−0.03	−0.35	0.02	−0.39
30-year US	0.17	0.60	0.15	−0.82
Average			0.60	0.29
Panel D: Currency forwards				
AUD/USD	−0.10	−0.47	0.08	−0.92
EUR/USD	0.17	1.08	0.29	−0.74
CAD/USD	0.14	1.18	0.42	−0.88
JPY/USD	0.33***	2.60	1.10	0.43*
NOK/USD	−0.01	−0.06	0.00	−0.59
NZD/USD	0.09	0.40	0.07	−0.94
SEK/USD	0.12	0.70	0.13	−0.82
CHF/USD	0.12	0.75	0.13	−1.42
GBP/USD	−0.05	−0.29	0.03	−0.82
Average			0.25	−0.75
Average across asset classes			0.39	−0.67
#(10% significance)	8			3

Table 1.3 *p*-value from the test that all assets have the same mean or Sharpe ratio

This table reports the p-value from the test that all assets have the same mean or Sharpe ratio. We perform four tests, including the analysis of variance (ANOVA) in Fisher (1918), Welch's ANOVA in Welch (1951), Kruskal-Wallis test in Kruskal and Wallis (1952), and bootstrap test. The sample period is 1985:01–2015:12.

	ANOVA	Welch's ANOVA	Kruskal-Wallis	Bootstrap
Mean	0.08	$< 10^{-3}$	$< 10^{-10}$	0
Sharpe ratio	$< 10^{-5}$	$< 10^{-5}$	$< 10^{-15}$	0

Table 1.4 *t*-statistic of pooled regression without controlling for fixed effects

This table reports the t-statistic of pooled regression with real data and the simulated t-statistics with wild and pairs bootstrap, respectively. For each asset, we bootstrap a path with T observations and run pooled regression without controlling for fixed effects to calculate the t-statistic. We repeat this procedure 1,000 times and obtain the distribution of the t-statistic for testing the null hypothesis that there is no time-series momentum. The bootstrapped t-statistic is defined as the 97.5% percentile of the simulated t-statistics. The sample period is 1985:01–2015:12.

		Bootstrapped t-stat			Bootstrapped t-stat	
h	t-stat	Wild	Pairs	t-stat	Wild	Pairs
Panel A: forecast with return lagged h months						
	$r^i_{t+1}/\sigma^i_t = \alpha_h + \beta_h r^i_{t-h+1}/\sigma^i_{t-h} + \varepsilon^i_{t+1}$			$r^i_{t+1}/\sigma^i_t = \alpha_h + \beta_h sign(r^i_{t-h+1}) + \varepsilon^i_{t+1}$		
1	3.11	9.26	3.63	2.90	8.18	3.41
2	1.31	4.98	1.98	1.62	4.44	2.31
3	2.89	8.61	3.45	2.83	6.84	3.45
4	0.24	2.46	1.06	1.20	2.12	1.99
5	−0.17	1.88	0.60	−0.34	1.83	0.54
6	0.97	4.18	1.71	1.58	3.62	2.28
7	−0.21	1.52	0.65	0.44	1.55	1.29
8	0.75	3.81	1.49	1.09	3.20	1.84
9	1.36	4.76	2.10	1.70	4.20	2.47
10	3.02	8.12	3.60	2.56	6.46	3.30
11	3.63	10.34	4.13	3.97	7.61	4.39
12	0.29	2.52	1.00	0.55	2.35	1.26
Panel B: forecast with past h-month return						
	$r^i_{t+1}/\sigma^i_t = \alpha_h + \beta_h r^i_{t-h,t}/\sigma^i_{t-1} + \varepsilon^i_{t+1}$			$r^i_{t+1}/\sigma^i_t = \alpha_h + \beta_h sign(r^i_{t-h,t}) + \varepsilon^i_{t+1}$		
1	3.11	9.26	3.63	2.90	8.18	3.41
2	2.92	9.46	3.46	3.07	8.32	3.61
3	3.74	11.45	4.22	4.15	10.20	4.61
4	3.49	10.71	3.97	4.57	9.49	4.96
5	3.11	9.58	3.63	4.24	8.85	4.72
6	3.29	9.65	3.80	3.88	8.88	4.39
7	3.03	9.30	3.62	3.93	8.31	4.40
8	3.05	9.44	3.62	3.74	8.33	4.24
9	3.38	9.85	3.95	4.44	8.99	4.78
10	3.94	11.38	4.46	5.27	10.22	5.63
11	4.64	13.12	5.08	5.71	11.73	6.05
12	4.34	12.53	4.83	5.14	11.05	5.53

Table 1.5 *t*-**statistic of pooled regression within each asset class without controlling for fixed effects**

This table reports the t-statistic of pooled regression with real data and the simulated t-statistics with wild and pairs bootstrap, respectively. For each asset, we bootstrap a path of T observations and run pooled regression without controlling for fixed effects to calculate the t-statistic. We repeat this procedure 1,000 times and obtain the distribution of the t-statistic for testing the null hypothesis that there is no time-series momentum. The bootstrapped t-statistic is defined as the 97.5% percentile of the simulated t-statistics. The sample period is 1985:01–2015:12.

		Bootstrapped t-stat			Bootstrapped t-stat	
h	t-stat	Wild	Pairs	t-stat	Wild	Pairs
	$r^i_{t+1}/\sigma^i_t = \alpha_h + \beta_h r^i_{t-h,t}/\sigma^i_{t-1} + \varepsilon^i_{t+1}$			$r^i_{t+1}/\sigma^i_t = \alpha_h + \beta_h sign(r^i_{t-h,t}) + \varepsilon^i_{t+1}$		
Panel A: Commodity futures						
1	1.74	5.09	2.49	1.66	4.86	2.40
3	2.32	6.28	2.97	2.95	5.82	3.52
6	2.98	7.08	3.65	2.89	6.59	3.54
12	3.46	8.20	4.13	4.20	7.43	4.68
Panel B: Equity index futures						
1	1.77	5.56	2.41	0.46	4.85	1.27
3	1.97	6.07	2.68	1.03	5.33	1.79
6	1.92	5.88	2.57	2.29	5.37	2.89
12	2.20	6.49	2.92	3.00	6.05	3.60
Panel C: Government bond futures						
1	2.35	6.58	3.04	2.04	5.78	2.75
3	2.37	6.68	2.99	2.87	5.75	3.41
6	0.60	3.22	1.53	0.73	2.93	1.61
12	1.68	5.39	2.44	1.69	4.77	2.42
Panel D: Currency forwards						
1	1.67	4.97	2.46	2.09	4.32	2.88
3	2.46	6.95	3.09	2.46	5.97	3.12
6	1.40	4.73	2.19	2.23	4.27	2.94
12	1.73	5.49	2.61	1.96	5.08	2.74

Table 1.6 t**-statistic of pooled regression without volatility scaling and without controlling for fixed effects**

This table reports the t-statistic of pooled regression with real data and the simulated t-statistics with wild and pairs bootstrap, respectively. For each asset, we bootstrap a path with T observations and run pooled regression without volatility scaling and without controlling for fixed effects to calculate the t-statistic. We repeat this procedure 1,000 times and obtain the distribution of the t-statistic for testing the null hypothesis that there is no time-series momentum. The bootstrapped t-statistic is defined as the 97.5% percentile of the simulated t-statistics. The sample period is 1985:01–2015:12.

		Bootstrapped t-stat			Bootstrapped t-stat	
h	t-stat	Wild	Pairs	t-stat	Wild	Pairs
Panel A: forecast with return lagged h months						
	$r^i_{t+1} = \alpha_h + \beta_h r^i_{t-h+1} + \varepsilon^i_{t+1}$			$r^i_{t+1} = \alpha_h + \beta_h sign(r^i_{t-h+1}) + \varepsilon^i_{t+1}$		
1	1.80	5.49	2.51	2.20	6.13	2.85
2	0.52	2.58	1.47	1.65	2.67	2.45
3	1.43	4.57	2.19	1.84	4.58	2.58
4	0.67	3.21	1.58	1.47	3.21	2.26
5	−1.33	−0.10	−0.14	−0.89	−0.08	0.28
6	1.03	3.37	1.92	1.77	3.45	2.48
7	−1.21	−0.47	−0.18	−0.18	−0.50	0.77
8	−0.64	0.60	0.42	0.17	0.54	1.12
9	−0.97	0.23	0.28	0.22	0.19	1.30
10	2.52	6.11	3.21	2.72	5.87	3.42
11	5.04	9.88	5.30	5.17	9.89	5.51
12	−1.04	−0.17	0.08	−0.11	−0.06	0.85
Panel B: forecast with past h-month return						
	$r^i_{t+1} = \alpha_h + \beta_h r^i_{t-h,t} + \varepsilon^i_{t+1}$			$r^i_{t+1} = \alpha_h + \beta_h sign(r^i_{t-h,t}) + \varepsilon^i_{t+1}$		
1	1.80	5.49	2.51	2.20	6.13	2.85
2	1.39	4.56	2.21	2.57	5.10	3.18
3	1.71	5.26	2.45	3.06	5.81	3.62
4	1.82	5.30	2.59	3.75	5.94	4.25
5	1.27	4.27	2.09	3.23	4.75	3.77
6	1.55	4.85	2.39	2.71	5.38	3.32
7	1.04	3.89	1.90	2.54	4.26	3.19
8	0.78	3.49	1.58	2.24	3.64	2.94
9	0.62	3.12	1.50	2.57	3.31	3.29
10	1.08	3.75	1.96	3.62	4.23	4.14
11	2.09	5.61	2.84	4.17	6.41	4.72
12	1.68	4.96	2.50	3.72	5.64	4.16

Table 1.7 t-statistic of pooled regression without controlling for fixed effects over 1985:01–2009:12

This table reports the t-statistic of pooled regression with real data and the bootstrapped t-statistics with wild and pairs bootstrap, respectively. For each asset, we bootstrap a path with T observations and run pooled regression without controlling for fixed effects to calculate the t-statistic. We repeat this procedure 1,000 times and obtain the distribution of the t-statistic for testing the null hypothesis that there is no time-series momentum. The bootstrapped t-statistic is defined as the 97.5% percentile of the simulated t-statistics.

		Bootstrapped t-stat			Bootstrapped t-stat	
h	t-stat	Wild	Pairs	t-stat	Wild	Pairs
Panel A: forecast with return lagged h months						
	$r^i_{t+1}/\sigma^i_t = \alpha_h + \beta_h r^i_{t-h+1}/\sigma^i_{t-h} + \varepsilon^i_{t+1}$			$r^i_{t+1}/\sigma^i_t = \alpha_h + \beta_h sign(r^i_{t-h+1}) + \varepsilon^i_{t+1}$		
1	3.71	10.68	4.20	3.75	9.31	4.19
2	0.97	4.07	1.68	1.34	3.65	2.02
3	2.48	7.43	3.11	2.44	6.09	3.01
4	0.22	2.40	1.14	0.65	2.28	1.59
5	−0.15	1.53	0.67	−0.38	1.56	0.66
6	0.52	3.08	1.30	1.35	2.78	2.15
7	0.39	3.07	1.24	0.95	2.74	1.84
8	0.59	3.32	1.37	1.20	2.84	2.06
9	1.68	5.26	2.42	1.96	4.59	2.66
10	2.70	7.37	3.32	2.11	5.83	2.84
11	3.70	10.37	4.23	4.04	7.61	4.54
12	0.37	2.74	1.14	0.54	2.39	1.37
Panel B: forecast with past h-month return						
	$r^i_{t+1}/\sigma^i_t = \alpha_h + \beta_h r^i_{t-h,t}/\sigma^i_{t-1} + \varepsilon^i_{t+1}$			$r^i_{t+1}/\sigma^i_t = \alpha_h + \beta_h sign(r^i_{t-h,t}) + \varepsilon^i_{t+1}$		
1	3.71	10.68	4.20	3.75	9.31	4.19
2	3.09	9.53	3.54	3.19	8.39	3.70
3	3.74	11.53	4.27	4.43	9.96	4.94
4	3.45	10.37	3.98	4.78	9.19	5.19
5	3.06	9.27	3.63	4.36	8.39	4.85
6	3.17	9.31	3.73	4.03	8.52	4.46
7	3.05	9.09	3.60	4.19	8.32	4.62
8	3.12	9.06	3.69	4.13	8.11	4.58
9	3.59	10.31	4.13	4.70	9.27	5.13
10	4.00	11.68	4.54	5.46	10.44	5.85
11	4.69	13.16	5.14	5.64	11.77	6.07
12	4.48	12.76	4.96	5.24	11.17	5.62

Table 1.8 t**-statistic of pooled regression controlling for fixed effects**

This table reports the t-statistic of pooled regression with real data and the bootstrapped t-statistics with wild and pairs bootstrap, respectively. For each asset, we bootstrap a path with T observations and run pooled regression controlling for fixed effects to calculate the t-statistic. We repeat this procedure 1,000 times and obtain the distribution of the t-statistic for testing the null hypothesis that there is no time-series momentum. The bootstrapped t-statistic is defined as the 97.5% percentile of the simulated t-statistics. The sample period is 1985:01–2015:12.

		Bootstrapped t-stat			Bootstrapped t-stat	
h	t-stat	Wild	Pairs	t-stat	Wild	Pairs
Panel A: forecast with return lagged h months						
	$r^i_{t+1}/\sigma^i_t = \alpha^i_h + \beta_h r^i_{t-h+1}/\sigma^i_{t-h} + \varepsilon^i_{t+1}$			$r^i_{t+1}/\sigma^i_t = \alpha^i_h + \beta_h sign(r^i_{t-h+1}) + \varepsilon^i_{t+1}$		
1	2.80	8.51	3.39	2.66	7.60	3.19
2	0.96	4.17	1.66	0.94	3.85	1.67
3	2.53	7.77	3.12	2.17	6.36	2.83
4	−0.19	1.56	0.70	0.36	1.56	1.27
5	−0.56	1.00	0.25	−0.94	1.03	0.02
6	0.58	3.26	1.36	1.07	2.90	1.79
7	−0.62	0.59	0.27	0.20	1.01	1.04
8	0.37	2.90	1.14	0.80	2.64	1.53
9	0.94	3.84	1.73	0.94	3.53	1.79
10	2.57	7.21	3.22	1.87	5.71	2.61
11	3.22	9.40	3.75	3.53	7.03	4.17
12	−0.12	1.63	0.65	0.37	1.70	1.13
Panel B: forecast with past h-month return						
	$r^i_{t+1}/\sigma^i_t = \alpha^i_h + \beta_h r^i_{t-h,t}/\sigma^i_{t-1} + \varepsilon^i_{t+1}$			$r^i_{t+1}/\sigma^i_t = \alpha^i_h + \beta_h sign(r^i_{t-h,t}) + \varepsilon^i_{t+1}$		
1	2.80	8.51	3.39	2.66	7.60	3.19
2	2.51	8.43	3.07	2.62	7.41	3.19
3	3.23	10.17	3.74	3.56	9.08	4.17
4	2.89	9.24	3.46	3.60	8.36	4.11
5	2.44	7.89	2.99	3.17	7.45	3.66
6	2.53	7.97	3.12	3.15	7.27	3.70
7	2.22	7.32	2.86	2.97	6.86	3.43
8	2.19	7.35	2.80	2.55	6.67	3.24
9	2.49	7.75	3.09	3.43	7.19	3.92
10	3.00	9.23	3.58	3.94	8.34	4.38
11	3.68	10.80	4.20	4.49	9.71	4.94
12	3.37	10.13	3.93	4.04	9.14	4.53

Table 1.9 TSM vs. TSH at the asset level

This table reports the mean returns and Sharpe ratios of the time-series momentum (TSM) and time-series history (TSH) strategies, as well as their difference, on the basis of individual assets. TSM refers to the strategy that buys the future contract if its past 12-month return is non-negative and sells it if its past 12-month return is negative, and TSH refers to the strategy that buys the futures contract if its historical sample mean is non-negative and sells it if its historical sample mean is negative. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. The investment period is 1986:01–2015:12.

Asset	TSM return	TSH return	TSM Sharpe ratio	TSH Sharpe ratio	Return difference	p-value	Sharpe ratio difference	p-value
Aluminum	0.27	−0.47	0.05	−0.08	0.74**	0.04	0.13**	0.04
Brentoil	0.80	0.32	0.09	0.04	0.48	0.44	0.05	0.44
Cattle	0.28	0.08	0.07	0.02	0.20	0.48	0.05	0.48
Cocoa	−0.46	0.13	−0.06	0.02	−0.59	0.32	−0.08	0.31
Coffee	0.18	−0.55	0.02	−0.05	0.73	0.30	0.07	0.30
Copper	0.77	0.94	0.10	0.12	−0.17	0.74	−0.02	0.73
Corn	0.11	0.14	0.01	0.02	−0.04	0.94	−0.01	0.94
Cotton	1.04	−0.07	0.14	−0.01	1.11**	0.04	0.15**	0.04
Crude	1.07	0.21	0.11	0.02	0.86	0.19	0.09	0.19
Gasoil	0.98	0.53	0.10	0.05	0.45	0.49	0.05	0.49
Gold	0.55	0.05	0.12	0.01	0.50*	0.10	0.11*	0.10
Heatoil	1.09	0.30	0.12	0.03	0.79	0.21	0.09	0.21
Hogs	0.29	−0.17	0.04	−0.02	0.46	0.37	0.06	0.37
Natgas	1.26	0.05	0.09	0.01	1.21	0.25	0.08	0.25
Nickel	0.72	0.43	0.07	0.04	0.29	0.70	0.03	0.70
Platinum	0.30	0.53	0.05	0.09	−0.23	0.61	−0.04	0.61
Silver	0.33	−0.09	0.04	−0.01	0.42	0.46	0.05	0.47
Soybean	−0.15	0.02	−0.02	0.01	−0.17	0.68	−0.03	0.67
Soymeal	0.20	0.51	0.02	0.06	−0.31	0.58	−0.04	0.58
Soyoil	0.42	0.14	0.06	0.02	0.28	0.57	0.04	0.57
Sugar	0.05	0.49	0.01	0.05	−0.44	0.50	−0.04	0.50
Unleaded	1.00	0.83	0.10	0.08	0.17	0.77	0.02	0.77
Wheat	0.38	0.26	0.05	0.03	0.12	0.81	0.02	0.81
Zinc	0.67	−0.22	0.09	−0.03	0.89*	0.08	0.12*	0.08
SPI 200	0.18	0.55	0.04	0.12	−0.37	0.17	−0.08	0.17
DAX	0.79	0.68	0.13	0.11	0.11	0.80	0.02	0.79
IBEX 35	0.71	0.72	0.11	0.11	−0.01	0.98	0.00	0.98
CAC 40	0.43	0.49	0.08	0.09	−0.06	0.89	−0.01	0.88
FTSE/MIB	0.90	0.36	0.14	0.05	0.54	0.27	0.09	0.27
TOPIX	0.84	0.25	0.15	0.04	0.59	0.18	0.11	0.18
AEX	0.73	0.55	0.13	0.10	0.18	0.66	0.03	0.66
FTSE 100	0.27	0.52	0.06	0.11	−0.25	0.40	−0.05	0.40
S&P 500	0.67	0.73	0.15	0.16	−0.06	0.84	−0.01	0.83
3-year AUS	0.24	0.28	0.24	0.28	−0.04	0.39	−0.04	0.37
10-year AUS	0.32	0.42	0.15	0.21	−0.10	0.27	−0.06	0.26
2-year EURO	0.13	0.10	0.13	0.10	0.03	0.57	0.03	0.56
5-year EURO	0.08	0.13	0.06	0.11	−0.05	0.46	−0.05	0.45
10-year EURO	0.05	0.25	0.02	0.09	−0.20	0.18	−0.07	0.18
30-year EURO	0.14	0.56	0.05	0.19	−0.42***	0.01	−0.14***	0.01
10-year CAN	0.34	0.52	0.11	0.17	−0.18	0.32	−0.06	0.31
10-year JP	0.21	0.08	0.06	0.02	0.13	0.55	0.04	0.56
10-year UK	0.34	0.28	0.14	0.12	0.06	0.68	0.02	0.68
2-year US	0.13	0.12	0.28	0.26	0.01	0.65	0.02	0.63
5-year US	0.19	0.23	0.15	0.19	−0.04	0.41	−0.04	0.40
10-year US	0.19	0.28	0.09	0.13	−0.09	0.40	−0.04	0.40
30-year US	0.54	0.89	0.12	0.20	−0.35*	0.07	−0.08*	0.06
AUD/USD	0.06	−0.16	0.02	−0.05	0.22	0.37	0.07	0.37
EUR/USD	0.11	0.11	0.03	0.03	0.00	1.00	0.00	1.00
CAD/USD	0.23	−0.08	0.11	−0.04	0.31**	0.05	0.15**	0.05
JPY/USD	0.46	0.09	0.14	0.03	0.37	0.11	0.11	0.11
NOK/USD	0.06	−0.06	0.02	−0.02	0.12	0.58	0.04	0.58
NZD/USD	0.24	0.02	0.07	0.01	0.22	0.38	0.06	0.38
SEK/USD	0.04	−0.05	0.01	−0.02	0.09	0.70	0.03	0.70
CHF/USD	0.18	0.19	0.05	0.06	−0.01	0.96	−0.01	0.97
GBP/USD	0.00	−0.03	0.00	−0.01	0.03	0.87	0.01	0.87
#(significance)					7		7	

Table 1.10 TSM vs. TSH at the portfolio level

This table reports the average and risk-adjusted returns of the TSM and TSH strategies, where we restrict portfolio weights on individual assets to be the same for comparison when constructing these two strategies. TSM refers to the strategy that buys futures contracts with non-negative past 12-month return and sells futures contracts with negative past 12-month return, and TSH refers to the strategy that buys futures contracts with non-negative historical sample mean and sells futures contracts with negative historical sample mean. The benchmarks are the Fama-French four-factor model that includes MSCI world index, SMB, HML, and UMD, and the Asness, Moskowitz, and Pedersen (2013) three-factor model. Newey-West t-statistics and p-values are reported in parentheses and brackets, respectively. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. The investment period for is 1986:01–2015:12.

Equal weighting, i.e., portfolio weight $= \frac{1}{N}$

		Fama-French four-factor model						Asness, Moskowitz, and Pedersen (2013) three-factor model				
	Mean	Alpha	MSCI world	SMB	HML	UMD	R^2	Alpha	MSCI world	VAL everywhere	MOM everywhere	R^2
	TSM strategy											
Long leg	0.34***	0.12**	0.16***	0.05**	0.09***	0.32***	42.57%	0.09	0.16***	0.14***	0.38***	41.92%
	(4.92)	(2.29)	(7.73)	(2.24)	(2.62)	(7.19)		(1.60)	(7.21)	(2.99)	(7.33)	
Short leg	−0.05	−0.03	0.14***	0.11***	0.03	−0.28***	48.31%	0.02	0.13***	−0.09	−0.32***	45.85%
	(−0.72)	(−0.46)	(5.30)	(4.01)	(0.96)	(−8.34)		(0.23)	(5.08)	(−1.42)	(−6.89)	
Long-short	0.39***	0.15*	0.02	−0.06*	0.06	0.60***	46.03%	0.07	0.03	0.23**	0.70***	47.39%
	(4.73)	(1.94)	(0.61)	(−1.83)	(1.01)	(9.99)		(1.01)	(0.93)	(2.50)	(8.79)	
	TSH strategy											
Long leg	0.27***	0.07	0.28***	0.14***	0.10***	0.08*	49.07%	0.10	0.26***	0.01	0.08*	45.30%
	(2.56)	(0.95)	(8.79)	(4.41)	(3.90)	(1.93)		(1.13)	(7.73)	(0.24)	(1.65)	
Short leg	0.02	0.02	0.03***	0.03*	0.02	−0.04**	8.73%	0.01	0.03***	0.03	−0.03	8.04%
	(0.61)	(0.52)	(3.51)	(1.83)	(1.47)	(−2.01)		(0.25)	(3.17)	(1.15)	(−1.04)	
Long-short	0.25***	0.05	0.25***	0.11***	0.08***	0.13***	44.83%	0.09	0.23***	−0.02	0.11*	42.01%
	(2.70)	(0.80)	(8.54)	(3.70)	(2.96)	(2.76)		(1.14)	(7.59)	(−0.29)	(1.94)	
	Mean difference	Alpha difference						Alpha difference				
TSM vs. TSH	0.14	0.10						−0.02				
	[0.19]	[0.26]						[0.84]				

Table 1.10 (continued)

Panel B: Volatility weighting, i.e., portfolio weight $= \frac{1}{N}\frac{40\%}{\sigma_t^i}$

		Fama-French four-factor model						Asness, Moskowitz, and Pedersen (2013) three-factor model				
	Mean	Alpha	MSCI world	SMB	HML	UMD	R^2	Alpha	MSCI world	VAL everywhere	MOM everywhere	R^2
TSM strategy												
Long leg	1.02***	0.58***	0.28***	−0.04	0.12	0.68***	36.04%	0.48***	0.28***	0.36***	0.84***	37.46%
	(7.66)	(5.31)	(7.34)	(−0.67)	(1.85)	(8.02)		(4.29)	(7.96)	(3.21)	(8.41)	
Short leg	−0.14	−0.06	0.22***	0.19***	0.04	−0.52***	44.46%	0.02	0.20***	−0.17	−0.59***	42.42%
	(−1.07)	(−0.53)	(5.75)	(3.06)	(0.79)	(−8.67)		(0.13)	(5.52)	(−1.48)	(−9.19)	
Long-short	1.16***	0.64***	0.05	−0.22**	0.08	1.19***	40.62%	0.46***	0.08	0.53***	1.43***	41.58%
	(6.31)	(3.68)	(0.80)	(−2.24)	(0.85)	(10.31)		(2.90)	(1.25)	(2.75)	(10.81)	
TSH strategy												
Long leg	0.81***	0.42***	0.48***	0.12**	0.16**	0.25***	40.10%	0.43***	0.46***	0.13	0.29***	38.80%
	(4.72)	(3.06)	(12.17)	(2.29)	(2.48)	(2.97)		(2.71)	(10.96)	(1.03)	(3.05)	
Short leg	0.07	0.10*	0.02	0.03	−0.01	−0.08***	4.27%	0.07	0.02	0.06	−0.04	4.35%
	(1.50)	(1.93)	(1.28)	(1.33)	(−0.11)	(−2.73)		(1.39)	(1.42)	(1.07)	(−1.06)	
Long-short	0.74***	0.32**	0.46***	0.09	0.16**	0.33***	36.23%	0.36**	0.44***	0.06	0.33***	35.01%
	(4.36)	(2.18)	(9.81)	(1.46)	(2.17)	(3.76)		(2.25)	(8.85)	(0.43)	(3.21)	
	Mean difference	Alpha difference						Alpha difference				
TSM vs. TSH	0.42*	0.32*						0.10				
	[0.07]	[0.08]						[0.56]				

Table 1.10 (continued)

Panel C: Past 12-month return weighting, i.e, portfolio weight = $\frac{r^i_{t-12,t}}{\sum_{i=1}^{N}|r^i_{t-12,t}|}$

	Mean	Fama-French four-factor model Alpha	MSCI world	SMB	HML	UMD	R^2	Asness, Moskowitz, and Pedersen (2013) three-factor model Alpha	MSCI world	VAL everywhere	MOM everywhere	R^2
						TSM strategy						
Long leg	0.50***	0.08	0.27***	0.17***	0.14**	0.66***	32.83%	0.06	0.25***	0.15*	0.73***	30.95%
	(3.54)	(0.72)	(5.85)	(3.62)	(2.06)	(8.65)		(0.52)	(5.09)	(1.65)	(7.71)	
Short leg	−0.07	−0.01	0.19***	0.16***	0.01	−0.43***	39.93%	0.05	0.18***	−0.15	−0.49***	38.05%
	(−0.65)	(−0.08)	(5.06)	(3.61)	(0.18)	(−7.74)		(0.49)	(5.06)	(−1.36)	(−6.97)	
Long-short	0.57***	0.09	0.08	0.01	0.13	1.09***	40.17%	0.01	0.08	0.30**	1.22***	40.55%
	(3.79)	(0.69)	(1.48)	(0.14)	(1.35)	(12.55)		(0.08)	(1.57)	(2.04)	(11.33)	
						TSH strategy						
Long leg	0.30*	−0.03	0.43***	0.23***	0.14***	0.19***	48.23%	0.03	0.41***	−0.03	0.17**	44.26%
	(1.78)	(−0.23)	(8.10)	(4.71)	(3.22)	(2.77)		(0.21)	(6.70)	(−0.29)	(1.96)	
Short leg	0.02	0.02	0.02***	0.01	0.01	−0.04*	5.23%	0.01	0.02***	0.03	−0.03	5.18%
	(0.77)	(0.83)	(2.82)	(1.09)	(1.06)	(−1.70)		(0.50)	(2.64)	(1.08)	(−0.86)	
Long-short	0.28*	−0.05	0.41***	0.22***	0.13***	0.23***	46.18%	0.02	0.39***	−0.06	0.20**	42.65%
	(1.71)	(−0.42)	(7.87)	(4.39)	(2.93)	(3.10)		(0.10)	(6.85)	(−0.56)	(1.99)	
	Mean difference	Alpha difference						Alpha difference				
TSM vs. TSH	0.29	0.14						−0.01				
	[0.12]	[0.34]						[0.98]				

Table 1.10 (continued)

Panel D: Zero investment, i.e., long $= \frac{1}{N^{\text{buy}}}$ and short $= \frac{1}{N^{\text{sell}}}$

	Mean	Fama-French four-factor model						Asness, Moskowitz, and Pedersen (2013) three-factor model				
		Alpha	MSCI world	SMB	HML	UMD	R^2	Alpha	MSCI world	VAL everywhere	MOM everywhere	R^2
TSM strategy												
Long leg	0.60***	0.24***	0.25***	0.08**	0.11**	0.53***	39.74%	0.20**	0.24***	0.17**	0.61***	39.10%
	(5.29)	(2.65)	(6.93)	(2.02)	(2.26)	(7.60)		(2.14)	(6.41)	(2.19)	(7.17)	
Short leg	−0.12	−0.06	0.28***	0.23***	0.05	−0.56***	42.77%	0.02	0.26***	−0.17	−0.64***	40.27%
	(−0.74)	(−0.42)	(5.76)	(3.94)	(0.79)	(−7.40)		(0.10)	(5.33)	(−1.24)	(−6.73)	
Long-short	0.72***	0.30**	−0.03	−0.16**	0.06	1.10***	45.91%	0.18	−0.02	0.34**	1.25***	46.18%
	(4.32)	(1.96)	(−0.65)	(−2.43)	(0.69)	(10.26)		(1.25)	(−0.40)	(1.97)	(8.52)	
TSH												
Long leg	0.35***	0.10	0.35***	0.17***	0.12***	0.10*	49.49%	0.13	0.33***	0.02	0.10	45.78%
	(2.64)	(1.06)	(8.98)	(4.39)	(3.85)	(1.90)		(1.25)	(7.92)	(0.21)	(1.56)	
Short leg	0.08	0.04	0.15***	0.14**	0.09	−0.18*	8.16%	−0.01	0.14***	0.17	−0.09	7.37%
	(0.51)	(0.30)	(3.66)	(2.13)	(1.62)	(−1.86)		(−0.05)	(3.28)	(1.48)	(0.75)	
Long-short	0.27**	0.06	0.20***	0.03	0.03	0.28***	11.89%	0.14	0.19***	−0.16	0.19	12.27%
	(2.10)	(0.40)	(5.30)	(0.45)	(0.49)	(2.75)		(1.02)	(4.83)	(−1.24)	(1.44)	
	Mean difference	Alpha difference						Alpha difference				
TSM vs. TSH	0.45***	0.24						0.04				
	[0.03]	[0.13]						[0.76]				

Table 1.11 TSM and TSH forecast comparison

This table reports the results of regressing r^i_{t+1} on the expected return ($\hat{r}^{\text{TSM},i}_{t+1}$) estimated at time t with the TSM pooled regression (1.3), and regressing $\hat{r}^{\text{TSM},i}_{t+1}$ on the expected return ($\hat{r}^{\text{TSH},i}_{t+1}$) estimated with the TSH approach (i.e., historical sample mean), respectively. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	$r^i_{t+1} = \alpha + \beta \hat{r}^{\text{TSM},i}_{t+1} + \varepsilon^i_{t+1}$			$\hat{r}^{\text{TSM},i}_{t+1} = d\hat{r}^{\text{TSH},i}_{t+1} + u^i_t$		
Asset class	β	t-stat	R^2	d	t-stat	R^2
Panel A: $\hat{r}^{\text{TSM},i}_{t+1}$ is estimated with volatility-scaling						
Overall	0.19	0.61	0.04	1.09***	18.56	40.33
Commodity	0.15	0.42	0.02	1.24***	11.62	23.53
Equity	0.07	0.10	0.01	0.84***	14.90	45.06
Bond	0.23	0.60	0.08	0.99***	68.75	92.27
Currency	−0.08	−0.12	0.01	1.01***	14.95	4.45
Panel B: $\hat{r}^{\text{TSM},i}_{t+1}$ is estimated without volatility-scaling						
Overall	0.30	0.45	0.03	1.04***	41.89	54.96
Commodity	0.09	0.11	0.00	1.01***	26.53	37.65
Equity	−0.37	−0.32	0.07	0.93***	35.34	77.93
Bond	−0.49	−0.52	0.07	1.00***	72.40	91.38
Currency	0.03	0.03	0.00	1.64***	19.78	14.32

Chapter 2

Impacts of Disagreement[1]

Disagreement measures are known to predict cross-sectional stock returns but fail to predict market returns. This chapter proposes a partial least squares disagreement index by aggregating information across individual disagreement measures and shows that this index significantly predicts market returns both in- and out-of-sample. Consistent with the theory in Atmaz and Basak (2018), the disagreement index asymmetrically predicts market returns with greater power in high sentiment periods, is positively associated with investor expectations of market returns, predicts market returns through a cash flow channel, and can explain the positive volume-volatility relationship.

2.1 Introduction

Researchers in economics and finance have long been interested in studying the effects of expectations across investors. Investor disagreement, usually measured by the second moment of investor expectations, plays an important role in explaining stock returns, volatility, and trading volume. Due to its wide impacts, Hong and Stein (2007) conclude that disagreement represents "the best horse" for behavioral finance to obtain as many insights as classical asset pricing theories. However, unlike Baker and Wurgler's (2006) sentiment index that has been widely used to

[1]This is a joint work with Dashan Huang and Jiangyuan Li

capture the first moment of investor expectations (see, e.g., Stambaugh, Yu, and Yuan, 2012; Yu and Yuan, 2011), investor disagreement has only been approximated through various proxies in the literature.[2] To date, there is a lack of research that examines disagreement measures collectively and it is unclear as to whether they are able to predict (excess) market returns in real time.

