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Does Shareholder Activism Create Value? A Meta-Analysis*

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Abstract

We conduct a meta-analysis of 1,973 estimates of stock price responses to shareholder activism reported in 67 primary studies. We document publication bias in the literature. Corrected activism effects range from 0% to 1.5%. Effects are stronger when shareholder rights are better protected and when stock markets are smaller. Markets respond more positively to activism by individual investors, confrontational activism, and activism aimed at company sale. Estimates based on longer periods, simpler risk-adjustment approaches, more recent and longer datasets, as well as those published in more reputable journals tend to be larger.

Keywords: Shareholder activism, price response, value created, meta-analysis, publication bias, model averaging.

JEL Codes: G14, G30, G34, L20

*Corresponding author: Jiri Novak: e-mail: jiri.novak@fsv.cuni.cz. Data and code are available in an online appendix at meta-analysis.cz/activism. The views expressed here are ours and not necessarily those of the Czech National Bank.

1. Introduction

We study the determinants of the value created by shareholder activism. Shareholder activism has become an increasingly prominent feature of corporate governance. The *The Economist* (2023) argues that the weakening disciplining oversight by financial markets due to the rise of passive investing, lower interest rates, and the environmental, social and governance (ESG) considerations, make shareholder activism increasingly important. The *Financial Times* (2020) states that companies nowadays face more shareholder activism than ever before. The article cites Jim Rossman, the head of shareholder advisory at Lazard, who says that “activism has become a permanent feature of the corporate landscape”. In their recent review, Lazard’s Capital Market Advisory Group observes a global resurgence of shareholder activism as challenging macroeconomic conditions give urgency to performance improvements (Lazard, 2022). The report points out that much of the activism targets technology companies, which constitute the backbone of the modern economy. The report also mentions the increasing popularity of settlements relative to proxy fights to achieve board representation and the growing number of first-time activists. This implies that more owners than before are ready to take the initiative and actively influence the ways companies are run. Thus, shareholder activism has become increasingly important and widespread.

Observations of the recent trends made by Lazard (2022) are mostly consistent with prior academic research, which documents a broad trend away from shareholder proposals on remuneration or voting practices often initiated by pension funds (Holderness & Sheehan, 1985; Wahal, 1996; Smith, 1996), towards direct negotiation with management and potentially also litigation (Denes *et al.*, 2017). The increasing engagement of hedge funds and their readiness to coordinate the activity of like-minded shareholders has transformed the challenge activism poses (Boyson & Mooradian, 2011; Bessler *et al.*, 2015; Becht *et al.*, 2017). Given the evolving nature of shareholder activism, it is important to investigate how much value various forms of activism create and to study how their success depends on the

institutional setting.

Conceptually, there is considerable controversy over the merits and drawbacks of activism (Brav *et al.*, 2008b). The Economist (2023) suggests that “*Activist hedge funds are often seen as villains who are nasty, brutish and focused on the short term. Sometimes the shoe fits. But more often activists are playing a role that is essential for shareholder capitalism.*” Activist campaigns may mitigate agency problems that arise between firm owners and managers due to the separation of ownership and control (Jensen & Meckling, 1976; Jensen, 1986). The Investor Forum, founded in 2014 by UK institutional investors, is explicitly intended to serve “as an ‘escalation mechanism’ when firms ignore individual investors or exhibit problems that worry many shareholders” (The Economist, 2018). Shareholder activism may thus create value by initiating efficiency improvements, such as refocusing on profitable activities and reduced “empire-building” (Brav *et al.*, 2008a, 2018; Klein & Zur, 2009; Bebchuk *et al.*, 2015; Becht *et al.*, 2017; Brochet *et al.*, 2021; Maffett *et al.*, 2022) and by improving tax efficiency (Cheng *et al.*, 2012). Kang *et al.* (2022) observe that appointing independent directors nominated by activists tends to be associated with increases in firm value. Consistent with the proposition that activism tends to address cases when managers are not sufficiently responsive to owners’ requests, Chapman *et al.* (2022) show that firms with a dedicated investor relations function are less often challenged by activists.

