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DOES SHAREHOLDER ACTIVISM HAVE A LONG-LASTING IMPACT ON COMPANY VALUE? A META-ANALYSIS

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Does Shareholder Activism Have a Long-Lasting Impact on Company Value? A Meta-Analysis

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Abstract:
I collect 1,186 reported estimates of long-run value creation from 49 studies and present the first meta-analysis of the literature on shareholder activism and its effect on firm value in the longrun. This synthesis is necessary because shareholder activism is increasing over time and across countries. The study shows that the conclusion as to whether activism is beneficial depends on the benchmark with which the targeted firm is compared. Second, the estimates are also overstated due to publication bias. Third, the value added by activism depends on the country-specific regulátory framework.

JEL: G14, G30, G34, L20
Keywords: Abnormal return, event study, long run, meta-analysis, model averaging, publication bias, shareholder activism

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All errors and omissions remain the fault of the author.

1. Introduction

Shareholder activism refers to action taken by shareholders – either individuals or institutions such as pension funds and hedge funds – to improve a firm's efficiency, change its capital structure, affect managerial decisions, or protect minority shareholders (Denes et al., 2017). It is an increasingly prominent feature of corporate governance. Financial Times (2020) cites Jim Rossman, head of shareholder advisory at Lazard, as saying that "activism has become a permanent feature of the corporate landscape." Furthermore, Lazard's Capital Market Advisory Group recently noted a revival of shareholder activism in their review of shareholder activism in the first half of 2022. Such activism usually takes the form of shareholder proposals, direct negotiations, proxy contests, and/or media pressure. The latest campaigns are targeted mainly at technology firms, which are the backbone of the modern economy (Lazard, 2022). Moreover, the report highlights a growing number of investors initiating activist campaigns for the first time ("first timers"). This means that more and more shareholders are engaging actively in the debate about how their firms are run. The report also mentions a strong upsurge of activism in Europe caused by challenging macroeconomic conditions, which are heightening the urgency of enhancing corporate governance.

In this paper, therefore, I aim to quantify the effect of long-run shareholder activism on firm value and examine how it varies with different activism characteristics. For the analysis, I chose the meta-analytical approach, which is useful predominantly in areas of extensive prior empirical research. A meta-analysis is a quantitative review of all the empirical results in a given field of research. It is an effective way to determine "true" effects in a realm of contradictory findings. Since it works with many independent variables from primary studies, it allows for addressing endogeneity problems while showing the most critical drivers of the relationship under scrutiny. Last, it sheds light on the impact of economic fundamentals such as the degree of market development (Kim et al., 2019).

I collected 1,186 reported estimates of long-run value creation from 49 studies published between 1983 and 2022. I identified 44 variables that may explain the heterogeneity in the harvested estimates. They include the types of sponsors initiating activism campaigns, their attitudes, objectives, and success, and researcher and methodology data. In addition, I control for external factors such as anti-director rights and regulatory thresholds (Djankov et al., 2008; Becht et al., 2017). In performing my meta-analysis, I follow the seminal work in the field, such as Havranek et al. (2017), Bajzik et al. (2020), and Gechert et al. (2022). I use state-of-the-art techniques to correct for publication bias. I start with the funnel plot proposed by Egger et al. (1997) and add several formal tests, such as panel fixed effects and weighted least squares (Stanley and Doucouliagos, 2012). Furthermore, I use the latest advances in the field, for example, the kinked approach (Bom and Rachinger, 2019), the stem-based method (Furukawa, 2019), and nonlinear estimation (Andrews and Kasy, 2019). Consistent with my expectations, I find publication selection bias in the reported estimates of long-run price responses to shareholder activism events. This bias means that the unconditional mean of the estimates reported in the literature is substantially overstated. In other words, the prior empirical literature exaggerates the value that shareholder activism creates.

The second part of my analysis studies the substantial heterogeneity in the characteristics of the individual estimates. The meta-analysis allows me to contribute to the literature by investigating the collected control variables simultaneously and therefore to measure their combined effect. I use Bayesian Model Averaging (BMA), which addresses the omitted variable problem, parsimony issues, and the best model selection problem all at once (Raftery et al., 1997; Eicher et al., 2011; Feldkircher and Zeugner, 2012; Moral-Benito, 2015; Steel, 2020). I find that the type of activism,

activism by hedge funds and individual investors, and the objective of activism matter little in long-run value creation by shareholder activism. On the other hand, the estimate of value added is affected by the choice of the benchmark with which the targeted firm is compared. Specifically, when one takes the overall market as the benchmark, the abnormal returns reported for the targeted firm are about 6% (9%) higher than when one compares the targeted firm with its non-targeted counterparts (industry mates). In addition, the value added by shareholder activism is lower in countries with stronger "antidirector" rights (La Porta et al., 1997; Djankov et al., 2008). Specifically, with each one percentage point increase in rights, the value created by shareholder activism is lowered by 5.9%. Conversely, with each one percentage point increase in the regulatory reporting threshold, the value added by activism decreases by 7.1%. The other significant findings are more or less intuitive. When one uses a simple market-adjusted model, the results are overstated. Next, the value added is higher when activism is successful than when it is unsuccessful or when its outcome is not defined. Last, I propose implied estimates based on the most recent and best articles published in the field. I conclude that the value creation by shareholder activism corrected for the publication bias is positive, but only in case when one compares the targeted firm with the market. When one compares the firm of interest with a similar but non-targeted firm or with the firm's industry, the long-term benefits of shareholder activism are inconclusive.

The remainder of the article is organized as follows. Section 2 explains in detail the motivation for studying this topic. Section 3 discusses the process of data collection from the primary studies and how the studies were chosen. Section 4 discusses the presence of publication bias in this area of research. Section 5 explains the heterogeneity across the estimates from the primary studies and proposes implied estimates. Section 6 summarizes the paper. The Appendix provides additional data information and robustness checks of the results.

2. Motivation

Two broad approaches have usually been used to study the value created by shareholder activism. The first is a short-run one using the widely accepted event study methodology (e.g., Brown and Warner, 1985). This approach approximates the value created by shareholder activism using the direct price response to the announcement of activism or to the initial filing. The event window ranges from a few days to one month around the announcement date (Denes et al., 2017; Brav et al., 2008b). The short-run approach captures how investors evaluate their expected income from shareholder activism. The second, long-run approach reveals the overall impact on the firm's operating performance and governance through superior long-run stock returns (Mitchell and Stafford, 2000). On the one hand, this approach seems appealing, since it measures the actual impact of activism instead of mere expectations. On the other, it has several limitations. First, there needs to be a consensus on the methodology for long-run returns. Croci (2007) states: "Differently from the measurement of short-run abnormal returns, there is no generally accepted methodology for long-run returns." Second, long-run estimates might be subject to other influences unrelated to shareholder activism itself (Filatotchev and Dotsenko, 2015). I therefore compare the different measures of long-run returns and model the legal frameworks in which the various estimates were collected.

Besides the motivation provided by Lazard (2022) and Financial Times (2020), both the literature and the report (Lazard, 2022) detect other motivations for studying shareholder activism.[1] For

[1] Needless to say, the literature only captures legal events (filings, proposals, buying shares above threshold) and public events (open letters, media pressure).

example, the various tools that activist investors use have evolved over time. In the past, the standard form of activism involved pension funds submitting shareholder proposals on remuneration or voting practices (Holderness and Sheehan, 1985; Wahal, 1996; Smith, 1996). Subsequently, direct negotiation with management and litigation increased in importance (Denes et al., 2017). After the turn of the millennium, the whole concept of shareholder activism became dominated by an increasingly aggressive approach of hedge funds (Boyson and Mooradian, 2011; Bessler et al., 2015; Becht et al., 2017). Lastly, Lazard (2022) draws attention to the recent growing popularity of proxy contests directed at board representation. Based on these two observations – the increasing importance of shareholder activism and its evolving nature over time – it is crucial to study how much value is created by shareholder activism and whether and how value creation differs with various forms of activism.

Moreover, the benefits and shortcomings of shareholder activism are still surrounded by substantial controversy (Brav et al., 2008a). Activist campaigns can indeed spur a firm's management into making changes they would otherwise resist, and can thus enhance its economic performance and increase its value (Brav et al., 2008a; Klein and Zur, 2009; Becht et al., 2017; Maffett et al., 2022). For instance, UK institutional investors in 2014 founded a body that "serves as an "escalation mechanism" when firms ignore individual investors or exhibit problems that worry many shareholders" (Economist, 2018). In addition, activist raids and campaigns may reduce agency problems between managers and owners due to the detachment of ownership and control (Jensen and Meckling, 1976). Conversely, for instance, Maffett et al. (2022) note that shareholder activism can have negative consequences. Challenging managerial practices may destabilize the company (O'Rourke, 2003) or distract management from long-term projects (Brav et al., 2008b). Shareholder activism may thus suppress innovation and force managers to resort to more defensive strategies (Maffett et al., 2022). O'Rourke (2003) says that even well-intended activism may lead to confrontation and cause a negative relationship between the activist and the company. Hence, activism may lead to reputational damage and increase litigation risk. Targets of activism generally experience more accounting-related lawsuits and pay higher audit fees (Bourveau and Schoenfeld, 2017; Guo et al., 2021).

The negative consequences of shareholder activism have also been noticed by regulators worldwide. They may thus consider imposing restrictions on activists' ability to accumulate substantial ownership stakes and impose their will on how a company is managed. For example, O'Rourke (2003) point out that in the US "there are some legal restrictions that limit the use of resolutions, such as the SEC restricting proposals to 'ordinary business' issues that are not mundane." The regulation of activism has evolved in parallel with successful forms of activism (Maffett et al., 2022). In addition, in Europe, some prominent legal commentators, European regulators, and leading corporate lawyers have called for restrictions on hedge fund activism because of its possible short-term orientation (Brav et al., 2008a). In Japan, "it has not been uncommon for courts to allow incumbent managers to enforce poison pill arrangements that prevent activists from forcing change on incumbent managers" (The Economist, 2007). The importance of various legal frameworks is pointed out in an international study by Becht et al. (2017), who note that the country-specific legal framework and institutions are critical factors in determining positive returns to activism.

3. Data Collection

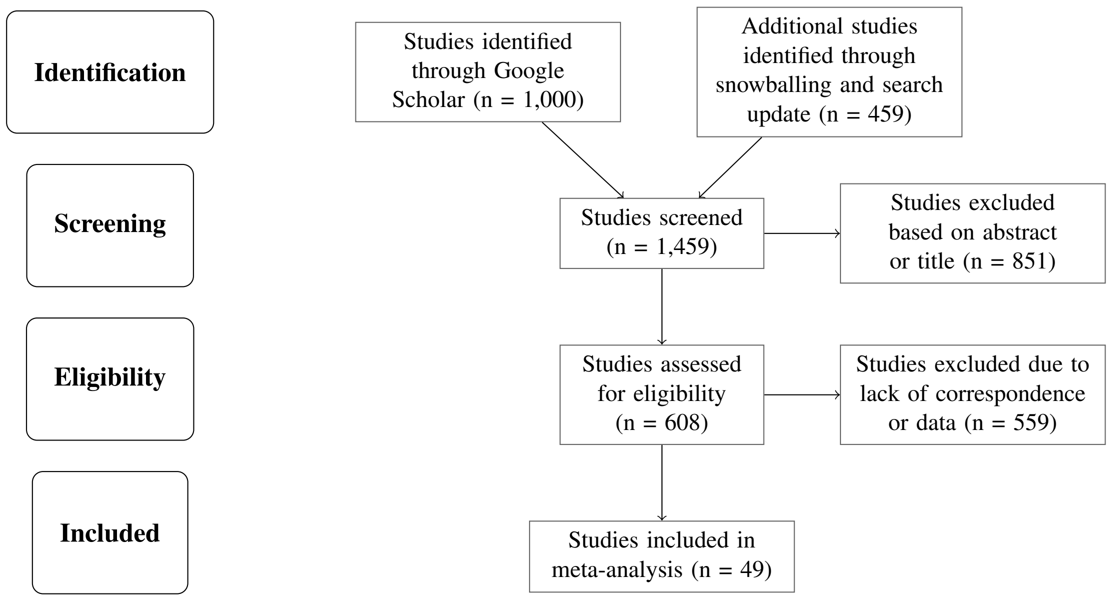
During the data collection process, I followed the guidelines proposed by Havránek et al. (2020), Steel et al. (2021), and Kepes et al. (2013). First, I scrutinized the seminal overviews in this research area to identify studies that might be relevant for further analysis. These overviews include Filatotchev and Dotsenko (2015), Denes et al. (2017), and Albuquerque et al. (2021). Second, based on this knowledge, I developed a search query for Google Scholar to identify all relevant studies. The search query was calibrated using the following combination of keywords: "abnormal return" AND "activist investor" OR "investor activism" OR "activist shareholder" OR "shareholder activism" OR "shareholder proposal" OR "contested proposal" OR "hedge fund activism" OR "proxy contest" OR "proxy fight" OR "negotiation" OR "litigation" OR "takeover." The advantage of the Google Scholar algorithm is its ability to consider an entire study regardless of the precise formulation of its title, abstract, and keywords. This increases the coverage of relevant published estimates relative to the simple title or abstract algorithms. I downloaded and examined the first 1,000 articles recommended by the search query. I then conducted a "snowballing" procedure on all the articles identified, meaning I investigated the reference lists of the relevant studies to make the search as comprehensive as possible. I ran the search in November 2021 and re-ran it at the end of March and August 2022 to check for articles written from 2020 onward. Finally, I snowballed the new articles and then terminated the search.