This chapter examines whether extant disagreement measures can become agreeable. If extant measures capture disagreement, they should display commonality and have a common factor. To aggregate information across 24 individual measures, we propose a disagreement index by using the partial least squares (PLS) method in Kelly and Pruitt (2013, 2015). Empirically, we show that the 24 individual measures do have a common factor and the disagreement index significantly predicts market returns up to 12 months. Over the sample period of 1969:12–2018:12, a one-standard deviation increase in the disagreement index is associated with a 0.83% decrease in the next one-month market return and a 7.04% decrease in the next 12-month market return, where the latter is comparable to 6.6% in Yu (2011) who measures investor disagreement with analyst forecast dispersion. The in- and out-of-sample R^2s are 2.52% and 1.56% at the one-month horizon and 13.88% and 13.26% at the 12-month horizon. In contrast, there are only four individual disagreement measures that are significant at the one-month horizon and four others significant at the 12-month horizon for in-sample forecasting, but none of the 24 individual measures exhibits any out-of-sample forecasting power.

PLS is chosen for information aggregation due to its simplicity and efficacy. PLS is initially proposed by Wold (1966) and further developed by Kelly and Pruitt (2013, 2015), which extracts the disagreement index with a three-pass regression filter to reduce common noises in the individual disagreement measures. Theoretically, PLS outperforms PCA in extracting factors for prediction if individual

[2] Professional forecast dispersions (Anderson, Ghysels, and Juergens, 2009; Bordalo et al., 2020; Li, 2016), analyst forecast dispersions (Diether, Malloy, and Scherbina, 2002; Hong and Sraer, 2016), household forecast dispersions (Li and Li, 2017), unexplained trading volume (Garfinkel, 2009), and stock idiosyncratic volatility (Boehme, Danielsen, and Sorescu, 2006) are some of the most prominent disagreement measures to date.

predictors contain a common (noise) component that is unrelated with future market returns. The intuition is that, as a supervised learning technique, PLS incorporates the target information—market returns—in the factor extracting procedure and teases out any common component that is uncorrelated with future market returns. Empirically, Kelly and Pruitt (2013), Lyle and Wang (2015), Huang et al. (2015), Giglio, Kelly, and Pruitt (2016), Light, Maslov, and Rytchkov (2017), and Gu, Kelly, and Xiu (2020), among others, show that PLS is effective in extracting factors for predicting stock returns and economic activities in the time series and cross-section.

The forecasting power of the disagreement index is not subsumed by economic predictors and uncertainty measures. It remains significant after controlling for the 14 economic predictors in Welch and Goyal (2008), output gap in Cooper and Priestley (2009), and aggregate short interest in Rapach, Ringgenberg, and Zhou (2016). Also, while the disagreement index represents one type of uncertainty (see, e.g., Anderson, Ghysels, and Juergens, 2009; Atmaz and Basak, 2018), it is distinct from extant uncertainty measures, such as economic uncertainty (Bali, Brown, and Caglayan, 2014), treasury implied volatility (Choi, Mueller, and Vedolin, 2017), financial uncertainty and macro uncertainty (Jurado, Ludvigson, and Ng, 2015), economic policy uncertainty (Baker, Bloom, and Davis, 2016), news implied volatility (Manela and Moreira, 2017), sample variance (Welch and Goyal, 2008), and the Chicago Board Options Exchange (CBOE) volatility index (VIX).

The ability of disagreement in predicting market returns is robust to alternative econometric methods. In addition to PLS, we explore six LASSO-related machine learning methods (see, e.g., Chinco, Clark-Joseph, and Ye, 2019; Diebold and Shin, 2019; Freyberger, Neuhierl, and Weber, 2020; Han, He, Rapach, and Zhou, 2019; Kozak, Nagel, and Santosh, 2020; Rapach, Strauss, and Zhou, 2013), and find that all of them generate significant out-of-sample R^2s, although the magnitudes are slightly smaller than that with the PLS disagreement index. [3] For example, the

[3]The reason why the PLS disagreement index performs the best is that the PLS forecast is asymptotically consistent and can generate the minimum mean squared forecast error (MSFE) so

out-of-sample R^2 by using the elastic net is 1.36% at the one-month horizon and 8.43% at the 12-month horizon, respectively, which are both significant at the 5% level. These results suggest genuine predictability of extant disagreement measures on market returns.

After providing evidence on the forecasting power of the disagreement index, we show that it is indeed consistent with the theory in Atmaz and Basak (2018). In their equilibrium model with infinite heterogeneous investors, Atmaz and Basak show that the overall effect of belief heterogeneity depends on two sufficient statistics, average bias and disagreement, which can be intuitively defined as the mean and cross-sectional standard deviation of investor expectation biases. Suppose investors are risk averse and exhibit a wealth effect that endogenously limits their risk taking. Atmaz and Basak (2018) show that, in equilibrium, investor disagreement affects stock returns via two channels. The first channel is a direct effect: disagreement represents uncertainty and investors require a higher expected return to hold a stock when disagreement on the stock increases, suggesting a positive disagreement-return relation. The second channel is an indirect effect: investor disagreement affects stock returns via an amplification effect on the average bias. That is, higher disagreement leads to higher average bias and more overvaluation, thereby suggesting a negative disagreement-return relation. With these two channels, Atmaz and Basak (2018) reconcile the mixed disagreement-return relation documented in the empirical finance literature (see, e.g., Carlin, Longstaff, and Matoba, 2014; Chen, Hong, and Stein, 2002; Diether, Malloy, and Scherbina, 2002; Yu, 2011). Since investors, regardless of whether they are sophisticated or not, are generally upward biased (see, e.g., Barber and Odean, 2008; DeVault, Sias, and Starks, 2019; Edelen, Ince, and Kadlec, 2016; Engelberg, McLean, and Pontiff, 2020), the second channel is more likely to dominate the first channel, thereby explaining why the disagreement index negatively predicts market returns in this chapter.

long as the consistency condition is satisfied (Kelly and Pruitt, 2015).

In the following, we test four implications raised by Atmaz and Basak (2018). The first, and most important, implication is that the forecasting power of disagreement is asymmetric: it is stronger when investors are optimistic or among stocks with optimistic investor expectations, and weaker or insignificant otherwise. The intuition is that when investors are relatively pessimistic, the first and second channels have different forecasting signs and are likely to offset each other, making the disagreement-return relation insignificant. In contrast, when investors are overly optimistic, the second channel dominates the first channel, and as a consequence, disagreement negatively predicts future stock returns. To test this implication, we perform two tests. First, in time series, we show that the forecasting power of the disagreement index is concentrated in high investor sentiment periods and nonexistent in low sentiment periods. Second, cross-sectionally, we form ten decile portfolios based on firm level investor expectation, which is measured by the analyst long-term growth rate (LTG) forecast (Bordalo et al., 2019), and find that the disagreement index displays much stronger power in predicting portfolios with higher LTG forecasts, especially in high sentiment periods.

The second implication is that disagreement should be linked to investor optimism about market returns and ex post forecast errors. To capture investor expectations of market returns, we consider four measures, including aggregate analysts' return forecast (Engelberg, McLean, and Pontiff, 2020), Michigan survey of consumers attitudes (Das, Kuhnen, and Nagel, 2019), Graham-Harvey's survey of CFOs and Shiller's survey of individual investor confidence (Greenwood and Shleifer, 2014). We find that all these four measures positively correlate with the disagreement index. For example, a one-standard deviation increase in disagreement is associated with a 3.26% increase in the analysts' return forecast about the following 12-month market return. Since investor expectations are upward biased, the disagreement index negatively predicts ex post return forecast errors.

The third implication is that the predictive ability of disagreement on market returns is more likely to operate via a cash flow channel in the sense of Campbell

(1991). According to Atmaz and Basak (2018), after positive cash flow news, investors whose beliefs are supported by the cash flow news become relatively wealthier, which makes them more optimistic about future cash flows or discount rates or both, and consequently, increases investor disagreement. For this reason, both the cash flow news and discount rate news can have a positive effect on disagreement. Empirically, we find that the cash flow news-based disagreement index displays strong forecasting power, while the discount rate news-based disagreement index does not.

The fourth, and last, implication is that disagreement plays an important role for the positive relationship between trading volume and market volatility. In Atmaz and Basak (2018), disagreement is the only driver of trading volume and market volatility. In the absence of disagreement, there is no trade and the market volatility is constant. In the presence of disagreement, however, both trading volume and market volatility increase as disagreement increases. Empirically, we find that the disagreement index is positively related to the volume-volatility elasticity. Intuitively, a one-standard deviation increase in disagreement predicts a 5.22% increase in the volume-volatility correlation in the following month. Overall, our empirical results are consistent with the theoretical implications of Atmaz and Basak (2018).

This chapter contributes to the disagreement literature by showing that disagreement predicts market returns in- and out-of-sample. While many papers have explored the relationship between disagreement and stock returns at the firm level, studies at the market level are relatively rare. There are two exceptions, Yu (2011) on the stock market and Carlin, Longstaff, and Matoba (2014) on the mortgage market, but they do not investigate the out-of-sample forecasting power and the economic value for a real time investor. Also, Yu (2011) documents a negative forecasting sign whereas Carlin, Longstaff, and Matoba (2014) find a positive forecasting sign, and therefore, they interpret their results with different theories. This chapter reconciles the seemingly conflicting results by using the unified theory of Atmaz and Basak (2018).

This chapter is also related to the broad literature on return predictability. Since Welch and Goyal (2008), a large number of variables have been identified to significantly predict market returns in- and out-of-sample, such as the output gap (Cooper and Priestley, 2009), 52-week high and historical high (Li and Yu, 2012), aggregate implied cost of capital (Li, Ng, and Swaminathan, 2013), disaggregate book-to-market ratio (Kelly and Pruitt, 2013), aggregate short interest (Rapach, Ringgenberg, and Zhou, 2016), aggregate liquidity (Chen, Eaton, and Paye, 2018), fourth quarter consumption (Møller and Rangvid, 2015), metal prices (Jacobsen, Marshall, and Visaltanachoti, 2019), dividend-price ratio (Golez and Koudijs, 2018), variance risk premium (Pyun, 2019), gold to platinum price ratio (Huang and Kilic, 2019), aggregate skewness (Jondeau, Zhang, and Zhu, 2019), and many others. This chapter does not aim at identifying a new variable to predict market returns, but proposes to aggregate predictive information from extant individual disagreement measures.

The rest of the chapter is organized as follows. Section 2.2 considers 24 extant disagreement measures and shows that they fail to predict the stock market at the one- to 12-month horizons. Section 2.3 proposes a PLS disagreement index by aggregating information across individual measures and shows that it significantly predicts market returns in- and out-of-sample. Section 2.4 shows that the predictability of disagreement on market returns is consistent with the theoretical implications of Atmaz and Basak (2018), which is followed by Section 2.5 with a brief conclusion.

2.2 Forecasting Power of Extant Disagreement Measures

At the one- to 12-month horizons, we show in this section that most of the extant disagreement measures fail to predict market returns in-sample and none of them displays significant out-of-sample forecasting power.

2.2.1 Extant disagreement measures

We consider 24 disagreement measures, among which, 13 are based on professional forecasts on eight macro variables, two based on analyst forecasts, six based on household forecasts on macroeconomic conditions, and three based on market information. While these measures originate from different time periods, dating as early as December 1968, all of them conclude by December 2018.

13 disagreement measures based on professional forecasts

The disagreements between professional forecasts on macro variables are based on the oldest quarterly survey of professional forecasters (SPF) in the US. The survey begins in 1968Q4 and is typically released in the mid-to-late second month of each quarter.[4] However, the accurate release dates before 1990Q2 are unavailable, and therefore, to be conservative, we assume that all surveys are made known in the last month of each quarter in our analysis. Also, because most of our analyses are performed on a monthly frequency, we convert the quarterly measures into monthly frequencies by assigning the most recent quarterly value to each month. For example, the observation in the first quarter of 2018 is assigned to the months of March, April, and May, respectively.

We consider professional forecasts on eight macro variables, including gross domestic production (GDP), industrial production (IP), consumption (CON), investment (INV), housing starts (HSG), unemployment (UEP), consumer price index (CPI), and the 3-month Treasury bill rate (TBL). As the forecasts on GDP, IP, CON, INV, and HSG include both level and growth rate, we therefore have 13 disagreement measures in total. In each quarter, the forecasters predict macro variables for horizons ranging from the current up to four quarters ahead. Following Li (2016) and documents from the SPF, we define disagreement on each macro

[4] Three exceptions with delayed releases are 1990Q2, 1996Q3, and 2013Q4, respectively.

variable as the difference between the 75th percentile and 25th percentile forecasts for each horizon, taking the average across all horizons as the disagreement measure of that macro variable. In the literature, Anderson, Ghysels, and Juergens (2009), Bali, Brown, and Tang (2020), and many others use the SPF in a similar fashion in constructing aggregate uncertainty and disagreement measures, and find significant power for pricing the cross-section of stock returns.

Two disagreement measures based on analyst forecasts

Numerous studies have employed analyst forecast dispersion as the measure of investor disagreement. Following Yu and Yuan (2011) and Hong and Sraer (2016), we adopt the "bottom-up" approach by defining disagreement in month t as:

$$D_t^{\mathrm{Yu}} = \frac{\sum_i \mathrm{MKTCAP}_{i,t} \cdot D_{i,t}}{\sum_i \mathrm{MKTCAP}_{i,t}}, \tag{2.1}$$

and

$$D_t^{\mathrm{HS}} = \frac{\sum_i \beta_{i,t} \cdot D_{i,t}}{\sum_i \beta_{i,t}}, \tag{2.2}$$

where $D_{i,t}$ is the analyst forecast dispersion on the earnings per share (EPS) long-term growth rate (LTG) of firm i, and MKTCAP and $\beta_{i,t}$ are firm i's market cap and market beta. We only include common stocks (with CRSP item SHRCD = 10 or 11) listed on the NYSE, NASDAQ, and AMEX. As explained in Yu (2011), the LTG forecast features prominently in valuation models and is less affected by a firm's earnings guidance than the short-term forecast. When constructing D_t^{HS}, we follow Hong and Sraer (2016) and focus on all-but-micro stocks, stocks that are larger than the 20th percentile of the market cap of NYSE stocks. For each firm i in month t, we regress the daily returns of the past one year on contemporaneous and one to five lagged market returns, and use the sum of the slopes as the estimate of $\beta_{i,t}$.

Six disagreement measures based on household forecasts

Empirical studies often focus on how the trading of securities is affected by disagreement among institutional investors (see, e.g., Chen, Hong, and Stein, 2002; Diether, Malloy, and Scherbina, 2002; Jiang and Sun, 2014), but seldom explore the disagreement effect of households or retail investors on the stock market. From the Flow of Funds Accounts, households own about 60% of outstanding equities in the US (about 40% direct holding and additional 20% indirect holding through mutual funds), and therefore, their opinions should play a similarly important role as those of institutional investors. Li and Li (2017) show that the effect of household disagreement remains significant after controlling for professional forecast dispersions and even dominates the professional forecast dispersion measures.

We construct household disagreement based on the Michigan survey of consumers attitudes (SCA). The SCA starts conducting monthly surveys on a minimum of 500 households in January 1978, with accurate release dates available after January 1991. In each survey, the SCA collects responses to 50 core questions that are generally related to households opinions on current economic conditions and their expectations about future economic conditions. In this chapter, we construct our disagreement measures from six questions. The first question is about households' realized opinions on current personal financial condition compared with those of the prior year, while the other five are about households' expectations about the following year, consisting of the expected personal financial condition, business condition, unemployment condition, interest rate condition, and house purchase condition.

For each question, the surveyed households' replies are classified into three categories, better (good), same (depends), and worse (bad). In a consistent way, we rename the categories as positive, neutral, and negative, respectively, and define the proportion of each category as P_{positive}, P_{neutral}, and P_{negative}. We follow Li and Li (2017) and define disagreement as the unevenly weighted negative Herfindahl index,

$$D = -\sum w_i P_i^2, \quad i = \text{positive, neutral, negative}, \tag{2.3}$$

where w_i is the weight of each category as $w_{\text{positive}} = 1$, $w_{\text{neutral}} = 2$, and $w_{\text{negative}} = 1$. We assign a higher weight to the neutral category to avoid the unfavourable feature of the evenly weighted Herfindahl index. For example, if 50% of households indicate the positive response and 50% indicate the negative response, the evenly weighted Herfindahl index would be the same as if the responses are 50% positive and 50% neutral. However, the disagreement in the former situation is obviously more dispersed than in the latter.

Disagreement based on unexplained stock trading volume

Ajinkya, Atiase, and Gift (1991) find that high trading volume is associated with an increase in the analyst forecast dispersion, suggesting that trading volume may measure investor disagreement. We follow Garfinkel (2009) and construct a disagreement measure with the standardized unexplained volume. Specifically, we obtain the monthly aggregate trading volume data of the NYSE from Pinnacle and define volume as the residual of applying an AR(4) to the log turnover with the past 120-month observations (Hamilton, 2018).[5] Then, we run the following time series regression with data from the past 120-month period at the end of each month on a rolling basis as

$$\text{Volume}_t = \alpha + \beta_1 R_t^+ + \beta_2 R_t^- + \varepsilon_t, \tag{2.4}$$

and use the last value of the residuals as the estimate of unexpected volume. In Eq. (2.4), the plus and minus signs in the superscript indicate that market returns can be either positive or negative, and capture the empirical fact that positive and negative returns generate different levels of trading volume. Thus, investor disagreement can

[5]The results are quantitatively similar with alternative specifications, such as using AR(12) or using the past 60-month observations.

be defined by the standardized unexplained volume:

$$D_t^{\mathrm{SUV}} = \frac{\varepsilon_t}{\sigma_{\varepsilon,t}}, \tag{2.5}$$

where $\sigma_{\varepsilon,t}$ is the standard deviation of the regression residuals.

Disagreement based on idiosyncratic volatility

Inspired by theoretical studies that construct a close relation between belief dispersion and volatility, Boehme, Danielsen, and Sorescu (2006) and Berkman et al. (2009) propose idiosyncratic volatility as a disagreement measure at the firm level. We extend this measure to the market level. Specifically, following Ang et al. (2006), we regress daily stock returns on the Fama and French (1993) three factors with a 12-month rolling window and estimate the firm level idiosyncratic volatility at the end of each month. We then define investor disagreement as the value-weighted idiosyncratic volatility.

Disagreement based on option open interest

Disagreement can also be constructed from the option market. Investors who hold call options have a bullish view, whereas investors who hold put options have a bearish view. Following Ge, Lin, and Pearson (2016), we define disagreement as one minus the scaled difference between the OEX call and put open interests:

$$D_t^{\mathrm{OID}} = 1 - \frac{|\mathrm{COI}_t - \mathrm{POI}_t|}{|\mathrm{COI}_t + \mathrm{POI}_t|}, \tag{2.6}$$

where COI_t (POI_t) is the call (put) option open interest. The scaled call and put option open interest difference $|\mathrm{COI}_t - \mathrm{POI}_t|/|\mathrm{COI}_t + \mathrm{POI}_t|$ ranges from zero to one. The explanation is that when disagreement is low, investors' beliefs polarize into bullish or bearish extremes. The difference between the call and put option open interests diverges and the scaled difference approaches one. As a result, one minus this scaled difference is accordingly low. When disagreement is high, the opinions between optimists and pessimists diverge. The call and put option open interests should be commensurable. The scaled difference between the call and put option open interests approaches zero. Hence, one minus the scaled difference is accordingly large.

2.2.2 Summary statistics

Table 2.1 presents summary statistics of the 24 disagreement measures, including the sample period, mean, standard deviation, minimum, maximum, skewness, and kurtosis. It is apparent that the scales across disagreement measures vary dramatically due to the nature of macro variables. For instance, the mean of disagreement on GDP is 61.32 billion, while the mean of disagreement on TBL is only 0.46%. Thus, to make them comparable and to avoid forward-looking bias, we standardize each disagreement measure in month t by its last six-year mean and standard deviation, with a requirement of at least one year data. For this reason, the analyses in all other tables start from December 1969. To remove possible fundamental information, we measure disagreement as the residuals from the regression of each individual disagreement measure on the six macro variables in Baker and Wurgler (2006), consisting of the growth of industrial production, the growth of durable consumption, the growth of nondurable consumption, the growth of service consumption, the growth of employment, and a dummy variable for NBER dated recessions (we recursively do so when performing out-of-sample tests).

Table A1 in the Online Appendix presents pairwise correlations between individual disagreement measures. Most of the measures are positively correlated, with several exceptions of negative values. For example, professional forecast dispersions are generally positively correlated, and they are also positively correlated with the two analyst forecast dispersion measures. Business condition forecast dispersion is an exception, and it is negatively correlated with other measures in general. Overall, this table indicates that extant measures capture both the common and different aspects of individual disagreement measures across the whole economy, and an individual measure is unlikely to completely capture the aggregate effect of disagreement on the stock market.

2.2.3 Forecasting market returns with extant disagreement measures

We explore the forecasting power of disagreement on market returns with the following predictive regression,

$$R_{t+1} = \alpha + \beta D_t + \varepsilon_{t+1}, \tag{2.7}$$

where R_{t+1} is the log excess return of the S&P 500 index in month $t+1$ and D_t is one of the 24 individual disagreement measures.[6] When the forecast horizon is h months, we denote the cumulative market return as $R_{t,t+h} = \sum_{j=1}^{h} R_{t+j}$.

The predictive power is assessed based on the regression slope β or the R^2 statistic. If β is significantly different from zero or if the R^2 is significantly larger than zero, it means that D_t is a predictor of the market returns. The out-of-sample forecast of the next one-month market return is recursively computed as

$$\hat{R}_{t+1} = \hat{\alpha}_t + \hat{\beta}_t D_t, \tag{2.8}$$

where $\hat{\alpha}_t$ and $\hat{\beta}_t$ are the ordinary least squares estimates of α and β based on data from the start of the available sample through month t. The in-sample forecast is computed similarly as before, except that $\hat{\alpha}_t$ and $\hat{\beta}_t$ are replaced by those estimated by using the entire sample. For ease of exposition, we always normalize the time series of disagreement in all the in-sample predictive regressions, so that the regression slope measures the change in response to a one-standard deviation increase in disagreement.

[6] For brevity, returns in this chapter always refer to excess returns except for Section 2.4.3, where we follow Campbell (1991) to decompose the total market returns into three subcomponents.

We use the out-of-sample R^2 statistic in Campbell and Thompson (2008) as the out-of-sample performance evaluation criterion and define it as:

$$R_{OS}^2 = 1 - \frac{\sum_{t=M+1}^{T}(R_t - \hat{R}_t)^2}{\sum_{t=M+1}^{T}(R_t - \bar{R}_t)^2}, \tag{2.9}$$

where M is the size for in-sample parameter training and $T - M$ is the number of out-of-sample observations. $\hat{R}_t$ is the market return forecast with Eq. (2.8), and $\bar{R}_t$ is the historical return mean, both of which are estimated using data up to month $t-1$. If D_t is a valid predictor, its MSFE is lower than the MSFE with the historical return mean and the R_{OS}^2 will be positive. Campbell and Thompson (2008) show that a monthly R_{OS}^2 of 0.5% can generate a significant economic value. The null hypothesis of interest is therefore $R_{OS}^2 \leq 0$ against the alternative hypothesis that $R_{OS}^2 > 0$. We test this hypothesis by using the MSFE-adjusted statistic as proposed by Clark and West (2007).

Panel A of Table 2.2 presents the regression slope β, Newey-West t-value, in-sample R^2, and out-of-sample R_{OS}^2. Throughout this chapter, the out-of-sample period is from February 1991 to December 2018 because the accurate release dates of household dispersion measures are only available as of January 1991. 20 out of 24 disagreement measures have a negative forecasting sign, among which, however, only four measures reveal significant in-sample predictive power at the 5% level, which are the housing starts forecast dispersion, CPI forecast dispersion, TBL forecast dispersion, and business condition forecast dispersion. The out-of-sample performance is more dismal, with all R_{OS}^2 values being negative. For instance, the TBL forecast dispersion exhibits the highest in-sample R^2 of 1.94%, but generates a -4.21% out-of-sample R_{OS}^2. These results suggest that none of the extant individual disagreement measures can predict market returns in real time at the one-month forecast horizon.

Panels B and C of Table 2.2 present similar results as Panel A when the forecast horizon is extended to three months or 12 months. The in-sample regression slopes are seldom significant and the R_{OS}^2 values are all negative. For in-sample prediction over the 1981:12–2005:12 sample period, Yu (2011) shows that analyst forecast dispersion exhibits insignificant forecasting power at the one-month horizon but significant forecasting power at the 12-month or longer horizons. Panel C suggests that when we extend the sample to the most recent period, analyst forecast dispersion becomes insignificant. Yu (2011) does not show out-of-sample forecasting performance and our results suggest that analyst forecast dispersion cannot generate meaningful real time forecasting value either.

Overall, Table 2.2 shows that while all of the extant disagreement measures may have cross-sectional forecasting power, they are unable to predict market turns in general, especially for out-of-sample forecasting.

2.3 PLS Disagreement Index

In this section, we construct a disagreement index by aggregating information across individual disagreement measures and show that it significantly predicts market returns in- and out-of-sample.

2.3.1 Methodology

The method we choose for information aggregation is PLS, which consists of three steps. In the first step, we run a time series regression of each individual disagreement measure on the realized subsequent market returns (as a proxy of expected return) with the full sample, denoted as:

$$D_{t-1}^k = \pi_{k,0} + \pi_k R_t + u_{k,t-1}, \quad k = \text{GDP}, \cdots, \text{OID}, \tag{2.10}$$

where π_k captures the sensitivity of proxy D_{t-1}^k to the expected market return. In the second step, we run a cross-sectional regression of D_t^k on $\hat{\pi}_k$ at the end of each month:

$$D_t^k = a_t + D_t \hat{\pi}_k + v_{k,t}, \tag{2.11}$$

where the regression slope D_t is the PLS disagreement index in month t. In the last and third step, to predict R_{t+1}, we run the following predictive regression:

$$R_{t+1} = \alpha + \beta D_t + \varepsilon_{t+1}. \tag{2.12}$$

The above three steps are for in-sample analysis. For out-of-sample forecasting, the standard approach is to repeat the three steps by truncating the observations that are not known at month $t+1$. Specifically, consider a forecast for return R_{t+1} that is realized in month $t+1$. A properly constructed forecast can only use information known through month t. In the first step, the latest return that can be used on the right-hand side is R_t and the last observation of disagreement on the left-hand side is, therefore, D^k_{t-1}. In the second step, the cross-sectional regressions are run from months 1 through t. In the last step, the latest return on the left-hand side entering the predictive regression is R_t and the forecast for R_{t+1} is $\hat{\alpha}_t + \hat{\beta}_t D_t$, where $\hat{\alpha}_t$ and $\hat{\beta}_t$ are the estimates using information up to month t. In summary, for out-of-sample forecasting, all inputs to the forecast are constructed using data that are observed no later than month t.

To iron out extreme outliers, we smooth the disagreement index with its six-month moving average values and plot the time series in Fig. 2.1. There are two interesting observations. First, the disagreement index is time-varying and does not diminish over time, which is consistent with the finding in Hong and Stein (2007) and Cookson and Niessner (2020) that permanent disagreement can arise even when investors have common priors and observe the same time series of public information, so long as they interpret information differently. Second, the disagreement index value can be large in bad times, such as the recessions of 1981 to 1982 and 2007 to 2008, and also in good times, such as the dot-com boom of the late 1990s. This evidence is consistent with the beta-weighted analyst forecast dispersion in Hong and Sraer (2016).

2.3.2 Forecasting performance

This section explores the in- and out-of-sample forecasting performance of the disagreement index. For comparison, we consider two alternative disagreement indexes as benchmarks. The first alternative disagreement index is constructed based on PCA, which extracts the first principal component of the 24 individual disagreement measures as the aggregate index. This method has been widely used in finance, such as Baker and Wurgler (2006) who construct an investor sentiment index as the first principal component of six individual sentiment proxies. The second alternative disagreement index is constructed by simply equal-weighting the 24 (standardized) individual disagreement measures. The intuition is that if each individual measure is unbiased, equal-weighting will efficiently reduce the idiosyncratic errors.

Panel A of Table 2.3 reports the results of predicting market returns with the three disagreement indexes. At the one-month horizon, a one-standard deviation increase in disagreement leads to a 0.38% decrease in the next one-month market return with the PCA disagreement index (t-value $= -1.96$), a 0.60% decrease with the equal-weight disagreement index (t-value $= -2.87$), and a 0.83% decrease with the PLS disagreement index (t-value $= -3.96$). When turning to out-of-sample forecasting, the R^2_{OS} with the PCA disagreement index is 0.20% and not significant. In contrast, the R^2_{OS} is 0.90% with the equal-weight disagreement index and 1.56% with the PLS disagreement index, which are both significant at the 5% level.

Panels B and C of Table 2.3 report the results when the forecast horizons are three and 12 months, respectively. In these two cases, all the three disagreement indexes display significant in- and out-of-sample forecasting power. For example, a one-standard deviation increase in disagreement leads to 2.92%, 4.93%, and 7.04% decreases in the next 12-month market returns with the three disagreement indexes, respectively. The R^2 and R^2_{OS} of the PLS disagreement index, 13.88% and 13.26%, are comparable with the most powerful predictor to date, the aggregate short interest in Rapach, Ringgenberg, and Zhou (2016), whose corresponding values are 12.89% and 13.24%, respectively.

Why does the PLS disagreement index have stronger forecasting power than the two alternative disagreement indexes? The reason is that while the PCA and equal-weight disagreement indexes can efficiently reduce the idiosyncratic measurement and observation errors in the individual disagreement measures, they cannot tease out the common errors that are unrelated to expected market returns. In contrast, as a supervised learning technique, the PLS aggregates information relevant to expected market returns and is supposed to perform the best.

To better understand their differences in forecasting power, Fig. 2.2 depicts the forecasted 3-month market returns based on the PCA, equal-weight, and PLS disagreement indexes for the 1991:02–2018:12 out-of-sample period (the results with other forecasting horizons are similar and omitted for brevity). The PLS disagreement index generates more volatile forecasts than the other two and naturally does a better job in capturing the variation of expected market returns. To explore the dominant variables in constructing the PLS index, Fig. 2.3 exhibits the top five individual disagreement measures at each point in time when conducting the out-of-sample forecasting. In general, consumption growth forecast dispersion, TBL forecast dispersion, realized personal financial improvement dispersion, business condition forecast dispersion, and house purchase condition forecast dispersion are more likely to be chosen.

In this chapter, we measure disagreement with the first PLS factor. One natural question is how many PLS factors we should use in our setting. Following Kelly and Pruitt (2015), we calculate the Bayesian Information Criterion (BIC) via the Krylov representation method and find that only one factor is chosen statistically. To see this is true, Table A2 reports the R^2s and R^2_{OS}s with the first to sixth moment PLS factors in predicting market returns, where the PLS factors are extracted by using the automatic proxy-selection algorithm in Kelly and Pruitt (2015). The results show that the second to sixth PLS factors do not have any in- and out-of-sample forecasting power, thereby supporting our choice of focusing on the first PLS factor.

In summary, extant disagreement measures do have a common component that is able to predict market returns, and the forecasting power depends on how we aggregate information across individual measures.

2.3.3 Controlling for economic predictors

This section examines whether the forecasting power of the disagreement index on market returns remains significant after controlling for extant economic predictors. In so doing, we consider the 14 economic predictors in Welch and Goyal (2008), output gap in Cooper and Priestley (2009), and aggregate short interest in Rapach, Ringgenberg, and Zhou (2016), and run the following regression:

$$R_{t+1} = \alpha + \beta D_t + \psi Z_t + \varepsilon_{t+1}, \tag{2.13}$$

where Z_t is one of the 16 economic predictors.

Table 2.4 reports the results. For comparison, Panel A considers the predictive power of the 16 economic predictors and shows that only four variables are able to significantly predict market returns, including the long-term bond return, term spread, output gap, and aggregate short interest. Panel B shows that controlling for extant economic predictors does not reduce the forecasting power of the disagreement index. For example, when controlling for output gap, the corresponding slope slightly decreases to -0.75 in absolute value and is significant at the 1% level. When the disagreement index and aggregate short interest are jointly used as predictors, the regression slope on the disagreement index remains at a value of -0.84, which is almost the same as without controlling for the aggregate short interest. In the last row, we consider a kitchen sink regression by including all the economic predictors. To handle highly correlated predictors, we estimate the regression slopes with the elastic net method, which has been successfully used in Rapach, Strauss, and Zhou (2013) and Kozak, Nagel, and Santosh (2020) for time series and cross-sectional predictability. The result shows that the forecasting power of the disagreement index remains quantitatively the same as the case of using the disagreement index alone. Therefore, the predictive ability of the disagreement index is not subsumed by extant economic predictors and it contains independent information beyond these economic predictors.

2.3.4 Controlling for uncertainty measures

In the literature, disagreement has two alternative interpretations: investor heterogeneity and uncertainty. For example, Anderson, Ghysels, and Juergens (2005) show theoretically and empirically that investor heterogeneity matters for asset pricing and measure it with analyst forecast dispersion. In contrast, Wang, Yan, and Yu (2017) proxy analyst forecast dispersion for uncertainty. While these two alternative explanations can be reconciled by the theory of Atmaz and Basak (2018), it remains empirically interesting to explore whether the disagreement index is different from extant uncertainty measures. Specifically, we employ eight uncertainty measures, including economic uncertainty (Bali, Brown, and Caglayan, 2014), treasury implied volatility (Choi, Mueller, and Vedolin, 2017), financial uncertainty and macro uncertainty (Jurado, Ludvigson, and Ng, 2015), economic policy uncertainty (Baker, Bloom, and Davis, 2016), news implied volatility (Manela and Moreira, 2017), sample variance (Welch and Goyal, 2008), and VIX.

We investigate the forecasting power of disagreement by controlling for macro uncertainty as:

$$R_{t+1} = \alpha + \beta D_t + \psi U_t + \varepsilon_{t+1}, \tag{2.14}$$

where U_t is one of the eight uncertainty measures. As a benchmark, Panel A of Table 2.5 shows that extant uncertainty measures cannot significantly predict market returns with one exception, namely financial uncertainty in Jurado, Ludvigson, and Ng (2015). However, the forecasting sign of financial uncertainty seems inconsistent with asset pricing theories that higher uncertainty implies higher risk premium.

Panel B shows that the disagreement index remains significant in predicting market returns after controlling for extant uncertainty measures. For example, in the kitchen sink regression that includes all the eight uncertainty measures, the slope on the disagreement index is still -0.89, close to the case without any controls (-0.83%). Overall, while the disagreement index is positively correlated with extant uncertainty measures in general, it contains different information for future market returns.