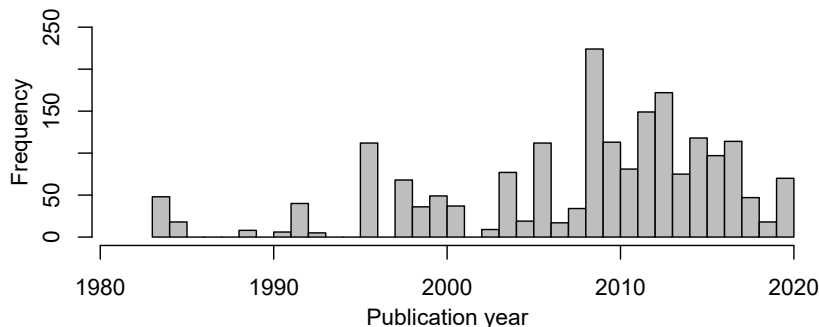
On the other hand, shareholder activism may be ineffective (Song & Szewczyk, 2003) and distract managers from beneficial long-term projects (Brav *et al.*, 2008b). Coffee & Palia (2016) mention concerns that shareholder activism (especially by hedge funds) may pursue short-termistic “pump and dump” schemes that temporarily boost dividend payouts but ultimately are detrimental to firms’ long-term profitability. The threat of shareholder activism may lead to undesirable preemptive behavior and defensive responses by management. Cherkes *et al.* (2014) provide evidence that managers are ready to take action to deter a potential challenge from activist shareholders. Prior research shows that shareholder activism constrains managerial control over the firm, lowers executive compensation, and in-

creases executive turnover (Ferri & Sandino, 2009; Brav *et al.*, 2010; Edmans & Holderness, 2017). Thus, to prevent becoming an activism target, managers may prioritize high current dividend payouts over investments. Activism may thus stifle innovation by reducing research and development (R&D) and other investments that generate long-term value (Bourveau & Schoenfeld, 2017; Maffett *et al.*, 2022). Ordóñez-Calafi & Bernhardt (2022) argue that in some settings, the costs of the reduced investment may outweigh the benefits of managerial disciplining. In addition, activism may turn confrontational, destabilize the company (O’Rourke, 2003) and lead to unintended adverse outcomes that damage the firm’s reputation (O’Rourke, 2003) and increase the likelihood of lawsuits (Guo *et al.*, 2021). Consistent with the greater risk of negative publicity, Guo *et al.* (2021) finds that shareholder activism targets pay higher audit fees even though they are more transparent and provide more frequent voluntary disclosures (Bourveau & Schoenfeld, 2017). Activism may also hurt other stakeholders. Agrawal & Lim (2022) suggest that shareholder wealth gains from activism are partly wealth transfers from employees.

Due to these conflicting perspectives, it is not *a priori* obvious whether shareholder activism, on average, enhances firm value and how the change in firm value varies with activism characteristics. Corporate governance regulation design should optimally trade off the benefits of efficiency improvements against the cost of forgoing beneficial innovative long-term projects. The magnitude of value created by activism is an important input for regulatory decisions. In this paper, we provide such an estimate by aggregating and synthesizing prior empirical evidence. We also examine how the value created by shareholder activism varies with its characteristics.

The increasing prominence of shareholder activism is reflected in extensive empirical research that analyzes its impact. Figure 1 shows the number of estimates of price responses to shareholder activism announcements published in research articles between 1980 and 2020. Our meta-analysis aggregates and synthesizes these diverse results to draw more robust inferences about the value shareholder activism typically creates. A meta-analysis represents

Figure 1: Interest in shareholder activism increases over time



Note: The figure displays the number of estimates of short-term stock returns surrounding shareholder activism campaigns published in each individual year.

an effective way of estimating the “true effect” in settings where there is an extensive pool of prior estimates based on different data samples and estimated using various methodologies (Stanley & Doucouliagos, 2012; Habersang *et al.*, 2019). Systematic coverage of activism campaigns differs across jurisdictions and over time (Becht *et al.*, 2017; Maffett *et al.*, 2022), which makes it challenging for researchers to comprehensively cover the phenomenon. Data on the impact of shareholder activism are scattered, and many primary studies use diverse and often fairly small data sets. For example, a well-published study by Matsusaka *et al.* (2019) uses a hand-collected data set covering only six years. Furthermore, Weber & Zimmermann (2013) and Krishnan *et al.* (2016) use a data set covering only four years, and Cai & Walkling (2011) uses data for only three years. The median length of the sample period in the primary studies on shareholder activism is only 8.3 years. Empirical results based on these samples may be affected by the regulatory framework and macroeconomic conditions specific to a given setting and time. This may compromise the generalizability of the reported findings and contribute to substantial heterogeneity in the reported estimates.