To enhance the reliability of the results, I discarded any papers not published in a research journal, i.e., I withdrew any theses, discussion papers, and working papers. On the one hand, publication bias should not differ significantly between published and unpublished studies (Doucouliagos and Stanley, 2013). On the other, my *a priori* expectation is that published estimates are less likely to be affected by flawed methodology or measurement errors. The rest of the articles have to meet the following criteria. (1) The article has to discuss shareholder activism. (2) The article has to present abnormal returns surrounding the shareholder activism event date. (3) The t-statistic, the standard error, or other statistics from which the standard error can be calculated, must be reported for each estimate. (4) The effect of shareholder activism on stock returns has to be measured in the long-run, i.e., the end period of the abnormal returns window must be more than one month after the event date (Brav et al., 2008b; Denes et al., 2017). A comprehensive overview of the paper selection process, PRISMA, is provided in Figure 1.

Altogether, the collection process yields 1,186 estimates of long-run value creation by shareholder activism from 49 studies. An overview is given in Table A1. Figure 2 shows the distribution of the collected estimates. The studies are based on cross-sectional data with study-specific time periods and countries.

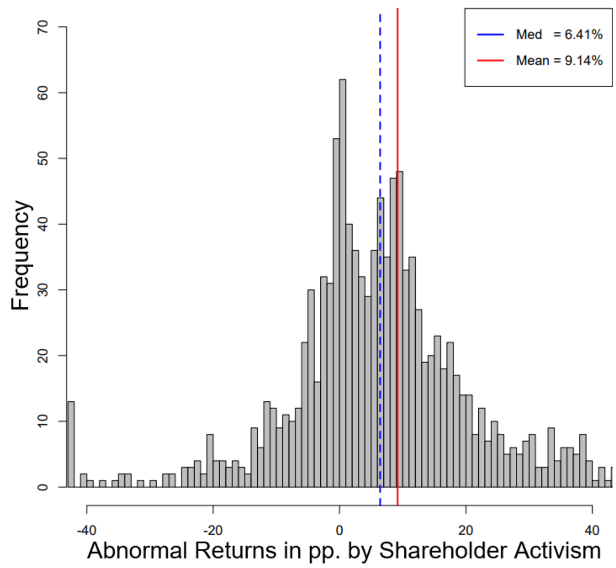
The distribution has an overall mean of 9.14% and a median of 6.41%, so one can conclude that the skewness is positive (numerically 1.076). Moreover, as can be seen in Figure 2, the distribution has two peaks, one around zero and the other around 9%. Such bimodality is probably caused by high heterogeneity in the primary data. For example, diverse activists might be associated with diverse returns (Mietzner et al., 2011; Filatotchev and Dotsenko, 2015). Next, there are many approaches to activism, from shareholder proposals to litigation and media pressure (Hamao and Matos, 2018; Bassen et al., 2019). In addition, activism might be successful or unsuccessful and depend on the country-specific legal framework (La Porta et al., 1997; Djankov et al., 2008; Goodwin and Rao, 2014). The model chosen by the researcher might also cause differences (Del Guercio and Hawkins, 1999; Gao and Brooks, 2010). I will study these and other factors in section 5. However, before that, I will scrutinize whether the effect has the value reported in the primary literature or whether it is subject to publication bias.

Figure 1: Preferred Reporting Items for Systematic Reviews and Meta-Analyses (PRISMA) Flow Diagram



Note: Preferred Reporting Items for Systematic Reviews and Meta-Analyses (PRISMA) is an evidence-based set for reporting in meta-analyses and systematic reviews. Havránek et al. (2020) provide details on PRISMA and expected meta-analysis standards.

Figure 2: Distribution of Value Creation Estimates



Note: Figure 2 depicts the distribution of long-run value creation by shareholder activism together with its mean and median. Abnormal returns above 40% and below -40% were discarded for ease of exposition, but are included in every computation in this paper after 1% winsorization.

4. Publication Bias

Despite the fact that recent articles discuss the costs and benefits of shareholder activism (Maffett et al., 2022), the articles that study the relationship empirically are usually very positive about the effects of shareholder activism on firm value. For example, Albuquerque et al. (2021) write:

> *"The consensus is that these returns are significantly larger than those following the announcement of new passive positions (e.g., Holderness and Sheehan, 1985; Brav et al., 2008a; Klein and Zur, 2009; Edmans et al., 2013). Indeed, in our data, the average return following the public announcement of activist intent via the filing of a Schedule 13D with the Securities and Exchange Commission (SEC) is 6.34%. Following announcements of passive investment, i.e., Schedule 13G filings, the return is only 0.59%."*

This assertion does not seem misleading until one realizes that this *consensus* omits findings from other articles, such as Song and Szewczyk (2003), which declares that there is no statistically significant difference between the findings from the targeted firm and the control firm. There is consequently a danger that at least part of the literature is built on prior knowledge that is biased toward positive estimates. I therefore want to study the publication bias phenomenon in this area of research.[2]

Publication bias describes the fact that the results reported by researchers differ from those they actually achieved. The phenomenon probably originates in a conscious or subconscious inclination to publish statistically significant estimates corresponding to the *a priori* expectation regarding the nature of the relationship of interest. It may seem rational to withhold implausible results. However, the systematic discarding of some results, even from independent publications, creates a bias in the reported results and distorts the average magnitude and significance of the effect. Prior research exposes selective reporting in various topics in economics (Ugur et al., 2018; Campos et al., 2019; Blanco-Perez and Brodeur, 2020; Brown et al., 2021; Gechert, 2022) and finance (Zigraiova and Havranek, 2016; Geyer-Klingeberg et al., 2018; Astakhov et al., 2019; Gric et al., 2023).

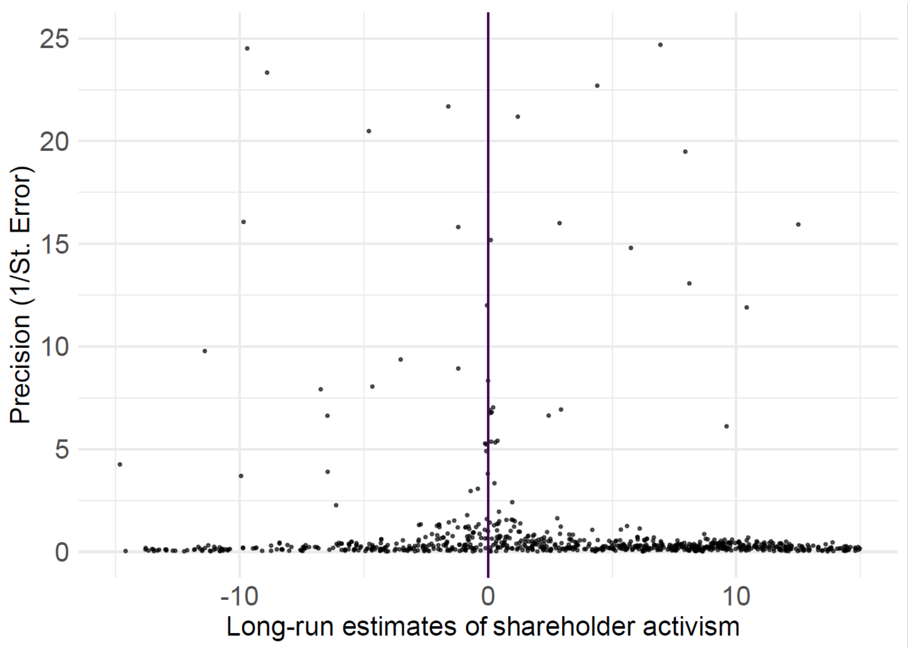
Therefore, in investigating the long-run value effects of activism, I utilize the merits of meta-analysis and evaluate how much the published results are affected by selective reporting. In the analysis, I follow a common research practice. I start the analysis with the simple graphical visualization proposed by Egger et al. (1997). I then move to formal tests of the relationship via linear and state-of-the-art nonlinear tests (Andrews and Kasy, 2019; Furukawa, 2019). While the graphical visualization indicates the possible presence of publication bias, the formal tests show whether publication bias is present and provide an estimate of the mean corrected for publication bias. I complete the analysis by testing for selective reporting around typical significance thresholds (Bruns et al., 2019).

The first insights into the publication bias phenomena are provided by Figure 3, which plots the 1,186 collected estimates on the horizontal axis and the corresponding inverse of their standard error (1/*SE*) on the vertical axis. The expected shape of the graph is an inverted funnel, since the most precise estimates should be close to the underlying mean effect and less precise ones should be

[2] I study publication bias on the whole sample, since there is no clue on how to divide the sample into positive and negative values, because even negative estimates can come from studies that suppose positive outcomes. I consider this to be the correct approach, since there is a strong consensus in the literature on the positive effect of such activism and because the disadvantages have been discussed only recently and without empirical estimates (Maffett et al., 2022). Moreover, there are not enough observations with negative values (about 30% of the sample).

scattered. Without a tendency to publication selection, the funnel plot will be symmetrical, because there is an equal probability of estimates with the "correct" and "wrong" signs being published. If the literature is biased, for instance, shareholder proposals will act in a negative direction, or hedge fund activism will be skewed to the left; both of these phenomena would make the funnel asymmetric. If imprecise estimates are discarded, the funnel will show some hollow parts.

Figure 3: Funnel Plot Indicates Publication Bias in the Sample



Note: Without publication bias, the plots would create an inverted funnel plot symmetric around the most precise estimates. Estimates with precision above 25 are excluded for ease of exposition. Nevertheless, they are included in the analysis.

As can be seen from Figure 3, the overall distribution of long-run value creation by shareholder activism has an atypical distribution. Besides the classical funnel shape around zero and a fatter right tail of the distribution, which might indicate the presence of publication bias, there is a larger number of positive and negative estimates that are very precise, i.e., with precision above 5. This observation shows that there is considerable heterogeneity across the estimates and that primary studies found – probably under different circumstances – both strongly negative and positive significant estimates. Nevertheless, the presence of publication bias should be tested for formally using the following equation suggested by Stanley and Doucouliagos (2012):

$$\hat{x}_{ij} = \beta_0 + \beta_1 SE_{ij} + e_{ij}, e_{ij} \sim N(0, \sigma^2), \quad (1)$$

where $\hat{x}_{ij}$ stands for the i-th estimate of the value created by shareholder activism in the j-th study. $SE_{i,j}$ is its corresponding standard error. Parameter β_1 represents the intensity of the publication bias, while coefficient β_0 denotes the effect beyond this bias. Without any publication bias in the data, β_1 does not differ statistically from zero. If it does differ, there is a relationship between the estimate of interest and its standard error, indicating the presence of publication bias. Equation 1 also assumes a linear association between the estimate and its standard error. When both these assumptions hold, coefficient β_1 can be interpreted as a measure of publication bias, and the intercept, β_0, captures the mean coefficient corrected for publication bias.