2.3.5 Economic value with disagreement prediction

In this section, we examine the economic value of forecasting market returns with the disagreement index from the perspective of investing. Following Ferreira and Santa-Clara (2011) and many others, we explore the certainty equivalent return (CER) gain and Sharpe ratio. The higher the CER gain and Sharpe ratio, the larger the risk-rewarded returns by using the disagreement index.

Suppose a mean-variance investor invests her wealth between the stock market and the one-month T-bill rate. At the start of each month, she allocates a proportion of w_t to the stock market to maximize her next one-month expected utility

$$U(R_p) = \mathrm{E}(R_p) - \frac{\gamma}{2}\mathrm{Var}(R_p), \tag{2.15}$$

where R_p is the return of the investor's portfolio, $\mathrm{E}(R_p)$ and $\mathrm{Var}(R_p)$ are the mean and variance of the market returns, and γ is the investor's risk aversion.

Let R_{t+1} and $R_{f,t+1}$ be the market return and T-bill rate. The investor's portfolio return at the end of each month is

$$R_{p,t+1} = w_t R_{t+1} + R_{f,t+1}, \tag{2.16}$$

where $R_{f,t+1}$ is known at t. With a simple calculation, the optimal portfolio weight is

$$w_t = \frac{1}{\gamma}\frac{\hat{R}_{t+1}}{\hat{\sigma}^2_{t+1}}, \tag{2.17}$$

where $\hat{R}_{t+1}$ and $\hat{\sigma}^2_{t+1}$ are the investor's estimates on the mean and variance of the market returns based on information up to time t.

The CER of the portfolio is

$$\mathrm{CER} = \hat{\mu}_p - \frac{\gamma}{2}\hat{\sigma}^2_p, \tag{2.18}$$

where $\hat{\mu}_p$ and $\hat{\sigma}^2_p$ are the mean and variance of the investor's portfolio over the out-of-sample evaluation period. The CER can be interpreted as the compensation to the investor for holding the stock market. The difference between the CERs for the investor using the predictive regression based on disagreement and the historical return mean is naturally an economic measure of predictability significance.

Table 2.6 presents the economic value generated by optimally trading on the disagreement index for the investor with a risk aversion of 3 and 5, respectively. That is, we report the CER difference between the strategy using the disagreement forecast and the strategy using the historical return mean. We annualize the CER by multiplying it by 1,200 so that the CER difference denotes the percentage gain per year for the investor to use the disagreement index forecast instead of the historical return mean. Following Campbell and Thompson (2008), we assume that the investor uses a ten-year moving window of past monthly returns to estimate the variance of market returns, and constraints w_t to lie between 0 and 1 in order to exclude extreme cases.

For comparison, we also consider the alternative PCA and equal-weight disagreement indexes. The results show that among the three disagreement indexes, the PLS disagreement index performs the best and the PCA index performs the worst, which is consistent with the results in Table 2.3 that both the PLS and equal-weight disagreement indexes can generate significant R^2_{OS}s at the one-month horizon. In Panel A, when there is no transaction cost, the annualized CER gain by using the PLS disagreement index is 2.50%, suggesting that investing with the PLS disagreement index forecast can generate 2.50% more risk-adjusted return relative to the historical return mean. The monthly Sharpe ratio is 0.18 and much higher than the market Sharpe ratio of 0.10 in our sample period. When there is a transaction cost of 50 basis points, the CER gain by using the PLS disagreement index is 1.92%, which is still economically sizeable. The corresponding Sharpe ratio is 0.16. Panel B shows similar results when the investor's risk aversion is 5. For example, the CER gain is 2.68% without transaction costs and is 1.88% with atransaction cost of 50 basis points. In summary, the PLS disagreement index is able to deliver considerable economic value for a mean-variance investor.

2.3.6 Alternative econometric methods

In the previous sections, we have shown that market returns can be significantly predicted by the PLS disagreement index. This section examines whether the result is robust to alternative econometric methods. Particularly, we consider six LASSO-related machine learning methods: equal-weight LASSO, combination LASSO (Han, He, Rapach, and Zhou, 2019), encompassing LASSO (Han, He, Rapach, and Zhou, 2019), adaptive LASSO (Freyberger, Neuhierl, and Weber, 2020), egalitarian LASSO (Diebold and Shin, 2019), elastic net (Kozak, Nagel, and Santosh, 2020). These six methods are introduced in detail in the Online Appendix.

Table 2.7 reports the results. There are three observations. First, the out-of-sample R^2_{OS}s are all significant at the one- to 12-month horizons, which confirms the predictability of the PLS disagreement index on market returns. For example, with the elastic net method, the R^2_{OS}s are 1.36% and 8.43% at the one- and 12-month horizons, respectively, and significant at the 5% level. Second, the disagreement index by using the equal-weight LASSO method significantly improves the forecasting power of the equal-weight disagreement index in Section 2.3.2. The R^2_{OS} increases from 0.90% to 1.26% at the one-month horizon and from

9.41% to 12.08% at the 12-month horizon, thereby suggesting that machine learning techniques are useful for return predictability. Finally, while these six alternative methods work well for predicting market returns, they underperform the PLS. This finding lends empirical support to Kelly and Pruitt (2015) that the PLS forecast is asymptotically consistent and will generate the minimum MSFE so long as the consistency condition is satisfied.

To explore which individual disagreement measures are important in predicting market returns, Figs. A1 and A2 in Online Appendix plot the selected measures and their frequencies according to the six LASSO-related methods at each point in time when conducting out-of-sample forecasting. Over the 1991:02–2018:12 out-of-sample period, some measures are commonly and frequently selected by all the methods. For example, the housing starts forecast dispersion and business condition forecast dispersion are the two most important individual measures and selected by all the six methods with a probability of 100%. The next three important measures are the CPI forecast dispersion, TBL forecast dispersion, and value-weighted analyst forecast dispersion, which are commonly selected with a probability of around 50%. In contrast, the disagreement measures based on the standardized unexplained volume, idiosyncratic volatility, and option open interest are rarely selected by any of the six LASSO-related methods. These results are generally consistent with Fig. 2.3 and suggest that disagreement measures that are based on professional and household forecasts are equally important in predicting market returns, whereas measures that are based on market information are not.

2.4 Economic Implications

This section shows that the predictability of the disagreement index is consistent with the theory in Atmaz and Basak (2018). In particular, we test four implications. The disagreement index 1) predicts market returns asymmetrically, with stronger power in high sentiment periods, 2) negatively predicts investors' ex post return forecast errors, 3) predicts market returns via a cash flow channel in the sense of Campbell (1991), and 4) can explain the positive relationship between trading volume and market volatility.

2.4.1 Asymmetric forecasting power

One key implication in Atmaz and Basak (2018) is that disagreement should display an asymmetric forecasting pattern in different market states. The reason is that when investors are relatively pessimistic, the first and second channels have different forecasting signs and are likely to offset each other, making the disagreement-return relation insignificant. In contrast, when investors are overly optimistic, the second channel dominates the first channel and hence disagreement should negatively predict future stock returns. In the following, we use the investor sentiment index of Baker and Wurgler (2006) to test whether the forecasting power of the disagreement index is asymmetric over the high and low sentiment periods, where a month is defined as high if the past 18-month average sentiment index is positive, and low otherwise. The results by using the PLS sentiment index in Huang et al. (2015) are quantitatively similar and omitted for brevity.

Time series evidence

Following Rapach, Strauss, and Zhou (2010), we calculate the in-sample R^2s in high and low sentiment periods as:

$$R_c^2 = 1 - \frac{\sum_{t=1}^{T} S_t^c (\hat{\varepsilon}_t)^2}{\sum_{t=1}^{T} S_t^c (R_t - \bar{R})^2}, \quad c = \text{high, low}, \tag{2.19}$$

where $S_t^{\text{high,}}$ (S_t^{low}) is an indicator that takes a value of one when month t is in a high (low) sentiment period and zero otherwise, $\hat{\varepsilon}_t$ is the fitted residual based on the in-sample estimates, $\bar{R}$ is the full-sample mean of R_t, and T is the number of observations for the full sample. Note that, unlike the full-sample R^2 statistic, the R^2_{high} and R^2_{low} statistics can be either positive or negative. Similarly, we can also calculate the R^2_{OS} in high and low sentiment periods separately. Another way to test the forecasting asymmetry is to run the following state-dependent regression:

$$R_{t+1} = \alpha + \beta_{\text{high}} S_t^{\text{high}} D_t + \beta_{\text{low}} S_t^{\text{low}} D_t + \varepsilon_{t+1}. \tag{2.20}$$

Table 2.8 shows that the forecasting power of the disagreement index is concentrated in high sentiment periods. In Panel A, the R^2 and R^2_{OS} are 5.28% and 3.69% in high sentiment periods, and 0.80% and -0.55% in low sentiment periods, respectively. In Panel B, the regression slope of the disagreement index in high sentiment periods is -1.12 with a t-value of -4.71, but it is only -0.42 with an insignificant t-value of -1.21 in low sentiment periods. Therefore, the predictability of disagreement on market returns is asymmetric and concentrated in high sentiment periods.

Cross-sectional evidence

In a multiple-stock economy, Atmaz and Basak (2018) suggest that the forecasting power of disagreement should be asymmetric across stocks, stronger among stocks with optimistic investor expectations and weaker or insignificant among stocks with pessimistic investor expectations. Different from Miller (1977), this implication holds even in the absence of short-sale constraints, so long as there are infinite risk averse investors. For this reason, we test the implication based on portfolios sorted by expectation directly.

Following Bordalo et al. (2019), we proxy the analyst LTG forecast for investor expectation at the firm level and construct 10 decile portfolios at the end of December for each year. The portfolios are subsequently held for one year. In the 1982–2018 period, the portfolio with low LTG forecast earns an annual return of 13.58% and the portfolio with high LTG forecast earns an annual return of 7.89%, with the difference between the high and low LTG forecast portfolios equal to 5.69% per year. Panel A of Fig. 2.4 plots the regression slopes of predicting the ten decile portfolio returns with the disagreement index. Apparently, the slope increases in magnitude from -0.61 for the portfolio with low LTG forecast to -1.09% for the portfolio with high LTG forecast.

Also, to explore the time-varying effect of the average bias of investor expectations, we run the following state-dependent regression:

$$R_{i,t+1} = \alpha_i + \beta_{\text{high},i} S_t^{\text{high}} D_t + \beta_{\text{low},i} S_t^{\text{low}} D_t + \varepsilon_{i,t+1}. \tag{2.21}$$

Panels B and C of Fig. 2.4 plot the regression slopes in high and low sentiment periods, respectively. As expected, the forecasting power of the disagreement index is concentrated in high sentiment periods. β_{high} monotonically increases in magnitude from -0.57 for the low LTG forecast portfolio to -1.96 for the high LTG forecast portfolio. In contrast, β_{low} is flat and displays a slightly upward trend. Also, Fig. A3 in the Online Appendix shows that portfolios with lower institutional ownership, higher beta, or higher IVOL earn lower average returns among high disagreement periods and confirms the argument that disagreement and arbitrage costs have an interaction effect (see, e.g., Hong and Sraer, 2016). In general, the predictability of disagreement is both time series and cross-sectionally asymmetric, stronger among stocks with optimistic cash flow expectation in high sentiment periods.

2.4.2 Disagreement and expectations of market returns

In the previous section we have linked disagreement with investor expectation (measured by investor sentiment) in an indirect manner. In this section we examine the relation of disagreement with investor expectations of market returns directly. According to Atmaz and Basak (2018), since disagreement amplifies investor optimism, it should be negatively related to ex post return forecast errors.[7]

Specifically, we consider four measures of investor expectations of 12-month ahead market returns. The first measure is the value-weighted aggregate analysts'

[7] We thank the anonymous referee for this and many other intriguing suggestions

return forecast, where the analysts' return forecast of an individual stock is defined as the mean of 12-month ahead analysts' target prices divided by current price (Engelberg, McLean, and Pontiff, 2020), and the target prices are restricted to those reported in the past one month. The second measure is Michigan survey of consumers attitudes. Following Das, Kuhnen, and Nagel (2019), we use the responses to "Suppose that tomorrow someone were to invest one thousand dollars in a type of mutual fund known as a diversified stock fund. What do you think is the percent chance that this one thousand dollar investment will increase in value in the year ahead, so that it is worth more than one thousand dollars one year from now?" The third and last measures are Graham-Harvey's survey of CFOs and Robert Shiller's survey of individual investor confidence in the stock market, which are constructed strictly following Greenwood and Shleifer (2014). The first three expectation measures have the same measurement unit as the realized market returns, whereas the last one, Shiller's survey, is based on binary variables. As such, when calculating the ex post return forecast errors, we project the aggregate analysts return forecast on Shiller's survey, so that the projected time series has the same measurement unit as the realized returns.

Table 2.9 reports the results. In Panel A, the disagreement index is positively related to the four investor expectation measures, with significant correlations ranging from 0.24 to 0.35. In terms of economic magnitude, a one-standard deviation increase in disagreement is associated with 3.26%, 2.71%, 1.57%, and 2.16% increases in investor expectations of 12-month ahead market returns with the analyst's return forecast, Michigan survey, Graham-Harvey's survey, and Shiller's survey, respectively. Untabulated results also confirm Greenwood and Shleifer (2014) that these investor expectation measures negatively predict 12-month ahead market returns. In Panel B, when regressing ex post return forecast errors on the disagreement index, we find that all the regression coefficients are significantly negative. For example, a one-standard deviation increase in disagreement is associated with a 7.34% increase in the analysts' return forecast error (i.e., the deviation of analysts' return forecast from the realized return increases by 7.34%), and the disagreement index explains about one quarter of variations of analysts' return forecast errors (i.e., $R^2 = 23.26\%$). Overall, Table 2.9 suggests that disagreement is closely linked with investor expectations of market returns, for both sophisticated and retailed investors.

2.4.3 Relation of disagreement with cash flow news and discount rate news

The section examines the contemporaneous relation of disagreement with cash flow news and discount rate news, so that we can disentangle the forecasting channel in the sense of Campbell (1991).

Following Campbell (1991), the log total market return can be decomposed into three components,

$$\tilde{R}_t \approx \mathrm{E}_{t-1}(R_t) + \mathrm{CF}_t - \mathrm{DR}_t, \tag{2.22}$$

where CF_t and DR_t are cash flow news and discount rate news, and they are defined as

$$\mathrm{CF}_t = (\mathrm{E}_t - \mathrm{E}_{t-1}) \sum_{j=0}^{\infty} \kappa^j \Delta d_{t+j} = (\Delta d_t - \mathrm{E}_{t-1} \Delta d_t) + (\mathrm{E}_t - \mathrm{E}_{t-1}) \sum_{j=1}^{\infty} \kappa^j \Delta d_{t+j}, \tag{2.23}$$

$$\mathrm{DR}_t = (\mathrm{E}_t - \mathrm{E}_{t-1}) \sum_{j=1}^{\infty} \kappa^j \tilde{R}_{t+j}, \tag{2.24}$$

where Δd_{t+j} and $\tilde{R}_{t+j}$ are the log dividend growth and log total market return at time $t+j$, and κ is a log-linearization constant slightly less than one. In another word, CF_t and DR_t are return innovations due to updates in expectations of current and future cash flows and future expected returns, respectively.

Atmaz and Basak (2018) posit that, after positive cash flow news, say $\Delta d_t - \mathrm{E}_{t-1} \Delta d_t > 0$, investors whose beliefs are supported by the cash flow news become relatively wealthier, which makes them more optimistic about future cash flows or discount rates or both, and consequently, increases disagreement. In contrast, after negative cash flow news, investors who have been optimistic become relatively poorer and pessimistic, thereby shrinking disagreement. This suggests that both CF_t and DR_t can positively affect disagreement and drive its movements.

To explore which component is the main driver of disagreement, we use contemporaneous CF_t and DR_t as the targets in Eq. (2.10) to extract a cash flow news-based PLS disagreement index and a discount rate news-based PLS disagreement index, and then examine their power in predicting future market returns. The results are reported in Table 2.10, where the cash flow news and discount rate news are estimated based on individual VARs comprising the total market return, dividend-price ratio, and one of the rest 15 economic predictors explored in Table 2.4. We always include the dividend-price ratio in the VARs because Engsted, Pedersen, and Tanggaard (2012) show that it is important to include this variable to properly estimate the cash flow and discount news components. In the last row of Table 2.10, we also consider the decomposition based on a VAR comprising the total market return, log dividend-price ratio, and the first three principal components extracted from the 15 economic predictors.

Table 2.10 shows that only the cash flow news-based disagreement index has forecasting power on market returns. For example, when the cash flow news and discount rate news are estimated with the VAR comprising the total market return and dividend-price ratio, a one-standard deviation increase in the cash flow news-based disagreement index predicts a 0.65% decrease in the next one-month market return, while the discount rate news-based disagreement index displays nil power. This finding echoes Section 2.3.2 in which we show statistically that there is only one PLS factor exhibiting forecasting power on market returns. Thus, we conclude that the ability of disagreement in predicting market returns is more likely to operate via a cash flow channel in the sense of Campbell (1991).

2.4.4 Relation of disagreement with trading volume and market volatility

In Atmaz and Basak (2018), in the absence of disagreement, trading volume is zero and market volatility is constant. In the presence of disagreement, however, higher disagreement leads to both higher trading volume and higher market volatility, thereby suggesting that disagreement is the driver of the positive volume-volatility relationship.

To test the implication, we estimate the volume-volatility elasticity in month t as the slope of regressing the daily change in market turnover on the daily change in volatility within month t, and then regress the monthly elasticity on the lagged disagreement index. For robustness, we consider four daily volatility measures, including realized volatility, realized semi-volatility, and median realized volatility based on the S&P 500 index returns from 5-minute intervals from Andersen, Dobrev, and Schaumburg (2012), and realized volatility of the S&P 500 index futures contract returns from 5-minute intervals from Johnson (2019).

Panel A of Table 2.11 shows that the disagreement index positively predicts the volume-volatility elasticity. The intuition is that increased disagreement increases the average bias of investor expectations, which in turn increases both the fluctuation of stock price and the trading demand (due to the increased weight of investors with relatively different beliefs), thereby increasing the volume-volatility elasticity. In a more intuitive way, we show in Table A3 in the Online Appendix that the disagreement index positively predicts the correlation between trading volume and market volatility. For example, a one-standard deviation increase in disagreement predicts a 5.22% increase in the volume-volatility correlation of next month when market volatility is estimated with the realized volatility.

To corroborate Panel A, we decompose market volatility into two components: one is contemporaneously related to disagreement and extracted via the PLS method and the other is unrelated to disagreement. Then we regress the one-month ahead trading volume on these two volatility components and report the results in Panel B of Table 2.11. As expected, the disagreement-related volatility significantly positively predicts future trading volume, whereas the disagreement-unrelated volatility does not have any predictive power. Similarly, when decomposing trading volume into disagreement-related and unrelated components, we find that the disagreement-related volume predicts future market volatility but the disagreement-unrelated volume fails to do so, which is reported in Panel B of Table A3.

In sum, this section provides empirical support to Atmaz and Basak (2018) that disagreement seems a key driver of the positive volume-volatility relationship.

2.5 Conclusion

This chapter examines whether extant individual disagreement measures are agreeable and proposes a disagreement index by using the PLS methodology in Kelly and Pruitt (2013, 2015). We show that this PLS disagreement index significantly predicts market returns both in- and out-of-sample. Consistent with the theory in Atmaz and Basak (2018), the disagreement index asymmetrically predicts market returns with greater power in high sentiment periods, is negatively related to investors' ex post return forecast errors, predicts market returns through a cash flow channel, and is able to explain the positive volume-volatility relation.

There are some open issues for future research. First, it will be valuable to apply the disagreement index to other markets, such as bonds, commodities, and currencies, to see whether the forecasting power remains significant. Second, it will be of interest to construct aggregate disagreement indexes at different frequencies, such as daily or weekly, so that investors can use them for real time investing. Finally, as Hong and Stein (2007) posit that there are two main sources of disagreement—differences in information sets and differences in models that investors use to interpret information, it will be interesting to disentangle them.

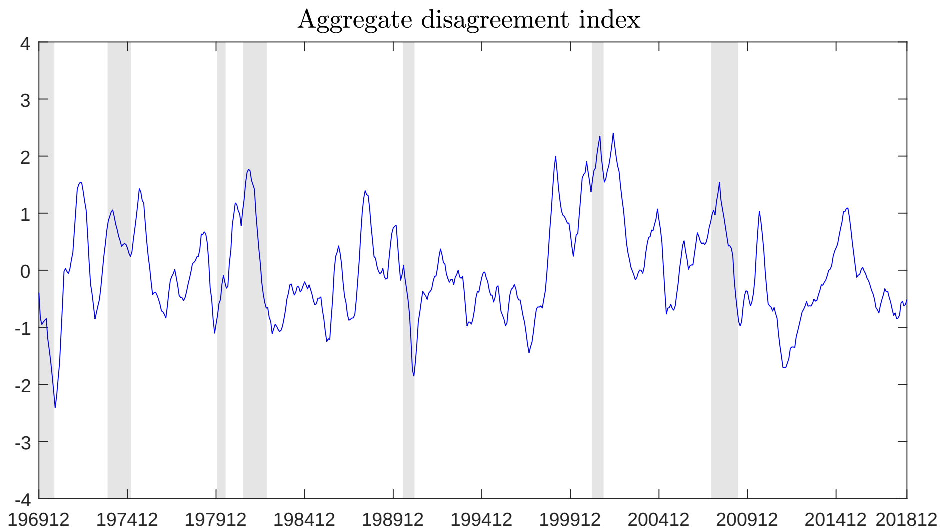

Fig. 2.1 This figure plots the time series dynamics of the disagreement index constructed by the PLS method in Kelly and Pruitt (2013, 2015). Grey shadow bars denote NBER recessions. The sample period is 1969:12–2018:12.

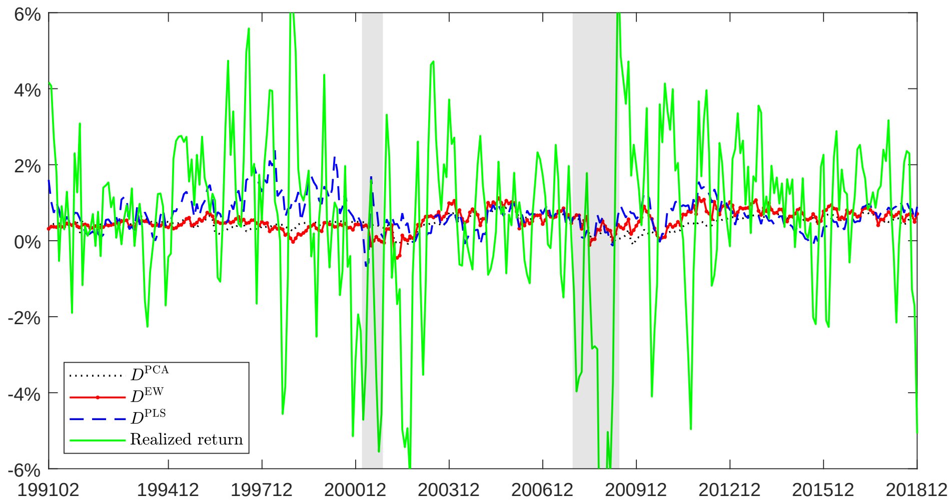

Fig. 2.2 This figure plots the out-of-sample 3-month market return forecasts with the PCA, equal-weight, and PLS disagreement indexes, respectively. For comparison, the figure also plots the realized 3-month market returns. Grey shadow bars denote NBER recessions. The out-of-sample period is 1991:02–2018:12.

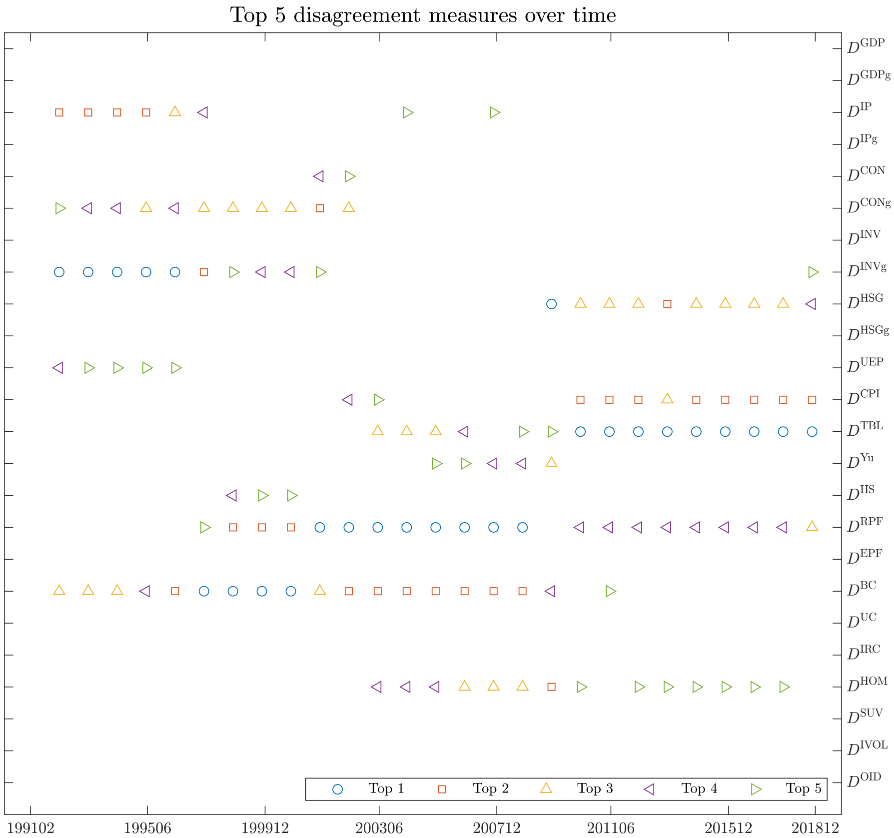

Fig. 2.3 This figure plots top five individual disagreement measures in the PLS disagreement index at each point in time when conducting out-of-sample forecasting.

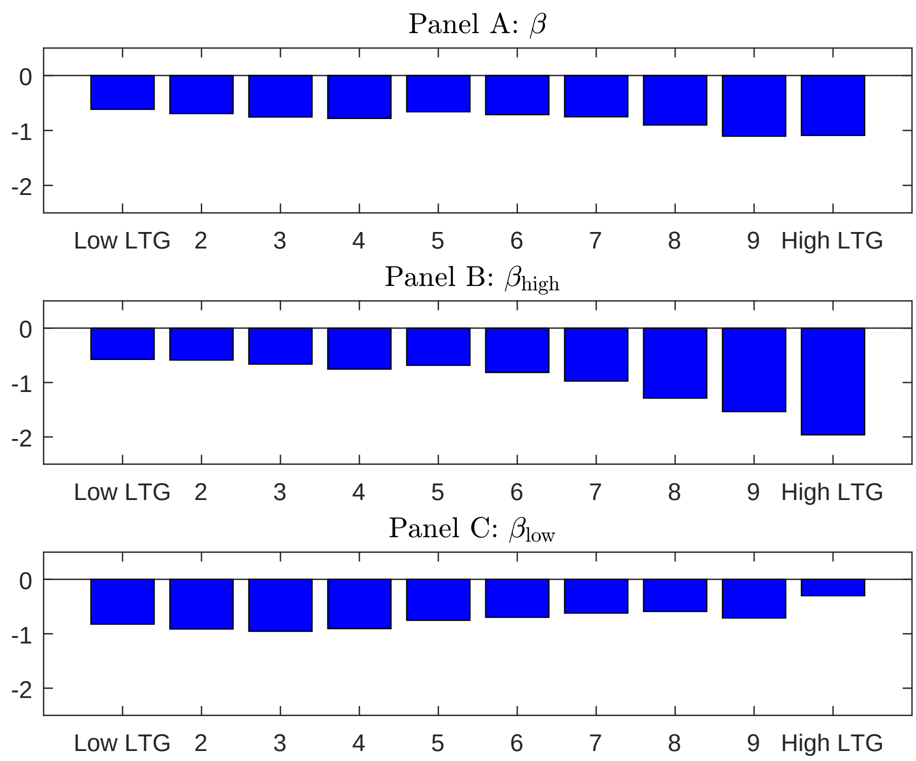

Fig. 2.4 This figure plots the regression slopes of predicting portfolio excess returns sorted by analyst long-term growth rate (LTG) forecast with the disagreement index as

$$R_{i,t+1} = \alpha_i + \beta_i D_t + \varepsilon_{i,t+1}$$

for Panel A, and

$$R_{i,t+1} = \alpha_i + \beta_{\text{high},i} S_t^{\text{high}} D_t + \beta_{\text{low},i} S_t^{\text{low}} D_t + \varepsilon_{i,t+1}$$

for Panels B and C. Low (high) LTG refers to the portfolio with low (high) analyst LTG forecast and is constructed the same as Bordalo et al. (2019). S_t^{high} (S_t^{low}) is a dummy variable that equals 1 if month t is in high (low) sentiment periods and 0 if month t is in low (high) sentiment periods (Baker and Wurgler, 2006). The sample period is 1982:01–2018:12.

Table 2.1 Summary statistics of individual disagreement measures

This table reports the summary statistics of 24 individual disagreement measures used in this chapter. The first 13 measures are obtained from the survey of professional forecasters (SPF) at a quarterly frequency, each of which is defined by the level or growth difference between the 75th and 25th percentiles of the forecasts. D^{Yu} and D^{HS} are value- and beta-weighted analyst forecast dispersions (Hong and Sraer, 2016; Yu, 2011). The next six are household belief dispersions on macroeconomic conditions from the Michigan survey of consumers attitudes. D^{SUV} is a disagreement measure based on the standardized unexplained trading volume of NYSE stocks (Garfinkel, 2009). D^{IVOL} is the value-weighted idiosyncratic volatility proposed by Boehme, Danielsen, and Sorescu (2006) for measuring investor disagreement. D^{OID} is a disagreement measure defined by the open interest difference of OEX call and put options (Ge, Lin, and Pearson, 2016).