Performing a meta-analysis also allows us to adjust the reported empirical results for biases that may arise when prior evidence is published selectively in academic journals. Selective publication may result from authors’ and editors’ tendency to publish results that are

(i) consistent with their *a priori* expectations and/or with prior empirical findings and (ii) statistically significant. Despite the skepticism expressed in social discourse regarding the benefits of shareholder activism (Coffee & Palia, 2016), researchers in the fields of economics and finance may be prone to perceive activism as an essential corporate governance mechanism that plays a vital economic role in overcoming agency problems (Jensen & Meckling, 1976; Jensen, 1986). Hence, economists may mistrust findings suggesting that activism is ineffective or even detrimental in enhancing economic efficiency. They may also be reluctant to deviate from several prominent studies that document a positive impact of shareholder activism on firm value (Brav *et al.*, 2008a; Klein & Zur, 2009; Edmans *et al.*, 2013; Denes *et al.*, 2017; Albuquerque *et al.*, 2022). Consequently, authors and editors may consciously or subconsciously select for publication results that conform with the view that shareholder activism is beneficial and it enhances firm value (Gillan & Starks, 2007). Some of the results that would contradict this perspective may get discarded as implausible, which may bias the pool of estimates reported in research journals.

Furthermore, researchers may choose to examine the types of shareholder activism that are prominently featured in the financial press and other media. These forms of activism may have attracted media attention, particularly because they were associated with dramatic stock price responses. If academic journals include estimates of the impact of shareholder activism based on these pre-selected samples, the pool of published results might be biased upwards and fail to provide a balanced view of activism's overall effect. This concern may be particularly pressing for studies that investigate the activities of individual shareholder activists and successful hedge funds, whose performance is not systematically tracked. The decision to include these investors in a data sample may be affected by their prior success, leading to a self-selection problem. Results published in academic journals likely constitute the primary source of information that shapes researchers' and practitioners' perceptions of how beneficial or detrimental shareholder activism actually is. Adjusting for the publication selection bias is thus an important step in evaluating the economic role activism plays.

Finally, collecting a large data set of value creation estimates also allows us to exploit the heterogeneity across the primary studies and examine how various characteristics affect the value shareholder activism creates. Our approach is inspired by Holderness (2018) who perform a meta-analysis exploiting regional variation in regulatory requirements concerning equity issues. Pooling the data from numerous studies allows them to identify the fundamental role of shareholder approval of equity issues. Similarly, our study offers novel insights into the relevance of institutional setting quality for the effectiveness of shareholder activism. This analysis is infeasible in a study using data from a single regulatory setting.

We collect 1,973 estimates of stock price responses to shareholder activism campaigns from 67 studies published between 1983 and 2021. We use several methods to detect and correct for the impact of potential publication selection bias, including recent state-of-the-art approaches that detect selective publication even when the conventional assumption of a linear association between the estimates and their standard errors is violated (Andrews & Kasy, 2019; Bom & Rachinger, 2019; Bruns *et al.*, 2019; Furukawa, 2019; Simonsohn *et al.*, 2014b,a). Using this multitude of detection techniques we find a publication selection bias. We observe that numerically imprecise estimates are more likely published when they are high rather than low or negative. We also observe clustering of test statistics above the conventional levels of statistical significance at 5% and 1%. This tendency biases the pool of published empirical results. After correcting for this bias the value created by shareholder activism seems to be positive, but much smaller than commonly proposed. Our estimates range from 0.000% to 1.473%.

Furthermore, we construct more than 50 variables that capture various aspects of the individual estimates, such as the sponsors of the activism, its types, stated objectives, success in achieving these objectives, as well as a number of other factors related to the data sample and estimation methodology. We test how these characteristics affect the conclusions about the magnitude of value created by shareholder activism while simultaneously controlling for the differences in research designs used in the individual primary studies. We use Bayesian

Model Averaging (BMA) to address the model uncertainty problem that arises when the set of determinants of the dependent variable is not *a priori* defined (Steel, 2020). BMA considers various combinations of “candidate” explanatory variables and identifies those that are most important in explaining the variation in the dependent variable (Eicher *et al.*, 2011; Feldkircher & Zeugner, 2012; Moral-Benito, 2015; Raftery *et al.*, 1997). This technique allows us to address multi-collinearity issues that may arise when considering numerous potentially relevant variables (George, 2010). Due to its flexibility, BMA is frequently used for meta-analyses (Bajzik *et al.*, 2020; Bajzik, 2021; Cazachevici *et al.*, 2020; Gric *et al.*, 2021; Matousek *et al.*, 2021).