I use five different approaches to estimate Equation 1. Employing various estimation techniques allows me to evaluate the robustness of the results. First, I employ OLS estimation with two-way clustering at the study and country level as suggested by Cameron et al. (2012). Using two-way clustering is recommended because higher or lower price response estimates may be concentrated in results from specific studies or countries. Next, I run fixed-effects (FE) and between-effects (BE) regressions. Study-level FE absorbs idiosyncratic study-level variation, and study-level BE treats the differences in the size of the 49 primary studies. As the fourth and fifth approaches, I follow Stanley and Doucouliagos (2012) and Astakhov et al. (2019) and weigh the equation. In the fourth specification, I use $1/SE(r_{ij})$ as the weight to assign more weight to more precise estimates. This specification, called "Precision", adjusts for potential heteroskedasticity in the sample. In the fifth specification, I use the inverse of the number of estimates per study as the weight. This specification, which assigns each study the same importance, I call "Study". The results for these approaches are summarized in Panel A of Table 1.

The tests presented in Panel A of Table 1 assume that the price responses and their standard errors are independent in the case of no publication bias and that the relationship between the price response estimate and the publication bias is linear. These assumptions hold in most research settings but might be violated in some cases (Stanley and Doucouliagos, 2014; Havranek et al., 2021). Therefore, I complete the publication bias analysis using four recent state-of-the-art techniques that do not insist on the independence and linearity assumptions – the Top10 method developed by Stanley et al. (2010), the stem-based model (Furukawa, 2019), kinked-regression by Bom and Rachinger (2019), and the selection model by Andrews and Kasy (2019). The Top10 method estimates the "true effect" in the studied relationship using only the most precise 10% of the estimates from the primary studies. The technique is based on the idea that the most precise estimates are unlikely to be affected by selective reporting. Furukawa (2019) builds on the Top10 method, but instead of discarding the least precise 90% of the estimates, he suggests an algorithm that identifies the optimal trade-off between the potential bias and the size of the data sample. He then estimates the "true effect" based on the subset detected. The technique proposed by Bom and Rachinger (2019) searches for an "endogenous kink" in the reported precision in the primary studies, which would be characteristic of selective reporting. This approach draws on the idea that the intention to withhold some results might change with the precision of the estimate, making the scrutinized relationship linear in only some intervals. Last but not least, the selection model proposed by Andrews and Kasy (2019) allows potential discontinuities in the distribution of p-values around conventional cut-offs for statistical significance. Some argue that this approach should remain unbiased under publication bias (Havranek and Sokolova, 2020). The results for these state-of-the-art approaches are summarized in Panel B of Table 1.

The results presented in Panel A of Table 1 point to substantial publication bias across the empirical studies scrutinizing the effect of shareholder activism on created value. All five β_1 coefficients are positive, consistent with pressure to publish the positive effects of activism campaigns. Four of these five coefficients are significant even at the 1% level. The remaining estimate is only slightly below the 10% level (coef. 0.329, std. err. 0.205).

Panel A of Table 1 also contains the effect beyond bias, meaning what the value added by shareholder activism would be in the absence of publication bias. The effect beyond bias is measured by the intercepts β_0. Again, β_0 is positive and significant in four out of the five cases. Two of these coefficients are significant at the 5% level and the other two even at the 1% level.

Table 1: Publication Selection Bias

	OLS	FE	BE	Study	Precision
Panel A – Linear Estimation Methods					
Effect beyond bias	3.743***	4.347**	3.494**	4.389***	1.274
	(1.265)	(1.986)	(1.564)	(1.305)	(0.825)
	[1.752, 5.969]			[2.289, 6.485]	[-0.592, 3.454]
Publication bias	0.582***	0.517***	0.508***	0.329	0.848***
	(0.196)	(0.186)	(0.184)	(0.205)	(0.226)
	[0.216, 0.877]			[0.033, 0.686]	[0.496, 1.252]
Panel B – Nonlinear Techniques					
	Stanley et al. (2010)	Furukawa (2019)	Bom & Rach. (2019)	Andrews and Kasy (2019)	
Effect beyond bias	1.223	0.176	1.447***	2.901***	
	(0.974)	(0.675)	(0.165)	(0.067)	
Nobs.	1,186				
Studies	49				

Note: The uncorrected mean value creation by shareholder activism is 9.14%. The presented results are from regression $\hat{x}_{ij} = \beta_0 + \beta_1 S\hat{E}_{i,j} + e_{ij}$, where $\hat{x}_{ij}$ stands for the i-th value creation estimated in the j-th study and $\beta_1 S\hat{E}_{i,j}$ denotes the corresponding standard error. FE stands for fixed-effects and BE for between-effects. The Study column uses the inverse of the number of observations per study as the weight. Precision uses the inverse of the standard error as the weight. Standard errors clustered at the study level and 90% confidence intervals for the wild bootstrap in square brackets are reported wherever possible. The Stanley et al. (2010) Top10 method, the Bom and Rachinger (2019) approach, and the Andrews and Kasy (2019) and Furukawa (2019) techniques are described in the text. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Specifically, the corresponding β_0 coefficients range from 3.494% for the BE model to 4.389% for the model treating each study equally. The only insignificant (and also the lowest) estimate is from the *Precision* specification. This inconsistency may indicate that the most precise estimates are lower on average and that higher positive estimates are often less significant. This also is indicated by the fatter tale of Figure 3, where the more positive and less significant estimates are concentrated.

The results for recent state-of-the-art techniques are summarized in Panel B of Table 1. These techniques focus on the effect beyond bias and do not directly measure the significance and magnitude of the publication bias. Similarly to the *Precision* approach, the Top10 method by Stanley et al. (2010) and the stem-based method by Furukawa (2019) indicate that the most precise estimates are lower on average and that more positive estimates are often less significant. However, I do not suggest relying on these two models, since they are based upon the most precise estimates, which are clearly distributed across both positive and negative values (see Figure 3). These methods do not work well in samples with large heterogeneity. On the other hand, the effect beyond bias derived from the selection model by Andrews and Kasy (2019) attains almost the same range as the effect derived from the linear models.

4.1 Caliper Test

Besides testing whether the authors tend to publish estimates going in one direction while withholding those going in the other, I scrutinize a similar but slightly different phenomenon called p-hacking. This describes the situation where researchers tend to overreport statistically significant results compared to insignificant ones. Such behavior is usually evident around the conventional statistical thresholds of 10%, 5%, and 1%, in terms of t-statistics of value 1.65, 1.96, and 2.58.

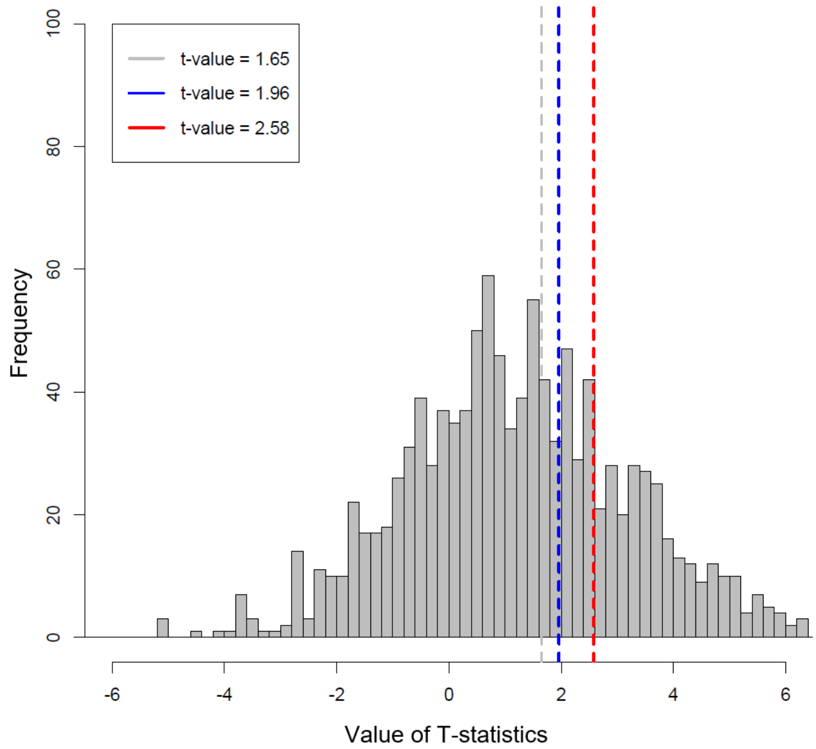
The reason for such overreporting is that a statistically insignificant result might be less attractive for publication because it does not provide clear evidence about the scrutinized relationship. This can occur where the empirical tests lack statistical power or where the relationship does not exist. Distinguishing between these two explanations is very challenging and often unfeasible. Therefore, statistically insignificant estimates are less informative than their statistically significant counterparts. Journal editors might thus be more inclined to publish significant results. As a result, authors are driven to select statistically significant results, and this causes bias (Harvey, 2017).

The tendency to underreport statistically insignificant results compared with significant ones might be observable from graphical visualization of the t-statistics, which in the ideal case should achieve normality. , in case of underreporting the insignificant estimates and overreporting significant ones, one may observe “jumps” in the distribution. Such discontinuities are expected around the mentioned conventional statistical levels. The distribution of the t-statistics for the estimates of long-run value creation by shareholder activism depicts Figure 4.

This simple graphical test shows that there is probably no p-hacking around the 1% and 10% levels but that there is probably a “jump” at the 5% level, considered the most crucial measure for significance in finance. This finding would support the previous conclusions about publication bias in shareholder activism and stock returns. Nevertheless, for such a conclusion, it is better to proceed with some formal tests rather than rely solely on graphical visualization.

To assess the test formally, I employ the Caliper test suggested by Gerber et al. (2008) and Gerber and Malhotra (2008) and improved by Bruns et al. (2019). In the case of no p-hacking, the volume of reported t-statistics above a threshold (the “over caliper”) should not be statistically different from

Figure 4: Distribution of the T-Statistics



Note: The figure depicts the distribution of the t-statistics corresponding to the long-run stock returns surrounding shareholder activism campaigns. The vertical dashed lines indicate the t-values of 1.645, 1.96, and 2.58, corresponding to statistical significance at the 10%, 5%, and 1% levels.

the volume of reported t-statistics below that threshold (the "under caliper"). Hence, the resulting ratio should not be statistically different from 0.5 (50:50). In addition, given that economic research is frequently underpowered (Ioannidis et al., 2017), Bruns et al. (2019) suggest a more lenient rule. They propose an over-to-under caliper ratio of 0.4. Following this logic, the frequency of t-statistics should decrease if p-hacking is absent. I performed the test using three Caliper sizes (0.025, 0.1, and 0.2).[3]

Table 2 summarizes the results of the Caliper tests. The evidence is not convincing. In the closest surroundings, there is obvious p-hacking at H1: $C \leq 0.4$ for both the 1.65 and 1.96 confidence levels, and even at H0: $C \leq 0.5$ for the 2.58 level. However, these findings are not confirmed in the broader surroundings, so one cannot draw a firm conclusion from this area of research.

Altogether, I ran one visual and nine formal tests for publication bias. These indicate a strong presence of publication bias in the sample. The results suggest that shareholder activism creates value, but the evidence is biased. After controlling for the bias, the "true effect" ranges between

[3] The lower is the caliper size, the more visible are the researchers' efforts to achieve the desired significance level (if the Caliper test is significant). The larger are the caliper sizes one uses, the weaker are the conclusions for overreporting, but they are still valid. This is noted, for example, by Gerber et al. (2008), who say that: "The fact that the disparities are most dramatic in the narrow regions nearest to the critical value provides additional evidence the imbalance is from publication bias rather than a chance occurrence."

1.447% and 4.389%. The bias exaggerates the estimates by about 0.33% to 0.58%. In addition, I broadened my search for bias in the literature to include a search in the area of p-hacking using the Caliper test, which suggests there is also some p-hacking in the sample, but the results are not solid and convincing. Lastly, one may object that my Equation 1 and, consequently, the results for publication bias suffer from the omitted variable problem. Therefore, I expand the search to include these possible drivers of heterogeneity and investigate them thoroughly in Section 5.