Disagreement measure	Sample Period	Obs	Avg	Std	Min	Max	Skew	Kurt
GDP forecast dispersion (D^{GDP})	1968Q4–2018Q4	201	61.32	41.13	6.80	248.60	1.28	2.53
GDP growth forecast dispersion (D^{GDPg})	1968Q4–2018Q4	201	1.65	0.71	0.71	4.25	1.07	0.81
Industrial production forecast dispersion (D^{IP})	1968Q4–2018Q4	201	1.97	1.03	0.52	6.10	1.14	1.30
Industrial production growth forecast dispersion (D^{IPg})	1968Q4–2018Q4	201	2.73	1.43	0.84	8.04	1.14	0.99
Consumption forecast dispersion (D^{CON})	1968Q4–2018Q4	150	31.61	18.31	5.00	101.87	1.08	1.91
Consumption growth forecast dispersion (D^{CONg})	1968Q4–2018Q4	150	0.97	0.40	0.39	2.79	1.33	2.07
Investment forecast dispersion (D^{INV})	1981Q3–2018Q4	150	22.46	12.47	3.40	57.92	0.48	−0.39
Investment growth forecast dispersion (D^{INVg})	1981Q3–2018Q4	150	3.63	1.19	1.43	8.62	0.71	1.33
Housing starts forecast dispersion (D^{HSG})	1968Q4–2018Q4	201	0.12	0.04	0.05	0.27	0.90	0.53
Housing starts growth forecast dispersion (D^{HSGg})	1968Q4–2018Q4	201	18.70	10.00	6.46	57.34	1.38	1.52
Unemployment rate forecast dispersion (D^{UEP})	1968Q4–2018Q4	201	0.32	0.13	0.15	1.04	1.75	5.10
CPI forecast dispersion (D^{CPI})	1981Q3–2018Q4	150	0.83	0.30	0.38	2.02	1.35	1.89
TBL forecast dispersion (D^{TBL})	1981Q3–2018Q4	150	0.46	0.36	0.04	2.96	3.36	17.15
Value-weighted analyst forecast dispersion (D^{Yu})	1981:12–2018:12	445	3.67	0.61	2.64	5.79	1.04	0.60
Beta-weighted analyst forecast dispersion (D^{HS})	1981:12–2018:12	445	5.15	1.29	3.41	9.62	1.39	1.87
Realized personal financial improvement dispersion (D^{RPF})	1978:01–2018:12	492	−0.44	0.02	−0.50	−0.39	−0.42	−0.37
Expected personal financial improvement forecast dispersion (D^{EPF})	1978:01–2018:12	492	−0.64	0.05	−0.80	−0.50	−0.22	0.24
Business condition forecast dispersion (D^{BC})	1978:01–2018:12	492	−0.42	0.07	−0.69	−0.28	−0.96	1.04
Unemployment condition forecast dispersion (D^{UC})	1978:01–2018:12	492	−0.63	0.08	−0.95	−0.47	−0.60	0.14
Interest rate condition forecast dispersion (D^{IRC})	1978:01–2018:12	492	−0.53	0.08	−0.77	−0.35	−0.17	−0.59
House purchase condition forecast dispersion (D^{HOM})	1978:01–2018:12	492	−0.59	0.08	−0.80	−0.42	−0.02	−0.64
Standardized unexplained volume (D^{SUV})	1968:12–2018:12	589	0.14	1.25	−3.45	3.17	−0.15	−0.68
Idiosyncratic volatility (D^{IVOL})	1968:12–2018:12	589	0.02	0.00	0.01	0.03	1.72	3.25
OEX call/put open interest difference (D^{OID})	1984:02–2018:12	419	0.86	0.09	0.55	1.00	−0.99	0.73

Table 2.2 Forecasting market returns with individual disagreement measures

This table presents the regression slope, Newey-West t-value, in-sample R^2, and out-of-sample R^2_{OS} of predicting market returns with individual disagreement measures:

$$R_{t,t+h} = \alpha + \beta D_t + \varepsilon_{t,t+h},$$

where $R_{t,t+h}$ is the cumulative market return between months t and $t+h$ ($h = 1, 3$, or 12), and D_t is one of the 24 individual disagreement measures. The in-sample period is 1969:12–2018:12 and the out-of-sample period is 1991:02–2018:12 (because the accurate release dates of the Michigan survey of consumers attitudes are only available as of January 1991). Statistical significance for R^2_{OS} is based on the p-value of the Clark and West (2007) MSFE-adjusted statistic for testing $H_0 : R^2_{OS} \leq 0$ against $H_A : R^2_{OS} > 0$. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	Panel A: $h = 1$				Panel B: $h = 3$				Panel C: $h = 12$			
Disagreement	β	t-value	R^2	R^2_{OS}	β	t-value	R^2	R^2_{OS}	β	t-value	R^2	R^2_{OS}
D^{GDP}	−0.11	−0.50	0.06	−1.88	−0.22	−1.35	0.72	−5.40	-0.36^{***}	−3.54	7.43	−13.61
D^{GDPg}	−0.27	−1.53	0.38	−3.44	−0.24	−1.62	0.89	−7.18	-0.24^{**}	−2.56	3.21	−16.47
D^{IP}	−0.06	−0.24	0.02	−2.38	−0.07	−0.42	0.08	−5.04	−0.04	−0.31	0.09	−12.18
D^{IPg}	0.04	0.20	0.01	−2.23	−0.21	−1.46	0.65	−9.50	−0.14	−1.09	1.13	−20.03
D^{CON}	−0.11	−0.49	0.07	−1.79	−0.24	−1.21	0.88	−4.34	−0.17	−1.27	1.72	−15.22
D^{CONg}	−0.10	−0.41	0.05	−2.38	−0.19	−0.92	0.59	−5.73	−0.17	−1.29	1.73	−19.59
D^{INV}	−0.24	−1.36	0.31	−2.73	-0.27^{*}	−1.75	1.12	−8.11	−0.14	−0.88	1.15	−12.17
D^{INVg}	0.19	1.31	0.21	−1.60	0.02	0.17	0.01	−4.60	0.02	0.14	0.04	−8.25
D^{HSG}	-0.40^{**}	−2.11	0.85	−5.39	-0.26^{*}	−1.70	1.04	−12.29	−0.21	−1.57	2.48	−23.20
D^{HSGg}	−0.21	−0.99	0.23	−6.49	-0.34^{**}	−2.08	1.70	−24.59	-0.33^{*}	−1.90	5.73	−28.78
D^{UEP}	0.16	0.76	0.13	−0.73	0.13	0.82	0.27	−2.64	0.16	1.47	1.48	−3.38
D^{CPI}	-0.36^{*}	−1.90	0.73	−6.39	-0.27^{**}	−2.26	1.18	−27.39	−0.11	−1.32	0.74	−19.33
D^{TBL}	-0.60^{**}	−2.23	1.94	−4.21	-0.48^{**}	−2.12	3.60	−9.36	−0.25	−1.53	3.84	−13.52
D^{Yu}	−0.30	−1.23	0.35	−2.72	−0.30	−1.32	1.05	−4.07	−0.27	−1.25	3.06	−25.93
D^{HS}	−0.12	−0.42	0.06	−3.67	−0.16	−0.55	0.30	−7.92	−0.23	−0.85	2.52	−11.16
D^{RPF}	−0.22	−1.26	0.26	−3.35	−0.09	−0.55	0.11	−6.13	−0.16	−1.57	1.52	−17.10
D^{EPF}	−0.22	−1.03	0.26	−4.49	−0.16	−1.11	0.40	−9.37	−0.05	−0.49	0.17	−17.36
D^{BC}	-0.44^{**}	−2.48	1.05	−5.77	−0.23	−1.56	0.86	−10.44	−0.08	−0.66	0.37	−19.99
D^{UC}	−0.06	−0.27	0.02	−3.40	−0.03	−0.19	0.02	−6.35	−0.05	−0.37	0.17	−19.27
D^{IRC}	−0.17	−0.72	0.16	−2.64	-0.41^{**}	−2.12	2.66	−12.05	-0.44^{**}	−2.52	11.72	−25.38
D^{HOM}	0.11	0.65	0.07	−0.86	0.01	0.04	0.00	−3.71	−0.19	−1.12	1.94	−20.53
D^{SUV}	−0.30	−1.24	0.23	−2.80	−0.34	−1.48	0.86	−6.86	-0.43^{*}	−1.86	4.81	−18.40
D^{IVOL}	−0.03	−0.15	0.00	−5.46	−0.02	−0.07	0.00	−14.61	0.00	−0.02	0.00	−14.82
D^{OID}	−0.20	−0.73	0.10	−2.73	−0.15	−0.57	0.15	−6.09	−0.24	−0.93	1.47	−15.53

Table 2.3 Forecasting market returns with aggregate disagreement indexes

This table presents the regression slope, Newey-West t-value, in-sample R^2, and out-of-sample R^2_{OS} of predicting market returns with disagreement as

$$R_{t,t+h} = \alpha + \beta D_t + \varepsilon_{t,t+h},$$

where $R_{t,t+h}$ is the cumulative market return between months t and $t+h$ ($h = 1, 3$, and 12), and D_t is the PCA, equal-weight (individual measures), or PLS disagreement index. Statistical significance for R^2_{OS} is based on the p-value of the Clark and West (2007) MSFE-adjusted statistic for testing $H_0 : R^2_{OS} \leq 0$ against $H_A : R^2_{OS} > 0$. The in- and out-of-sample periods are 1969:12–2018:12 and 1991:02–2018:12, respectively. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Method	β	t-value	R^2	R^2_{OS}
Panel A: $h = 1$				
PCA	-0.38^{**}	-1.96	0.61	0.20
Equal-weight	-0.60^{***}	-2.87	1.46	0.90^{**}
PLS	-0.83^{***}	-3.96	2.52	1.56^{**}
Panel B: $h = 3$				
PCA	-1.13^{**}	-2.14	1.74	1.71^{***}
Equal-weight	-1.73^{***}	-2.99	3.88	3.74^{***}
PLS	-2.24^{***}	-3.82	5.98	7.68^{***}
Panel C: $h = 12$				
PCA	-2.92^{**}	-1.99	2.78	3.04^{***}
Equal-weight	-4.93^{***}	-3.06	7.49	9.41^{***}
PLS	-7.04^{***}	-4.16	13.88	13.26^{***}

Table 2.4 Controlling for economic variables

Panel A presents the results of predicting market returns as

$$R_{t+1} = \alpha + \psi Z_t + \varepsilon_{t+1},$$

where Z_t is one of the 14 economic predictors in Welch and Goyal (2008), output gap in Cooper and Priestley (2009), or aggregate short interest in Rapach, Ringgenberg, and Zhou (2016). Panel B reports the results of forecasting market returns with the disagreement index and one economic predictor as

$$R_{t+1} = \alpha + \beta D_t + \psi Z_t + \varepsilon_{t+1}.$$

The last row reports the slope of the disagreement index from an elastic net regression by including all the economic predictors, where the t-value is calculated following Tibshirani et al. (2016) and Lee et al. (2016). The sample period is 1969:12–2018:12. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	Panel A: Univariate		Panel B: Bivariate		
Economic predictor	ψ	R^2	β	ψ	R^2
Dividend-price ratio (DP)	0.15	0.11	−0.82***	0.02	2.52
Dividend yield (DY)	0.17	0.15	−0.82***	0.03	2.52
Earning-price ratio (EP)	0.08	0.03	−0.83***	−0.01	2.52
Dividend payout ratio (DE)	0.08	0.03	−0.82***	0.03	2.53
Sample variance (SVAR)	−0.22	0.26	−0.81***	−0.07	2.54
Book-to-market ratio (BM)	0.00	0.00	−0.83***	−0.03	2.53
Net equity expansion (NTIS)	−0.06	0.02	−0.83***	−0.07	2.55
Treasury bill rate (TBL)	−0.26	0.36	−0.81***	−0.21	2.76
Long-term bond yield (LTY)	−0.15	0.11	−0.82***	−0.12	2.60
Long-term bond return (LTR)	0.42**	0.93	−0.82***	0.41**	3.41
Term spread (TMS)	−0.41**	0.89	−0.81***	−0.39**	3.31
Default yield spread (DFY)	−0.16	0.13	−0.84***	−0.21	2.74
Default return spread (DFR)	0.36	0.68	−0.80***	0.32	3.06
Inflation rate (INFL)	0.01	0.00	−0.83***	0.05	2.54
Output gap (OG)	−0.46***	1.08	−0.75***	−0.33**	3.07
Short interest (SI)	−0.55**	1.48	−0.84***	−0.46*	3.85
Kitchen sink (via elastic net)	–	–	−0.72***	–	5.50

Table 2.5 Controlling for uncertainty measures

We consider eight uncertainty measures, including economic uncertainty (Bali, Brown, and Caglayan, 2014), treasury implied volatility (Choi, Mueller, and Vedolin, 2017), financial uncertainty and macro uncertainty (Jurado, Ludvigson, and Ng, 2015), economic policy uncertainty (Baker, Bloom, and Davis, 2016), news implied volatility (Manela and Moreira, 2017), sample variance (Welch and Goyal, 2008), and the Chicago Board Options Exchange (CBOE) volatility index (VIX). Panel A presents the results of predicting market returns with one uncertainty measure as

$$R_{t+1} = \alpha + \psi U_t + \varepsilon_{t+1}.$$

Panels B presents the results of predicting market returns with the disagreement index and one uncertainty measure as

$$R_{t+1} = \alpha + \beta D_t + \psi U_t + \varepsilon_{t+1}.$$

The last row reports the slope of the disagreement index from an elastic net regression by including all the uncertainty measures, where the t-value is calculated following Tibshirani et al. (2016) and Lee et al. (2016). ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	Panel A: Univariate		Panel B: Bivariate		
Uncertainty	ψ	R^2	β	ψ	R^2
Economic uncertainty	−0.13	0.09	−1.02***	−0.04	4.88
Treasury implied volatility	−0.37	0.70	−1.01***	−0.06	4.72
Financial uncertainty	−0.62**	2.01	−0.69***	−0.49*	3.68
Macro uncertainty	−0.45	1.06	−0.74***	−0.30	2.96
Economic policy uncertainty	0.25	0.32	−0.86***	0.10	3.02
News implied volatility	0.09	0.04	−0.85***	0.14	2.74
Sample variance	−0.22	0.26	−0.81***	−0.07	2.54
VIX	0.00	0.00	−1.06***	0.24	4.66
Kitchen sink (via elastic net)	–	–	−0.89**	–	5.09

Table 2.6 Asset allocation results

This table reports portfolio gains of a mean-variance investor with risk-aversion $\gamma = 3$ or 5 for predicting market returns with the PCA, equal-weight (individual measure), and PLS disagreement indexes, respectively. The investor allocates her wealth monthly among the stock market and the risk-free asset by applying the out-of-sample forecasts based on one of the three disagreement indexes. CER gain is the annualized certainty equivalent return difference between applying a disagreement index forecast and applying the historical return mean forecast. Sharpe ratio is the monthly average portfolio excess return divided by its standard deviation. The portfolio weight is estimated recursively using data available at the forecast formation month t. The investment period is 1991:02–2018:12. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	No transaction cost		50 bps transaction costs	
	CER gain (%)	Sharpe ratio	CER gain (%)	Sharpe ratio
Panel A: Risk aversion $\gamma = 3$				
PCA	0.71	0.14**	0.52	0.13
Equal-weight	1.70**	0.16**	1.33*	0.15**
PLS	2.50***	0.18***	1.92**	0.16**
Panel B: Risk aversion $\gamma = 5$				
PCA	0.96**	0.12**	0.81**	0.11**
Equal-weight	2.10***	0.16***	1.69**	0.14**
PLS	2.68***	0.17***	1.88**	0.14**

Table 2.7 Out-of-sample R^2_{OS}s of forecasting market returns with alternative methods

This table presents the out-of-sample R^2_{OS}s of forecasting h-month ahead market returns with six alternative information aggregation methods: equal-weight LASSO, combination LASSO, encompassing LASSO, adaptive LASSO, egalitarian LASSO, and elastic net. Statistical significance for R^2_{OS} is based on the p-value of the Clark and West (2007) MSFE-adjusted statistic for testing $H_0 : R^2_{OS} \leq 0$ against $H_A : R^2_{OS} > 0$. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Method	$h = 1$	$h = 3$	$h = 12$
Equal-weight LASSO	1.26**	6.09***	12.08***
Combination LASSO	1.08**	2.87***	9.67***
Encompassing LASSO	1.09**	2.92***	9.41***
Adaptive LASSO	0.71*	2.34***	7.47***
Egalitarian LASSO	1.30*	2.69***	8.42***
Elastic net	1.36**	2.69***	8.43***

Table 2.8 Asymmetric forecasting power of disagreement

Panel A reports the in- and out-of-sample R^2s of predicting market returns with the disagreement index in different time periods, which are calculated as Eq. (2.19). Panel B presents the results of predicting market returns with a state-dependent regression:

$$R_{t+1} = \alpha + \beta_{\text{high}} S_t^{\text{high}} D_t + \beta_{\text{low}} S_t^{\text{low}} D_t + \varepsilon_{t+1},$$

where S_t^{high} (S_t^{low}) is a dummy variable that equals 1 if month t is in high (low) sentiment periods and 0 if month t is in low (high) sentiment periods (Baker and Wurgler, 2006). Statistical significance for R_{OS}^2 is based on the p-value of the Clark and West (2007) MSFE-adjusted statistic for testing $H_0 : R_{OS}^2 \leq 0$ against $H_A : R_{OS}^2 > 0$. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Forecasting performance in different periods

In-sample R^2		Out-of-sample R_{OS}^2	
High sentiment	Low sentiment	High sentiment	Low sentiment
5.28	0.80	3.69^{**}	-0.55

Panel B: State-dependent regression

	β_{high}	t-value	β_{low}	t-value	R^2
Sentiment-based state	-1.12^{***}	-4.71	-0.42	-1.21	2.96

Table 2.9 Disagreement and expectations of market returns

Panel A reports the results of regressing expectations of market returns on the disagreement index:

$$\text{Expectation}_{t:t+12} = \alpha + \beta D_t + \varepsilon_t,$$

where $\text{Expectation}_{t:t+12}$ is investor expectation of 12-month ahead market return at time t, which is measured by (value-weighted) aggregate analysts' return forecast (Engelberg, McLean, and Pontiff, 2020), Michigan survey of consumers attitudes, Graham-Harvey survey of CFOs, or Robert Shiller's survey of individual investor confidence. Panel B reports the results of regressing return forecast errors on the PLS disagreement index:

$$\text{Realized return}_{t:t+12} - \text{Expectation}_{t:t+12} = \alpha + \beta D_t + \varepsilon_t,$$

where the $\text{Expectation}_{t:t+12}$ in the case of Shiller's survey is the projection of analysts' return forecast on Shiller's survey, so that the projection has the same measurement unit as the realized return. The sample periods all end in 2018:12, but start differently, from 1999:04 for analysts return forecast, 2002:06 for Michigan's survey, 2000:10 for Graham-Harvey's survey, and 2001:07 for Shiller's survey, respectively. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Expectations of market returns				
	$\text{Corr}(\text{Expectation}_{t:t+12}, D_t)$	β	t-value	R^2
Analysts' return forecast	0.35***	3.26***	2.61	12.45
Michigan survey	0.24***	2.71*	1.69	5.55
Graham-Harvey's survey	0.26**	1.57**	2.40	6.58
Shiller's survey	0.25***	2.16**	2.31	6.50
Panel B: Market return forecast errors				
		β	t-value	R^2
Analysts' return forecast		−7.34***	−2.73	23.26
Michigan survey		−8.42***	−3.36	21.17
Graham-Harvey's survey		−9.65***	−4.41	31.61
Shiller's survey		−9.18***	−4.45	30.34

Table 2.10 Forecasting market returns with cash flow news- and discount rate news-based disagreement indexes

This table reports the slopes and Newey-West t-values from the regression of

$$R_{t+1} = \alpha + \beta_{\mathrm{CF}} D_t^{\mathrm{CF}} + \beta_{\mathrm{DR}} D_t^{\mathrm{DR}} + \varepsilon_{t+1},$$

where D_t^{CF} (D_t^{DR}) is the PLS disagreement index that uses the contemporaneous cash flow news (discount rate news) as the regressor in Eq. (2.10). The cash flow news and discount rate news are estimated by using the Campbell (1991) VAR approach. In the leftmost column, "$\tilde{R}$" represents the total market return, economic variables are defined in Table 2.4, and "PC" represents the first three principal components extracted from all the economic variables (except for DP). ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

VAR variables	β_{CF}	t-value	β_{DR}	t-value	R^2
$\tilde{R}$, DP	-0.65^{***}	-2.92	-0.01	-0.08	2.15
$\tilde{R}$, DP, DY	-0.65^{***}	-3.08	-0.01	-0.03	2.16
$\tilde{R}$, DP, EP	-0.67^{***}	-2.73	-0.11	-0.51	2.06
$\tilde{R}$, DP, DE	-0.67^{***}	-2.73	-0.11	-0.51	2.06
$\tilde{R}$, DP, RVOL	-0.70^{**}	-2.52	-0.06	-0.23	2.23
$\tilde{R}$, DP, BM	-0.65^{***}	-3.29	0.11	0.61	2.16
$\tilde{R}$, DP, NTIS	-0.66^{***}	-3.02	-0.02	-0.11	2.21
$\tilde{R}$, DP, TBL	-0.55^{***}	-2.86	0.10	0.59	1.62
$\tilde{R}$, DP, LTY	-0.64^{***}	-3.23	0.09	0.53	2.13
$\tilde{R}$, DP, LTR	-0.65^{***}	-3.13	0.02	0.09	2.24
$\tilde{R}$, DP, TMS	-0.57^{***}	-2.87	0.07	0.39	1.81
$\tilde{R}$, DP, DFY	-0.65^{***}	-2.82	-0.02	-0.11	2.16
$\tilde{R}$, DP, DFR	-0.65^{***}	-2.70	-0.03	-0.13	2.10
$\tilde{R}$, DP, INFL	-0.65^{***}	-3.29	0.03	0.16	2.20
$\tilde{R}$, DP, OG	-0.57^{***}	-2.83	-0.06	-0.32	1.57
$\tilde{R}$, DP, SI	-0.47^{**}	-2.08	0.27	1.28	1.93
$\tilde{R}$, DP, PC	-0.66^{***}	-2.78	0.04	0.16	2.26

Table 2.11 Relation of disagreement with market volatility and trading volume

Panel A presents the results of predicting the volume-volatility elasticity with the disagreement index:

$$\text{Elasticity}_{t+1} = \alpha + \beta D_t + \varepsilon_{t+1},$$

where the elasticity in month $t+1$ is the slope of regressing the daily change in turnover of NYSE stocks on the daily change in volatility within month $t+1$. Realized volatility, realized semi-volatility, and median realized volatility are estimated based on the S&P 500 index returns from 5-minute intervals (Andersen, Dobrev, and Schaumburg, 2012), and futures realized volatility is estimated based on the S&P 500 index futures contract returns from 5-minute intervals (Johnson, 2019). Panel B presents the results of the following regression:

$$\text{Volume}_{t+1} = \alpha + \beta_1 \text{D\_Volatility}_t + \beta_2 \text{Volatility}_t^{\circ} + \varepsilon_{t+1}.$$

D_Volatility is the disagreement-related volatility and extracted with the PLS method, and Volatility$^{\circ}$ is the residual of regressing volatility on D_Volatility. Following Hamilton (2018), we apply AR(4) to both trading volume and market volatility to remove potential trends and expected information. Reported are regression coefficient, Newey-West t-value, and R^2. The sample period is 2000:01–2018:12 for the first three volatility measures and 1990:01–2015:12 for the last one. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Predicting volatility-volume elasticity					
Volatility measure	β	t-value	R^2		
Realized volatility	4.01***	4.41	4.94		
Realized semi-volatility	1.85**	2.08	1.83		
Median realized volatility	1.99**	1.98	1.25		
Futures realized volatility	2.24**	2.17	1.49		
Panel B: Predicting trading volume					
Volatility measure	β_1	t-value	β_2	t-value	R^2
Realized volatility	2.38***	3.32	−1.30	−1.36	5.31
Realized semi-volatility	2.53***	3.44	−1.16	−1.32	5.58
Median realized volatility	2.60***	3.31	−1.44*	−1.77	6.36
Futures realized volatility	1.13**	2.15	−0.46	−0.75	1.18

Chapter 3

Partisan Conflict and Stock Price[1]

Partisan conflict has been one dominant theme in U.S. politics in recent years. By using a textual index following Azzimonti (2018), this chapter shows that partisan conflict positively predicts market returns, controlling for economic predictors and proxies for uncertainty, disagreement, geopolitical risk, and political sentiment. A one standard-deviation increase in partisan conflict is associated with a 0.54% increase in next month market return. The forecasting power is symmetric across political cycles and operates via a discount rate channel. Increased partisan conflict is associated with increased fiscal policy and healthcare policy uncertainties, and leads investors to switch their investments from equities to bonds.

3.1 Introduction

Partisan conflict has been one dominant theme in U.S. politics in recent years. According to the survey conducted by Pew Research Center in 2017, the gap between the political values of Republicans and Democrats is now larger than any point dating back to 1994. On top of that, in terms of another survey conducted by the Business Roundtable in 2013, half of all chief executive officers (CEOs) claim that political conflict within the federal government over the upcoming budget negotiations and the looming debt ceiling crisis is likely to have an adverse effect

[1]This is a joint work with Dashan Huang

on their short-term hiring decisions, suggesting that partisan conflict may have a side-effect on the real economy.

This chapter investigates the asset pricing implications of partisan conflict, with a focus on its predictability for future U.S. stock market returns. The rise in partisan conflict has been widely discussed by commentators (e.g., Krugman, 2004) and scholars (e.g., McCarty, Poole, and Rosenthal, 2006), because partisan conflict has substantive policy consequences, associated with increased levels of political gridlock (Jones, 2001), implying much reduced rates of policy innovation and a decreased ability to adapt to changes in economic, social, or demographic circumstances (McCarty, 2007). However, it is an open empirical question whether partisan conflict has a real effect on the stock market.

To answer our research question, we use a recently proposed partisan conflict index from Azzimonti (2018) to track the degree of political conflict among U.S. politicians at the federal level, by measuring the frequency of newspaper articles reporting disagreement in a given month. Naturally, higher index values indicate greater conflict among political parties, Congress, and the President. By focusing on partisan conflict about government policy, both within and between national parties, Azzimonti (2018) shows that the increases in partisan conflict are associated with presidential elections and well-known fiscal policy debates, such as the debt ceiling debate and debates on the Affordable Care Act (both related to its approval and potential repeal in early 2017).

We find that the partisan conflict index significantly and positively predicts future market returns. Over the sample period from January 1981 to December 2018, a one-standard deviation increase in partisan conflict is associated with a 0.54% increase in the next one month expected market return. This predictive power remains after controlling for the well-know economic predictors (Welch and Goyal, 2008), uncertainty measures (Baker, Bloom, and Davis, 2016), disagreement measures (Bali, Brown, and Tang, 2017), political sentiment (Addoum and Kumar, 2016), and geopolitical risk (Caldara and Iacoviello, 2018).

With a battery of robustness tests, we show that partisan conflict 1) has stronger predictive power for industry portfolios in chemicals, durables, construction, retail, and finance, 2) is able to predict big firms' returns rather than small firms, and 3) predicts the U.K. stock market returns, whereas the counterpart is unable to predict the U.S. market returns.

In Azzimonti (2018), an article is considered about partisan conflict if it covers a keyword in the political disagreement dictionary and a keyword in the government dictionary. To show that our index does measure disagreement about government policy, we construct two alternative indexes – one based on the political disagreement dictionary and the other based on the government dictionary – and find that they have a correlation of 0.65. When predicting future market returns, both indexes significantly predict future market returns and the index based on the political disagreement dictionary displays slightly stronger power, suggesting that the predictability is resultant from political disagreement on government policy, rather than disagreement about something else.

The predictability of partisan conflict is symmetric over political cycles and is different from the presidential puzzle. In an influential paper, Santa-Clara and Valkanov (2003) show that stock returns are much higher under Democratic presidents than under Republican ones, which continues to hold in an out-of-sample assessment (Pastor and Veronesi, 2019). By using a dummy that equals one if the president is affiliated with the Democratic party or the majority of House/Senate is Democrats, we find that the prediction of partisan conflict is independent of political cycles, suggesting that our result cannot be explained by interpretations to political cycles.

As a proxy for political uncertainty, one natural question is which type of uncertainty is more related to partisan conflict. By regressing the change in partisan conflict on the change in EPU or the change in one of the 11 EPU components, we find that partisan conflict is positively related to fiscal policy uncertainty, tax policy uncertainty, government spending policy uncertainty, healthcare policy uncertainty, entitlement program policy uncertainty, and regulation policy uncertainty. When using an elastic net regression to include all the uncertainty measures as independent variables, we find that fiscal policy uncertainty and healthcare policy uncertainty are positively correlated with partisan conflict, suggesting that partisan conflict does play a role for the real economy.

To explore economic implications of partisan conflict, we perform four tests. First, the predictability of partisan conflict operates via a discount rate channel. By using the decomposition of Campbell et al. (2018), we find that partisan conflict can only predict discount rates, rather than cash flows and variance shocks. Moreover, partisan conflict cannot predict economic activities, such as industrial production, consumption, unemployment, and investment. Our interpretation is that the government implements policies to affect the environment in which firm operate. When political parties are polarized and the government is divided, partisan conflict is elevated and the quality of policies opted is lower. Thus, partisan conflict exacerbates economic risk by increasing the level of uncertainty and dampens stock prices. When the uncertainty is resolved, stock prices will go up, suggesting a positive relation between partisan conflict and future stock returns.

Second, investors do pay attention to partisan conflict. We collect search interest data from Google trends for each keyword in the partisan conflict dictionary and construct an equally-weighted search interest index on partisan conflict. Contemporaneously, we find that the search interest index is positively associated with partisan conflict, with a correlation of 0.16. With regression analysis, we find that a one standard deviation increase in partisan conflict is associated with a 15% increase in search interest, suggesting that increased partisan conflict attracts more attention from investors.

Third, partisan conflict is strongly associated with the US presidential approval rates. In the political science literature, public assessments of presidential job are shown to be relevant for policy outcomes and help capture the aggregate public preferences. According to Liu and Shaliastovich (2018), approval rates are weakly related to current and past economic conditions. In this chapter, we find that high partisan conflict predicts a persistent decrease in future US presidential approval rates, suggesting that economic agents rationally incorporate information about political conflict in their assessments of government policies.

Finally, we show that investors respond to partisan conflict by switching their investments from equities to bonds. Following Ben-Rephael, Kandel, and Whol (2012), by using the Investment Company Institute (ICI) data of aggregate flows to US mutual funds, we find that partisan conflict negatively predict net mutual fund flows into equities and positively predict net mutual fund flows to bonds. Therefore, increased partisan conflict makes investors more conservative and flight to safety.

The rest of this chapter is organized as follows. Section 3.2 presents some basic facts about U.S. partisan conflict, which are the motivation of this chapter. Section 3.3 shows that the Azzimonti (2018) partisan conflict index positively predicts market returns and is robust with different set of controls. Section 3.4 explores an economic channel by showing that partisan conflict is positively related with uncertainty and that investors make more conservative investments when partisan conflict increases, thereby positively predicting market returns. Section 3.5 concludes.

3.2 U.S. Partisan Conflict

3.2.1 Evidence from surveys

The conflicts between Republicans and Democrats on fundamental political values—on government, race, immigration, national security, environmental protection and other areas—reached record levels during Obama's presidency. Republicans talk about "death taxes", "illegal aliens", and "tax reform", whereas Democrats refer to "estate taxes", "undocumented workers", and "tax breaks for the wealthy".

Based on surveys of more than 5,000 adults conducted over the summer of 2017, Pew Research Center finds widening differences between Republicans and

Democrats on a range of measures the Center has been asking about since 1994. Figures 3.1 and 3.2 show that the gap between the political values of Republicans and Democrats is now larger than at any point dating back to 1994, a continuation of a steep increases in the ideological divisions between the two parties over more than two decades. For example, in 2017, the median (middle) Republican is now more conservative than 97% of Democrats, and the median Democrat is more liberal than 95% of Republicans. By comparison, in 1994, the two corresponding numbers are only 70% and 64%, respectively. That is, 64% of Republicans are to the right of the median Democrat, while 70% of Democrats are to the left of the median Republican.

3.2.2 Partisan conflict index

Azzimonti (2018) constructs a partisan conflict index by using a semantic search approach to measure the frequency of newspaper coverage of articles reporting political disagreement about government policy both within and between national parties normalized by the total number of news articles to average 100 in 1990. For self-contained, in Online Appendix we present the newspaper coverage and the set of words used by Azzimonti (2018) in constructing the index.

Figure 3.3 plots the monthly partisan conflict index over the period from January 1981 to December 2018. As expected, the rise of this index accelerates with partisan debates, such as Obamacare and debt ceiling, and peaks around the 2013 government shutdown and the 2016 Trump-Clinton president election. The index also dramatically shrinks on some remarkable political and military incidences, such as the 1987 Beirut Bombing, 1990 Gulf War, and 2001 9/11. The partisan conflict remains relatively stable from 1981 to the late 2009, but appears to display an upward trend thereafter, during Obama's presidency and it has grown even larger in Donald Trump's first year as president. To ensure it to be stationary in use, Figure 3.3 also plots the log linear detrended series, which will be used throughout the chapter. In Section 3.3.1, we show that our results remain the same when alternative detrending methods are used.

3.3 Partisan Conflict and Return Predictability

3.3.1 Forecasting market returns

The market return to be predicted is the continuously compounded log return of the S&P 500 index in excess of the risk-free rate. To formally test this conjecture, we estimate variants of the following standard predictive regression:

$$R_{t,t+h} = \alpha + \beta \text{Partisan conflict}_t + \varepsilon_{t+1}, \tag{3.1}$$

where $R_{t,t+h}$ is cumulative market return from month t to $t+h$.

Panel A of Table 3.1 presents the major results of this chapter. For easy of exposition, we normalize partisan conflict for in-sample prediction so that the regression slope measures the variation of next month expected market return in response to one standard deviation increase in partisan conflict. Apparently, partisan conflict has substantial forecasting power for future market returns. In the first row, a one standard deviation increase in partisan conflict in month t is positively associated with a 0.54% increase in expected market return in month $t+1$.

In addition to in-sample forecasting, Table 3.1 also reports the out-of-sample performance. We use Campbell and Thompson (2008) out-of-sample R^2 statistic as the out-of-sample performance evaluation criterion. In this chapter, we use the data from 1981:01 to 1999:12 as in-sample training and the rest starting from 2000:01 as out-of-sample evaluation. Following Welch and Goyal (2008) and Campbell and Thompson (2008), we recursively estimate the expected market returns using the expanding window approach to reduce estimation error. The last column of Table 3.1 shows that the predictive ability remains significant with R^2_{OS} of 1.75% under one-month horizon.[2] We also extend the horizon to 3, 6, and 12 months, and find

[2]It should be mentioned that, although the out-of-sample R^2_{OS} can be negative and is usually smaller than the in-sample R^2, theoretically they do not have a strict relationship (Campbell and

that partisan conflict successfully predicts the stock market up to one year.

To ensure that our results are not driven by the specific log linear detrending approach, Table 3.1 also explores the predictive ability of partisan conflict without detrending, with log quadratic detrending and with stochastic detrending. Following Campbell (1991) and Rapach, Ringgenberg, and Zhou (2016), stochastic detrending is based on a five-year window and the detrended partisan conflict in month t is the difference between the raw partisan conflict in month t minus its average from month t-59 to month t. The results show that the predictive power of partisan conflict is robust to different detrending methods and at all horizons.

3.3.2 Controlling for economic predictors, disagreement and uncertainties

In this subsection, we show that the predictive power of partisan conflict is not subsumed by extant economic predictors, disagreement measures or uncertainty measures. Firstly, we include the 14 known return predictors one by one as a control variable to explore whether the partisan conflict index has incremental forecasting power.

$$R_{t,t+1} = \alpha + \beta \text{Partisan conflict}_t + \psi Z_t + \varepsilon_{t+1}, \tag{3.2}$$

where Z_t is one of the well-known return predictors in Welch and Goyal (2008), including dividend-price ratio, dividend yield, earnings-price ratio, dividend-payout ratio, book-to-market ratio, net equity expansion, treasury bill rate, long-term bond yield, long-term bond return, term spread, default yield spread, default return spread, inflation rate, and stock sample variance.

Panel A of Table 3.2 presents the regression results. The regression slope on partisan conflict is virtually unchanged from its value of Table 3.1. Consistent with Welch and Goyal (2008), none of the 14 known predictors is significant. Thus, the predictive power of partisan conflict for future market returns appears to be

Thompson, 2008; Welch and Goyal, 2008). One reason is that they are based on different sample periods with different econometric criteria.

somewhat more robust than known return predictors, at least in our sample period.

As partisan conflict is one type of disagreement, which raises a concern as to whether its predictive power is subsumed by traditional macroeconomic disagreement measures (Anderson, Ghysels, and Juergens, 2005; Atmaz and Basak, 2018; Banerjee, 2011). To differentiate partisan conflict from macroeconomic disagreement, we construct ten measures based on the Blue Chip Economic Indicator surveys, including disagreement on gross domestic product, consumer price index, 3-month treasury bill rate, unemployment rate, industrial production, disposable personal income, non-residential fixed investment, housing starts, 10-year treasury bond rate, and personal consumption expenditure, respectively. Each disagreement measure is computed as the standard deviation of economists forecasts on these macroeconomic variables.

Panel B of Table 3.2 reports the results from the bivariate regression (3.2) by replacing Z_t with a macroeconomic disagreement measure. The results show that the predictive ability of partisan conflict remains the same as the standalone case, while none of the macroeconomic disagreement measures has any predictive power.

As argued by Azzimonti (2018), high levels of partisan conflict are interpreted as situations where agreement between the two parties is hard to reach, so policies are expected to be less effective at preventing recessions and tail risks. Moderate levels of partisan conflict should be associated with positive economic policy uncertainty, as investors cannot predict which policies will be undertaken. Examples are the debt ceiling debate (will the government change taxes to avoid a fiscal cliff?), the passage of Obamacare (will Congress modify the health care system effectively, or will this result in an explosion of public debt?), or the uncertainty associated with tax expirations (will tax cuts expire or will the two parties agree on further extensions?) In these situations, partisan conflict should be correlated with macroeconomic risk and uncertainty, such as the EPU in (Baker, Bloom, and Davis, 2016).

To reduce the concern that the information embedded in partisan conflict largely overlaps with macroeconomic uncertainty, we consider the bivariate regression (3.2) by replacing Z_t with a proxy for economic policy uncertainty. For comprehensive, EPU and its 11 categorical uncertainty measures. Panel C of Table 3.2 shows that the predictive power of partisan conflict is not affected by any of the uncertainty measures, and its regression slope is similar to, or even larger in some cases than, the univariate case in which partisan conflict is used alone. It is also interesting that all of economic policy uncertainty proxies are not statistically significant in predicting future market returns, with national security uncertainty as an exception.

In Panel D of Table 3.2, we pool all the economic predictors, uncertainty measures, and disagreement measures together and run a kitchen sink regression. The results show that the predictive power of partisan conflict is unchanged from the standalone case, which further establish the predictability of partisan conflict on stock market return.