Our results identify several important explanatory variables. We observe more positive price responses to shareholder activism announcements in settings that better protect shareholder rights and where the aggregate stock market capitalization is lower relative to the gross domestic product (GDP). Furthermore, stock markets react more positively to activism by individual investors, conducted through more confrontational approaches, aimed at selling the target company, and those that eventually achieve their goals. In addition, studies using longer measurement windows, simpler risk-adjustment approaches, more recent and longer data sets, and those published in more influential academic journals report higher estimates of value created by shareholder activism.

Our results offer valuable insights to regulators in designing an optimal legal framework and to researchers in making appropriate methodology choices. The controversy in prior literature as well as the general public discourse about the merits and drawbacks of shareholder activism constitute a challenge for capital market regulators aiming to design an appropriate framework that trades off the benefits and the costs. Our research informs regulatory decisions by quantifying the average impact shareholder activism has on firm value and by identifying several relevant conditioning variables. For example, regulators may benefit from our finding that the value created by shareholder activism is positively associated with the quality of the institutional framework in a given country. Our findings also help researchers

identify relevant research design characteristics and quantify how they affect the computed estimates. These results facilitate the interpretation and comparison of prior empirical results and are instrumental for making research design choices in future studies.

The remainder of the paper is organized as follows. Section 2 describes our data collection procedure. Section 3 presents the results of our tests of selective publication. In Section 4, we show the results on the relevance of various estimate characteristics. Section 5 concludes.

2. Data Sample

Prior literature uses two broad approaches to measure the value created by shareholder activism. The event study approach, e.g., Brown & Warner (1985), uses price responses to shareholder activism announcements as a proxy for the value activism creates. The price response is typically measured over fairly short return windows ranging from several days to a few months (Denes *et al.*, 2017; Brav *et al.*, 2008b). This approach essentially captures how marginal investors update their estimates of firm value based on the expected impact of shareholder activism. Provided that financial markets are reasonably efficient, the short-term stock price response reflects the re-evaluation of the firm’s intrinsic value, i.e., the incremental value activism creates. Thus, in this paper, we use the terms “price response” and “value creation” interchangeably.

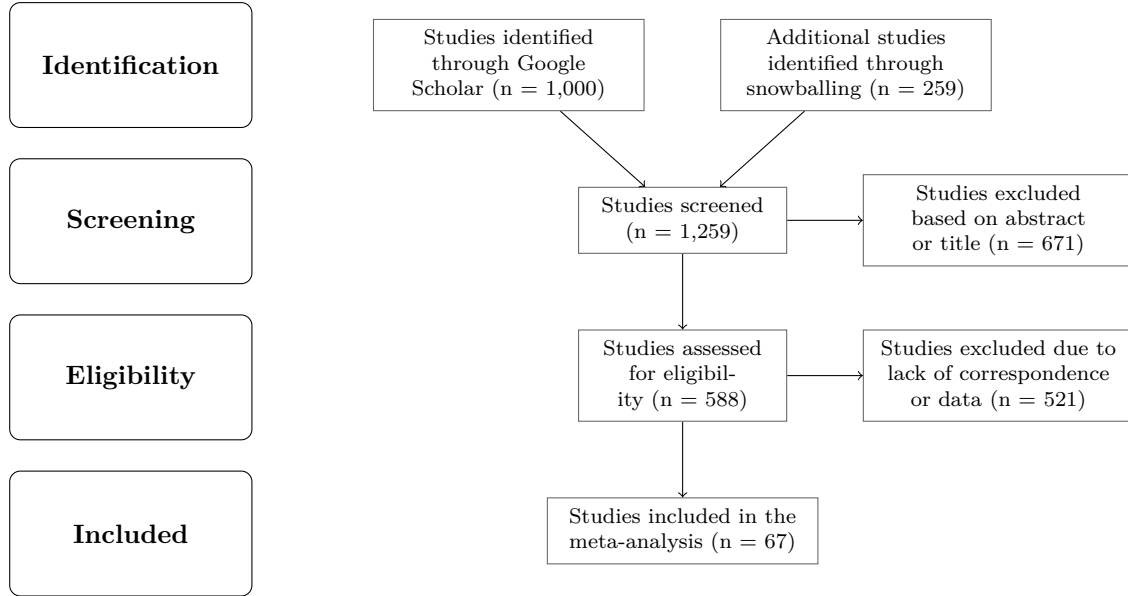
The second approach examines performance improvements and long-run stock returns following activism campaigns (Mitchell & Stafford, 2000). Even though the second approach is appealing due to its focus on the actual economic impact, it is subject to several important limitations. First, long-term estimates may be confounded with other factors unrelated to shareholder activism, e.g., performance reversal in target companies (Filatotchev & Dotsenko, 2015). Second, it is inherently challenging to estimate long-term abnormal stock returns (Croci, 2007). Third, some activism is explicitly aimed at making the company an acquisition target (Greenwood & Schor, 2009). This implies that its future performance as