Table 2: Caliper Test

T-statistics	C	All	
1.65	0.025	*0.750*	*(0.440)*
	0.1	0.510	(0.389)
	0.2	0.465	(0.375)
1.96	0.025	*0.833*	*(0.497)*
	0.1	0.455	(0.327)
	0.2	*0.500*	*(0.412)*
2.58	0.025	**0.680**	**(0.517)**
	0.1	*0.500*	*(0.406)*
	0.2	0.446	(0.376)

Note: The table shows the results of the Caliper test for three caliper sizes: 0.025, 0.1, and 0.2, around three significant thresholds: 1.65, 1.96, and 2.58. The figures denote the share of the observations above and below the given threshold for statistical significance, i.e., the over-to-under caliper ratio. The numbers in brackets indicate the lower bounds of the 95% confidence intervals. I follow Bruns et al. (2019) and interpret confidence intervals with lower bounds above 0.4 as evidence of "*p*-hacking."

5. Drivers of the Relationship

In the second part of the analysis, I scrutinize the heterogeneity of the sample of value creation estimates. I collect more than 40 variables that capture various aspects of the primary estimates. These aspects include different sponsors of activism, activism type, declared objectives of activism, success of activism, event type, event window, and estimation methodology. Table 3 and Table A2 in the Appendix summarize their summary statistics and definitions. After a short discussion of the variables, I investigate how these aspects affect the magnitude of the value created by shareholder activism. As a result, I can propose estimates of the implied effect of shareholder activism.

Table 3: Description and Summary Statistics of the Additional Variables Explaining the Heterogeneity Across the Primary Studies

	Frequency		Standard statistics		Weighted statistics	
Variable	#Nobs	#Articles	Mean	SD	W.Mean	W.SD
All	1,186	49	9.14	19.51	6.93	16.57
Activism sponsors						
Hedge_funds	367	16	10.46	19.35	6.26	17.23
Pension_funds_&_oth._inst.	210	13	12.49	20.66	7.93	18.11
Individuals	51	6	9.63	13.24	10.87	13.81
Sponsor_na (*)	558	19	6.97	19.44	5.89	15.39
Activism approaches						
All_types (*)	306	18	9.10	17.43	5.91	15.43
Shareholder_proposal	54	5	7.06	11.88	11.2	14.19
Direct_negotiation	133	8	17.53	17.00	13.23	13.02

Continued on next page

Table 3: Description and Summary Statistics of the Additional Variables Explaining the Heterogeneity Across the Primary Studies (continued)

	Frequency		Standard statistics		Weighted statistics	
Variable	#Nobs	#Articles	Mean	SD	W.Mean	W.SD
Proxy_contest	568	17	7.64	20.44	5.79	17.02
Multiple_strategies	72	4	4.10	17.21	0.57	21.30
Media_pressure	53	3	13.43	28.13	9.85	18.03
Activism success						
Successful	183	22	10.07	21.53	6.76	19.86
Unsuccessful	133	15	4.34	16.12	4.76	14.68
Outcome_na (*)	870	45	9.68	19.46	7.25	15.83
Activism objectives						
Objective_general (*)	329	19	12.11	21.57	8.08	13.90
Performance	190	8	9.19	18.94	9.66	15.63
Governance	100	8	16.32	23.10	5.90	24.59
Board_seats	384	16	3.82	18.36	2.69	18.38
Sale	154	14	11.93	12.85	8.10	13.97
Other_objective	29	6	6.00	10.51	7.46	10.03
Institutional setting						
Hi_antidirector_rights	866	43	7.19	17.29	5.80	15.98
Lo_antidirector_rights	320	7	14.43	23.78	13.75	18.34
Hi_mrkt_cap	614	29	8.95	18.12	6.99	14.96
Lo_mrkt_cap	572	21	9.35	20.91	6.84	18.52
Hi_threshold	870	43	7.56	17.96	6.53	16.34
Lo_threshold	316	7	13.50	22.73	9.36	17.75
Event windows						
Around (*)	435	24	5.75	8.33	7.58	8.72
Half_post	159	17	8.35	10.07	4.67	8.64
Year_post	222	26	9.28	19.80	7.85	15.16
Two_y_post	71	16	9.4	15.28	9.52	17.08
3plus_post	299	17	14.33	31.14	5.62	29.38
Sample characteristics						
Long_sample	715	26	7.67	18.82	5.49	16.75
Short_sample	471	29	11.38	20.33	8.27	16.29
Recent_sample	655	30	10.52	18.26	7.87	15.01
Older_sample	531	20	7.45	20.85	5.44	18.67
Returns models						
Market_model (*)	291	16	6.36	11.32	7.82	10.14
Market_adjusted	110	8	15.09	21.97	5.88	19.08
3F_&_4F	184	9	11.96	14.75	7.95	12.03
BHAR	278	14	3.92	16.25	4.79	18.48
CTAR	150	6	16.00	29.34	18.61	22.66
Other_model	173	12	9.48	23.85	4.31	19.48
Benchmark setting						
Control_market (*)	1019	42	10.01	20.02	8.21	15.21
Control_industry	87	5	5.38	9.45	4.47	9.86
Control_firm	80	9	2.18	19.30	-0.90	24.57
Index weighting						
Equally	460	20	8.17	19.66	6.32	14.97
Value	231	16	7.24	18.07	7.52	12.68
Weighting_na (*)	495	29	10.94	19.90	7.02	18.79
Publication characteristics						
Hi_impact_fact	675	27	9.90	19.77	4.83	17.69
Lo_impact_fact	511	22	8.14	19.13	9.49	14.69
Hi_cited	689	29	10.07	19.54	5.96	17.65

Continued on next page

Table 3: Description and Summary Statistics of the Additional Variables Explaining the Heterogeneity Across the Primary Studies (continued)

	Frequency		Standard statistics		Weighted statistics	
Variable	#Nobs	#Articles	Mean	SD	W.Mean	W.SD
Lo_cited	497	20	7.85	19.42	8.33	14.76

Note: All the variables are described in Table A2 in the Appendix. SD = standard deviation. The table contains the set of variables describing the study designs used by the primary studies. Asterisks (*) denote the default categories for the regression analysis. Variables denoted by Hi-/Lo- are divided by their mean into upper and lower halves. They enter the final analysis in their continuous form.

Activism sponsors. The literature indicates that the value added by shareholder activism varies with different types of activists (Denes et al., 2017). For example, Smith (1996) and Prevost and Rao (2000) report statistically insignificant long-run returns for firms targeted by public pension funds. Song and Szewczyk (2003) find the same for firms from the Council of Institutional Investors' Focus List. The story is different for hedge fund activism. Greenwood and Schor (2009), Klein and Zur (2009), and Boyson et al. (2017) find positive and significant stock returns for long-run hedge fund activism. Brav et al. (2008a) argue that hedge fund managers are more incentivized to create value for their investors, since they have more space to launch and succeed with campaigns because of longer investment horizons and lock-up periods.

Different forms of activism can arise from investors' different objectives and tactics. For example, individual investors often address compensation-related topics via shareholder proposals, while institutional shareholders (e.g., pension funds) press for changes in performance and governance (Karpoff et al., 1996; Gillan and Starks, 2000). Hedge funds also focus on performance issues and changes in governance through proxy contests (Brav et al., 2008a; Klein and Zur, 2009; Bessler et al., 2015; Krishnan et al., 2016; Bebchuk et al., 2020). It is therefore vital to study different activists and their approaches and objectives.

Activism approaches. Studying shareholder proposals and negotiations, Del Guercio and Hawkins (1999), Prevost and Rao (2000), and Barber (2009) find insignificant results for shareholder activism. On the contrary, Mietzner et al. (2011) and Bessler et al. (2015) find direct negotiations to be associated with highly positive returns. This is also indicated by Table 3, which shows direct negotiations to be the most promising approach in the long-run. The literature assigns almost as high long-run abnormal returns to media pressure (17.53% for direct negotiations vs 13.43% for media pressure). This approach is studied, for example, by Filatotchev and Dotsenko (2015) and Bassen et al. (2019), but with its high standard deviation it proves to be very volatile or unstable (based on only three studies).

Proxy contests are at the same end of the spectrum as shareholder proposals. This finding is surprising when one considers the short-run literature discussing the effects of shareholder activism. In their survey, Denes et al. (2017) find proxy contests to be one of the most successful approaches, with returns of about 6.77%. Moreover, even in the long-run, some authors (e.g., Denis and Serrano, 1996; Croci, 2007) find proxy contests to be significantly positive in most cases. Nevertheless, others (e.g., Ikenberry and Lakonishok, 1993) find the long-run returns following proxy contests to be negative. Mulherin and Poulsen (1998) highlight a possible bias in proxy contest studies caused by the fact that acquired firms (successful activism) do not appear in long-run samples. Long-run proxy contest estimates are thus generally concentrated among firms that were not taken over and did not change their managers.

Activism success. Prior related literature also suggests that the value created by shareholder activism varies with the success of activist investors (e.g., Mulherin and Poulsen, 1998; Boyson et al., 2017). Some authors distinguish between successful and unsuccessful activism (e.g., Greenwood and Schor, 2009; Gao and Brooks, 2010; Wu and Chung, 2022), while others do not (e.g., Stadler et al., 2015; Boyson and Pichler, 2019). It is natural to expect the value created by activists to be greater when they succeed in their efforts (Boyson et al., 2017). Similarly, it is natural to expect unsuccessful activism to destroy some value, due to reputational damage to the targeted firm and subsequent greater auditor interest in it (Guo et al., 2021).

On the other hand, as mentioned in the previous paragraphs, it is not always trivial to distinguish between successful and unsuccessful activism. For instance, DeAngelo and DeAngelo (1989) find "extensive turnover, asset sales, mergers, liquidations, and other major changes, even in companies at which proxy contests are deemed unsuccessful." The same is found, for example, by Boyson et al. (2017). In addition, some firms might be targeted several times in consecutive years; this, too, can affect abnormal long-run returns (Wahal, 1996; Prevost and Rao, 2000). I also tried to distinguish between these differences, but I was forced to discard my efforts since I lacked knowledge of the primary data. I could therefore only study the basic distinction between successful and unsuccessful activism and the case where the researcher does not distinguish the success of the activism.

Activism objectives. The effect of shareholder activism may also differ based on the objectives pursued by individual activists (Denes et al., 2017). My baseline classification of activism objectives follows Brav et al. (2008a) and Greenwood and Schor (2009), who suggest that activists may target performance or governance issues, board seats, capital structure, business strategy, or even the sale of the company (Greenwood and Schor, 2009). Even at first glance, it can be seen from Table 3 that the literature reports different abnormal returns for particular activism objectives.

The abnormal returns are lowest on average for board seat representation. However, one can see from studying the distribution of the estimates that this low average is caused by some negative estimates (Bhabra and Wood, 2014), while in other cases the literature provides highly positive estimates (Alexander et al., 2010). The literature also suggests there is a difference between the performance and governance objectives (9% and 16% abnormal returns on average, respectively). In the case of the performance objective, the targeted company is usually classified as underperforming, hence one might expect positive returns to shareholder activism, whereas in the case of the governance objective, this is not necessarily so (Strickland et al., 1996; Anson et al., 2003; Nelson, 2006). Last but not least, the abnormal returns linked to the sale of the company are usually positive, as suggested by Brav et al. (2008a) and Greenwood and Schor (2009).

Institutional setting. Since there are 852 observations for the US, 331 for European countries, and only three for Asia (Japan), I decided to scrutinize the impact of differences in institutional setting across countries instead of focusing on the hollow classification of geographic regions. This should be the right way to address most of the variability in the sample from an institutional point of view. First, the institutional setting evolves, so within-country differences have to be captured. Second, the institutional setting differs from country to country, so cross-country variation needs to be addressed as well.

A better institutional framework empowers shareholder activists and helps them achieve their goals. In addition, a high-quality institutional framework promotes corporate transparency. It enables shareholder activists to better estimate whether a company is a suitable activism target. The quality of the institutional framework thus affects the long-run value created by shareholder activism. Prior meta-analyses in finance successfully show that a diverse institutional setting across countries affects the relationship of interest (e.g., Holderness, 2018; Bajzik et al., 2023).

I use three measures of institutional framework quality. First, I use the anti-director rights index constructed by La Porta et al. (1997, 1998) and refined by Djankov et al. (2008). It reflects the strength the country-level institutional setting gives shareholders to promote their interests in negotiating with management. Second, I use the ratio of stock market capitalization to gross domestic product to measure the size of the country's financial market. Third, I use the threshold at which shareholders must report their stake in the company to the market regulator. I even considered employing the origin of the country's law, i.e., whether it is of English, German, Scandinavian, or French type. However, I abandoned this attempt due to a lack of observations for law of Scandinavian origin and a lack of articles using countries with law of French origin.