3.3.3 Controlling for political sentiment and geopolitical risk

Recent literature shows that changes in political climate influence stock prices. For instance, Addoum and Kumar (2016) argue that investor demand can be shifted if there is a change of the majority party. To offer a clear comparison between political sentiment and partisan conflict, we construct political-sensitivity portfolios following Addoum and Kumar (2016) and construct a political sentiment index as the return differential between high and low political-sensitivity portfolios. Figure 3.4 provides a graphical illustration on the difference between partisan conflict and political sentiment. Clearly, these two indexes capture different information, with a correlation of -0.12.

According to Caldara and Iacoviello (2018), geopolitical risk refers to the risk associated with wars, terrorist acts, and tensions between states that affect the normal and peaceful course of international relations, and is constructed by counting the frequency of articles related to geopolitical risks in leading international newspapers published in the U.S., the United Kingdom, and Canada. Figure 3.5 offers an intuitive comparison between partisan conflict and geopolitical risk.

Obviously, the two indexes capture different aspects of the U.S. economy and society with a correlation of 0.12.

To formally examine whether the predictive power of partisan conflict can be subsumed by political sentiment or geopolitical risk, we run the following bivariate regression,

$$R_{t,t+h} = \alpha + \beta \text{Partisan conflict}_t + \psi Z_t + \varepsilon_{t+1}, \tag{3.3}$$

where Z_t is either political sentiment, geopolitical risk or both.

Table 3.3 presents the results of predicting market returns with partisan conflict after controlling for political sentiment and geopolitical risk. When used as a standalone predictor, a one-standard deviation increase in political sentiment suggests a 0.40% decrease in the next one month expected market return. This magnitude is statistically significant and economically sizeable. When partisan conflict is included, the regression slope of political sentiment remains -0.34% significant. Similarly, the slope on partisan conflict is still as large as 0.50%. Similarly, controlling for geopolitical risk does not significantly influence the predictive power of partisan conflict. In sum, partisan conflict, political sentiment and geopolitical risk capture different aspects of the U.S. politics and deserve separate examination.

3.3.4 Forecasting industry and size portfolio returns

Belo, Gala, and Li (2013) show that industry government spending exposure reflects predictable variations in cash flow and stock returns over political cycles. Firms with higher (lower) government spending exposure experience higher (lower) cash flows and stock returns during Democratic (Republican) presidencies. In addition, Addoum and Kumar (2016) show that shifts in political climate generate predictable patterns in industry returns, which are more pronounced at the aggregate level than at firm level. In this section, we investigate how the predictive power of partisan conflict varies across industry portfolios. Specifically, we consider the following predictive regression:

$$R^i_{t+1} = \alpha + \beta \text{Partisan conflict}_t + \varepsilon_{t+1}, \tag{3.4}$$

where R^i_{t+1} is the excess return of one of the 17 industry portfolios in Fama and French (1997). Results in panel A of Table 3.4 show that partisan conflict significantly predicts eight industries both in-sample and out-of-sample, including Durbl (Consumer durables), Chems (Chemicals), Cnsum (Consumer goods), Cnstr (Construction and Construction Materials), FabPr (Fabricated Products), Retail, Finan (Finance) and Other. Apparently, most of these sectors are heavily regulated by the government and therefore related to the variations of partisan conflict.

We proceed to investigate how the predictive power of partisan conflict varies across size portfolios. Specifically, we consider the following predictive regression:

$$R^i_{t+1} = \alpha + \beta \text{Partisan conflict}_t + \varepsilon_{t+1}, \tag{3.5}$$

where R^i_{t+1} is the excess return of one of the 5 size portfolios in Fama and French (1993). Results in panel B of Table 3.4 show that partisan conflict significantly predicts the returns of three large size portfolios. Especially for the firms with highest market value, a one standard deviation increase in the level of partisan conflict predicts a 0.54% step-up in expected returns. The predictive power is significant at 1% level. Obviously, large firms are more sensitive to changes in government policy and also they are heavily regulated by the government, which implies variations of partisan conflict could have more pronounced impacts.

3.3.5 Partisan conflict for United Kingdom

To construct the partisan conflict index for United Kingdom, we use a search-based approach which measures the frequency of newspaper articles reporting political disagreement in United Kingdom. We perform the monthly search in Factiva, covering 11 major U.K. newspapers. Following Azzimonti (2018), we search for articles containing at least one keyword in the following two categories: (1) political disagreement and (2) government. The keywords related to political disagreement, such as standstill and gridlock, are retained as Azzimonti (2018). We adjust the keywords on government to better fit the political environment of United Kingdom. The set of words used in Factiva search query is listed in Online Appendix. We focus on articles including keywords at the intersection of those two categories. On top of that, we also include some specific terms related to political disagreement, such as “divided party”, “partisan divisions”, and “divided Congress”. The search is restricted with news written exclusively in English and events occurring in, or related to, the United Kingdom.

Due to the scarce availability of newspaper sources at the beginning periods, we skip the first several months, and start the sample period from 1998. Since the volume of digitized news varies over time, we scale the raw count by the total number of articles in the same newspaper over the same time interval. Finally, we normalize the scaled index to average 100 in 2000.

Figure 3.6 plots the monthly partisan conflict index for United Kingdom from January 1998 to December 2018. The index successfully captures nation-wide political debates, such as Mays election and Brexit referendum . It peaks in 2003 Iraq war, when there was severe partisan conflict between the two parties. The vertical lines indicate months in which general elections are held. Obviously from Figure 3.6, the index spikes during elections, which is as expected since newspapers increases the proportion of articles covering political debates during those periods. Over the 1998 - 2018 sample period, the U.K. partisan conflict shows a slightly downward trend and an upward trend around 2005. As a result, we use the quadratic detrended series to remove the potential time trend.

To further examine whether UK’s partisan conflict could influence its stock market or whether there is any spillover effect of partisan conflict between the U.K.

and the U.S., we estimate the standard predictive regression:

$$R_{t,t+1} = \alpha + \beta_1 \text{Partisan conflict}_t^{UK} + \beta_2 \text{Partisan conflict}_t^{US} + \varepsilon_{t,t+1}, \tag{3.6}$$

The market return for UK is computed based on UK equity market index from MSCI via Datastream.

The first row of Table 3.5 show that the U.K. partisan conflict has forecasting power for future the U.K. market returns. For instance, a one standard deviation increase in partisan conflict in month t is positively associated with a 0.48% increase in expected market return in month t+1. Interestingly, the U.S. partisan conflict index can also predict the U.K. market return, and it subsumes the U.K. conflict index when including the two partisan conflict together.

3.3.6 Alternative partisan conflict indexes

In constructing the partisan conflict index, Azzimonti (2018) considers an article about political conflict if it covers at least one word from the political disagreement dictionary and one word from the government dictionary. To show that our index does measure disagreement about government policy, we construct two alternative indexes one based on the political disagreement dictionary and the other based on the government dictionary and find that they have a correlation of 0.65.

Table 3.6 reports the results. When predicting future market returns, both indexes significantly predict future market returns and the index based on the political disagreement dictionary displays slightly stronger power, suggesting that the predictability is resultant from political disagreement on government policy, rather than disagreement about something else

3.3.7 Forecasting the market over different political regimes

Santa-Clara and Valkanov (2003) and Pastor and Veronesi (2018) show that market returns exhibit a striking pattern: they are much higher under Democratic presidents than under Republican ones. In this section, we examine whether the predictive power of partisan conflict differs over different political regimes. Specifically, we predict the market return with a state-dependent regression as

$$\begin{aligned} R_{t+1} = \alpha + \beta_1 \text{Partisan conflict}_t + \beta_2 \text{Democratic}_t \times \text{Partisan conflict}_t \\ + \beta_3 \text{Democratic}_t + \varepsilon_{t+1} \end{aligned} \tag{3.7}$$

where Democratic is a dummy variable that equals one if the president is affiliated with Democratic party or the majority of House/Senate is Democrats, and zero otherwise. Results in Table 3.7 show that the predictive power of partisan conflict is symmetric over political cycles, independent of which party is in power. This result suggests that our finding cannot be explained by those proposed for the presidential puzzle.

3.3.8 Relationship with uncertainty

As a proxy for political uncertainty, one natural question is which type of uncertainty is more related to partisan conflict. Table 3.8 regresses the change in partisan conflict on the change in EPU or the change in one of the 11 EPU components. The results show that partisan conflict is positively related to fiscal policy uncertainty, tax policy uncertainty, government spending policy uncertainty, healthcare policy uncertainty, entitlement program policy uncertainty, and regulation policy uncertainty. When using an elastic net regression to include all the uncertainty measures as independent variables, we find that fiscal policy uncertainty and healthcare policy uncertainty are positively correlated with partisan conflict, suggesting that partisan conflict does play a role for the real economy.

3.4 Economic Implications

3.4.1 Cash flow shocks versus discount rate shocks

Campbell et al. (2018) show that if a variable predicts market return, it must predict the cash flow shock, discount rate shock, variance shock or all of the three. We consider the following regression:

$$y_{q+1} = \alpha + \beta \text{Partisan conflict}_q + \psi \text{P/E}_q + \varepsilon_{q+1}, \tag{3.8}$$

where the dependent variable is future discount rate shock, cash flow shock, or variance shocks, and P/E is the price earnings ratio.[3] We take the average of the partisan conflict index within one quarter as the quarterly partisan conflict measure. Panel A of Table 3.9 reports the results of predicting the three shocks with partisan conflict. The results show that partisan conflict positively predicts future discount rate shocks, rather than cash flow shocks or variance shocks, which is consistent with Table 3.8 that increased partisan conflict increases uncertainty and as a consequence, investors require a higher risk premium for taking more risk.

If partisan conflict cannot predict future cash flow and variance shocks, it will not be able to predict future economic activities. To test this hypothesis, we forecast economic activities with partisan conflict as

$$y_{t+1} = \alpha + \beta \text{Partisan conflict}_t + \sum_{i=1}^{12} \lambda_i y_{t-i+1} + \varepsilon_{t+1} \tag{3.9}$$

for monthly data, and

$$y_{q+1} = \alpha + \beta \text{Partisan conflict}_t + \sum_{i=1}^{4} \lambda_i y_{q-i+1} + \varepsilon_{q+1} \tag{3.10}$$

for quarterly data. We consider eight proxies for economic activities, including the Chicago fed national activity index (CFNAI), industrial production growth,

[3]We thank Christopher Polk for providing these data on his webpage.

real personal consumption expenditure (consumption), unemployment rate, private gross domestic investment (investment), real GDP growth, business inventory, and capacity utilization (Greenwood, Hercowitz, and Huffman, 1988). In the regressions, these economic variables are adjusted for seasonality and annualized for ease of exposition. Except for the gross private domestic investment and real GDP growth, all of them are measured at monthly frequency.

Panel B of Table 3.9 shows that partisan conflict cannot predict any of the eight economic activity measures. This result is not necessarily inconsistent with Azzimonti (2018), who shows that, to some extent, partisan conflict represents one type of uncertainty and negatively predicts future firm investments. The main difference is that we focus on the aggregate market level analysis, while Azzimonti (2018) considers the firm level analysis. Since firms' investments are highly heterogenous (Clementi and Palazzo, 2018), the firm level pattern does not necessarily hold at the aggregate level.

3.4.2 Do investors pay attention to partisan conflict?

The partisan conflict index is constructed by counting the number of articles related to political disagreement published in widely-circulated newspapers. It reflects the opinions from a relatively small group of professionals with sophisticated knowledge. One natural question is whether investors pay attention to partisan conflict. To address this issue, we collect the search interest index data for each keyword used in Azzimonti (2018) from Google trends and construct an equally-weighted search interest index on partisan conflict. To formally test whether partisan conflict is correlated with public's search interest, we run the regression as follows:

$$\begin{aligned}\Delta\text{Attention}_t =&\alpha+\beta\Delta\text{Partisan conflict}_t+\psi X_t+\sum_{i=1}^{4}\lambda_i\Delta\text{Attention}_{t-i}\\&+\sum_{i=1}^{4}\lambda_i\Delta\text{Partisan conflict}_{t-i}+\varepsilon_t,\end{aligned}\tag{3.11}$$

where Attention refers to the search interest index constructed as above. X includes control variables following Lian, Ma, and Wang (2018). Specifically, we control the Campbell-Shiller price-earnings ratio (P/E10), the past 12-month excess stock market return, VIX^2 (the square of VIX, which measures the expected variance of S&P500 index), real GDP growth, the credit spread (Gilchrist and Zakrajšek, 2012), and surplus consumption ratio.

Table 3.10 shows that partisan conflict is positively correlated with search attention. Consider column (3) with the most comprehensive control variables, one standard deviation increase in partisan conflict leads to 14.12% increase in search attention. This demonstrates that investors do pay attention to intensified partisan conflict.

3.4.3 Partisan conflict and presidential approval rates

This subsection shows that partisan conflict is strongly related with the US presidential approval rates. In the political science literature, public assessments of presidential job are shown to be relevant for policy outcomes and help capture the aggregate public preferences. According to Liu and Shaliastovich (2018), approval rates are weakly related to current and past economic conditions. To formally examine the relationship between partisan conflict and presidential approval rates, we run the regression as follows:

$$\begin{aligned}\Delta\text{Approval}_t = & \alpha + \beta\Delta\text{Partisan conflict}_t + \psi X_t + \sum_{i=1}^{4}\lambda_i\Delta\text{Approval}_{t-i} \\ & + \sum_{i=1}^{4}\lambda_i\Delta\text{Partisan conflict}_{t-i} + \varepsilon_t,\end{aligned} \tag{3.12}$$

where Approval is the U.S. presidential approval rates from Gallup Analytics. X includes control variables following Lian, Ma, and Wang (2018). Specifically, we control the Campbell-Shiller price-earnings ratio (P/E10), the past 12-month excess stock return, VIX^2 (the square of VIX, which measures the expected variance of S&P500 index), real GDP growth, the credit spread (Gilchrist and Zakrajšek, 2012), and surplus consumption ratio.

Table 3.11 reports the results of predicting the presidential approval rates with partisan conflict. The results show that high partisan conflict predicts a persistent decrease in future US presidential approval rates, suggesting that economic agents rationally incorporate information about partisan conflict in their assessments of government policies.

3.4.4 Do investors respond to partisan conflict?

If investors pay attention to partisan conflict, do they adjust their investments accordingly? To answer this question, we offer some evidences from observational data. Investment Company Institute (ICI) provides the data on mutual fund flows and total net asset at monthly frequency. The flow data of bonds (stocks) are divided into four categories: "exchanges in", "exchanges out", "sales", and "redemptions". We focus on the first two categories which are "exchanges in" and "exchanges out" since they are transfers between different funds in the same fund family. The "net exchanges" of flows into bonds (stocks) are defined as "exchanges in" of bonds (stocks) minus the "exchanges out" of the same group. As Ben-Rephael, Kandel, and Whol (2012), we focus on the relation between "net exchanges" and partisan conflict since "net exchanges" reflect asset allocation decisions of mutual fund investors on shifting between bonds and equity. We compute the quarterly net exchange of mutual fund flows into bonds (stocks) at quarter q normalized by the total net asset at quarter q -1. In terms of Lian, Ma, and Wang (2018), we control the Campbell-Shiller price-earnings ratio (P/E10), the past 12-month excess stock return, VIX^2 (the square of VIX, which measures the expected variance of S&P500 index), real GDP growth, and the credit spread (Gilchrist and Zakrajšek, 2012). We take the average of partisan conflict index within one quarter as the quarterly partisan conflict measure and run the following regression:

$$MF_q = \alpha + \beta \Delta \text{Partisan conflict}_{q-1} + X'_{q-1}\psi + \sum_{i=1}^{4} \lambda_i MF_{q-i} + \varepsilon_q \tag{3.13}$$

where MF_q denotes the net exchange of mutual fund flows into bonds or stocks. X includes all the control variables which are lagged by one period. All regressions include four lags of MF_q in case that net exchange of mutual fund flows are persistent over time.

Results in Table 3.12 show that with controls on macroeconomic variables, change in partisan conflict negatively (positively) predicts the net exchange into stocks (bonds) for the next quarter. Such evidence from mutual fund flows further verifies our conjecture that when the two parties fail to compromise with each other, investors may feel uncertain about what policy will be implemented in the future. As a result, they may shift their demands from equity to bonds as to at least guarantee their payoff under high level of uncertainty.

3.5 Conclusion

Using the textual index from Azzimonti (2018), this chapter shows that partisan conflict positively predicts market returns, controlling for economic predictors and proxies for uncertainty, measures of disagreements, political sentiment and geopolitical risk. A one standard-deviation increase in partisan conflict is associated with a 0.54% increase in next month market return. The forecasting power is symmetric over political cycles and operates via a discount rate channel. Increased partisan conflict is associated with increased fiscal policy and healthcare policy uncertainties. Investors do pay attention to intensified partisan conflict and switch investments from equities to bonds.

There are a number of subjects that are of interest for future research. First, while we focus on the stock market, it is interesting to examine the predictability of partisan conflict in other markets. Second, while it is beyond the scope of this chapter, it would be interesting to explore the effect of partisan conflict in a general equilibrium model. Finally, what drives the movements of partisan conflict deserves further research.

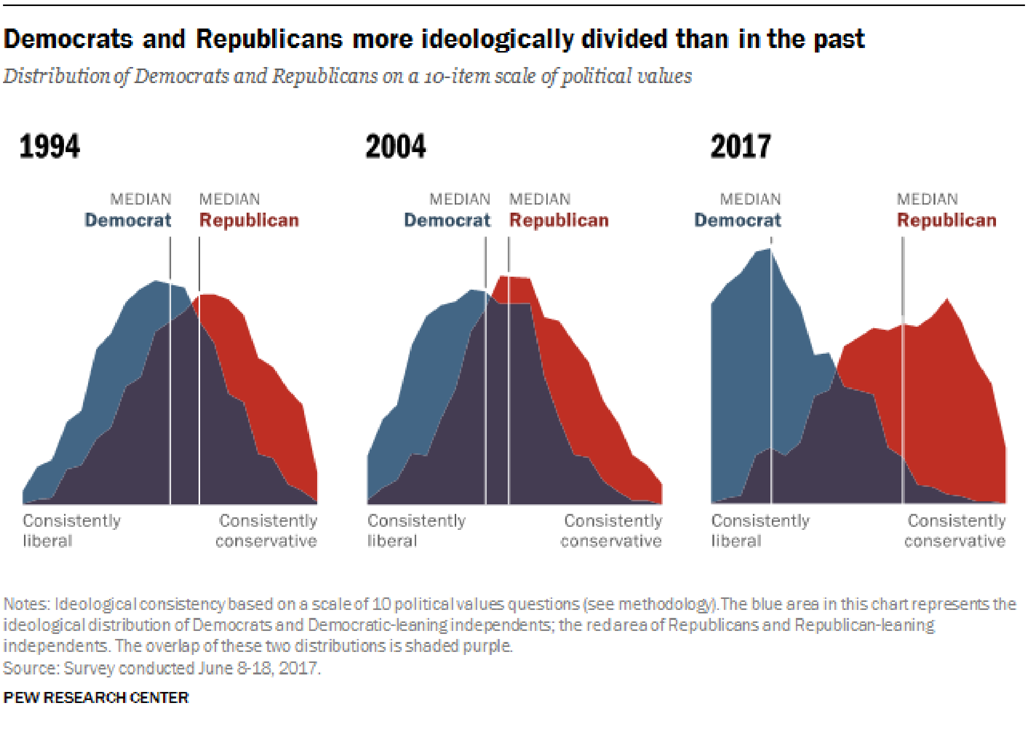

Fig. 3.1 Political polarization in the American public (2017, Pew Research Center)

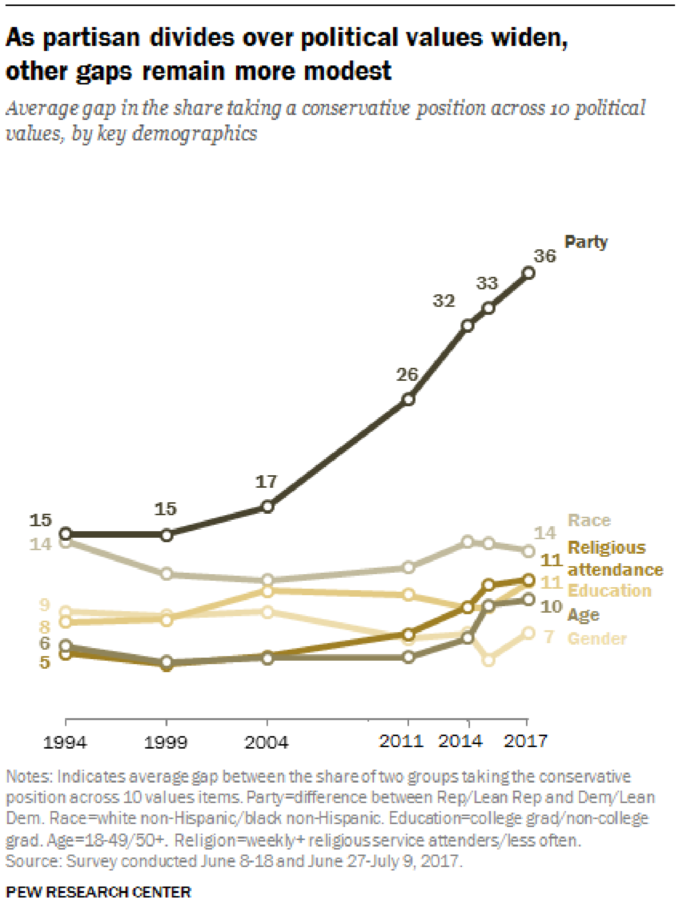

Fig. 3.2 Political polarization vs. other gaps in the American public (2017, Pew Research Center)

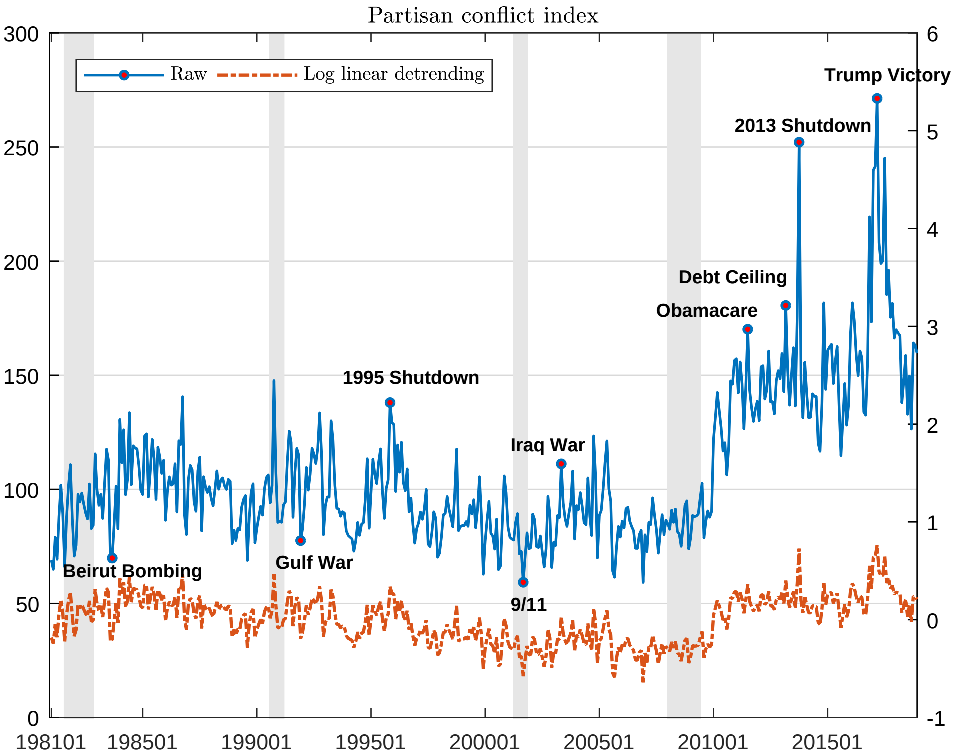

Fig. 3.3 Partisan conflict index

This figure plots the partisan conflict index from Azzimonti (2018). The sample period is 1981:01–2018:12

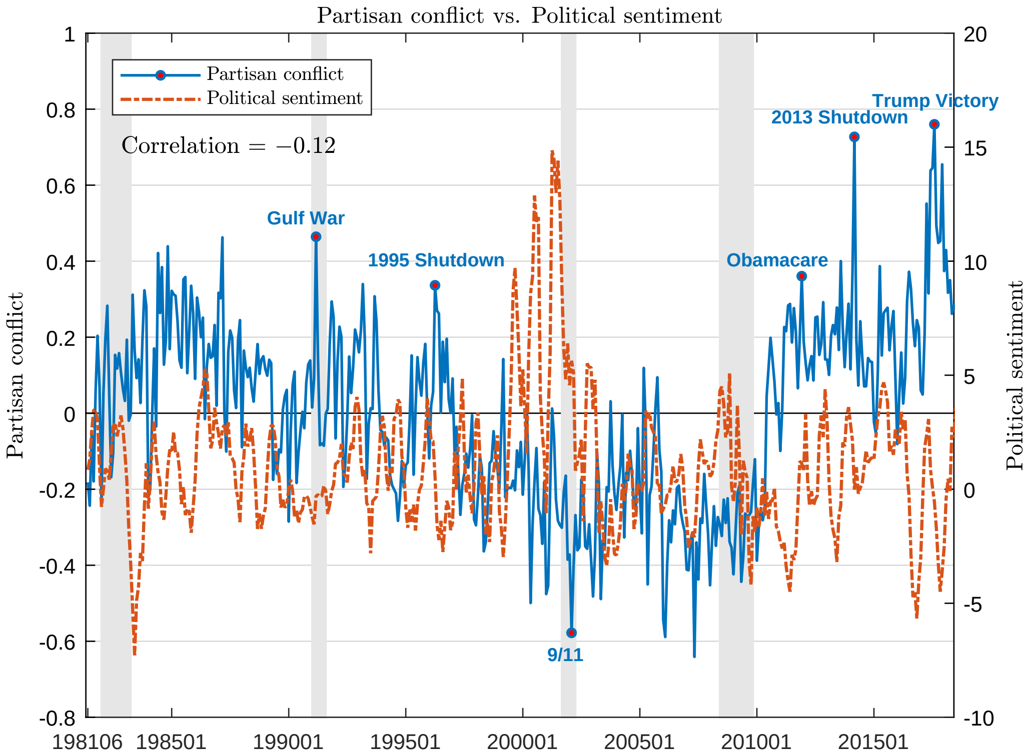

Fig. 3.4 Partisan conflict vs. political sentiment

This figure plots the partisan conflict and political sentiment indexes, where the former is from Azzimonti (2018) and the latter is measured as the return differential between high and low political-sentiment portfolios following Addoum and Kumar (2016), which is smoothed by the 6-month moving averages to iron out idiosyncratic noises. The sample period is 1981:01–2018:12.

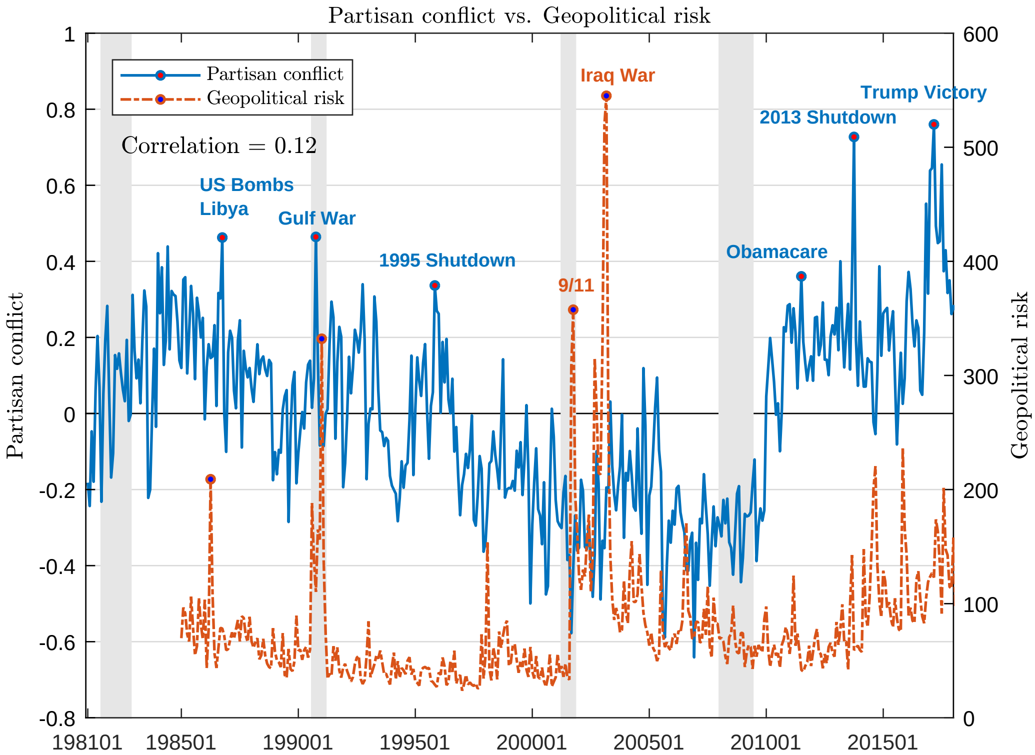

Fig. 3.5 Partisan conflict vs. geopolitical risk

This figure plots the partisan conflict and geopolitical risk indexes, where the former is from Azzimonti (2018) and the latter is from Caldara and Iacoviello (2018). The sample period is 1981:01–2018:12 for partisan conflict index and 1985:01–2018:12 for geopolitical risk index.

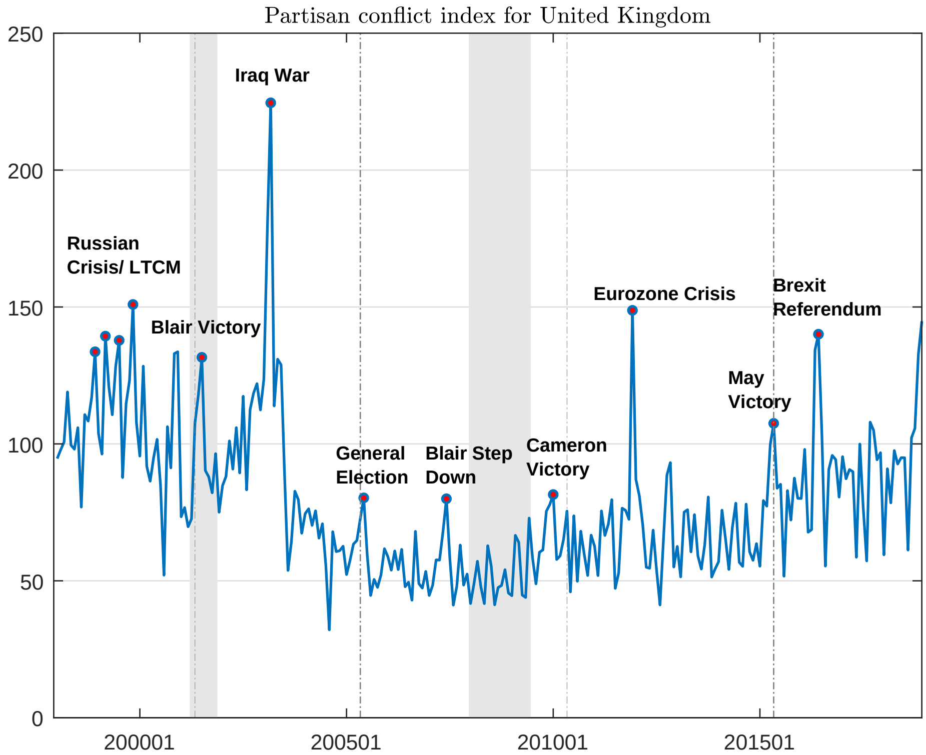

Fig. 3.6 Partisan conflict index for United Kingdom

This figure plots the partisan conflict index for United Kingdom. We follow the semantic search approach in Azzimonti (2018), and the key words used are listed in Online Appendix. The sample period is 1998:01–2018:12

Table 3.1 Forecasting market returns with different horizons and alternative detrending methods

This table presents the results of predicting market returns with partisan conflict index as:

$$R_{t,t+h} = \alpha + \beta \text{Partisan conflict}_t + \varepsilon_{t,t+h},$$

where $R_{t,t+h}$ is the cumulative market return between months t and $t+h$ ($h = 1, 3, 6, 12$). The partisan conflict index is from Azzimonti (2018). We consider the raw partisan conflict index without detrending and with log linear detrending, log quadratic detrending and stochastic detrending approach. Reported are regression slope, t-value, in-sample R^2, and out-of-sample R^2_{OS}. Statistical significance for R^2_{OS} is based on the p-value of the Clark and West (2007) MSFE-adjusted statistic for testing: $H_0 : R^2_{OS} \leqslant 0$ against $H_A : R^2_{OS} > 0$. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. The in-sample period is 1981:01–2018:12 and the out-of-sample period is 2000:01–2018:12.

Horizon	β	t-value	R^2	R^2_{OS}
Panel A: Log linear detrending				
$h = 1$	0.54***	2.70	1.56	1.75***
$h = 3$	1.07**	2.18	1.99	2.02***
$h = 6$	2.62***	2.75	5.53	3.86***
$h = 12$	5.14***	2.79	9.55	6.79***
Panel B: No detrending				
$h = 1$	0.42***	2.63	0.96	1.21***
$h = 3$	0.90**	2.44	1.41	1.97***
$h = 6$	2.14***	2.91	3.70	3.99***
$h = 12$	4.09***	2.63	6.03	7.84***
Panel C: Log quadratic detrending				
$h = 1$	0.64***	3.28	2.26	1.24**
$h = 3$	1.22**	2.55	2.58	0.76*
$h = 6$	2.90***	3.14	6.81	−0.79
$h = 12$	5.27***	3.11	10.02	0.30*
Panel D: Stochastic detrending				
$h = 1$	0.48**	2.59	1.21	1.59***
$h = 3$	0.77*	1.91	1.05	1.17**
$h = 6$	1.83***	2.65	2.86	3.20***
$h = 12$	3.58***	2.79	4.98	6.08***

Table 3.2 Controlling for economic predictors, disagreements, and uncertainties

This table reports the results of predicting market returns with partisan conflict index as

$$R_{t+1} = \alpha + \beta \text{Partisan conflict}_t + \psi Z_t + \varepsilon_{t+1},$$

where Z_t includes the economic predictors in Welch and Goyal (2008), economic disagreement measures, and uncertainty measures. Economic disagreement measures are calculated as the standard deviations of economists' forecasts from the Blue Chip Economic Indicator survey, including gross domestic product (GDP), consumer price index (CPI), 3-month treasury bill rate (TB3), unemployment rate (UNPR), industrial production (IP), disposable personal income (DPI), non-residential fixed investment (NRFI), housing starts (HST), 10-year treasury bond rate (TN10), and personal consumption expenditure (PCE). Economic uncertainty measures include economic policy uncertainty (EPU) and categorical EPU from Baker, Bloom, and Davis (2016). The partisan conflict index is from Azzimonti (2018). The last row reports the slope of partisan conflict index from the elastic net regression by including all the predictors. Reported are the regression slope, t-value, and R^2. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. The sample period is 1981:01–2018:12.