a stand-alone entity will no longer be available, which likely biases the available data. Thus, we restrict our attention to short-term price responses to shareholder activism campaigns.

Our data sample collection follows the guidelines proposed by Havranek *et al.* (2020). We provide a comprehensive overview of our sample collection procedure in the PRISMA diagram shown in Figure 2. First, we inspect the lists of references in the most influential review articles: Albuquerque *et al.* (2022), Denes *et al.* (2017), and Filatotchev & Dotsenko (2015). We then develop combinations of subsequent keywords for our search queries. We observe the list of articles generated by each query, and we iteratively modify the set of keyword combinations to most effectively identify relevant studies. This process generates 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”. We verify that these keyword combinations successfully identify the relevant articles cited in the above-mentioned reviews. We also screen the lists of references in these articles to potentially identify additional relevant studies. We concluded our data collection at the end of March 2022. The final data set (including code) is available in an online appendix at meta-analysis.cz/activism.

We run our search query using Google Scholar because of its ability to consider the entire full-text contents of articles. We download and examine the first 1,000 entries in the list generated by the above-described search query. To increase the likelihood that the estimates we collect are reliable, we disregard any sources that are not published in research journals (e.g., working papers, student theses). To get published in an academic journal, a study must successfully undergo a demanding peer-review process intended to assure the quality and reliability of the reported findings. Furthermore, empirical evidence published in academic journals tends to be the most influential in shaping researchers’ and professionals’ views of the impact of shareholder activism. Thus, limiting our analysis to estimates published in research articles lets us focus on the empirical findings that matter most for the academic

Figure 2: Schematics of study inclusion and exclusion (PRISMA)



Note: The figure shows the Preferred Reporting Items for Systematic Reviews and Meta-Analyses (PRISMA) diagram that depicts the process we follow to identify relevant estimates in the primary studies that constitute our sample. Our sample collection procedure follows the guidelines proposed by Havranek *et al.* (2020).

and business communities. We do not expect this research design choice to have a dramatic impact on our results because prior research shows that publication selection bias is fairly comparable across published and unpublished studies (Doucouliagos & Stanley, 2013).

We only collect stock returns estimates measured over short-run windows that we define as being fully contained within the two-month period starting 30 days prior to the event day and ending 30 days after it (Brav *et al.*, 2008b; Denes *et al.*, 2017). To be able to perform our tests of the publication selection bias, we require the price response estimates to be accompanied by corresponding t -statistics, standard errors, or other statistics from which standard errors can be computed. When several measures are provided, we preferably collect corresponding standard errors over t -statistics and p -values.

Table 1 provides a list of the primary studies from which we source our estimates. In total, our data collection procedure yields 1,973 estimates that we collect from 67 research articles. Figure 3 shows the histogram of the price response estimates in our sample. The number of primary studies that contain relevant estimates and the range of these estimates

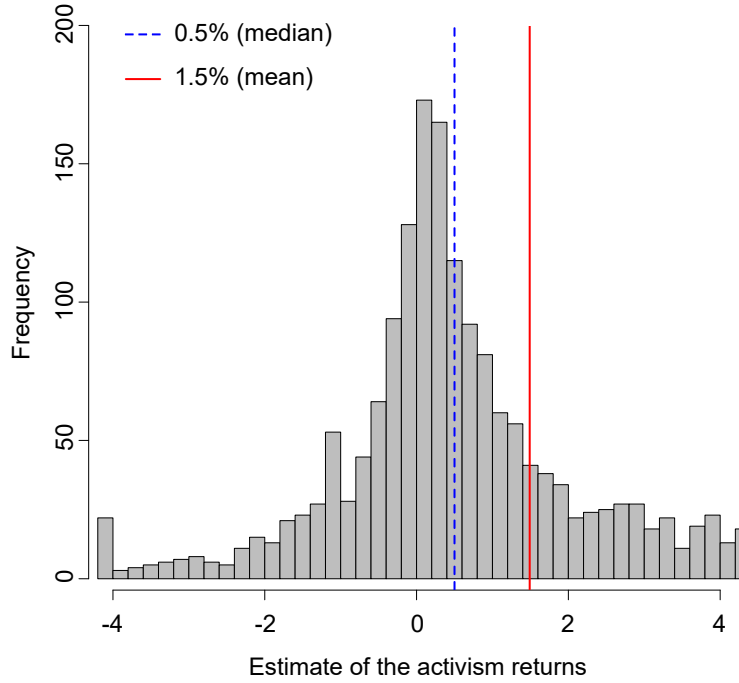
Table 1: Primary studies included in our meta-analysis