Event windows. The subject of interest here is whether different time windows affect the magnitude and significance of shareholder activism. On the one hand, the focus of the analyses is to capture the cumulative abnormal returns around one event. Authors thus try to optimize the signal-to-noise ratio. Therefore, it is not *a priori* obvious whether longer or shorter windows are more suitable for the analysis. Shorter windows are less likely to be infested by confounding events. Conversely, longer periods might capture the process of pass-through of activism to firm value.

On the other hand, for example, Greenwood and Schor (2009) point out that the benefits of activism might accumulate over time, so the cumulative abnormal returns might do likewise. Their study found that activism created 3.84% abnormal returns in the filing month, increasing to 4.20% one month after and 10.28% 18 months after the activism. I therefore decided to split the sample according to windows into five groups: around the activism campaign and then half a year after, one year after, two years after, and three or more years after. The literature also suggests controlling for different event types that stand for day zero in measuring abnormal returns to shareholder activism. Such events usually include filing dates, press announcements, and proxy mail. These are very important in discussing the short-run effects of shareholder activism, since, by this measure, single days are important. However, I view this approach as unnecessary from the long-run point of view (Wahal, 1996; Weber and Zimmermann, 2013).

Sample characteristics. Further, I focus on two characteristics of the datasets used in the primary studies. I approximate the novelty of the data sample by the midpoint year of the sample period, and I control for the length of the data samples in years. The midpoint year of the data sample varies from 1964 (Dodd and Warner, 1983) to 2014 (Bessler and Vendrasco, 2022), and the length of the dataset ranges from one year (the GFC subsample in Bessler and Vendrasco 2022) to 30 years (Goodwin and Rao, 2014). These two characteristics help me detect any novelty bias (referring to the fact that some effects crop up when they are studied for the first time but are ultimately found to be non-existent) and small-sample bias (due to the unreliability of small samples). For ease of exposition, I convert these measures into indicator variables representing observations above and below the median, but they enter the final analysis in their continuous form. As can be seen from Table 3, the literature observes higher abnormal returns using short samples (11.38% vs 7.67%) and using recent data (10.52% vs 7.45%). Similar distinctions, but with lower price responses, are observable with the weighted means as well. I tried to collect information, for instance, about the industries of the targeted firms, but most of the primary studies collect information across the industries in their datasets.

Returns models. One of the main merits of this study, besides providing a comprehensive analysis of the diverse characteristics of the activism and institutional settings in the primary studies, is its investigation of diverse study designs and estimation methodology settings. Barber and Lyon (1997) and Kothari and Warner (1997) underline the importance of the choice of returns model, declaring that the simple market-adjusted returns model (e.g., Becht et al., 2017; Boyson et al., 2017) yields misspecified test statistics. Besides abnormal returns from the classical market-adjusted model and market model (e.g., Smith, 1996; Del Guercio and Hawkins, 1999), for example, Bhabra and Wood (2014) discusses the use of the buy-and-hold abnormal returns model (BHAR) and the calendar time abnormal returns model (CTAR). While the BHAR model (e.g., Bhabra and Wood, 2014; Stadler et al., 2015; Hamao and Matos, 2018) still suffers from cross-sectional dependence in sample observations and is classified as a poorly specified asset-pricing model by Kothari and Warner (1997) and Mitchell and Stafford (2000), the CTAR should outperform both the BHAR and market models. Besides these four models, the literature often uses the Fama-French three-factor and Carhart four-factor models (e.g., Smythe et al., 2015; Krishnan et al., 2016). I thus also distinguish these in the sample.

Benchmark setting. Besides the choice of model, the literature differs in the choice of model benchmark. Most authors compare the targeted firm with the overall market (e.g., Boyson et al., 2017; Bessler and Vendrasco, 2022). Recently, however, some authors have used the firm's industry as the benchmark (e.g., Greenwood and Schor, 2009; Venkiteshwaran et al., 2010; Gonzalez-Mulé and Aguinis, 2018) or have chosen a control firm based on similar market characteristics for comparison (e.g., Boyson and Mooradian, 2011; Goodwin and Rao, 2014; Wu and Chung, 2022). That the last two approaches should provide more reliable estimates is supported, for instance, by Barber and Lyon (1997), who suggest that the control firm should be of a similar size and book-to-market ratio. So, I control for this variation, too.

Some authors (e.g., Albuquerque et al., 2021) compare the targeted firms with the market and then the control firm with the market and, in the final step, test for the difference between the abnormal returns of the targeted and control firms. This addresses the Barber and Lyon (1997) critique as well. However, capturing this distinction is beyond the scope of this study.

Index weighting. In addition, I discern whether the primary studies use a value-weighted or an equally weighted index. Brown and Warner (1985) and Campbell and Wesley (1993) demonstrate that an equally weighted index yields more precise estimates of abnormal returns. Along these lines, Ikenberry and Lakonishok (1993) find the estimates from an equally weighted index to be lower on average than those from a value-weighted one. On the other hand, Denis and Serrano (1996), Nelson (2006), and Chen et al. (2020) do not detect any meaningful differences resulting from the use of one or the other index. Nevertheless, since the literature draws attention to the importance of model selection, I find it necessary to include these characteristics in the estimation as well.

Publication characteristics. The study only includes journal articles. This should not affect the results substantially (Doucouliagos and Stanley, 2013; Rusnak et al., 2013). However, even among journal articles, there are publication characteristics that might be correlated with unobserved features of the quality of the paper. For example, in a previous meta-analysis about stock returns, Gric et al. (2023) find that the journal's impact factor significantly affects the published results on the relationship between sentiment and stock returns, in the sense that articles published in higher-ranked journals contain less negative effects in their area of research. Similarly, Bajzik et al. (2020) find evidence that the results on Armington elasticity are affected by the number of citations contained in the relevant papers. These variables indicate whether some studies tend to be more cited than others. Thus, to make the estimation complete and comprehensive, I distinguish between the quality of the journal articles. I include the discounted recursive RePEc impact factor of the respective journal series, and I use the logarithm of the number of citations in Google Scholar normalized by the number of years since the first version of the study appeared in Google Scholar.

5.1 Model Averaging

Many variables that might explain the variation in shareholder activism were discussed in the previous section. Now I turn to examining these variables to evaluate which are the most powerful in explaining the variation in stock returns to shareholder activism. Furthermore, since the prior literature does not offer clear conclusions regarding the variables expected to be the most relevant in such an explanation, I consider all the variables mentioned in the previous section to be potentially relevant.

I acknowledge that such a large number of variables, namely 44, may be correlated in various ways. Moreover, due to such collinearity issues, it would be dubious to include them in a single regression model or in a structural equation model (Bergh et al., 2016). I need to find a way to address this problem together with the omitted variable problem, parsimony issues, and the best model selection problem. I found such an approach for my analysis among model-averaging techniques – Bayesian model averaging (BMA). BMA was pioneered in the social sciences by Raftery (1995) and Raftery et al. (1997). Its use in economics is summarized, for instance, by Moral-Benito (2015). In meta-analysis, it is used, for example, by Bajzik et al. (2020), Cazachevici et al. (2020), and Matousek et al. (2021).

BMA is aimed at finding the best approximation of the parameters. Based on my data, it would use the 2^{36} possible combinations of the variables (without interaction terms). It would be unfeasible to run such a computation, so I employ the Markov Chain Monte Carlo (MCMC) process with the Metropolis-Hastings algorithm (Zeugner et al., 2015). This algorithm only takes into account the most probable models. BMA assigns each model a weight with regard to its goodness of fit compared to the other models. This weight is called the posterior model probability (PMP). Next, the posterior inclusion probability (PIP) of each variable is computed from the PMPs of all the models estimated. This probability ranges from zero – the variable is not included in any of the models – to one – the variable is included in every model considered. The coefficients and standard deviations of every variable are computed as a weighted sum of the coefficients from the linear equations.

BMA setting. Following Eicher et al. (2011), I employ the unit information g-prior in my baseline specification. Its prior is that the regression coefficient is zero and has the same weight as one data observation. This setting reflects a lack of knowledge regarding the probability of each parameter value. Next, I employ the dilution model prior to tackle the potential collinearity problem across our 36 variables George (2010). The advantage of the dilution prior is that it gives models with highly collinear variables less weight. This is done by multiplying the model probabilities by the determinant of the correlation matrix, which equals one when there is no collinearity and is close to zero when the variables are highly collinear. This alleviation of collinearity is applied in meta-analysis by, for example, Bajzik et al. (2020); Gechert et al. (2022).

Robustness checks. As a robustness check, I apply the uniform model prior together with the unit information g-prior (UIP) (Eicher et al., 2011), the BRIC g-prior combined with the random model prior (Fernandez et al., 2001), and the Hannan-Quinn (HQ) g-prior together with the random model prior (Fernandez et al., 2001; Ley and Steel, 2009). In addition, I apply simple OLS to significant variables from the BMA exercise.

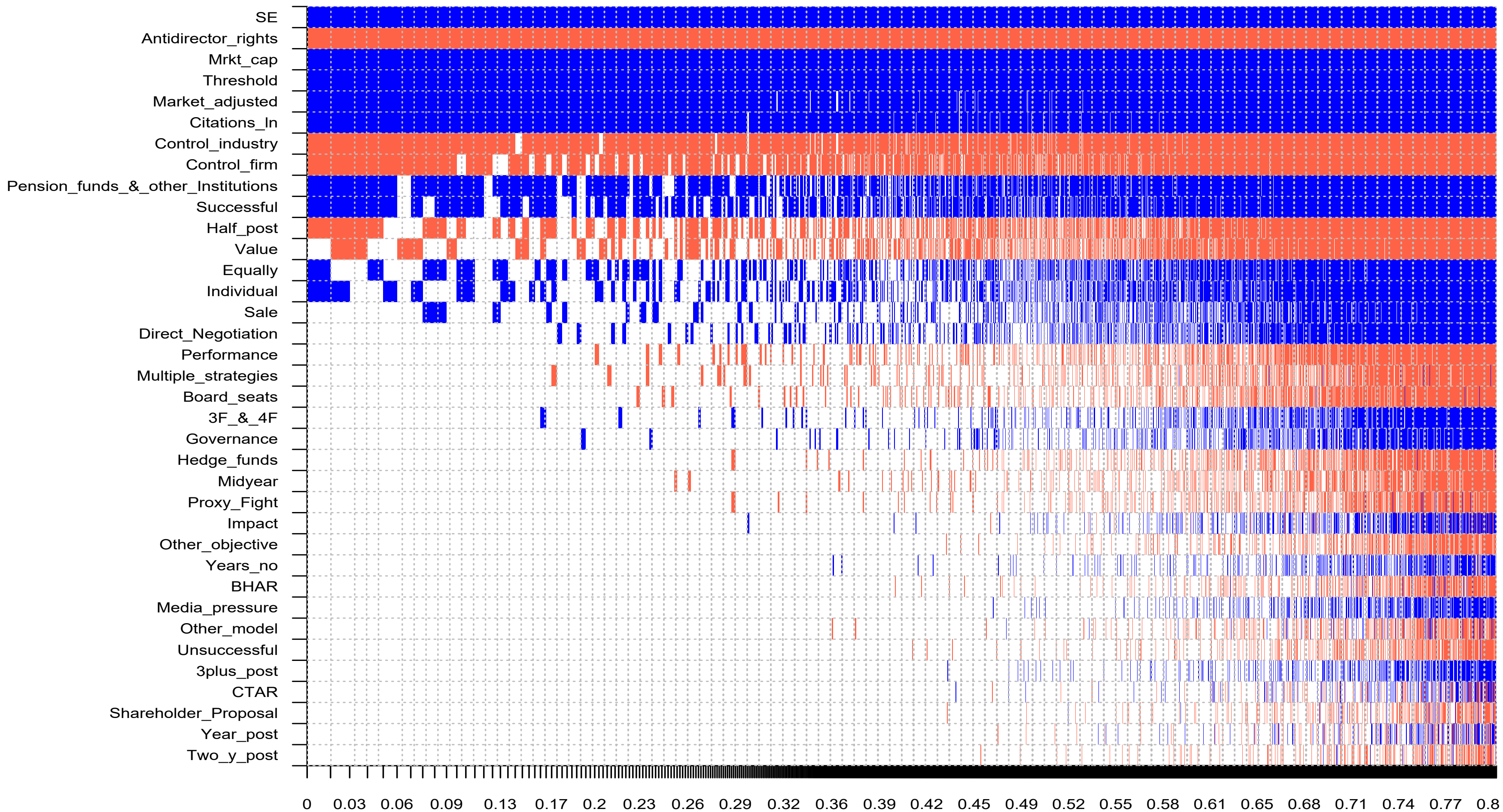
Reference model. Since many of the variables are coded as dummies, I define the reference model based on the principal characteristics. I drop the most frequent variable from each dummy variable group to avoid the dummy variable trap. I keep to this rule as strictly as possible; I drop another variable in a few cases just for smooth interpretation. The variables dropped and kept are summarized in Table 3.