	β	t-value	ψ	t-value	R^2	R^2_{OS}
Panel A: Controlling for economic predictors						
Dividend-price ratio	0.51**	2.37	0.08	0.34	1.59	2.26***
Dividend yield	0.50**	2.32	0.10	0.44	1.60	2.46***
Earning-price ratio	0.54***	2.87	−0.01	−0.05	1.56	1.13**
Dividend payout ratio	0.54***	2.78	0.09	0.30	1.60	−0.31
Book-to-market ratio	0.61***	2.98	−0.12	−0.55	1.81	1.89***
Net equity expansion	0.54***	2.61	0.08	0.31	1.59	0.25**
Treasury bill rate	0.53***	2.68	−0.17	−0.85	1.72	−0.19
Long-term bond yield	0.59***	3.01	−0.22	−1.09	2.01	0.22**
Long-term bond return	0.53***	2.64	0.24	1.17	1.87	1.80***
Term spread	0.58***	2.89	−0.32	−1.48	2.32	0.28**
Default yield spread	0.53***	2.71	0.39	1.22	2.40	−0.45
Default return spread	0.53***	2.71	0.39	1.22	2.40	−0.45
Inflation rate	0.53***	2.72	0.14	0.58	1.67	1.20***
Stock sample variance	0.59***	2.98	0.29	1.51	1.99	1.76***
Panel B: Controlling for disagreement						
Disagreement on GDP	0.62***	2.92	0.10	0.38	2.13	1.61***
Disagreement on CPI	0.63***	2.94	0.03	0.11	2.08	−0.81
Disagreement on TB3	0.62***	2.92	0.07	0.39	2.10	1.77***
Disagreement on UNPR	0.62***	2.89	0.32	1.41	2.61	3.41***
Disagreement on IP	0.65***	3.02	0.26	1.18	2.44	1.80***
Disagreement on DPI	0.62***	2.89	−0.09	−0.29	2.11	−0.13
Disagreement on NRFI	0.67***	3.07	0.23	0.97	2.35	0.31
Disagreement on HST	0.65***	2.92	−0.14	−0.63	2.18	0.10
Disagreement on TN10	0.99***	3.86	0.49	1.36	4.65	2.10**
Disagreement on PCE	0.77***	3.89	−0.04	−0.12	3.67	−0.69

Table 3.2 (continued)

	β	t-value	ψ	t-value	R^2	R^2_{OS}
Panel C: Controlling for uncertainty						
Economic policy uncertainty	0.56**	2.12	0.07	0.22	1.85	−0.54
Monetary policy uncertainty	0.60**	2.45	0.06	0.20	2.03	−2.52
Fiscal policy uncertainty	0.50**	2.11	0.28	1.18	2.20	1.01**
Tax uncertainty	0.52**	2.20	0.27	1.15	2.18	0.69**
Government spending uncertainty	0.52**	2.30	0.20	0.76	2.01	1.04**
Healthcare uncertainty	0.51**	2.15	0.28	1.22	2.21	−0.11
National security uncertainty	0.60***	2.88	0.37*	1.79	2.58	2.04***
Entitlement program uncertainty	0.55**	2.49	0.20	0.91	2.03	−0.30
Regulation uncertainty	0.58**	2.36	0.01	0.03	1.82	−1.20
Financial regulation uncertainty	0.58**	2.36	0.01	0.03	1.82	0.12*
Trade policy uncertainty	0.56***	2.71	0.15	1.28	1.94	1.45***
Sovereign debt uncertainty	0.57***	2.72	0.18	0.67	1.99	0.38**
Panel D: Kitchen sink						
Elastic net	0.58**	2.36			4.02	5.96***

Table 3.3 Controlling for political sentiment and geopolitical risk

This table reports the results of predicting market returns with partisan conflict index as

$$R_{t+1} = \alpha + \beta \text{Partisan conflict}_t + \psi Z_t + \varepsilon_{t+1},$$

where Z_t represents political sentiment , geopolitical risk, or both. Political sentiment is defined as the return differential between high and low political-sensitivity portfolios in Addoum and Kumar (2016). Geopolitical risk is from Caldara and Iacoviello (2018). The partisan conflict index is from Azzimonti (2018). ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. The sample period is 1981:01–2018:12.

Partisan conflict	t-value	Political sentiment	t-value	Geopolitical risk	t-value	R^2	R^2_{OS}
		−0.40*	−1.92			0.88	0.60
				0.26	1.22	0.36	−0.51
0.50**	2.51	−0.34*	−1.65			2.19	1.96***
0.59***	2.84			0.28	1.27	2.25	−0.61
0.55***	2.67	−0.31	−1.40	0.24	1.10	2.74	−0.46

Table 3.4 Forecasting industry and size portfolio returns

This table presents the results of predicting industry and size portfolio returns as:

$$R^i_{t+1} = \alpha + \beta \text{Partisan conflict}_t + \varepsilon_{t+1},$$

where R^i_{t+1} includes industry i's return (panel A) and size portfolio i's return (panel B) from Ken French's website. The partisan conflict index is from Azzimonti (2018). Reported are the regression coefficient, t-value, in-sample R^2, and out-of-sample R^2_{OS}. Statistical significance for R^2_{OS} is based on the p-value of the Clark and West (2007) MSFE-adjusted statistic for testing: $H_0 : R^2_{OS} \leqslant 0$ against $H_A : R^2_{OS} > 0$. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. The in-sample period is 1981:01–2018:12 and the out-of-sample period is 2000:01–2018:12.

	β	t-value	R^2	R^2_{OS}
Panel A: Industry portfolio				
Food	0.32*	1.85	0.60	−1.36
Mines	−0.17	−0.47	0.05	−0.18
Oil	0.19	0.79	0.12	−0.02
Clths	0.41*	1.69	0.46	0.47*
Durbl	0.55**	2.07	0.98	0.94**
Chems	0.51**	1.99	0.79	1.04**
Cnsum	0.35*	1.93	0.66	0.04
Cnstr	0.64***	2.70	1.16	1.85***
Steel	0.21	0.45	0.07	−0.22
FabPr	0.43*	1.82	0.65	1.68***
Machn	0.45	1.37	0.44	0.58*
Cars	0.51	1.63	0.63	−0.04
Trans	0.39*	1.66	0.55	1.03**
Utils	0.32	1.48	0.71	0.39*
Retail	0.45**	2.31	0.79	0.08
Finan	0.58**	2.19	1.14	0.54*
Other	0.56**	2.29	1.32	1.35***
Panel B: Size portfolio				
Low	0.19	0.68	0.10	−0.08
Quintile 2	0.37	1.61	0.44	0.56*
Quintile 3	0.40*	1.73	0.58	0.83**
Quintile 4	0.43**	1.97	0.76	1.09**
High	0.54***	2.66	1.65	1.76***

Table 3.5 Forecasting UK and US market returns with partisan conflict

This table presents the results of forecasting UK and US market return with UK or US partisan conflict or both. The partisan conflict index for US is from Azzimonti (2018). We construct the partisan conflict index for UK following the same logic as Azzimonti (2018). Reported are the regression slopes, t-values, and R^2. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. The sample period is 1981:01–2018:12.

Return	Partisan conflict^{US}	t-value	Partisan conflict^{UK}	t-value	R^2	R^2_{OS}
R^{UK}_{t+1}			0.48^{**}	1.97	1.53	1.01^{*}
R^{UK}_{t+1}	0.57^{**}	2.30			2.13	0.48^{*}
R^{UK}_{t+1}	0.53^{**}	2.09	0.43^{*}	1.85	3.33	0.38^{*}
R^{US}_{t+1}			0.52^{*}	1.78	1.44	0.55
R^{US}_{t+1}	0.74^{***}	2.80	0.45	1.54	4.27	2.16^{**}

Table 3.6 Forecasting market return with alternative partisan conflict indexes

This table presents the results of predicting market returns with partisan conflict that are constructed based on political disagreement dictionary or government dictionary (Azzimonti, 2018). We consider the raw indices without detrending and with log linear detrending and stochastic detrending approach. Reported are regression slope, t-value, in-sample R^2, and out-of-sample R^2_{OS}. Statistical significance for R^2_{OS} is based on the p-value of the Clark and West (2007) MSFE-adjusted statistic for testing: $H_0 : R^2_{OS} \leqslant 0$ against $H_A : R^2_{OS} > 0$. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. The in-sample period is 1981:01–2018:12 and the out-of-sample period is 2000:01–2018:12.

	β	t-value	R^2	R^2_{OS}
Panel A: Index based on political disagreement dictionary				
Log linear detrending				
$h = 1$	0.50***	2.56	1.34	1.88***
$h = 3$	1.11**	2.23	2.14	2.90***
$h = 6$	2.59***	2.75	5.40	5.36***
$h = 12$	4.78***	2.81	8.24	10.03***
No detrending				
$h = 1$	0.43**	2.53	1.01	0.98**
$h = 3$	0.99**	2.32	1.70	1.38**
$h = 6$	2.27***	2.73	4.15	2.05***
$h = 12$	4.06***	2.65	5.97	6.17***
Stochastic detrending				
$h = 1$	0.38**	2.07	0.78	0.67*
$h = 3$	0.73*	1.80	0.95	0.70*
$h = 6$	1.61***	2.63	2.22	1.83***
$h = 12$	3.21***	2.93	3.99	4.96***
Panel B: Index based on government dictionary				
Log linear detrending				
$h = 1$	0.38**	2.30	0.80	1.33***
$h = 3$	0.90*	1.90	1.41	2.73***
$h = 6$	2.12**	2.29	3.63	7.10***
$h = 12$	4.58***	2.60	7.58	14.29***
No detrending				
$h = 1$	0.38**	2.24	0.77	0.73**
$h = 3$	0.84*	1.77	1.21	0.89*
$h = 6$	1.94**	2.18	3.02	3.19**
$h = 12$	4.33***	2.60	6.77	6.51***
Stochastic detrending				
$h = 1$	0.35*	1.76	0.66	0.62
$h = 3$	0.69	1.40	0.84	0.83
$h = 6$	1.64**	2.28	2.31	3.03***
$h = 12$	3.99***	3.10	6.17	7.85***

Table 3.7 Forecasting power of partisan conflict over political cycles

This table reports the results of predicting market returns with state-dependent regression as

$$R_{t+1} = \alpha + \beta_1 \text{Partisan conflict}_t + \beta_2 \text{Democratic}_t \times \text{Partisan conflict}_t \\ + \beta_3 \text{Democratic}_t + \varepsilon_{t+1}$$

where Democratic_t is a dummy variable that equals one if the president is affiliated with the democratic party or the majority of House/Senate is democrats, and zero otherwise. The partisan conflict index is from Azzimonti (2018). ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. The sample period is 1981:01–2018:12.

State determination	β_1	t-value	β_2	t-value	β_3	t-value	R^2
Presidential affiliation	0.57^{**}	2.24	−0.12	−0.54	0.46^{**}	2.33	2.74
House majority	0.44^{**}	2.04	0.10	0.36	0.48^{**}	2.25	2.72
Senate majority	0.28	1.53	0.34	1.24	0.40^{**}	2.01	3.04

Table 3.8 Relationship with uncertainty

This table reports the regressions of partisan conflict on uncertainty measures as:

$$\Delta \text{Partisan conflict}_t = \alpha + \beta \Delta y_t + \psi \Delta \text{Partisan conflict}_{t-1} + \varepsilon_t,$$

where y_t is economic policy uncertainty (EPU) and categorical EPU from Baker, Bloom, and Davis (2016). The last column of Panel B reports the results of elastic net regression by including all the uncertainty measures. The partisan conflict index is from Azzimonti (2018). Reported are the regression slope, t-value, and R^2. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

y	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Economic policy uncertainty	1.72*												−−
	(1.88)												
Monetary policy uncertainty		1.90*											−−
		(1.95)											
Fiscal policy uncertainty			3.73***										1.60**
			(4.38)										(1.96)
Tax uncertainty				3.45***									−−
				(4.54)									
Government spending uncertainty					3.54***								−−
					(4.05)								
Healthcare uncertainty						4.24***							3.14***
						(4.30)							(2.68)
National security uncertainty							0.20						−−
							(0.27)						
Entitlement program uncertainty								3.08***					−−
								(3.03)					
Regulation uncertainty									1.45**				−−
									(2.08)				
Financial regulation uncertainty										−0.21			−−
										(−0.28)			
Trade policy uncertainty											1.56**		−−
											(2.03)		
Sovereign debt uncertainty												−0.44	−−
												(−0.90)	
R^2	6.06	5.91	11.12	10.15	10.55	12.89	4.65	9.05	5.63	4.65	5.78	4.72	13.53

Table 3.9 Forecasting economic activities

Panel A reports the results of predicting discount rate shock, cash flow shock, and variance shock of the market return with partisan conflict index and price earning ratio as

$$y_{q+1} = \alpha + \beta \text{Partisan conflict}_q + \psi \text{P/E}_q + \varepsilon_{q+1},$$

where the shocks and quarterly P/E data are from Campbell, Giglio, Polk, and Turley (2018). Panel B presents the results of predicting economic and corporate activities with partisan conflict index as:

$$y_{t+1} = \alpha + \beta \text{Partisan conflict}_t + \sum_{i=1}^{12} \lambda_i y_{t-i+1} + \varepsilon_{t+1}$$

for monthly data, and

$$y_{q+1} = \alpha + \beta \text{Partisan conflict}_q + \sum_{i=1}^{4} \lambda_i y_{q-i+1} + \varepsilon_{q+1}$$

for quarterly data. The economic activity measures include Chicago fed national activity index (CFNAI), industrial production growth, real personal consumption expenditure (consumption), unemployment rate, private gross domestic investment (investment), Real GDP growth, business inventory, and capacity utilization (Greenwood, Hercowitz, and Huffman, 1988). The partisan conflict index is from Azzimonti (2018). We take the average of the index within one quarter as the quarterly partisan conflict measure. Reported are the regression slopes, its Newey-West t-value, and R^2. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

y	β	t-value	R^2
Panel A: Forecasting market return component shocks			
Discount rate shock	0.93**	1.96	0.97
Cash flow shock	−0.02	−0.05	0.06
Variance shock	0.01	0.03	0.94
Panel B: Forecasting economic activities			
CFNAI	0.08	0.22	37.90
Industrial production	0.33	0.94	18.23
Consumption	0.22	0.91	9.85
Unemployment	−0.18**	−2.17	14.63
Investment (quarterly)	1.25	1.54	20.33
Real GDP (quarterly)	0.15	0.94	27.37
Business inventory	0.12	0.55	52.79
Capacity utilization	0.32	1.15	17.39

Table 3.10 Relationship with search attention

This table reports the results of contemporaneous regression of search attention index on partisan conflict as:

$$\Delta \text{Attention}_t = \alpha + \beta \Delta \text{Partisan conflict}_t + \psi X_t + \sum_{i=1}^{4} \lambda_i \Delta \text{Attention}_{t-i} + \sum_{i=1}^{4} \lambda_i \Delta \text{Partisan conflict}_{t-i} + \varepsilon_t,$$

where Attention is the search attention index (i.e., Google search interest) constructed by equally weighting the Google search volumes of the key words used in Azzimonti (2018). The partisan conflict index is from Azzimonti (2018). X includes control variables following Lian, Ma, and Wang (2018). Specifically, we control the Campbell-Shiller price-earnings ratio (P/E10), the past 12-month excess stock return, VIX^2 (the square of VIX, which measures the expected variance of S&P500 index), real GDP growth, the credit spread (Gilchrist and Zakrajšek, 2012), and surplus consumption ratio. Reported are the regression slope, t-value, and R^2. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	DepVar: Google search attention		
	(1)	(2)	(3)
Constant	−0.19	−0.19	−0.42
	(−0.09)	(−0.09)	(−0.23)
Partisan conflict	15.69***	15.65***	14.12***
	(6.84)	(6.92)	(5.72)
P/E 10		1.16	2.27
		(0.24)	(1.14)
12-month market return		0.99	3.50
		(0.22)	(1.08)
VIX^2		5.72*	5.31*
		(1.84)	(1.91)
T-bill		−1.94	−0.06
		(−0.40)	(−0.01)
Real GDP growth		0.85	1.15
		(0.27)	(0.46)
Credit spread		−3.88	4.21
		(−0.49)	(0.65)
Surplus consumption ratio		−0.21	−0.78
		(−0.04)	(−0.21)
Attention_{-1}			−0.09
			(−1.01)
Attention_{-2}			−0.22***
			(−3.25)
Attention_{-3}			−0.31***
			(−4.20)
Attention_{-4}			−0.21***
			(−3.33)
$\text{Partisan conflict}_{-1}$			−2.01
			(−0.87)
$\text{Partisan conflict}_{-2}$			−3.02
			(−1.17)
$\text{Partisan conflict}_{-3}$			4.85**
			(2.01)
$\text{Partisan conflict}_{-4}$			2.28
			(1.01)
R^2	22.29	38.43	41.19

Table 3.11 Forecasting presidential approval rates

This table reports the results of predicting presidential approval rates as:

$$\Delta\text{Approval}_{t+1} = \alpha + \beta\Delta\text{Partisan conflict}_t + \psi X_{t+1} + \sum_{i=1}^{4}\lambda_i\Delta\text{Approval}_{t-i+1} + \sum_{i=1}^{4}\lambda_i\Delta\text{Partisan conflict}_{t-i+1} + \varepsilon_{t+1},$$

where Approval is the U.S. Presidential approval rates from Gallup Analytics. The partisan conflict index is from Azzimonti (2018). X includes control variables following Lian, Ma, and Wang (2018). Specifically, we control the Campbell-Shiller price-earnings ratio (P/E10), the past 12-month excess stock return, VIX^2 (the square of VIX, which measures the expected variance of S&P500 index), real GDP growth, the credit spread (Gilchrist and Zakrajšek, 2012), and surplus consumption ratio. Reported are the regression slope, t-value, and R^2. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	DepVar: Presidential approval rates		
	(1)	(2)	(3)
Constant	0.01	-0.08	-0.04
	(0.04)	(-0.37)	(-0.23)
Partisan conflict	-0.35^{*}	-0.54^{**}	-0.53^{**}
	(-1.74)	(-2.50)	(-2.35)
P/E 10		-0.06	-0.09
		(-0.18)	(-0.32)
12-month market return		-0.24	-0.26
		(-0.66)	(-0.72)
VIX^2		-0.05	-0.11
		(-0.14)	(-0.41)
T-bill		0.66	0.90^{**}
		(1.32)	(2.16)
Real GDP growth		0.40^{**}	0.32^{**}
		(1.32)	(1.02)
Credit spread		0.93^{*}	1.01^{*}
		(1.69)	(1.89)
Surplus consumption ratio		0.37	0.48^{*}
		(1.15)	(1.79)
Approval_{-1}			0.10
			(1.21)
Approval_{-2}			-0.01
			(-0.17)
Approval_{-3}			-0.04
			(-0.69)
Approval_{-4}			-0.16^{***}
			(-2.96)
$\text{Partisan conflict}_{-1}$			-0.20
			(-0.98)
$\text{Partisan conflict}_{-2}$			0.16
			(0.75)
$\text{Partisan conflict}_{-3}$			-0.29
			(-1.38)
$\text{Partisan conflict}_{-4}$			0.14
			(0.72)
R^2	0.96	5.85	11.21

Table 3.12 Forecasting mutual fund flows

This table reports the results of predicting mutual fund flows as:

$$MF_{q+1} = \alpha + \beta \Delta \text{Partisan conflict}_q + \psi X_q + \sum_{i=1}^{4} \lambda_i MF_{q-i+1} + \varepsilon_{q+1}$$

where MF_q refers to quarterly net exchange of mutual fund flows into bonds (stocks) at quarter q normalized by the total net asset at quarter $q-1$ following Ben-Rephael, Kandel, and Whol (2012). X includes control variables following Lian, Ma, and Wang (2018). Specifically, we control the Campbell-Shiller price-earnings ratio (P/E10), the past 12-month excess stock return, VIX^2 (the square of VIX, which measures the expected variance of S&P500 index), real GDP growth, and the credit spread (Gilchrist and Zakrajšek, 2012). We lag all the control variables by one period. Mutual fund flow data are from Investment Company Institute (ICI). Partisan conflict measure is from Azzimonti (2018). We take the average of the index within one quarter as the quarterly partisan conflict measure. Reported are the regression slope, t-value, and R^2. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. The sample period is 1997Q1–2017Q4.

Mutual fund flows	Equity		Bond	
Constant	-1.25	-0.56	-0.08	-0.28
	(-0.94)	(-0.59)	(-0.06)	(-0.29)
Partisan conflict	-2.11^{**}	-2.81^{***}	2.13^{**}	2.42^{***}
	(-2.34)	(-3.08)	(2.43)	(2.94)
P/E 10	-3.43^{*}	-4.52^{***}	4.98^{***}	4.97^{***}
	(-1.76)	(-3.09)	(2.61)	(3.37)
12-month MKT return	1.95	0.72	-1.48	-0.36
	(1.08)	(0.52)	(-0.92)	(-0.31)
VIX^2	1.18	2.65^{*}	-1.12	-2.80^{**}
	(0.76)	(1.92)	(-0.74)	(-2.06)
T-bill	-0.08	1.74	1.15	-0.98
	(-0.03)	(1.19)	(0.52)	(-0.81)
Real GDP growth	2.86	3.67^{**}	-4.51^{**}	-3.76^{**}
	(1.49)	(2.50)	(-2.25)	(-2.39)
Credit spread	-1.25	0.13	2.15	0.50
	(-0.52)	(0.07)	(0.91)	(0.29)
Fund flow$_{-1}$		0.38^{***}		0.39^{***}
		(3.29)		(3.26)
Fund flow$_{-2}$		0.04		0.06
		(0.39)		(0.68)
Fund flow$_{-3}$		0.22^{***}		0.21^{***}
		(2.99)		(2.70)
Fund flow$_{-4}$		-0.28^{***}		-0.19^{**}
		(-3.07)		(-2.19)
R^2	11.95	32.87	20.11	38.46

Chapter 4

Partisan Conflict and Corporate Credit Spreads

In this chapter, I document a positive relationship between partisan conflict and corporate credit spreads. A one standard deviation increase in partisan conflict is associated with a 0.91% increase in the next one-month corporate credit spreads after controlling for bond-issue information, firm characteristics, macroeconomic variables, uncertainty measures, and sentiment measures. The result holds when using instrumental variable to resolve endogeneity concerns. I further find that partisan conflict has a greater impact on corporate credit spreads for firms with higher exposure to government policies, including government spending policy and tax policy, and for firms with higher dependence on external finance. Firms that are actively involved in political activities are also more sensitive to changes in political polarization.

4.1 Introduction

Partisan conflict and credit spreads have both been increasing in recent years. In this chapter, I examine the relationship between partisan conflict and corporate credit spreads using a comprehensive panel of bond dataset from 2002 to 2018. I focus on corporate credit spreads because corporate bonds are a major source of external financing for firms. Changes in borrowing costs may significantly influence a firm's operating and investment plans. Furthermore, a widening of credit spreads could disrupt credit supply, leading to a subsequent reduction in spending and production (Gilchrist and Zakrajšek, 2012). Importantly, corporate credit spread is a better signal of recessions than stock prices (Philippon, 2009). In addition, it has been argued that credit spreads also have quantitatively significant impact on investment dynamics and business cycles (Gilchrist, Sim, and Zakrajšek, 2014). Hence, it is important to study the effect of partisan conflict on the bond market, because it unveils a novel channel through which partisan conflict can have significant impact on the real economy.

I find that partisan conflict significantly and positively predicts corporate credit spreads. Within our sample period, a one standard deviation increase in partisan conflict is associated with a 1.01% increase in next-month corporate credit spreads after controlling for firm characteristics, bond-issue information, and macroeconomic variables. Our finding is novel, as it implies that rising partisan conflict leads to expectations of higher levels of risk in the corporate debt market. To resolve the concern that the predictive power of partisan conflict maybe driven by other related factors, I further include a battery of control variables, such as uncertainty measures (e.g., VIX, economic policy uncertainty, geopolitical risk) and sentiment measures (e.g., consumer confidence index, political sentiment), that are commonly regarded as determinants of credit spreads. Our principal results stand even under this extensive set of controls, which indicates that the predictability that partisan conflict has on credit spreads is robust and cannot be explained by existing factors suggested in the literature.

I further show that this impact is rather short-lived and alters at later months. In particular, I find that the impact of partisan conflict has on corporate credit spread remains positive at month t+2. However, this effect reverses later as the sign turns negative and significant in month t+3, and this negative sign becomes insignificant from month t+4 onwards. The interpretation of our results is as follows. When partisan conflict intensifies, corporate credit spreads surge in the month after as debt markets demand for a higher risk premium to compensate for the disagreement. In contrast, as the disagreement resolves three months later, this effect evaporates and decays as time passes by.

Next, our results show that the effect of partisan conflict is more pronounced for bonds with lower credit rating. We split the whole sample into speculative-grade issues, which have lower credit ratings, and investment-grade issues, which have better credit scores. We find that a one- standard deviation increase in partisan conflict widens credit spreads by 1.97% for speculative- grade issues and by 0.64% for investment-grade issues. The results are similar if we split the sample according to Standard & Poors credit rating classifications.

Having established that partisan conflict positively predicts corporate credit spreads, I proceed to propose three implications from this relationship. I conjecture that the impacts of partisan conflict on credit spreads should be more pronounced for firms with higher exposure to government policy, for firms with higher dependence on external finance, and for firms with higher amount of political donation. For the first implication, I identify two types of firms that are especially exposed to government policies, namely firms that are dependent on government spending, and firms with high effective tax rates. Our results show that the response of corporate credit spreads to changes in partisan conflict are stronger for firms with higher government spending exposure and for firms paying higher effective tax rates. These findings are consistent with the hypothesis that intensified partisan conflict leads to increasing uncertainty in government policy, which in turn, translates into higher credit risk.

Apart from government policies, partisan conflict may also affect corporate credit spreads indirectly through changes in economic conditions. Hence, for the second implication, I identify firms that are more dependent on external finance using the measure from Duchin, Ozbas, and Sensoy (2010). I find that firms that rely heavily on external finance are more sensitive to changes in partisan conflict. This effect is 5.21 times larger than that for firms that finance their operations internally. Our finding corroborates the conjecture that changes in partisan conflict distort the economy, which consequently raise external borrowing costs.

While all firms are passively influenced by government policies and external finance condition, some firms take active roles in political campaigns to hedge against political uncertainties. To test the third implication, I construct a measure of political involvement using the amount of a firm's political campaign contributions following Luo, Manconi, and Massa (2018). I then define "politically active" firms as firms with larger amount of political campaign donation. I find that firms with higher PAC (Political Action Committee) contributions are more responsive to changes in partisan conflict. The impact of partisan conflict is 4.90 times larger than that of firms that do not actively contribute to party candidates.

Our results so far document a positive relationship between partisan conflict and corporate credit spreads and this relationship differs across firms. I next resolve endogeneity issues to further establish causality. First, there are concerns that partisan conflict may be potentially correlated with other economic factors that are omitted in our regressions. To address these concerns, I leverage the similarities and linkages between U.S. and Canadian economies. To do so, I regress U.S. partisan conflict index on the index for Canada, together with some macroeconomic control variables. Next, I retain the residuals from such regressions as an alternative proxy for U.S. partisan conflict. Critical to our approach is the assumption that U.S. and Canadian economies are highly correlated but political system in the U.S. and Canada are mostly independent of one another. Hence, this regression allows us to eliminate the common economic factors that influence both American and Canadian politics, and it extracts the residuals that capture the variation in partisan conflict not explained by these economic factors. I then use these residuals as an alternative index and explore the pure impacts of partisan conflict on corporate credit spreads.

I also conduct a Two-Stage-Least-Square (TSLS) analysis, with several instrumental variables, including a measure of party polarization (McCarty, Poole, and

Rosenthal, 2006) and mass shooting incidents. While the former is widely used in the existing literatures on political uncertainty, the latter is innovative in our analysis. The occurrence of mass shootings is totally unanticipated, indicating that it is exogenous to economic developments. Moreover, the spill-over effects on economic policy afterwards have indirect influences on corporate credit spreads. These make the occurrence of mass shootings a suitable instrument for partisan conflict. The predictive power of partisan conflict survives under these approaches, which indicates that the positive relationship between partisan conflict and corporate credit spreads is not due to endogeneity issues.

Our analysis contributes to the literature in several important ways. First, we add to the growing literature on the impact of political and policy uncertainty. Existing works have been focusing on the impact of political polarization on economic outcomes, especially on stock market (Brogaard, Dai, Ngo, and Zhang, 2019; Brogaard and Detzel, 2015; Huang and Wang, 2019; Liu, Shu, and Wei, 2017; Pástor and Veronesi, 2013). In contrast, the impact on credit markets, and in particular corporate debt market, has rarely been studied. Using economic policy uncertainty measures from Baker, Bloom, and Davis (2016), Kaviani et al. (2018) document a positive relation between policy uncertainty and corporate credit spreads. In our analysis, we take one step further by focusing on partisan conflict, which maybe a potential driver of policy uncertainty (Baker, Bloom, Canes-Wrone, Davis, and Rodden, 2014). We provide fundamental explanations for the increasing policy uncertainty and its impacts on credit market. On top of that, EPU index mixes policymakers' discussing uncertainty in financial markets or the economy versus discussing uncertainty in policy action. In other words, EPU index mixes economic uncertainty and policy uncertainty. In addition, it may generate inconsistent implications as it captures various types of uncertainties. By using partisan conflict index, we separate the uncertainty caused by party polarization from other types of uncertainties. This enables us to perform clear-cut empirical tests on the implications of Pástor and Veronesi (2013) in credit market.

Furthermore, Gad et al. (2019) employ the firm-level political risk measure from Hassan et al. (2019) and document its effect on credit market. Our analysis differs from theirs in several aspects. First, their firm-level political risk measure is constructed from the transcripts of a firm's conference calls. Each individual firm has a score for political risk, which represents the belief of its managers or analysts regarding political risks. What they have identified is the level effect of individual firm's exposure to political risk on corporate credit spreads. In contrast, the partisan conflict index that we employ is constructed using key words related to political disagreement. It captures the intensity of dispute between Democrats and Republicans, which is identical to all firms. We identify the effect that our specification focuses on from the time-series variation in the intensity of partisan conflict and individual firm's credit spread. In short, Gad et al. (2019)'s specification focuses on the effect of firm-specific risk while our empirical design focuses on the effect of partisan conflict or the ideological dispersion among politicians. These two variables have different economic meanings and deserve separate explorations.

Second, firm-level political risk indicates the risk perceived by firm managers and participants on their conference calls. These subjective beliefs may differ from actual political risks. In contrast, partisan conflict index we use is constructed based on newspaper articles and the search excludes editorials and commentaries to reduce personal ideological biases. This way, our partisan conflict index better captures the objective facts regarding political risk and makes it a closer representation of what is happening in the Congress and the White House.

Third, political risk measure speaks to the importance of firm-specific exposure to political issues. It tells nothing about the heterogeneous exposure to aggregate political polarization. In other words, our research complements Gad et al. (2019) and illustrates how common shocks in political uncertainty influence financial institutions across the nation. Finally, controlling for both EPU and firm-level political risk in our main regression does not influence the predictive power of partisan conflict. Hence, our partisan conflict index has superior performance in capturing the variation of corporate credit spreads.

Existing literature also documents the impacts of political uncertainty on firms' activities and decision, including corporate investment (Gulen and Ion, 2016; Jens, 2017; Julio and Yook, 2017), and corporate financing activities (Francis, Hasan, and Zhu, 2014; Tran and Phan, 2017; Waisman, Ye, and Zhu, 2015). Our results contribute to the literature by showing that firms relying heavily on external finance are more sensitive to changes in partisan conflict. This implies that maintaining a healthy debt capital structure maybe helpful for firms to deal with the increasing cost of capital during periods of intensified party polarization. Furthermore, recent literature provides evidence that political connection and lobby activity affect firms' corporate decision and performances (Akey, 2015; Akey and Lewellen, 2016; Borisov, Goldman, and Gupta, 2015). Our analysis also adds to this literature by showing that firms take more active positions in political contribution to hedge against the uncertainty induced by party polarization.

Finally, we also contribute to the literature on the determinants of corporate credit risk. Merton (1974) proposes a structural model that relates credit risk to leverage and asset volatility. Following this seminal work, several researchers extend Merton's structural model and provide additional theoretical and empirical support (Campbell and Taksler, 2003; Chen, Lesmond, and Wei, 2007; Collin-Dufresne, Goldstein, and Martin, 2001; Gad, Nikolev, Tahoun, and van Lent, 2019; Kaviani, Kryzanowski, Maleki, and Savor, 2018). Eom, Helwege, and Huang (2004) show that structural models do not accurately predict credit spreads, suggesting that most of the variation in credit spreads is attributable to a missing systematic factor. This factor is not firm-specific and is orthogonal to both changes in credit risk and typical measures of liquidity. Our analysis provides new evidence to this line of research by proposing a potential candidate for this missing systemic factor. Using partisan conflict index, we show that party polarization has significant impacts on corporate credit spreads even after controlling for known credit spread determinants, including the EPU index from existing literature.

The rest of this chapter is organized as follows. Section 4.2 depicts the data and sample construction. Section 4.3 shows that partisan conflict positively predicts corporate credit spreads. Section 4.4 explores cross-sectional heterogeneity of the effects of changes in partisan conflict on credit spreads. Section 4.5 addresses endogeneity issues using several approaches. Section 4.7 concludes.

4.2 Data and Sample Construction

The primary source of bond data is the Trade Reporting and Compliance Engine (TRACE), which was established by the Financial Industry Regulatory Agency (FINRA) in July 2002. It provides bond transaction data on a daily basis, including features such as transaction price, size of the transaction, execution time, yield to maturity, bond maturity date. I also obtain the issuer and issue information from the master file provided by TRACE, which provides us with the bond characteristics and issue information.