5.2 Results

Figure 5 presents the BMA results graphically. The columns along the horizontal axis show the individual regressions. The column widths indicate their significance based on the respective PMPs. The columns are ranked from left to right in descending order of weight. The variables in the rows are likewise ordered by PIP from top to bottom. Where the model does not consider the variable, the corresponding cell is left blank. A cell is blue (darker in grayscale) if the variable has a positive sign and red (lighter in grayscale) if it has a negative sign. Figure 5 shows that roughly one-third of the variables are included in the best models, and these variables have stable signs across the specifications.

Table 4 converts the results to numbers. Besides the average estimate for each variable, the corresponding standard deviations and PIPs are presented. According to Eicher et al. (2011), a variable is decisive when the PIP is between 0.99 and 1, strong between 0.95 and 0.99, substantial between 0.75 and 0.95, and weak between 0.5 and 0.75. In addition, Table 4 shows alternative coefficients for simple OLS run only on variables identified as being at least weak. In the baseline BMA analysis, I identify nine variables that are at least substantial and 11 that are at least weak. This result provides me with ample space for discussion. The rule of thumb is that “blue” variables cause value creation to be more positive (less negative) and “red” ones cause it to be less positive (more negative).

Figure 5: Bayesian Model Averaging



Note: Figure 5 visualizes the BMA results. The columns along the horizontal axis show the individual regressions. The column widths indicate their significance based on the respective PMPs. The columns are ranked from left to right in descending order of weight. The variables in the rows are likewise ordered by PIP from top to bottom. Where the model does not consider the variable, the corresponding cell is left blank. A cell is blue if the variable has a positive sign and red if it has a negative sign.

Table 4: What Drives the Heterogeneity – Baseline Results

	Bayesian Model Averaging			**Frequentist Check (OLS)**		
	PIP	P. mean	P. SD	Coef.	SE	p-value
Intercept	1.000	-19.547	NA	-19.879	5.333	0.001
SE	1.000	0.573	0.057	0.554	0.178	0.003
Activism sponsors						
Hedge_funds	0.053	-0.242	1.211			
Pension_funds_&_Inst	0.762	5.537	3.779	6.590	2.475	0.011
Individual	0.371	2.832	4.045			
Activism approaches						
Shareholder_proposal	0.011	-0.021	0.397			
Direct_negotiation	0.194	0.968	2.183			
Proxy_contest	0.044	-0.162	0.904			
Multiple_strategies	0.099	-0.626	2.126			
Media_pressure	0.018	0.069	0.705			
Activism success						
Successful	0.678	3.380	2.664	4.792	3.061	0.124
Unsuccessful	0.018	-0.038	0.391			
Activism objectives						
Performance	0.135	-0.607	1.711			
Governance	0.069	0.434	1.823			
Board_seats	0.094	-0.400	1.401			
Sale	0.194	0.953	2.134			
Other_objective	0.021	-0.091	0.810			
Institutional setting						
Antidirector_rights	1.000	-5.898	0.906	-5.993	1.019	0.000
Threshold	1.000	7.072	1.459	7.277	1.655	0.000
Mrkt_cap	1.000	0.124	0.026	0.118	0.032	0.001
Event Windows						
Half_post	0.505	-2.724	2.993	-4.766	2.734	0.088
Year_post	0.009	0.001	0.152			
Two_y_post	0.009	-0.009	0.233			
3plus_post	0.017	0.027	0.282			
Sample characteristics						
Years_no	0.020	0.003	0.029			
Midyear	0.051	-0.011	0.054			
Returns models						
Market_adjusted	0.967	9.420	2.917	10.454	4.839	0.036
3F_&_4F	0.084	0.438	1.645			
BHAR	0.020	-0.037	0.363			
CTAR	0.011	0.004	0.247			
Other_model	0.018	-0.036	0.380			
Benchmark setting						
Control_firm	0.785	-5.775	3.626	-7.472	5.295	0.165
Control_industry	0.921	-9.298	3.863	-11.122	2.239	0.000
Index weighting						
Equally	0.401	1.619	2.163			
Value	0.425	-2.217	2.846			
Publication characteristics						
Impact	0.031	0.057	0.404			
Citations_ln	0.954	2.155	0.718	2.428	0.746	0.002

Note: The table presents the estimation results for Bayesian model averaging (BMA) with the dilution prior (George, 2010; Eicher et al., 2011) and the frequentist check (OLS). PIP: posterior inclusion probability; P.mean: posterior mean; P.SD: posterior standard deviation. OLS is based only on variables with a PIP higher than 0.5. A description of the variables is provided in Table 3.

In line with the observations in Section 4, publication bias exaggerates the results by about 0.57% according to the BMA results. This figure is approximately equal to the average publication bias for the whole sample proposed in Section 4 (it ranged from 0.329 to 0.848). This confirms that publication bias is present in this field. Moreover, it shows that the publication bias findings are not caused by possible omitted variable bias in equation 1.

Activism sponsors. The findings about sponsor type are in line with the current literature about long-run value creation by shareholder activism. The overall data, as well as my results, show that hedge fund activism and the activism of individuals do not cause higher value creation in the long run. This might be because these raiders (usually either individual investors such as Guy Wyser-Pratte and Rupert Murdoch, or hedge funds) aim to create short-run benefits for themselves (Holderness and Sheehan, 1985; Bassen et al., 2019). By contrast, the activism of pension funds and other institutions brings about 5.5% higher returns, but the PIP is only substantial, not decisive. A thorough reading of the literature reveals a possible explanation of this phenomenon, namely that CalPERS[4] and other institutions targeted underperforming firms, so their effect might seem large (English II et al., 2004; Barber, 2009).

Activism approaches. The other returns are not explained even by the different types of activism. Even though the literature suggests that direct negotiations and aggressive media pressure bring about higher returns (Table 3, Mietzner et al. 2011; Bessler et al. 2015), my comprehensive analysis shows that the differences in returns are caused mainly by other factors, not by the types of activism. None of the variables relating to activism type turns out to have a PIP above 0.5, or even above 0.2.

Activism success. The evidence on different outcomes of shareholder activism is relatively weak. In the BMA exercise, successful outcomes show about 3.378% higher returns, but the frequentist OLS check does not confirm the significance. The reason why I did not find that successful activism generates higher returns probably lies in the fact that changes happen even in firms where activism was not successful. For example, DeAngelo and DeAngelo (1989), as previously mentioned, find major changes even in companies where proxy contests were unsuccessful. A similar conclusion is drawn by Boyson et al. (2017), for instance. I tried to control for these differences in my analysis. However, it was impossible to do so, since most of the primary studies do not control for them.

Activism objectives. Different objectives do not cause different abnormal returns to activism, either. Similarly to activism type, Table 3 suggests higher returns, for example, for governance issues or the sale of the company. In the end, however, the differences turn out to be caused by different aspects of the studies. The highest PIP among these variables is lower than 0.2, which is very low.

Institutional setting. The BMA analysis provides convincing evidence that other structural factors are what affect the long-run estimates the most. Regarding shareholder activism, the critical structural factors are anti-director rights (La Porta et al., 1997, 1998) and regulatory threshold (Becht et al., 2017). The anti-director rights index proposed by La Porta et al. (1998) is based on aggregating six indicators capturing various aspects of shareholder rights protection. These comprise shareholders' ability to mail their proxy vote to the firm, the absence of a requirement to deposit their shares prior to a general meeting, the option of cumulative voting or proportional representation of minority shareholders on the board of directors, the presence of mechanisms to protect minority shareholders, a relatively low aggregate ownership threshold needed to call an

[4] CalPERS is the California Public Employees' Retirement System. It is recognized as having been a leader of shareholder activism during the 1980s and 1990s (Smith, 1996).

extraordinary meeting, and the existence of shareholders' preemptive rights. These measures are aimed at strengthening shareholders' bargaining position vis-à-vis the company and enhancing minority shareholders' ability to influence the governance of the company. The regulatory threshold is the country-specific regulatory disclosure threshold in percent.

Turning to the results, the returns to shareholder activism decrease with each increase in anti-director rights. This might seem counterintuitive at first glance but is plausible when one considers that in countries where shareholders have greater rights, firms are likely to be better prepared for activism. The logic for the threshold variable is much more straightforward. In countries with higher regulatory thresholds, activists might hide their intentions for longer, thus gaining higher returns. These results are remarkable, since it is unfeasible to draw such a conclusion from studies based on single-country observations. My analysis therefore allows me to reach new conclusions. My finding is similar to that in the meta-analysis by Holderness (2018), who studied cross-sectional differences in the quality of shareholder protection in the area of price responses to firms' announcements of new equity issues.

Event Windows. My results on different event windows are at odds with Greenwood and Schor (2009), who find increasing abnormal returns over time. The evidence in the literature is generally in favor of the "event-effect hypothesis" that "one event has one correct effect irrespective of the event length when correctly defined." The only results in contradiction with this point of view are those for the effect half a year past the event, but this variable does not have a stable PIP across the robustness checks. Another possible explanation for the significant decrease half a year after the initiation of activism is that activism has stable benefits after the primary negative shock.

Sample characteristics. Sample length and data age do not matter in this case. This means that short-sample bias and novelty bias do not distort the research into the long-run effects of shareholder activism.

Returns models. One of the most important factors influencing the differences between the estimates studied is that related to estimation methodology. The results indicate the importance of model choice and the choice of the benchmark with which one compares the results on abnormal returns to shareholder activism. The market-adjusted model is insufficient for making long-run estimates, since it does not control for other factors influencing the estimates. It is thus biased upward, as suggested by Barber and Lyon (1997) and Kothari and Warner (1997). Other models are comparable.

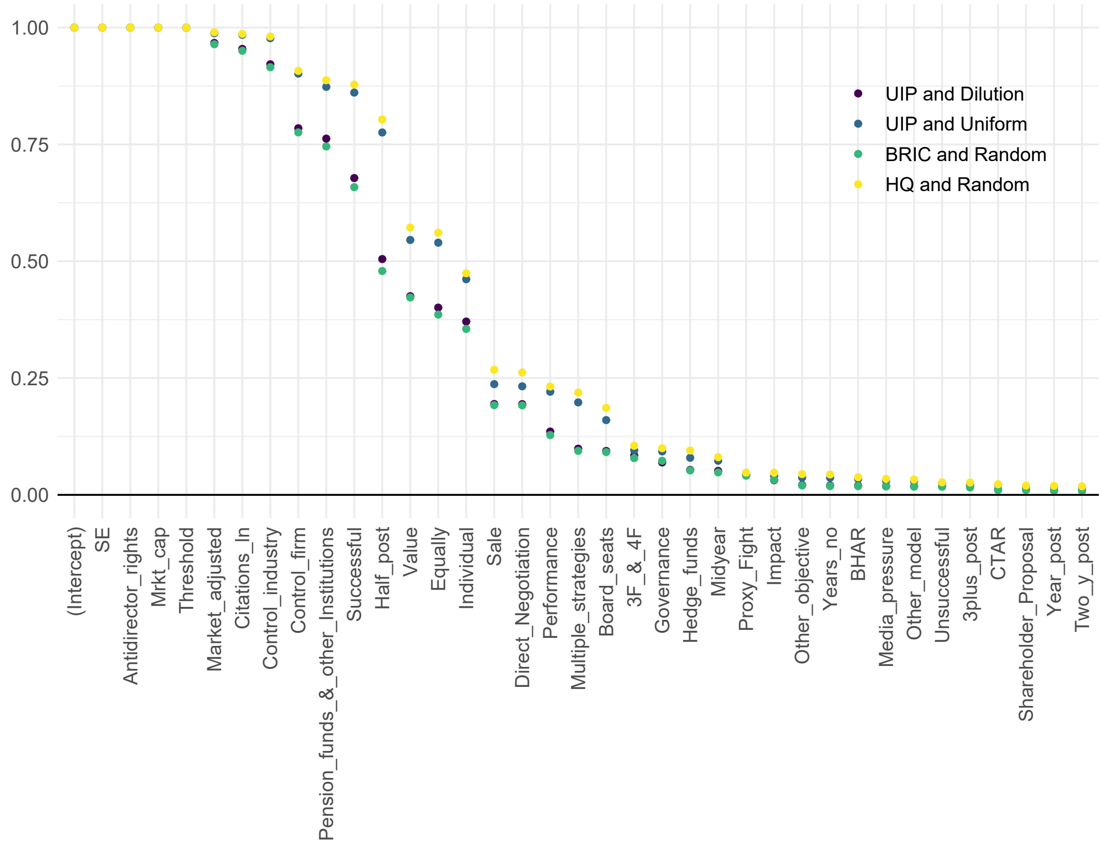
Benchmark setting. The second key finding concerning estimation methodology relates to the significance of the choice of benchmark. When the benchmark is a similar firm or the same industry as the targeted firm, the abnormal returns of the targeted firm increase less than when the general market is used as the benchmark. In other words, the general market is not the right benchmark choice and causes upward bias. This result lends support to the direction taken by the recent literature, which tends to use the firm's industry as the benchmark (e.g., Greenwood and Schor, 2009; Venkiteshwaran et al., 2010; Gonzalez-Mulé and Aguinis, 2018) or chooses a control firm based on similar market characteristics for comparison (e.g., Boyson and Mooradian, 2011; Goodwin and Rao, 2014; Wu and Chung, 2022). These two approaches generate significantly different estimates than those with simple market comparisons. The market seems to be a very general simplification for the "non-targeted benchmark". A proper comparison is provided by more similar counterparts based on the same industry or the same characteristics (for example, size and book-to-market ratio, Barber and Lyon 1997). More research should be done in this area to help set benchmarks better.

Publication characteristics. The results indicate a significant association between the number of citations and abnormal value creation by shareholder activism. Given this result, it is evident that articles that report higher returns to shareholder activism in the long run are cited more frequently, irrespective of journal quality (the impact factor variable is insignificant). This underscores the finding of upward bias in the literature (Gric et al., 2023).

5.3 Robustness Check

My comprehensive robustness checks confirm the baseline results. Even in Table 4, one can get a glimpse of this stability. Simple OLS run on variables with PIPs over 0.5 from the BMA baseline analysis yields the same result as the baseline specification. A few estimates are of greater magnitude, and the Successful and Control_firm variables turn insignificant. Nevertheless, the main results retain, for example, the crucial observation of publication bias.

Figure 6: Sensitivity of Posterior Inclusion Probabilities



Note: Figure 6 summarizes the PIPs for all four g-prior and model prior combinations used in our analysis. The baseline uses the UIP g-prior with the dilution model prior. Next, the UIP g-prior with the uniform model prior is presented alongside the BRIC g-prior with the random model prior and the HQ g-prior with the random model prior.

Additionally, I run a robustness check using different priors in the BMA settings – the UIP g-prior with the uniform model prior, the BRIC g-prior with the random model prior, and the HQ g-prior with the random model prior. These various specifications confirm my baseline results with almost the exact same estimates. Figure 6 indicates that the HQ g-prior and the uniform g-prior propose higher PIPs for the Halfpost, Value, and Equally variables, which might be deemed substantial (PIP between 0.75 and 0.95) and weak (PIP above 0.5), respectively.

In a nutshell, the results are stable across the robustness checks. They show that the value created by shareholder activism is significant but affected by publication bias, which exaggerates the "true" effect. Moreover, other factors – such the control sample, the choice of model, and the country's legal framework – affect the results. For example, researchers who compare their results with some benchmark firm usually get much lower estimates than researchers who compare them with the market. Similarly, researchers who use the simple market-adjusted model and thus do not control for risk factors obtain estimates that are about 10% higher than researchers who do control for it. The numerical results of these robustness checks are summarized in Table A3 in the Appendix.

5.4 Implied Value Creation

What does the BMA analysis imply for value creation across different sponsors and countries? The results presented so far indicate that value creation (i) is exaggerated by publication bias, (ii) is affected by the country-specific legal framework, (iii) varies with estimation methodology, and (iv) is not affected by different sponsors, activism types, and activism objectives. In order to provide some guidelines for future research, I stay focused on my main objectives and use the latest approaches published in top-rated journals (Boyson and Pichler (2019) in *The Review of Financial Studies* and Albuquerque et al. (2021) in *Journal of Financial Economics*). I also propose an overall estimate of the value that activism creates today (if any).

The estimates are summarized in Table 5. They are based on a linear combination of these two studies and the estimates of the baseline equation. Specifically, I follow the baseline specification of the BMA model, just setting the event window to the longest one. Moreover, for publication bias, I plug in zero, denoting no presence of publication bias. For the length of the data, the number of citations, and the impact factor, I plugged in the 90% percentiles of these variables, indicating a preference for high-quality studies. For the country characteristics, I used the 90% percentiles of the samples, indicating that I prefer newer studies with longer samples. In addition, I set anti-director rights to their 90% percentile to show a preference for shareholder protection. The 90% percentile for the threshold indicates the direction in which countries have been moving their thresholds over the last few decades. Based on these settings, together with correction for publication bias, I cannot replicate the results of Boyson and Pichler (2019) and Albuquerque et al. (2021) exactly. Nevertheless, the 95% confidence intervals of the results are positive in only one case, indicating that the value added by shareholder activism is somewhat inconclusive.

In addition to the implied estimates, I estimate the mean effect beyond bias by using all the variables from the BMA estimation but corrected for publication bias, meaning that the slope for publication bias is changed to zero. This result is displayed in the first row of Table 5. The result of this multivariate regression is an extension of the univariate estimations provided in Table 1 in Section 4. The multivariate and univariate results are very similar, but the confidence interval is much larger in the multivariate case due to the high number of explanatory variables in the estimated equation.

The implied estimates of value creation by shareholder activism confirm that shareholder activism only shows abnormal returns in a specific setting – researchers find significantly positive returns to shareholder activism only when they use the market return as the benchmark for their measurements. Conversely, when they use similar but non-targeted firms or industry mates, the returns tend to be positive but insignificant. Therefore, one should be very careful about how the research is conducted, as the results depend heavily on the choice of benchmark. They are also exaggerated by publication bias. The values suggested by the implied estimates are higher than the effect beyond bias and in some cases higher than the literature mean, but this is due to how the implied estimation procedure is set up.

Table 5: Implied Value Created across Sponsors, Activism Types, Success, and Countries

Shareholder Activism	Value Created in %	Lower 95% CI	Upper 95% CI
Effect beyond bias	3.830	-30.479	38.158
Control_market	18.339	2.912	33.766
Control_firm	11.305	-8.761	31.371
Control_industry	6.180	-9.839	22.199

Note: The figures represent the percentage increase in firm value in the long run following shareholder activism. The effect beyond bias is based on a multivariate estimation of publication bias. The other three rows build on the best study design of the current literature with different benchmarks.

6. Conclusions

I collect 1,186 reported estimates of long-run value creation from 49 studies and present the first meta-analysis of the literature on this phenomenon. Such a synthesis is useful, because shareholder activism is increasing over time and across countries. The study shows that, first, the conclusion as to whether activism is beneficial depends mainly on the benchmark with which one compares the firm "targeted" by activism. Second, the country-specific regulatory framework influences the value added by activism. Third, publication bias is present in this field, so the estimates in the literature are overstated.

I employ Bayesian model averaging with the dilution prior (Raftery et al., 1997) to investigate the heterogeneity of the estimates. I detect various sources of differences. Surprisingly, the type of activism, activism by hedge funds and individual investors, and the objective of activism matter little in long-run value creation by shareholder activism. On the other hand, the estimate of value added is affected by the choice of the benchmark with which the targeted firm is compared. Specifically, when one takes the overall market as the benchmark, the abnormal returns reported for the targeted firm are about 6% (9%) higher than when one compares the targeted firm with its non-targeted counterparts (industry mates).

In addition, the value added by shareholder activism is lower in countries with stronger "anti-director" rights (La Porta et al., 1997; Djankov et al., 2008). Conversely, with each one percentage point increase in the regulatory reporting threshold, the value added by activism decreases. The other significant findings are more or less intuitive. When one uses a simple market-adjusted model, the results are overstated. Next, the value added is higher when activism is successful than when it is unsuccessful or when its outcome is not defined. I propose implied estimates based on the most recent articles published in top-ranked journals. I conclude that the value created by shareholder activism corrected for publication bias is positive, but only when one compares the targeted firm with the market. When one compares the firm of interest with a similar but non-targeted firm or with the industry, the long-run benefits of shareholder activism are inconclusive.

Regarding future research and policy implications, one should be very careful about how the research is conducted, as the results depend heavily on the choice of benchmark and model, not only on the publication bias exaggeration. Moreover, one should be cautious about the country-specific regulatory framework. In the case of emerging shareholder-activism markets (such as the Czech Republic), one can learn from markets with a longer track record of such activism and its effects (such as the US and Western Europe).

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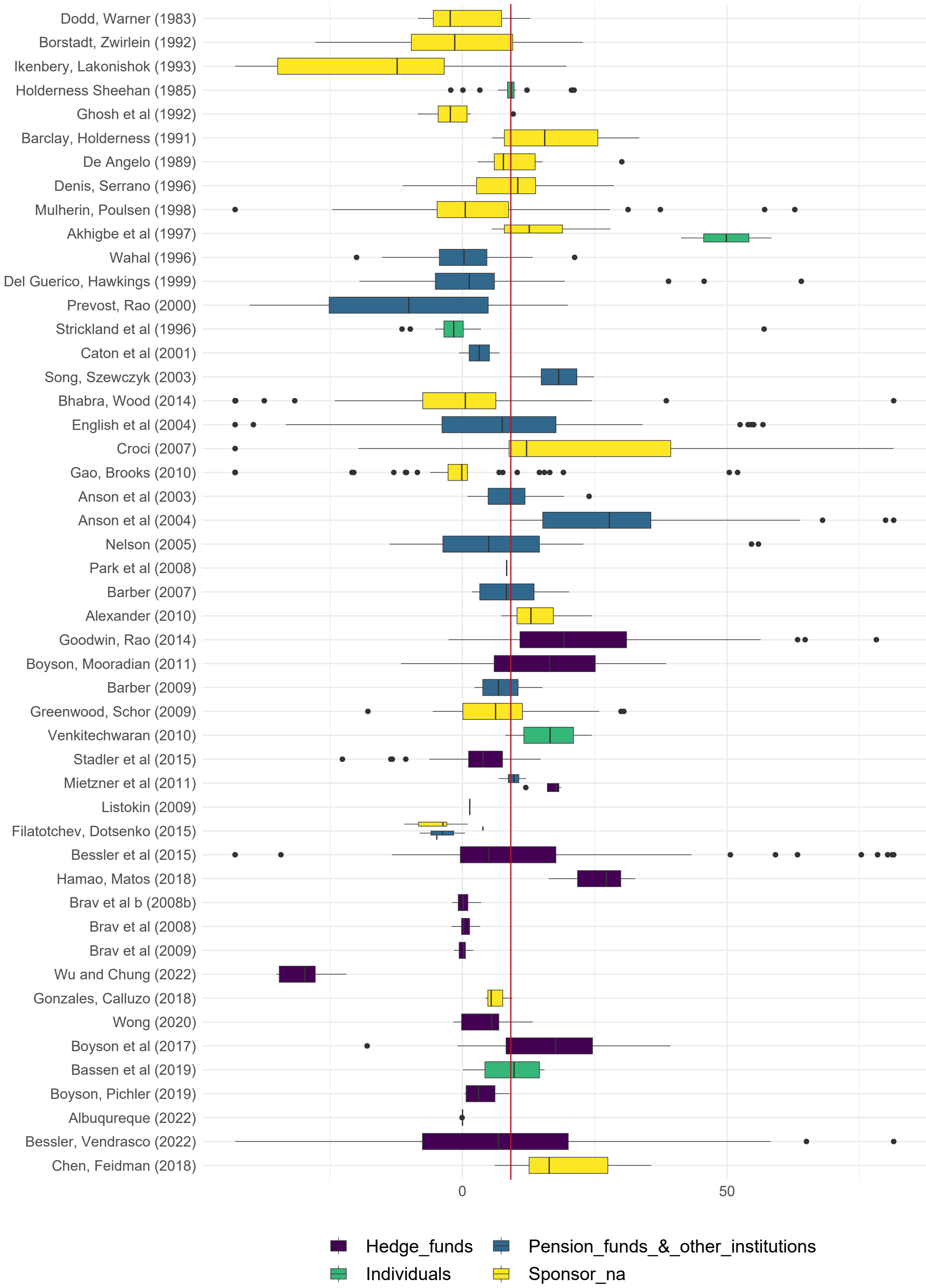
Appendix A