As in Chen, Fabozzi, and Sverdlove (2010), there appear to be some reporting errors in the TRACE yield and price data. I therefore recompute the bond yields using coupon rates and bond prices and remove the data points where yield reported by TRACE differ from our computed yield by more than 5%. Following Edwards, Harris, and Piwowar (2007) and Kaviani et al. (2018), I delete trade reports that are subsequently corrected, canceled or suspended, that are incorrectly reported, that are missing key information, and that are duplicately reported by multiple dealers. I also remove convertible bonds, since this option would have distorted the return calculation, as mentioned by Campbell and Taksler (2003). Bonds that are structured notes, mortgage-backed, asset-backed, agency-backed, equity-linked, and/or traded under \$5 or above \$1000, and bonds that have less than one year to maturity, are also excluded from the sample (as in the work of Bai, Bali, and Wen (2019)). If a bond is traded more than once in a given day, I compute the yield and price based on the average number of transactions completed on that day, and the trading volume as the summation of all transactions completed on that day. The data provided by TRACE is of daily frequency, so to convert this data frequency to monthly frequency, I construct the monthly measures of bond yields and prices as the transaction size weighted averages over each month of daily yields and bond prices.

Firm financial information, such as stock price, return, and the number of shares outstanding, is obtained from the standard source, the Center for Research in Security Prices (CRSP), and accounting information, such as total assets and net sales, is obtained from the quarterly COMPUSTAT North America database. I merge the cleaned TRACE database with the quarterly COMPUSTAT database using the standard matching table provided by Wharton Research Data Services (WRDS), and retain all the firms that have accounting information in COMPUSTAT. Yields of Treasury bonds are collected from the Federal Reserve Board Ibsite and used as the benchmark for calculating credit spreads. Thus, corporate credit spreads are computed as the yield difference between corporate bonds and Treasury notes with closest maturity.

The sample used in this study contains 16,300 bonds issued by 1,312 firms from July 2002 to December 2018. Panel A of Table 4.1 displays the summary statistics for the main variables used in our analysis. The average credit spread is 2.05%. The whole sample contains bonds with an average rating of 7.08 (i.e., BBB+) and an average time to maturity of 14.92 years. Comparing the right two sections of Panel A, investment-grade bonds have lower credit spread, higher credit rating, and more years to maturity, and are issued by firms with lower leverage and idiosyncratic risk. Panel B shows the summary statistics for the partisan conflict index, macroeconomic variables, uncertainty measures, and sentiment measures. All of these variables exhibit considerable variation over the sample period. Panel C reports the pairwise correlation between time-series variables, with the p-value indicated in parenthesis. Partisan conflict is positively correlated with term spread and expected unemployment, and negatively correlated with the 10-year Treasury rates and political sentiment. Details of these variables are provided in the Online Appendix.

4.3 Partisan Conflict and Corporate Credit Spreads

In this section, I report the results of our principal analysis on the impacts of partisan conflict on corporate credit spreads. Previous research show that there are a large set of potential determinants of credit spreads, so I include various bond-issue information, firm characteristics, macroeconomic control variables, uncertainty measures, and sentiment measures.

I first control maturity, coupon rates, and credit rating as they directly influence corporate credit spreads. I compute maturity as the time between when the bond is issued and the maturity date. Our primary credit-rating measure is the annual S&P Domestic Long-Term Issuer Credit Rating from COMPUSTAT North America. Since the credit rating ranges from AAA to D, I convert it to cardinal values and expect a positive relationship between credit rating and corporate credit spreads. I also control bond illiquidity. Following Acharya, Amihud, and Bharath (2013), I compute the bond returns, then calculate the bond illiquidity in accordance with the procedure described by Amihud (2002). Given the lack of consensus in measuring bond liquidity, I also construct the proxy following Corwin and Schultz (2012). The results are similar using this alternative liquidity measure.

Firm accounting characteristics include market leverage, total debt ratio, operating income to sales ratio, and pre-tax interest coverage, which are all computed as standard in accounting literature. Since changes in interest coverage have non-linear effects on credit spreads, I construct four dummy variables to indicate whether pre-tax interest coverage was less than 5, between 5 and 10, between 10 and 20, or greater than 20. I also control for the firms stock return and its idiosyncratic volatility.

Next, I control for two groups of macroeconomic variables that are commonly used in corporate bond literature. The first group contains indicators of market condition, including the S&P 500 index return, the term slope, and the 10-year Treasury rate. The second group of macroeconomic control variables measures the general economic conditions, including expected unemployment and expected one-year inflation from the Survey of Professional Forecasters by the Federal Reserve Bank of Philadelphia.

I also control for uncertainty measures. The first group of uncertainty measures includes the VIX index by the Chicago Board Options Exchange (CBOE) and macroeconomic uncertainty measure from Jurado, Ludvigson, and Ng (2015). The second group consists of political or policy uncertainty measures, including economic policy uncertainty (Baker, Bloom, and Davis, 2016), firm-level political risk (Hassan, Hollander, van Lent, and Tahoun, 2019), and geopolitical risk measure (Caldara and Iacoviello, 2018). The last set of control variables consists political sentiment (Addoum and Kumar, 2016), consumer sentiment (Consumer Attitude Survey from University of Michigan), and credit-market sentiment (López-Salido, Stein, and Zakrajšek, 2017).

4.3.1 Full sample analysis

I begin the analysis by estimating the relationship between partisan conflict and corporate credit spread as:

$$
\begin{aligned}
\text{Credit spread}_{i,t} = & \alpha + \beta_1 \text{Partisan conflict}_{t-1} + \beta_2 \text{Firm control}_{i,t} \\
& + \beta_3 \text{Bond control}_{i,t} + \beta_4 \text{Macro control}_t \\
& + \beta_5 \text{Uncertainty control}_t + \beta_6 \text{Sentiment control}_t \\
& + \gamma_i + \varepsilon_{i,t},
\end{aligned}
\tag{4.1}
$$

I lag partisan conflict by one period to rule out the possibility that changes in corporate credit spreads are driven by other economic factors that influence credit spreads and partisan conflict simultaneously. Firm control includes firms' market leverage, total debt ratio, operating income to sales, pre-tax interest coverage dummies, stock return, and idiosyncratic volatility. Bond control represents bond issue information, including bond liquidity, coupon rate, maturity, and credit rating. Macro control refers to two groups of macroeconomic control variables, consisting of the S&P 500 index return, the 10-year Treasury rate, the term slope, expected unemployment, expected inflation. Uncertainty control consists of the VIX index, macroeconomic uncertainty, economic policy uncertainty (EPU), geopolitical risk, and the firm-level political risk measure. Sentiment control includes the consumer confidence index, political sentiment, and the credit-market sentiment index. I control firm fixed effects by γ_i, which are aimed at capturing the unobserved effects of firm-level determinants that are not included in independent and control variables.

Table 4.2 displays the results of our principal analysis. I normalize all independent variables and present the coefficients in percentage to clearly show how a one-standard-deviation change in each of the independent variables could explain the variations in credit spreads. I double cluster the t-statistics by firm and time. Column (1) shows that partisan conflict is positively associated with corporate credit spreads controlling for firm characteristics, bond-issue information, and macroeconomic variables. A one-standard-deviation increase in partisan conflict is associated with a 1.01% step-up in corporate credit spreads for the following month. Moreover, the coefficients on other control variables are generally consistent with those in the previous literature, which suggests that our sample is representative and reliable. Higher market leverage and bond illiquidity widen the credit spreads. As mentioned by Campbell and Taksler (2003), a higher level of idiosyncratic risk implies a larger probability of default, which in turn increases the credit spreads. As argued by Baker, Bloom, and Davis (2016) and López-Salido, Stein, and Zakrajšek (2017), credit spreads are counter-cyclical, which suggests that credit spreads are lower under promising market condition (confirmed in our results by the negative and significant coefficient on S&P 500 index). Consistent with the findings of Longstaff and Schwartz (1995), the 10-year Treasury rate, which is a proxy of interest rate, is negatively and significantly correlated with corporate credit spreads.

To rule out the possibility that the impact of partisan conflict on corporate credit spreads mostly overlaps with uncertainty measures, I continue to control for VIX index, macroeconomic uncertainty, EPU, geopolitical risk, and firm-level political risk measure. Column (2) shows that the predictive power of partisan conflict is not subsumed by the uncertainty measures. Variables related to economic uncertainty, such as VIX index and macroeconomic uncertainty are positively and significantly associated with changes in credit spreads. As investor sentiment has a substantial influence on the financial market, I also add sentiment controls to our baseline regression, making it the most comprehensive specification in our analysis. The results in column (3) show that partisan conflict still has a positive and significant impact on credit spreads, with a coefficient of 0.91 (t-statistic = 2.25). The increased R^2 demonstrates that uncertainty measure and investor sentiment do have explanatory power for changes in corporate credit spreads. Although the insignificant coefficients on EPU and firm-level political risk seem to contradict the existing literature, when I exclude partisan conflict and examine their predictive poIr on corporate credit spreads separately, both of them become positively and significantly associated with corporate credit spreads (EPU: 0.28 with a t-statistic of 2.54; political risk: 0.11 with a t-statistic of 3.60). The magnitude and significance of these coefficients are consistent with Kaviani et al. (2018) and Gad et al. (2019) , which suggests that our sample is representative and methods are reliable. The insignificance of EPU and political risk, as shown in column (3), indicates that the information incorporated in partisan conflict better captures the variation in credit spreads. Although not significant, coefficients of the three sentiment measures all enter with negative signs, which is consistent with previous literature. In sum, results in Table 4.2 demonstrate a positive and significant relation between partisan conflict and corporate credit spreads, which is not influenced by bond-issue information, firm characteristic, macroeconomic condition, uncertainty, and sentiment.

4.3.2 Long term effects

Previous results show that uncertainty stemming from conflicts among party members positively influences corporate credit spreads; as partisan conflict is resolved, its impact may gradually lessen and allow credit spreads to return to their fundamental level. So in this section, I investigate the long-term effects to determine whether there is a strong reversal in credit spreads following increase in partisan conflict.

I repeat our main analysis as Eq.(4.1) for month $t+h$, where h equals 1,2,3,4. All the control variables take the same time stamp as dependent variable, while partisan conflict is still measured at time t. Table 4.3 presents results on longer term effects, from month t+1 to month t+4, of changes in partisan conflict at month t. The effect in month t+2 is still positive and significant (0.88 with t-statistics of 2.14). Then, from month t+3 the effect turns negative and becomes statistically significant at 5% level. The effect is not significant any more from month t+4 onwards. These findings regarding long-term effects demonstrate that intensified partisan conflict causes increasing policy uncertainty, which pushes corporate credit spreads away from their intrinsic value. Such an increase in a firm's credit risk is therefore not due to the deterioration of the firm's fundamentals. As political disagreement is resolved, the firm's borrowing costs should return to their fundamental level.

4.3.3 Investment-grade versus speculative-grade bonds

In this section, we estimate Eq.(4.1) separately for investment-grade bonds and speculative-grade bonds. By doing so, we are able to provide some insights into how the influences of partisan conflict vary with bonds' credit ratings. We first consider the credit rating provided by TRACE, which classifies issuers as investment grade or speculative (high-yield) grade. To make the results robust, we also use the S&P Domestic Long-Term-Issuer Credit Rating by COMPUSTAT. According to the S&P rating scheme, an investment-grade sample includes bonds with a credit rating of

BBB- and higher, and a speculative-grade sample includes those with a credit rating of BB+ and lower.

Table 4.4 presents the results on investment-grade and speculative-grade bonds. For brevity, we display the results for the most comprehensive specification, with the whole set of control variables and firm fixed effects. As expected, the coefficients on partisan conflict in each subsample are all economically meaningful and statistically significant. Comparing the results from the investment-grade sample with those of the speculative grades, we find that the impact of partisan conflict is more pronounced for speculative-grade bonds under both rating schemes. A one-standard-deviation increase in the level of partisan conflict leads to a 1.97% (1.67% in Panel B) step-up in corporate credit spreads for speculative-grade bonds, compared with 0.64% (0.74% in Panel B) for the investment-grade sample. This is not surprising since speculative-grade bonds are typically issued by firms without healthy financial structures. Investors require higher yields as the compensation for a higher possibility of default, and, as a consequence, speculative-grade bonds are more sensitive to the changes in partisan conflict than those with better credit ratings.

4.4 Empirical Implications

The previous results have established the positive predictive poIr of partisan conflict on corporate credit spreads across all firms. HoIver, it is also quite likely that shifts in the level of partisan conflict will not influence all firms in the same way. Hence, in the second part of our empirical analysis, I continue to examine whether all firms are equally affected by changes in partisan conflict. I propose several possibilities, including exposure to government policies, dependence on external finance, and involvement in political activities, to explain why the influence of partisan conflict varies across firms.

4.4.1 Exposure to government policy

As stated in last chapter, a higher level of partisan conflict causes increased policy uncertainty. Moreover, I have shown that partisan conflict is closely related to fiscal policy uncertainty (tax + government spending) and healthcare policy uncertainty. In this section, I examine two types of government policies, including government spending policy and tax policy.

Exposure to government spending As in Ulrich (2013), uncertainty about future government spending is a first-order risk factor in the bond market, leading to an increase in bond premia. For the firms whose profitability relies heavily on government spending, a higher level of partisan conflict will increase the uncertainty about what policy to be implemented, which will in turn, translate to higher demand uncertainty. Hence, if partisan conflict widens corporate credit spreads, the effect should be more significant for firms with higher sensitivity to government spending.

Following Belo, Gala, and Li (2013), I construct the measure of government spending exposure (GSE, details are described in Online Appendix). Briefly, a firm's dependence on government purchases is measured as sales to government sector divided by its total sales. Next, I sort the whole sample into quintiles based on firms' GSE and define a GSE dummy which equals one for firms belonging in the top GSE quintile, and zero for firms in the bottom GSE quintile. I add this variable and its interaction with partisan conflict to the main specification and run the regression as follows:

$$
\begin{aligned}
\text{Credit spread}_{i,t} = & \alpha + \beta_1 \text{Partisan conflict}_{t-1} \times \text{GSE}_{i,t-1} \\
& + \beta_2 \text{Partisan conflict}_{t-1} + \beta_3 \text{GSE}_{i,t-1} \\
& + \beta_4 \text{Firm control}_{i,t} + \beta_5 \text{Bond control}_{i,t} \\
& + \beta_6 \text{Macro control}_t + \beta_7 \text{Uncertainty control}_t \\
& + \beta_8 \text{Sentiment control}_t + \gamma_i + \varepsilon_{i,t},
\end{aligned} \tag{4.2}
$$

where all the control variables are the same as Eq.(4.1). I also obtain the results from the bottom and top subsamples by splitting the sample with respect to GSE.

Panel A of Table 4.5 shows that firms that are more dependent on government spending have credit spreads that are more sensitive to partisan conflict. The effect is 3.75 times larger than that for firms with the lowest GSE. Considering both the bottom and top GSE quintiles, the coefficient of interactive term is 3.09, which is significant at the 5% level. This implies that the difference in response to changes in partisan conflict between the top and bottom GSE quintiles is non-trivial. Consequently, our results confirm that the impact of partisan conflict on credit spreads is more salient for firms with higher exposure to government spending.

Effective tax rate Tax policy is another important channel through which government policies affect firms. Since interest payments are tax deductible, firms have incentives to finance their operations by issuing bonds, which indirectly leads to higher credit risk. I hypothesize that firms with high tax expenses are more exposed to tax policy changes, and as a result their borrowing costs are more sensitive to changes in partisan conflict. To test our hypothesis, I compute a firm's effective tax rate (ETR) by dividing its total tax expense by its pre-tax income, as is standard in the literature. I sort the whole sample into quintiles based on firms' ETRs, and define an ETR dummy, which equals one for firms belonging to the top ETR quintile, and zero for firms in the bottom ETR quintile. I add this variable and its interaction with partisan conflict to the specification given by Eq.(4.1). I also present results in bottom and top subsamples based on splitting the sample with respect to effective tax rate.

Panel B of Table 4.5 shows that firms with higher ETRs are more sensitive to changes in partisan conflict. The effect is 6.21 times larger than that for firms in the bottom ETR quintile. Considering both the bottom and top ETR quintiles, the coefficient of interactive term is 2.85, which is significant at the 5% level, implying that the difference in response to changes in partisan conflict between the top and bottom ETR quintiles is significant. Hence, if partisan conflict positively influences corporate credit spreads, the impacts should be more pronounced for firms that are more exposed to changes in tax policy.

4.4.2 Dependence on external finance

The previous empirical analysis has shown that partisan conflict influences firms' performance and bond spreads directly through changes in government policy. In addition, partisan conflict may affect the conditions of the whole economic market, which will have indirect effects on corporate credit spreads. As partisan conflict intensifies, creditors are uncertain about what policies will be implemented, and will demand higher compensation when lending to firms, which will incur higher borrowing costs. However, financial market frictions do not influence all firms in the same way; only firms that depend heavily on external finance are more exposed to market frictions. Harsher financial market conditions may force them to postpone profitable investment plans, and they may also have problems refinancing existing debts. As a consequence, the firms' value will be reduced, making them less creditworthy to investors. In view of this, I expect firms with high dependence on external finance to be more responsive to changes in partisan conflict.

Following Duchin, Ozbas, and Sensoy (2010), I construct a measure of external finance dependence (EFD). I sort the whole sample into quintiles based on firms' EFD and define a EFD dummy which equals one for firms belonging to the top EFD quintile, and zero for firms in the bottom EFD quintile. I add this variable and its interaction with partisan conflict to the specification given by Eq.(4.1). I also present results in bottom and top subsamples based on splitting the sample with respect to firm's external finance dependence.

The results, displayed in Panel A of Table 4.6, show that firms with a higher EFD are more sensitive to changes in partisan conflict. For instance, a one-standard-deviation increase in partisan conflict is associated with a 3.23% increase in corporate credit spreads for firms that rely heavily on external finance. In contrast, the influence of partisan conflict is much weaker for firms with the lowest EFD.

Comparing the top and bottom quintiles, the difference in response to partisan conflict is non-trivial since the coefficient of the interaction term is significant at the 5% level (t-statistic: 2.02). Therefore, the results in Panel A of Table 4.6 confirm another indirect channel through which partisan conflict influences corporate credit spreads. Intensified partisan conflict may aggravate financial market friction, which increases firm's borrowing costs, and consequently, widens their credit spreads.

4.4.3 Political donation

In response to changes in government policies, some firms tend to actively engage with the policy makers or party candidates to obtain some insider information. In addition, some American businesses seem to derive part of their brand identity from their political affiliations, and it is common for such companies to make donations to candidates they hope would advocate for their interests. By this means, firms could not only benefit from the changes in public policy, but also hedge the potential risk from partisan conflict. I therefore examine whether firms' political donations influence their responses to intensified political polarization.

Following Luo, Manconi, and Massa (2018), I use the dollar amount that a firm voluntarily contribute to party candidates each year to measure its involvement in political activities. The data for corporate political contributions are retrieved from the detailed files of the Federal Election Commission (FEC). The database contains detailed information on the dollar amount of each firm's political action committee (PAC) or individual contributions to Republican or Democratic Party candidates. I focus on all the contributions in the "Corporation" group and aggregate all contributions made by a firm to Republican and Democratic Party candidates each year. As there is no common identifier between FEC and COMPUSTAT, I manually match the two databases using company names. The final matched sample consisted of 1114 unique firms with political contribution information.

I sort the whole sample into quintiles based on firms' political donations (DON) and define a DON dummy, which equals one for firms belonging to the top DON quintile, and zero for firms in the bottom DON quintile. I add this variable and its interaction with partisan conflict to the specification given by Eq.(4.1). I also present results in bottom and top subsamples based on splitting the sample with respect to DON.

Panel B of Table 4.6 shows that firms that actively contribute to political candidates display higher sensitivity to changes in partisan conflict. The effect is 4.90 times larger than that for firms with lower amounts of donations. Comparing the top and bottom DON quintiles, the difference in response to partisan conflict is non-trivial since the coefficient of the interactive term is significant at the 5% level (t-statistic: 2.19). In sum, the results in this section confirm that firms that generously contribute to political campaigns are more closely tied to party candidates and, as a consequence, are more responsive to conflict among party members.

4.5 Establishing Causality

The previous sections have established the significantly positive predictive power of partisan conflict on corporate credit spreads. However, as corporate credit spreads are sensitive to various economic and social factors, although I have controlled for a wide range of variables in our primary analysis, it is still likely that the partisan conflict index captures factors other than just dispute among party members. This, in turn, would have caused our findings to suffer from measurement error bias. Therefore, the remainder of this section presents several approaches to alleviate the concerns of endogeneity.

4.5.1 Canada-U.S. residual analysis

One possible concern about the partisan conflict index is that it may unwittingly capture economic uncertainty that also affects corporate credit spreads. Although I control for various proxies for economic uncertainty, I also take an additional step to further alleviate the measurement error concern.

According to Romalis (2007), the Canada-U.S. Free Trade Agreement (CUS-FTA) and the North American Free Trade Agreement (NAFTA) make Canada the largest trade partner of the United States. The economies of these two countries are closely related, and so the chances are that shocks causing economic uncertainty in the U.S. may have extended influence on Canada's economy. However, the two countries do not share a common political system. As a result, the impact of political polarization does not easily travel across the country's borders. Taking advantage of this characteristic, I could eliminate the economic-uncertainty-related part of the partisan conflict index by regressing the U.S. partisan conflict index on its Canadian counterpart, and controlling for a set of macroeconomic variables. I then retain the regression residuals as an alternative partisan conflict index, which eliminate the potential influence of general economic uncertainty.

To construct the partisan conflict index for Canada, I perform a similar search-based approach similar to Azzimonti (2018), of which the details can be found in Online Appendix. Figure 4.1 plots the series of partisan conflict index for Canada from January 2000 to December 2018. The index successfully captures some events that raised heated political debates, such as the Gulf War and the Quebec independence referendum. Moreover, as expected, the level of partisan conflict is higher than average during elections, since newspapers increases the proportion of articles covering political debates and addressing the differences between candidates. Although the index remains relatively stable prior to 2010, it displays an upward trend afterward. We therefore used a log-linear detrended series to remove the potential influence of the time trend.

With this partisan conflict index for Canada in hand, I proceed to run the time series regression as follows:

$$\text{Partisan conflict}_t^{US} = \alpha + \beta_1 \text{Partisan conflict}_t^{CAN} + \beta_2 \text{Macro control}_t + \varepsilon_t \quad (4.3)$$

where Partisan conflict^{US} and Partisan conflict^{CAN} are partisan conflict indices for the U.S. and Canada. Macro control includes 3-month Treasury bill rate, term spread, S&P 500 return, expected GDP growth, and expected inflation. I retain the residual ε_t as a purer proxy for U.S. partisan conflict and rerun our main regression specification as:

$$\begin{aligned}\text{Credit spread}_{i,t} = & \alpha + \beta_1 \text{Partisan conflict}^R_{t-1} + \beta_2 \text{Firm control}_{i,t} \\ & + \beta_3 \text{Bond control}_{i,t} + \beta_4 \text{Macro control}_t \\ & + \beta_5 \text{Uncertainty control}_t + \beta_6 \text{Sentiment control}_t \\ & + \gamma_i + \varepsilon_{i,t},\end{aligned} \tag{4.4}$$

where Partisan conflict^R refers to the residual from Eq.(4.3). The results in Table 4.7 show that the orthogonalized partisan conflict index still has significant positive impacts on corporate credit spreads. Column (3) has the most comprehensive control variables and firm fixed effect; the coefficient estimate is both economically meaningful and statistically significant (0.51, with t-statistic of 2.02). The economic magnitude of all coefficients on partisan conflict is smaller than the corresponding ones in Table 4.2, which implies that getting rid of the influences of unobserved economic uncertainty does affect the predictive poIr of partisan conflict. However, these measurement errors do not fully explain the variation in credit spreads, which provides supports to our arguments.

4.5.2 Instrumental variable analysis

I proceed to resolve the endogeneity concerns using two-stage-least-square (TSLS) method. In the context of our analysis, I aim to find a variable that is directly correlated with partisan conflict but does not have a direct impact on corporate credit spreads. More importantly, it should influence credit spreads only through its significant relationship with partisan conflict. This section describes the application of instrumental variable analysis to further establish causality.

Party polarization Our first instrumental variable is the measure of party polarization, which is first proposed as the NOMINATE measure (a measure of liberalism or conservatism) in McCarty, Poole, and Rosenthal (1997). Subsequently, McCarty, Poole, and Rosenthal (2006) update the measure, renaming it DW-NOMINATE [1]. I focus on the first dimension, since it can be interpreted as the positions of legislators about government intervention in the economy. Following Gulen and Ion (2016), our instrument is constructed by subtracting the DW-NOMINATE score of the Democratic party in the House from that of the Republican party [2]. Hence, holding everything constant, a higher value of polarization implies a higher level of partisan conflict, and therefore this party polarization measure satisfies the relevance condition as an instrument (I would test this condition statistically later). On the other hand, there is no direct evidence to show that party polarization influences corporate credit spreads in a way other than through its effect on partisan conflict. Under such circumstance, I are confident of finding a valid instrument for our analysis.

Mass shootings Next, I novelly use the number of mass shooting in the U.S. as an instrumental variable. To the best of our knowledge, this may have been the first time that mass shooting incidents are used as an instrument to measure party polarization.

In recent years, the number of public mass shootings has increased substantially. The increase in such incidents, and the extent of the casualties, has contributed to a strong partisan divide on gun control policy. In other words, the occurrence of mass shootings directly correlates with an increasing level of partisan conflict, which satisfies the relevance condition, making mass shootings a suitable instrument. In addition, the economic costs of mass shootings have spill-over effects on economic policy, which indirectly influence the corporate credit spreads. As taxpayers bear the

[1] Data are from http://voteview.com/downloads.asp.

[2] I also construct an analogous measure of party polarization for the members in the Senate. The two measures of polarization (for the House and the Senate) are highly correlated with each other. Hence, I use the House polarization measure since it has slightly higher F-statistics and R^2 in the first stage.

cost by paying for the medical care of victims and paying more in taxes to fund law enforcement, there are usually heated discussions on economic policies (tax policy and healthcare policy) afterwards. More importantly, the occurrence of a mass shooting is completely unanticipated, and hence totally exogenous to economic cycles. The only channel through which it impacts economic development is its influence on partisan conflict and the spill-over effect on economic policies, which makes mass shootings as an instrument satisfy the exclusion restrictions.

I create a dummy variable that equals one for a month in which a mass shooting or mass shootings occurs and for the two following months, and zero otherwise.

Two stage least square analysis Since the partisan conflict measure and its instruments are all time-invariant in the cross-section, their values repeat for all firms within each month. I run a time-series regression first and a panel regression in the second stage. Specifically, in the first stage, I run the time-series regression of partisan conflict on our instruments, together with some macroeconomic variables as:

$$\text{Partisan conflict}_t = \alpha + \beta \text{Instrument}_t + \psi \text{Macroeconomic variables}_t + \varepsilon_t, \quad (4.5)$$

where macroeconomic variables include 3-month T-bill rate, expected inflation, and expected GDP growth. The F-statistics for the first stage regression are 116.73 and 51.38, respectively. This indicates that these instrumental variables satisfy the relevance condition and are suitable choices for our analysis.

Following this, in the second stage, I use the fitted value of partisan conflict from Eq.(4.5) as its replacement and rerun Eq.(4.1) as:

$$\begin{aligned}\text{Credit spread}_{i,t} =& \alpha + \beta_1 \widehat{\text{Partisan conflict}}_{t-1} + \beta_2 \text{Firm control}_{i,t} \\ & + \beta_3 \text{Bond control}_{i,t} + \beta_4 \text{Macro control}_t \\ & + \beta_5 \text{Uncertainty control}_t + \beta_6 \text{Sentiment control}_t \\ & + \gamma_i + \varepsilon_{i,t},\end{aligned} \quad (4.6)$$

where all the control variables are the same as Eq.(4.1). Two sets of results are presented in Table 4.8, with the first two columns using party polarization, and the last two columns using the dummy for mass shootings. Across the two sets of instruments, the fitted value of partisan conflict still positively and significantly predicts the changes in corporate credit spreads. With the most comprehensive specification, the coefficients are 3.81 (party polarization as IV) and 4.47 (mass shootings dummy as IV), both of which are significant at 1% level. The magnitudes of coefficients are larger than the ones I found without instrumental variables, which implies that our instruments do Ill in eliminating other unrelated factors in partisan conflict, and make the relationship between partisan conflict and corporate credit spreads more sound. The results in this section therefore provide additional supports to the hypothesis that partisan conflict has a significant causal effect on changes in credit risk.

4.6 Robustness

This section describes several robustness tests we conduct to confirm that our main results hold within subsamples and with alternative detrending methods.

4.6.1 Subsample analysis

We stratify the whole sample into several subgroups based on firm size, bond maturity, and market leverage following Guntay and Hackbarth (2010). We choose these three firm and bond characteristics because previous research has shown that they have the most pronounced impacts on corporate credit spreads.

Firm size

Left panel in Table 4.9 reports the results of regression Eq.(4.1) within subsamples sorted by firm size. All the coefficients on partisan conflict in the three subgroups are positive and significant, alleviating the concern that the impact of partisan conflict on credit spreads is driven by a group of extremely large (small) firms. In addition, the magnitude of coefficient estimates decreases as we move from small firms to firms with larger market values (1.63 to 0.65). The difference in slope coefficients of the small and medium groups is not statistically significant (-0.55 with t-statistics of -0.35), whereas the difference is significant between the small and large groups (-0.98 with t-statistics of -1.96). As expected, small firms are less resistant to economic turbulence than large firms, which makes them more sensitive and vulnerable to disagreement among party members.

Bond maturity

In the middle panel of Table 4.9, we sort the whole sample in terms of bond maturity, and analyze the impact of partisan conflict within each subsample. For our purposes, the significantly positive relation between partisan conflict and corporate credit spreads holds across the three subgroups. The difference between coefficients of short- and long-maturity groups is not significant with a t-statistics of 0.09. In contrast, comparing short- and medium-maturity groups and medium- vs. long-maturity groups, the differences between coefficients on partisan conflict are significant for both comparisons, at 1% level. Such a U-shaped relation is consistent with the findings of Merton (1974) and Chen, Lesmond, and Wei (2007), which state that corporate yield spreads can either increase or decrease with maturity, depending on the risk of the firm. Helwege and Turner (1999) also finds that within the same credit rating category, safer firms tend to issue long-term bonds, causing a negative relationship between average yield spread and maturity.

Firm leverage

To eliminate the leverage-induced biases in our baseline regressions, we split the sample into three groups, those with low (below the 33rd percentile), medium (between 33rd and 67th percentiles), and high (above 67th percentile) leverage. The right panel of Table 4.9 confirms that partisan conflict is positively related to credit spreads across firms with different level of market leverage. The difference between the slope coefficients of the low-leverage and medium-leverage groups is not significant (0.44 with t-statistics of 1.01). However, the difference is significant between the coefficients of the low- and high-leverage group (1.17 with t-statistics of 1.86). Such a monotonic relation is consistent with our previous arguments that higher market leverage indicates a higher possibility of default. As a result, firms with a high amount of leverage are more sensitive to changes in partisan conflict.

4.6.2 Alternative detrending methods

Next, we adopt two alternative detrending methods to confirm that our results are not driven by a specific detrending method. We repeat our main analysis as Table 4.2 using partisan conflict index without detrending and with log-quadratic detrending. Panels A and B in Table 4.10 show that the coefficients of partisan conflict are economically meaningful and statistically significant. This implies that our results are robust across different detrending methods.

4.6.3 Alternative dictionaries

In the work of Azzimonti (2018), an article is considered about partisan conflict if it contains a keyword in the political disagreement dictionary and a keyword in the government dictionary. To show that our index does measure disagreement about government policy, we construct two alternative indexes, one based on the political disagreement dictionary and the other based on the government dictionary, and find that they have a correlation of 0.65. Panels C and D in Table 4.10 show that both indexes significantly predict corporate credit spreads with slightly stronger power, suggesting that the predictability results from partisan conflict over government policy, rather than disagreement about something else.

4.7 Conclusion

I document a positive relationship between partisan conflict and corporate credit spreads. A one standard deviation increase in partisan conflict is associated with 0.91% step-up in the following month's corporate credit spreads after controlling for bond-issue information, firm characteristics, macroeconomic conditions, uncertainty measures, and sentiment measures. The effect is rather short-lived and reverses three months later. The impacts of partisan conflict on corporate credit spreads are more salient for firms with higher exposure to government policy, for firms with higher dependence on external finance and for firms actively engaged in political activities. I also adopt several approaches to further establish causality and confirm that our results are not due to endogeneity issues.

Our research has some important policy and strategy implications. Intensified dispute among party members may increase firms' borrowing costs, so companies that rely on external financing to maintain day-to-day operations should be alerted before anticipated periods of increase in party polarization (i.e. periods before a presidential election). On top of that, intensified party polarization may impede the implementation of economic and social policy and have some negative effects on the efficient functioning of the administrative state and the judiciary. One interesting question for future research may be what strategy should firms follow to hedge against the political risk under intensified partisan conflict.

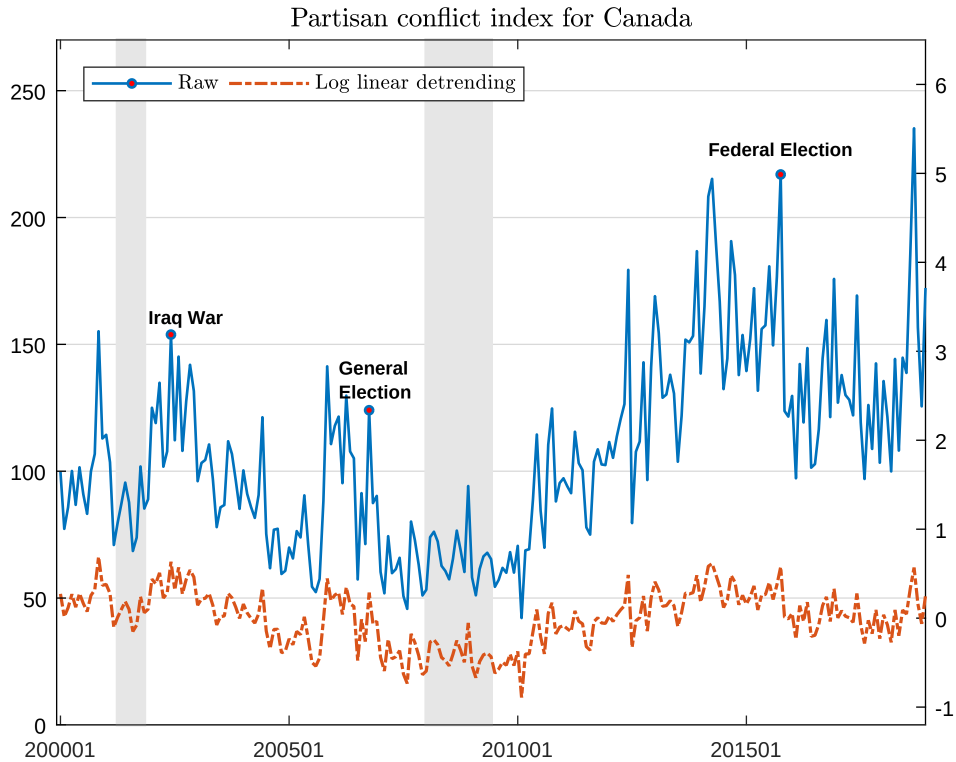

Fig. 4.1 Partisan conflict index for Canada

This figure plots the partisan conflict index for Canada, which is constructed following Azzimonti (2018). We cover 5 major Canadian newspapers following Baker, Bloom, and Davis (2016), and keywords used are adjusted to fit the Canadian political system. The sample period is 2000:01–2018:12.

Table 4.1 Summary statistics

This table reports the mean, median, and standard deviation for the main variables used in our analysis. Panel A reports the summary statistics for bond- and firm-characteristics for the entire sample, and then for investment- and speculative-grade bonds, respectively. Panel B reports the summary statistics for partisan conflict index, macroeconomic variables, uncertainty measures and sentiment measures. Panel C presents the pairwise correlation between time-series variables. The sample period is 2002:07–2018:12.

Panel A: Bond- and firm-specific variables									
	All			Investment grade			Speculative grade		
	Mean	Median	Std. Dev.	Mean	Median	Std. Dev.	Mean	Median	Std. Dev.
Credit spread (%)	2.05	1.82	1.33	1.79	1.64	1.09	3.15	3.09	1.65
Credit rating	7.08	7.00	3.35	6.27	6.00	2.69	10.49	11.00	3.65
Illiquidity (%)	2.04	0.32	6.54	1.67	0.28	5.59	2.34	0.40	6.89
Monthly stock return (%)	0.60	0.61	9.22	0.62	0.65	8.67	0.51	0.42	11.23
Idiosyncratic risk (%)	1.57	1.24	1.05	1.47	1.16	1.04	1.97	1.76	1.02
Political risk	175.78	105.52	237.67	185.45	115.62	244.77	135.20	75.22	200.14
Market leverage (%)	36.42	34.78	18.39	35.32	33.91	17.87	41.16	40.09	19.90
Operating income to sales	0.15	0.12	0.17	0.15	0.13	0.14	0.11	0.09	0.26
Total debt ratio	0.63	0.65	0.33	0.62	0.66	0.25	0.64	0.59	0.57
Coupon (%)	5.07	5.38	1.68	4.85	5.25	1.60	6.00	6.13	1.69
Maturity (years)	14.92	9.72	8.89	15.67	10.00	9.05	11.79	9.51	7.36
Number of observations	325402			268979			56423		

Panel B: Macroeconomic variables and uncertainty indices			
	Mean	Median	Std. Dev.
Partisan conflict	122.42	121.56	41.23
Term spread (%)	1.48	1.61	0.85
Monthly S&P return (%)	0.47	0.98	0.40
10-year Treasury note rate (%)	4.59	4.52	1.91
Expected unemployment (%)	6.17	5.61	1.79
Expected inflation (%)	2.11	2.10	0.32
Consumer confidence	83.70	86.40	11.63
VIX index	19.20	16.39	9.07
Macroeconomic uncertainty	0.66	0.64	0.09
Economic Policy uncertainty	115.18	107.11	35.90
Geopolitical risk	103.94	83.21	66.98
Political sentiment	0.19	0.52	5.80
Number of observations		198	

Panel C: Correlation analysis												
	PC	TS	S&P	TN10	UNPR	CPI	MCC	VIX	MU	EPU	GPR	PSENT
Partisan conflict (PC)	1.00											
Term spread (TS)	0.20	1.00										
	(0.00)											
S&P market return (S&P)	0.12	−0.02	1.00									
	(0.01)	(0.71)										
10-year Treasury note (TN10)	−0.28	−0.34	0.01	1.00								
	(0.00)	(0.00)	(0.88)									
Expected unemployment (UNPR)	0.22	0.74	0.09	−0.16	1.00							
	(0.83)	(0.00)	(0.02)	(0.00)								
Expected inflation (CPI)	0.18	−0.38	0.00	0.84	0.23	1.00						
	(0.00)	(0.00)	(1.00)	(0.00)	(0.00)							
Consumer Confidence (MCC)	−0.02	−0.62	0.04	0.21	−0.66	−0.04	1.00					
	(0.66)	(0.00)	(0.39)	(0.00)	(0.00)	(0.41)						
VIX	−0.30	0.07	−0.40	0.02	0.14	−0.05	−0.17	1.00				
	(0.00)	(0.19)	(0.00)	(0.68)	(0.01)	(0.32)	(0.00)					
Macroeconomic uncertainty (MU)	−0.48	0.10	−0.23	0.24	0.29	−0.13	−0.56	0.66	1.00			
	(0.00)	(0.14)	(0.00)	(0.00)	(0.05)	(0.00)	(0.00)	(0.00)				
Economic policy uncertainty (EPU)	0.32	0.52	−0.11	−0.38	0.61	−0.16	−0.58	0.37	0.27	1.00		
	(0.00)	(0.00)	(0.12)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)			
Geopolitical risk (GPR)	−0.02	0.16	−0.04	−0.26	−0.14	−0.21	−0.03	0.10	−0.02	−0.08	1.00	
	(0.64)	(0.00)	(0.37)	(0.00)	(0.00)	(0.00)	(0.50)	(0.06)	(0.70)	(0.26)		
Political sentiment (PSENT)	−0.12	−0.09	−0.16	0.08	−0.11	0.01	0.13	0.03	0.01	−0.09	−0.12	1.00
	(0.02)	(0.08)	(0.00)	(0.15)	(0.03)	(0.81)	(0.01)	(0.50)	(0.88)	(0.19)	(0.02)	

Table 4.2 Partisan conflict and corporate credit spreads

This table reports the results of predicting corporate credit spreads with partisan conflict index as

$$\begin{aligned}\text{Credit spread}_{i,t} = \alpha &+ \beta_1 \text{Partisan conflict}_{t-1} + \beta_2 \text{Firm control}_{i,t} + \beta_3 \text{Bond control}_{i,t} \\ &+ \beta_4 \text{Macro control}_t + \beta_5 \text{Uncertainty control}_t + \beta_6 \text{Sentiment control}_t \\ &+ \gamma_i + \varepsilon_{i,t},\end{aligned}$$

where Firm $\text{control}_{i,t}$ includes firms' market leverage, total debt ratio, operating income to sales, pre-tax interest coverage, stock return, and idiosyncratic volatility. Bond $\text{control}_{i,t}$ represents bond issue characteristics, including bond liquidity, coupon rate, maturity, and credit rating. Macro control_t refers to the two groups of macroeconomic control variables, consisting of S&P 500 index return, 10-year Treasury rate, term slope, expected unemployment, expected inflation. Uncertainty control_t includes VIX index, macroeconomic uncertainty from Jurado, Ludvigson, and Ng (2015), economic policy uncertainty (EPU) from Baker, Bloom, and Davis (2016), geopolitical risk from Caldara and Iacoviello (2018), and firm-level political risk measure from Hassan et al. (2019). Sentiment control_t includes consumer confidence index from University of Michigan, political sentiment following Addoum and Kumar (2016), and credit-market sentiment following López-Salido, Stein, and Zakrajšek (2017). γ_i captures firm fixed effects. The partisan conflict index is from Azzimonti (2018). All independent continuous variables are normalized. Reported are regression slopes, t-statistics, and R^2s. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. The sample period is 2002:07–2018:12.

	(1)	(2)	(3)
Partisan conflict	1.01**	0.93**	0.91**
	(2.02)	(1.96)	(2.25)
Market leverage	3.01**	3.02**	2.99**
	(2.05)	(2.09)	(2.01)
Operating income to sales	−0.66	−0.64	−0.64
	(−1.27)	(−1.32)	(−1.13)
Total debt ratio	0.24*	0.25*	0.25*
	(1.76)	(1.77)	(1.73)
Pre-tax coverage D1	−2.45	−2.38	−2.43
	(−0.95)	(−0.96)	(−0.96)
Pre-tax coverage D2	−3.37*	−3.36*	−3.38*
	(−1.75)	(−1.78)	(−1.77)
Pre-tax coverage D3	−2.47**	−2.50*	−2.51*
	(−1.96)	(−1.95)	(−1.95)
Stock return	−0.14	−0.17	−0.19
	(−0.50)	(−0.55)	(−0.66)
Idiosyncratic risk	9.52***	9.64***	9.50***
	(4.94)	(4.90)	(4.73)
Illiquidity	3.28***	3.27***	3.28***
	(3.51)	(3.31)	(3.24)
Coupon	59.47***	59.49***	59.45***
	(25.99)	(26.04)	(25.93)
Credit rating	19.23***	19.03***	19.07***
	(2.66)	(2.68)	(2.66)
Maturity	−4.34***	−4.32***	−4.34***
	(−6.88)	(−6.89)	(−6.88)
S&P return	−2.46***	−2.40***	−2.47***
	(−4.15)	(−4.16)	(−3.52)
Term slope	12.25***	12.74***	12.52***
	(2.57)	(2.91)	(2.58)
10-year Treasury rate	−43.03***	−43.35***	−43.23***
	(−14.21)	(−14.83)	(−14.13)
Expected unemployment	0.85	0.67	−0.41
	(0.40)	(0.34)	(−0.19)
Expected one-year inflation	3.62	3.76	3.60
	(1.54)	(1.62)	(1.50)
VIX index		6.91***	6.80***
		(6.46)	(5.95)
Macroeconomic uncertainty		3.36*	3.67**
		(1.94)	(2.10)
Economic policy uncertainty		0.32	0.26
		(0.30)	(0.29)
Geopolitical risk		0.07	0.06
		(0.11)	(0.08)
Political risk		−0.57	−0.58
		(−0.89)	(−0.91)
Consumer confidence			−1.20
			(−1.32)
Political sentiment			−0.15
			(−0.31)
Credit-market sentiment			−0.23
			(−0.24)
Firm fixed effect	YES	YES	YES
Firm cluster	YES	YES	YES
Year cluster	YES	YES	YES
Observations	325,402	325,402	325,402
Adjusted R^2	0.72	0.76	0.81

Table 4.3 Long term effects

This table reports the results of panel regressions for partisan conflict on corporate credit spreads over various horizons as:

$$\begin{aligned}\text{Credit spread}_{i,t+h} = \alpha &+ \beta_1 \text{Partisan conflict}_t + \beta_2 \text{Firm control}_{i,t+h} + \beta_3 \text{Bond control}_{i,t+h} \\ &+ \beta_4 \text{Macro control}_{t+h} + \beta_5 \text{Uncertainty control}_{t+h} \\ &+ \beta_6 \text{Sentiment control}_{t+h} + \gamma_i + \varepsilon_{i,t+h},\end{aligned}$$

where Credit spread$_{i,t+h}$ refers to the credit spread of bond i at time $t+h$. Control variables are the same as Table 4.2. The partisan conflict index is from Azzimonti (2018). All independent continuous variables are normalized. Reported are regression slopes, t-statistics, and R^2s. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. The sample period is 2002:07–2018:12.

	t+1	t+2	t+3	t+4
Partisan conflict	0.91**	0.88**	−0.76**	−0.30
	(2.25)	(2.14)	(−1.97)	(−0.65)
Market leverage	2.99**	3.01**	2.99**	3.01***
	(2.01)	(2.01)	(2.01)	(2.64)
Operating income to sales	−0.64	−0.62	−0.55	−0.57
	(−1.13)	(−1.09)	(−0.95)	(−1.60)
Total debt ratio	0.25*	0.25*	0.25*	0.25*
	(1.73)	(1.75)	(1.68)	(1.67)
Pre-tax coverage D1	−2.43	−2.34	−2.50	−2.42
	(−0.96)	(−0.93)	(−0.98)	(−0.99)
Pre-tax coverage D2	−3.38*	−3.38*	−3.36*	−3.35*
	(−1.77)	(−1.77)	(−1.78)	(−1.86)
Pre-tax coverage D3	−2.51**	−2.53*	−2.52**	−2.51*
	(−1.95)	(−1.95)	(−1.97)	(−1.68)
Stock return	−0.19	−0.19	−0.23	−0.22
	(−0.66)	(−0.68)	(−0.80)	(−1.01)
Idiosyncratic risk	9.50***	9.44***	9.76***	9.62***
	(4.73)	(4.61)	(4.06)	(4.17)
Illiquidity	3.28***	3.32***	3.12***	3.19***
	(3.24)	(3.29)	(2.99)	(4.26)
Coupon	69.47***	69.48***	69.46***	69.47***
	(25.93)	(25.90)	(25.93)	(28.13)
Credit rating	19.07***	19.04***	18.82***	18.91***
	(2.66)	(2.64)	(2.60)	(2.94)
Maturity	−4.34***	−4.35***	−4.32***	−4.28***
	(−6.88)	(−6.87)	(−6.88)	(−6.49)
S&P return	−2.47***	−2.07***	−2.39**	−2.11***
	(−3.52)	(−3.08)	(−3.15)	(−5.14)
Term slope	12.52***	11.24**	13.13***	12.89***
	(2.54)	(2.41)	(2.71)	(2.82)
10-year Treasury rate	−43.25***	−41.74***	−43.65***	−43.39***
	(−14.13)	(−14.51)	(−14.72)	(−15.76)
Expected unemployment	−0.41	−0.08	−0.70	−0.87
	(−0.19)	(−0.03)	(−0.30)	(−0.41)
Expected one-year inflation	3.60	3.66	3.83	3.81
	(1.50)	(1.53)	(1.60)	(1.54)
VIX index	6.80***	6.44***	6.49***	6.26***
	(5.95)	(6.43)	(6.41)	(9.54)
Macroeconomic uncertainty	3.67**	3.54**	3.51**	3.71*
	(2.10)	(2.01)	(2.04)	(1.81)
Economic policy uncertainty	0.26	0.31	0.40	0.43
	(0.29)	(0.36)	(0.43)	(1.05)
Geopolitical risk	0.06	−0.05	−0.30*	−0.30*
	(0.08)	(−0.06)	(−1.86)	(−1.77)
Political risk	−0.58	−0.56	−0.59	−0.59
	(−0.91)	(−0.88)	(−0.93)	(−0.94)
Consumer confidence	−1.20	−1.18	−1.01	−1.16*
	(−1.32)	(−0.95)	(−0.87)	(−1.87)
Political sentiment	−0.15	−0.14	−0.16	−0.18
	(−0.31)	(−0.27)	(−0.32)	(−0.60)
Credit-market sentiment	−0.23	0.01	−0.59	−0.37
	(−0.24)	(0.01)	(−0.59)	(−0.64)
Firm fixed effect	YES	YES	YES	YES
Firm cluster	YES	YES	YES	YES
Year cluster	YES	YES	YES	YES
Observations	325,402	325,402	325,402	325,402
Adjusted R^2	0.81	0.81	0.82	0.80

Table 4.4 Partisan conflict and credit spreads for investment- and speculative-grade bonds

This table reports the results of panel regressions for credit spreads of investment- and speculative-grade bonds with partisan conflict index as

$$\begin{aligned}\text{Credit spread}_{i,t} = {} & \alpha + \beta_1\text{Partisan conflict}_{t-1} + \beta_2\text{Firm control}_{i,t} + \beta_3\text{Bond control}_{i,t} \\ & + \beta_4\text{Macro control}_t + \beta_5\text{Uncertainty control}_t + \beta_6\text{Sentiment control}_t \\ & + \gamma_i + \varepsilon_{i,t},\end{aligned}$$

where control variables are the same as Table 4.2. The partisan conflict index is from Azzimonti (2018). All independent continuous variables are normalized. Reported are regression slopes, t-statistics, and R^2s. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. The sample period is 2002:07–2018:12.

	DepVar: Corporate credit spreads			
	Panel A		Panel B	
	Investment	Speculative	Investment	Speculative
Partisan conflict	0.64*	1.97**	0.74*	1.67**
	(1.81)	(2.28)	(1.92)	(2.12)
Market leverage	3.06***	2.88	2.92***	2.08
	(2.92)	(1.12)	(2.74)	(0.86)
Operating income to sales	−0.60	−0.89***	−0.62	−0.91**
	(−1.11)	(−2.62)	(−1.19)	(−2.08)
Total debt ratio	0.51	0.21***	0.84	0.12
	(0.58)	(2.61)	(0.83)	(1.32)
Pre-tax coverage D1	−3.47	−1.89	−4.14	−8.48**
	(−1.19)	(−0.25)	(−1.54)	(−2.33)
Pre-tax coverage D2	−3.05	−4.80	−3.11	−3.89
	(−1.43)	(−0.69)	(−1.62)	(−1.14)
Pre-tax coverage D3	−2.37	−5.26	−2.60*	−2.67
	(−1.35)	(−0.83)	(−1.66)	(−1.24)
Stock return	−0.02	−0.30	−0.19	0.01
	(−0.05)	(−1.14)	(−0.54)	(0.04)
Idiosyncratic risk	14.26***	5.86***	16.12***	1.72
	(3.86)	(3.76)	(4.51)	(1.21)
Illiquidity	2.50***	2.59**	3.28***	1.88*
	(2.58)	(2.41)	(3.01)	(1.95)
Coupon	64.77***	66.15***	67.63***	67.11***
	(25.47)	(21.27)	(22.03)	(27.85)
Credit rating	4.39***	4.32***	4.18***	1.76
	(2.70)	(3.07)	(3.12)	(1.15)
Maturity	−4.09***	−4.06***	−4.31***	−3.56***
	(−14.57)	(−7.06)	(−16.66)	(−4.85)
S&P return	−2.54***	−2.01**	−2.26***	−2.46***
	(−6.25)	(−2.43)	(−5.67)	(−3.51)
Term slope	12.63***	7.35	14.78***	−2.35
	(2.58)	(0.88)	(2.98)	(−0.26)
10-year Treasury rate	−42.55***	−47.96***	−43.79***	−45.89***
	(−13.53)	(−8.65)	(−14.31)	(−6.93)
Expected unemployment	−1.08	−0.50	−2.29	2.99
	(−0.64)	(−0.15)	(−1.40)	(0.74)
Expected one-year inflation	4.33*	−2.61	3.72	−1.37
	(1.93)	(−1.23)	(1.63)	(−0.50)
VIX index	6.52***	6.58***	6.01***	7.72***
	(8.11)	(3.66)	(6.92)	(4.82)
Macroeconomic uncertainty	4.17**	−0.93	3.23	1.69
	(2.18)	(−0.37)	(1.49)	(0.57)
Economic policy uncertainty	0.60	−0.79	0.67	−1.43
	(1.18)	(−0.73)	(1.31)	(−1.34)
Geopolitical risk	0.15	−0.11	0.07	0.01
	(0.62)	(−0.20)	(0.28)	(0.01)
Political risk	−0.76	0.06	−0.86	0.72
	(−1.17)	(0.11)	(−1.31)	(1.46)
Consumer confidence	−1.31**	−1.67	−1.27*	−2.86
	(−2.12)	(−0.74)	(−1.93)	(−1.18)
Political sentiment	−0.16	−0.15	−0.15	−0.25
	(−0.59)	(−0.30)	(−0.55)	(−0.42)
Credit-market sentiment	−0.50	1.44	−0.46	1.43
	(−0.71)	(1.62)	(−0.66)	(1.29)
Firm fixed effect	YES	YES	YES	YES
Firm cluster	YES	YES	YES	YES
Year cluster	YES	YES	YES	YES
Observations	268,979	56,432	265,317	60,085
Adjusted R^2	0.80	0.82	0.80	0.81

Table 4.5 Government policy and the impact of partisan conflict

This table presents the results of predicting corporate credit spreads with partisan conflict and firm's exposure to government policy (government spending policy (GSE) and tax policy (ETR)). I report both results using an interaction between PC and GSE (ETR) dummy (columns 4, 7), as Ill as results in separate subsamples based on splitting the sample into quintiles with respect to GSE (ETR) (the remaining columns). Controls are the same as in column (3) of Table 4.2. All independent continuous variables are normalized. Reported are the regression slope, t-statistics, and R^2. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. Sample period is 2002:07–2018:12.

	DepVar: Corporate credit spreads					
	Panel A: Government spending (GSE)			Panel B: Effective tax (ETR)		
	Bottom GSE quintile	Top GSE quintile	Bottom and top GSE quintiles	Bottom ETR quintile	Top ETR quintile	Bottom and top ETR quintiles
PC	1.05^{*}	3.94^{***}	1.10	0.85^{*}	3.41^{***}	0.88
	(1.65)	(2.56)	(1.14)	(1.85)	(2.58)	(1.41)
PC $\times$ GSE			3.09^{**}			
			(1.98)			
GSE			-5.02^{***}			
			(-3.72)			
PC $\times$ ETR						2.85^{**}
						(1.98)
ETR						-3.24
						(-1.51)
Controls	YES	YES	YES	YES	YES	YES
Firm fixed effect	YES	YES	YES	YES	YES	YES
Firm cluster	YES	YES	YES	YES	YES	YES
Year cluster	YES	YES	YES	YES	YES	YES
Observations	52,866	52,878	105,744	63,323	63,485	126,808
R^2	0.78	0.80	0.76	0.74	0.84	0.74

Table 4.6 External finance dependence, political donation and the impact of partisan conflict

Panel A presents the results of predicting corporate credit spreads with partisan conflict (PC) and external finance dependence (EFD, as measured in Duchin, Ozbas, and Sensoy (2010)). Panel B presents the results of predicting corporate credit spreads with partisan conflict and political donation (DON) as measured from Federal Election Commission. I report both results using an interaction between PC and EFD (DON) dummy, as Ill as results in separate subsamples based on splitting the sample into quintiles with respect to EFD (DON). Controls are the same as in column (3) of Table 4.2. All independent continuous variables are normalized. Reported are the regression slope, t-statistics, and R^2. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. Sample period is 2002:07–2018:12.

	DepVar: Corporate credit spreads		
Panel A: External finance dependence			
	Bottom EFD quintile	Top EFD quintile	Bottom and top EFD quintiles
PC	0.62^{*}	3.23^{**}	0.68
	(1.76)	(2.47)	(1.62)
PC × EFD			2.88^{**}
			(2.02)
EFD			−0.71
			(−0.32)
Controls	YES	YES	YES
Firm fixed effect	YES	YES	YES
Firm cluster	YES	YES	YES
Year cluster	YES	YES	YES
Observations	63,701	63,728	127,429
R^2	0.80	0.81	0.83
Panel B: Political donation			
	Bottom DON quintile	Top DON quintile	Bottom and top DON quintiles
PC	0.81	3.97^{**}	0.85
	(1.34)	(2.34)	(1.38)
PC × DON			3.26^{**}
			(2.19)
DON			13.35^{***}
			(3.14)
Controls	YES	YES	YES
Firm fixed effect	YES	YES	YES
Firm cluster	YES	YES	YES
Year cluster	YES	YES	YES
Observations	36,412	36,420	72,832
R^2	0.80	0.82	0.81

Table 4.7 Partisan conflict and corporate credit spreads using U.S.-Canada regression residuals

This table reports the results of predicting corporate credit spreads with partisan conflict index as

$$\begin{aligned}\text{Credit spread}_{i,t} = \alpha &+ \beta_1 \text{Partisan conflict}^R_{t-1} + \beta_2 \text{Firm control}_{i,t} + \beta_3 \text{Bond control}_{i,t} \\ &+ \beta_4 \text{Macro control}_t + \beta_5 \text{Uncertainty control}_t + \beta_6 \text{Sentiment control}_t \\ &+ \gamma_i + \varepsilon_{i,t},\end{aligned}$$

where control variables are the same as Table 4.2. Partisan conflict^R refers to the residual of the regression of U.S. partisan conflict on Canadian partisan conflict from Eq.(4.3). All independent continuous variables are normalized. Reported are regression slopes, t-statistics, and R^2s. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. The sample period is 2002:07–2018:12.

	(1)	(2)	(3)
Partisan conflict	0.49**	0.44**	0.51**
	(1.97)	(1.96)	(2.02)
Market leverage	3.10***	3.11***	3.09***
	(2.75)	(2.81)	(2.79)
Operating income to sales	−0.44	−0.40	−0.35
	(−1.17)	(−1.10)	(−0.97)
Total debt ratio	0.25*	0.25*	0.25*
	(1.69)	(1.69)	(1.71)
Pre-tax coverage D1	−2.36	−2.28	−2.26
	(−0.92)	(−0.90)	(−0.91)
Pre-tax coverage D2	−3.26*	−3.25*	−3.24*
	(−1.80)	(−1.79)	(−1.78)
Pre-tax coverage D3	−2.42	−2.44*	−2.47*
	(−1.62)	(−1.64)	(−1.66)
Stock return	−0.38	−0.42	−0.36
	(−1.43)	(−1.52)	(−1.42)
Idiosyncratic risk	9.69***	9.84***	9.77***
	(4.26)	(4.22)	(4.18)
Illiquidity	3.22***	3.11***	3.18***
	(4.64)	(4.41)	(4.51)
Coupon	59.57***	59.58***	59.56***
	(17.39)	(17.78)	(17.64)
Credit rating	19.03***	18.84***	18.85***
	(2.94)	(2.90)	(2.89)
Maturity	−4.35***	−4.38***	−4.34***
	(−10.35)	(−10.32)	(−10.41)
S&P return	−2.14***	−2.07***	−1.94***
	(−3.63)	(−2.91)	(−2.76)
Term slope	11.21***	12.30***	11.17**
	(2.66)	(2.96)	(2.36)
10-year Treasury rate	−42.71***	−43.43***	−42.50***
	(−16.75)	(−17.44)	(−15.59)
Expected unemployment	0.40	−0.29	−0.77
	(0.22)	(−0.14)	(−0.37)
Expected one-year inflation	3.33	3.47	3.19
	(1.48)	(1.56)	(1.36)
VIX index		6.76***	6.43***
		(5.97)	(6.06)
Macroeconomic uncertainty		3.76*	3.79*
		(1.91)	(1.88)
Economic policy uncertainty		0.80	0.69
		(1.63)	(1.41)
Geopolitical risk		0.14	0.09
		(0.57)	(0.41)
Political risk		−0.56	−0.57
		(−0.95)	(−0.96)
Consumer confidence			−1.03*
			(−1.81)
Political sentiment			0.09
			(0.33)
Credit-market sentiment			0.32
			(0.68)
Firm fixed effect	YES	YES	YES
Firm cluster	YES	YES	YES
Year cluster	YES	YES	YES
Observations	325,402	325,402	325,402
Adjusted R^2	0.70	0.74	0.79

Table 4.8 Instrumental variable analysis

This table reports the results of two-stage-least-squares regressions of predicting corporate credit spreads with partisan conflict index as

$$
\begin{aligned}
\text{Credit spread}_{i,t} = \alpha &+ \beta_1 \widehat{\text{Partisan conflict}}_{t-1} + \beta_2 \text{Firm control}_{i,t} + \beta_3 \text{Bond control}_{i,t} \\
&+ \beta_4 \text{Macro control}_t + \beta_5 \text{Uncertainty control}_t + \beta_6 \text{Sentiment control}_t \\
&+ \gamma_i + \varepsilon_{i,t},
\end{aligned}
$$

where control variables are the same as Table 4.2. $\widehat{\text{partisan conflict}}$ refers to the fitted value of partisan conflict using party polarization and mass shooting dummy variable as instrumental variables. All independent continuous variables are normalized. Reported are regression slopes, t-statistics, and R^2s. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. The sample period is 2002:07–2018:12.

	Party polarization		Mass shootings	
Partisan conflict	3.67***	3.81***	3.52**	4.47***
	(2.84)	(2.82)	(2.16)	(2.63)
Market leverage	3.02**	3.01**	3.11**	3.10**
	(2.12)	(2.04)	(2.17)	(2.16)
Operating income to sales	−0.26	−0.19	−0.30	−0.24
	(−0.47)	(−0.33)	(−0.53)	(−0.44)
Total debt ratio	0.26*	0.26*	0.25*	0.25*
	(1.87)	(1.84)	(1.80)	(1.87)
Pre-tax coverage D1	−2.18	−2.12	−2.22	−2.14
	(−0.87)	(−0.86)	(−0.85)	(−0.85)
Pre-tax coverage D2	−3.25*	−3.24*	−3.15*	−3.13*
	(−1.74)	(−1.78)	(−1.65)	(−1.66)
Pre-tax coverage D3	−2.41*	−2.47*	−2.36*	−2.42*
	(−1.85)	(−1.88)	(−1.85)	(−1.87)
Stock return	−0.37	−0.34	−0.28	−0.31
	(−0.91)	(−0.95)	(−0.71)	(−0.80)
Idiosyncratic risk	9.83***	9.87***	9.81***	9.76***
	(4.98)	(4.89)	(4.93)	(4.71)
Illiquidity	3.31***	3.35***	3.16***	3.14**
	(3.15)	(3.06)	(3.31)	(3.19)
Coupon	59.25***	59.59***	59.52***	59.56***
	(15.32)	(14.94)	(16.05)	(16.11)
Credit rating	19.34***	19.14***	18.99***	18.64***
	(2.80)	(2.81)	(2.65)	(2.62)
Maturity	−4.46***	−4.38***	−4.36***	−4.35***
	(−6.47)	(−6.58)	(−6.87)	(−6.85)
S&P return	−2.32***	−2.13**	−2.19***	1.74
	(−4.16)	(−2.51)	(−2.80)	(1.43)
Term slope	5.83*	4.89**	9.75**	9.12**
	(1.91)	(2.04)	(2.26)	(2.08)
10-year Treasury rate	−46.69***	−45.68***	−49.88***	−48.44***
	(13.29)	(−17.30)	(−15.24)	(−15.38)
Expected unemployment	5.06***	4.41**	1.01	−0.60
	(2.97)	(2.29)	(0.46)	(−0.29)
Expected one-year inflation	1.70	1.53	2.05	1.64
	(1.21)	(1.02)	(0.96)	(0.73)
VIX index		6.12***		6.01***
		(4.51)		(3.89)
Macroeconomic uncertainty		3.28*		3.72**
		(1.70)		(1.97)
Economic policy uncertainty		0.27		0.68
		(0.26)		(0.62)
Geopolitical risk		0.04		−0.30
		(0.06)		(−0.44)
Political risk		−0.57		−0.56
		(−0.88)		(−0.90)
Consumer confidence		−1.19		−1.43*
		(−1.29)		(−1.75)
Political sentiment		−0.06		−0.26
		(−0.09)		(−0.41)
Credit-market sentiment		0.49		0.18
		(0.54)		(0.25)
Firm fixed effect	YES	YES	YES	YES
Firm cluster	YES	YES	YES	YES
Year cluster	YES	YES	YES	YES
Observations	325,402	325,402	325,402	325,402
Adjusted R^2	0.72	0.81	0.74	0.80

Table 4.9 Subsample analysis

This table reports the results of predicting corporate credit spreads with partisan conflict index as Eq.(4.1). The partisan conflict index is from Azzimonti (2018). We stratify the panel into subsets based on firm sizes, bond maturity, and firm leverage ratios. Controls are the same as in column (3) of Table 4.2. All independent continuous variables are normalized. Reported are regression slopes, t-statistics, and R^2s. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. The sample period is 2002:07–2018:12.

	DepVar: Corporate credit spreads								
	Size			Maturity			Leverage		
	Small	Medium	Large	Short	Medium	Long	Low	Medium	High
Partisan conflict	1.63^{***}	1.08^{*}	0.65^{**}	1.86^{**}	0.24^{*}	1.98^{**}	0.31^{**}	0.75^{*}	1.48^{**}
	(2.71)	(1.75)	(1.98)	(1.96)	(1.67)	(2.03)	(2.10)	(1.80)	(2.09)
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES
Firm fixed effect	YES	YES	YES	YES	YES	YES	YES	YES	YES
Firm cluster	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year cluster	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observation	100,276	139,815	85,311	46,822	105,201	173,379	99,226	140,375	85,801
Adjusted R^2	0.82	0.81	0.81	0.83	0.82	0.81	0.80	0.83	0.82

Table 4.10 Robustness: credit spreads and alternative partisan conflict indexes

This table reports the results of predicting corporate credit spreads with partisan conflict index using alternative detrending methods as

$$\begin{aligned}\text{Credit spread}_{i,t} = \alpha &+ \beta_1 \text{Partisan conflict}^{Alt}_{t-1} + \beta_2 \text{Firm control}_{i,t} + \beta_3 \text{Bond control}_{i,t} \\ &+ \beta_4 \text{Macro control}_t + \beta_5 \text{Uncertainty control}_t + \beta_6 \text{Sentiment control}_t \\ &+ \gamma_i + \varepsilon_{i,t},\end{aligned}$$

where control variables are the same as Table 4.2. Partisan conflict$^{Alt}_{t-1}$ refers to partisan conflict index without detrending (Panel A), with log quadratic detrending (Panel B), constructed using political disagreement dictionary (Panel C), and constructed using government dictionary (Panel D). All independent continuous variables are normalized. Reported are regression slopes, t-statistics, and R^2s. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. The sample period is 2002:07–2018:12.

	DepVar: Corporate credit spreads		
Panel A: No detrending			
	(1)	(2)	(3)
Partisan conflict	1.04**	0.98**	1.15***
	(2.21)	(1.98)	(2.69)
Controls	YES	YES	YES
Firm fixed effect	YES	YES	YES
Firm cluster	YES	YES	YES
Year cluster	YES	YES	YES
Observations	325,402	325,402	325,402
Adjusted R^2	0.71	0.75	0.80
Panel B: Log quadratic detrending			
	(1)	(2)	(3)
Partisan conflict	1.07***	1.17**	1.19***
	(2.61)	(2.45)	(3.17)
Controls	YES	YES	YES
Firm fixed effect	YES	YES	YES
Firm cluster	YES	YES	YES
Year cluster	YES	YES	YES
Observations	325,402	325,402	325,402
Adjusted R^2	0.72	0.75	0.82
Panel C: Political disagreement dictionary			
	(1)	(2)	(3)
Partisan conflict	1.61***	1.18**	1.50***
	(3.81)	(1.97)	(3.60)
Controls	YES	YES	YES
Firm fixed effect	YES	YES	YES
Firm cluster	YES	YES	YES
Year cluster	YES	YES	YES
Observations	325,402	325,402	325,402
Adjusted R^2	0.72	0.74	0.82
Panel D: Government dictionary			
	(1)	(2)	(3)
Political disagreement	1.58***	1.57**	1.93***
	(3.05)	(1.96)	(2.94)
Controls	YES	YES	YES
Firm fixed effect	YES	YES	YES
Firm cluster	YES	YES	YES
Year cluster	YES	YES	YES
Observations	325,402	325,402	325,402
Adjusted R^2	0.71	0.72	0.81

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