Table A1: Studies Included in the Meta-Analysis

Akhigbe et al. (1997)	Brav et al. (2008b)	Hamao and Matos (2018)
Albuquerque et al. (2021)	Brav et al. (2010)	Holderness and Sheehan (1985)
Alexander et al. (2010)	Caton et al. (2001)	Ikenberry and Lakonishok (1993)
Anson et al. (2003)	Chen and Feldman (2018)	Listokin (2009)
Anson et al. (2004)	Croci (2007)	Mietzner et al. (2011)
Barber (2007)	DeAngelo and DeAngelo (1989)	Mulherin and Poulsen (1998)
Barber (2009)	Del Guercio and Hawkins (1999)	Nelson (2005)
Barclay and Holderness (1991)	Denis and Serrano (1996)	Park et al. (2008)
Bassen et al. (2019)	Dodd and Warner (1983)	Prevost and Rao (2000)
Bessler et al. (2015)	English II et al. (2004)	Song and Szewczyk (2003)
Bessler and Vendrasco (2022)	Filatotchev and Dotsenko (2015)	Stadler et al. (2015)
Bhabra and Wood (2014)	Gao and Brooks (2010)	Strickland et al. (1996)
Borstadt and Zwirlein (1992)	Ghosh et al. (1992)	Venkiteshwaran et al. (2010)
Boyson et al. (2017)	Artiga González and Calluzzo (2019)	Wahal (1996)
Boyson and Mooradian (2011)	Goodwin and Rao (2014)	Wong (2020)
Boyson and Pichler (2019)	Greenwood and Schor (2009)	Wu and Chung (2022)
Brav et al. (2008a)		

Note**:** Table A1 lists all the primary studies investigating value creation by shareholder activism employed in this meta-analysis.

Figure A1: Value Created by Various Sponsors



Note: The figure displays a box plot of the value created by the various sponsors of shareholder activism in the primary studies, sorted by the mid-year of the primary data. It highlights both the median and the interquartile range (P25 – P75). The coverage of the whiskers is from (P25 – 1.5*interquartile range) to (P75 + 1.5*interquartile range). The dots stand for outlying estimates. The red vertical line signifies the mean value created in %. Winsorization of 1% is applied to the overall outliers before the computations.

Table A2: Definition of Variables

Variable	Definition
Estimate	The value of the estimate of interest (the impact of activism on abnormal returns)
SE	The value of the SE related to the estimate of interest
Activism sponsors	
Hedge_Funds	= 1 if the estimate is for hedge fund activism
Pension_Funds _&_other_inst.	= 1 if the estimate is for pension funds (CalPERS, CALSTRS, TIAA-CREFF, etc.), or for institutions other than pension funds
Individual_investors	= 1 if the estimate is for individual investors
Sponsor_na	= 1 if the sponsor is not specified
Activism approaches	
Activism_approach_na	= 1 if the activism approach is not mentioned
Shareholder_proposal	= 1 if the estimate relates to shareholder proposals
Direct_negotiation	= 1 if the estimate relates to direct negotiations with managers
Proxy_contest	= 1 if the estimate relates to proxy contests
Multiple_strategies	= 1 if the estimate relates to a combination of proposals, negotiations, and proxy contests
Media_pressure	= 1 if the estimate relates to media pressure, such as open letters and media campaigns
Activism success	
Successful	= 1 if the activism is successful or at least partially successful
Unsuccessful	= 1 if the activism is unsuccessful
Outcome_na	= 1 if successful and unsuccessful events are not distinguished
Activism objectives	
Objective_general	= 1 if the objective of activism is not specified
Performance	= 1 if the objective of activism is the performance of the targeted firm
Governance	= 1 if the objective of activism is the governance of the targeted firm
Board_seats	= 1 if the objective of activism is board seats
Sale	= 1 if the objective of activism is sale of the company
Other_objective	= 1 if the objective of activism is other than mentioned above (e.g., capital structure, business strategy)
Institutional setting	
Antidirector_rights	An index aggregating shareholder rights labeled as “antidirector company law or commercial rights” as in La Porta et al. (1998)
Threshold	The regulatory disclosure threshold
Mrkt_Cap	The average ratio of stock market capitalization to gross domestic product for the period, source WDI.
Event Windows	
Around	= 1 if the estimate relates to the abnormal returns one to three months after the activism
Half_post	= 1 if the estimate relates to the cumulative abnormal returns no longer than half a year after the activism
Year_post	= 1 if the estimate relates to the cumulative abnormal returns no longer than a year and a half after the activism
Two_y_post	= 1 if the estimate relates to the cumulative abnormal returns no longer than two and a half years after the activism
3plus_post	= 1 if the estimate relates to the cumulative abnormal returns at least two and a half years after the activism
Sample characteristics	
Years_no	The length of the primary dataset in years
Midyear	The median year of the time period of the data used to estimate the elasticity minus the minimum mid-year value in the whole meta-analytical sample.
Returns models	
Market_model	= 1 if the DV is from the market model
Market_adjusted	= 1 if the DV is market-adjusted returns
3F_&_4F	= 1 if the DV is measured by the FF three-factor or Carhart’s four-factor model
BHAR	= 1 if the DV is buy-and-hold abnormal returns
CTAR	= 1 the DV is calendar time abnormal returns

Continued on the next page

Table A2: Definition of Variables (continued)

Variable	Definition
Other_model	= 1 if the DV is measured using a different model
Benchmark setting	
Control_market	= 1 if the market serves as the benchmark for the computation of abnormal returns in the returns models
Control_industry	= 1 if a control industry serves as the benchmark for the computation of abnormal returns in the returns models
Control_firm	= 1 if a control firm serves as the benchmark for the computation of abnormal returns in the returns models
Index weightings	
Equally	= 1 if the DV is equally weighted
Value	= 1 if the DV is value-weighted
Not_eq_nor_value	= 1 if the DV is not weighted
Publication characteristics	
Impact	The recursive discounted impact factor from RePEc.
Citation_ln	The logarithm of the number of Google Scholar citations normalized by the number of years since the first draft of the paper appeared in Google Scholar.

Note: Mean = mean value of the given variable, SD = standard deviation, DV = dependent variable. The capital-to-GDP ratio is obtained from the World Development Indicators (WB, 2022). The impact factor is downloaded from RePEc, and the number of citations is from Google Scholar. The rest of the variables are collected from primary studies estimating value creation by shareholder activism. For ease of exposition, I group the variables according to their characteristics into sponsor type, activism type, success, country of origin, type of event, event window, methodology, and other publication characteristics.

Table A3: What Drives the Heterogeneity of the Estimates Collected – Robustness Checks

	UIP and Uniform			**BRIC and Random**			**HQ and Random**		
	PIP	P. mean	P. SD	PIP	P. mean	P. SD	PIP	P. mean	P. SD
Intercept	1.000	-18.768	NA	1.000	-19.601	NA	1.000	-18.514	NA
SE	1.000	0.568	0.056	1.000	0.573	0.057	1.000	0.567	0.056
Activism sponsors									
Hedge_funds	0.073	-0.351	1.517	0.052	-0.236	1.189	0.081	-0.385	1.582
Pension_funds_&_Inst	0.901	7.251	3.494	0.746	5.363	3.789	0.907	7.459	3.563
Individual	0.540	4.102	4.282	0.355	2.711	4.000	0.572	4.318	4.264
Activism approaches									
Shareholder_proposal	0.018	-0.029	0.472	0.010	-0.020	0.392	0.020	-0.034	0.501
Direct_negotiation	0.237	1.115	2.258	0.192	0.963	2.183	0.262	1.227	2.336
Proxy_contest	0.079	-0.309	1.257	0.041	-0.151	0.873	0.095	-0.366	1.351
Multiple_strategies	0.198	-1.294	2.937	0.092	-0.577	2.047	0.232	-1.532	3.156
Media_pressure	0.037	0.148	1.025	0.017	0.065	0.686	0.048	0.209	1.225
Activism success									
Successful	0.861	4.332	2.262	0.659	3.280	2.684	0.878	4.415	2.196
Unsuccessful	0.022	-0.031	0.360	0.018	-0.039	0.396	0.023	-0.029	0.355
Activism objectives									
Performance	0.232	-1.027	2.104	0.128	-0.574	1.672	0.268	-1.184	2.216
Governance	0.043	0.166	1.022	0.073	0.468	1.901	0.044	0.155	1.000
Board_seats	0.095	-0.377	1.359	0.094	-0.403	1.406	0.100	-0.383	1.357
Sale	0.221	1.002	2.110	0.191	0.944	2.132	0.219	0.965	2.058
Other_objective	0.037	-0.157	1.051	0.020	-0.087	0.793	0.044	-0.193	1.162
Institutional setting									
Antidirector_rights	1.000	-6.032	0.869	1.000	-5.883	0.909	1.000	-6.022	0.862
Threshold	1.000	6.837	1.466	1.000	7.092	1.458	1.000	6.788	1.474
Mrkt_cap	1.000	0.133	0.026	1.000	0.123	0.026	1.000	0.133	0.026
Event Windows									
Half_post	0.776	-4.294	2.803	0.479	-2.578	2.970	0.804	-4.466	2.736
Year_post	0.016	-0.006	0.191	0.009	0.002	0.150	0.018	-0.010	0.210
Two_y_post	0.017	-0.021	0.328	0.009	-0.008	0.225	0.019	-0.024	0.351
3plus_post	0.030	0.046	0.368	0.016	0.025	0.273	0.034	0.054	0.396
Sample characteristics									
Years_no	0.040	0.007	0.042	0.019	0.003	0.027	0.047	0.008	0.046
Midyear	0.093	-0.020	0.072	0.048	-0.010	0.053	0.105	-0.022	0.075
Returns models									
Market_adjusted	0.988	9.627	2.643	0.964	9.387	2.951	0.990	9.522	2.609
3F_&_4F	0.160	0.849	2.223	0.079	0.408	1.591	0.186	0.986	2.364
BHAR	0.034	-0.063	0.452	0.019	-0.036	0.356	0.038	-0.069	0.468
CTAR	0.022	0.016	0.376	0.010	0.003	0.236	0.026	0.026	0.436
Other_model	0.030	-0.055	0.494	0.017	-0.034	0.372	0.033	-0.058	0.514
Benchmark setting									
Control_firm	0.873	-6.372	3.233	0.776	-5.715	3.658	0.887	-6.444	3.148
Control_industry	0.977	-9.964	3.294	0.915	-9.235	3.920	0.981	-10.024	3.255
Index weighting									
Equally	0.545	2.232	2.283	0.386	1.556	2.141	0.561	2.289	2.284
Value	0.461	-2.380	2.895	0.422	-2.208	2.846	0.474	-2.446	2.920
Publication characteristics									
Impact	0.026	0.019	0.280	0.032	0.060	0.415	0.027	0.014	0.278
Citations_ln	0.985	2.246	0.624	0.950	2.146	0.729	0.987	2.252	0.621

Note: The table reports the results of the BMA robustness checks. The first part shows the estimates from the UIP g-prior with the uniform model prior (Eicher et al., 2011). The second part displays the results for the BRIC g-prior and the random model prior. The third and last part employs the Hannan-Quinn (HQ) g-prior with the random model prior (Gechert et al., 2022). A description of the variables is given in Table A2.

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