| | | |
|--------------------------------|--------------------------------|-------------------------------------|
| Alexander <i>et al.</i> (2010) | Croci (2007) | Lee <i>et al.</i> (2018) |
| Anson <i>et al.</i> (2003) | Cunat <i>et al.</i> (2012) | Lin <i>et al.</i> (2016) |
| Azizan & Ameer (2012) | Cziraki <i>et al.</i> (2010) | Matsusaka <i>et al.</i> (2019) |
| Barber (2007) | DeAngelo & DeAngelo (1989) | Mietzner <i>et al.</i> (2011) |
| Barber (2009) | Del Guercio & Hawkins (1999) | Morgan & Poulsen (2001) |
| Barclay & Holderness (1991) | Dodd & Warner (1983) | Mulherin & Poulsen (1998) |
| Bassen <i>et al.</i> (2016) | El-Khatib <i>et al.</i> (2017) | Nelson (2005) |
| Bassen <i>et al.</i> (2019) | English <i>et al.</i> (2004) | Nelson (2006) |
| Bebchuk <i>et al.</i> (2020) | Filatotchev & Dotsenko (2015) | Ong <i>et al.</i> (2010) |
| Becht <i>et al.</i> (2009) | Fortin <i>et al.</i> (2014) | Park <i>et al.</i> (2008) |
| Becht <i>et al.</i> (2017) | Ghosh <i>et al.</i> (1992) | Prevost & Rao (2000) |
| Bessler <i>et al.</i> (2015) | Gillan & Starks (2000) | Prevost <i>et al.</i> (2012) |
| Bhabra & Wood (2014) | González & Calluzzo (2019) | Renneboog & Szilagyi (2011) |
| Bizjak & Marquette (1998) | Goodwin & Rao (2014) | Smith (1996) |
| Borstadt & Zwirlein (1992) | Greenwood & Schor (2009) | Smythe <i>et al.</i> (2015) |
| Boyson <i>et al.</i> (2017) | Hamao & Matos (2018) | Stadler <i>et al.</i> (2015) |
| Boyson & Pichler (2019) | Holderness & Sheehan (1985) | Strickland <i>et al.</i> (1996) |
| Brav <i>et al.</i> (2008a) | Chen & Feldman (2018) | Venkiteshwaran <i>et al.</i> (2010) |
| Brav <i>et al.</i> (2008b) | Chen <i>et al.</i> (2020) | Wahal (1996) |
| Brav <i>et al.</i> (2010) | Ikenberry & Lakonishok (1993) | Weber & Zimmermann (2013) |
| Cai & Walkling (2011) | Karpoff <i>et al.</i> (1996) | Yang <i>et al.</i> (2012) |
| Carleton <i>et al.</i> (1998) | Krishnan <i>et al.</i> (2016) | Yeh (2014) |
| Caton <i>et al.</i> (2001) | | |

Note: This table shows a list of the 67 primary studies, from which we source 1,973 estimates of short-term stock price response to shareholder activism that constitute our sample.

demonstrate the extensive empirical research on this topic. This underscores the benefits of aggregating these diverse findings by means of a meta-analysis. Figure 3 shows that the distribution of price response estimates is somewhat more dispersed than the normal distribution (excess kurtosis: 2.358, not tabulated). This points towards a substantial heterogeneity in the coefficients that we collect from the primary studies. The distribution is positively skewed (skewness: 1.499, not tabulated) with the mean value of 1.52% above the sample median of 0.50%. Our data set thus features a higher-than-expected frequency of positive observations, while the corresponding low or negative observations are less common. This finding is consistent with a propensity to discard some low or negative estimates of price responses to shareholder activism.

Figure 3: Activism returns concentrate around zero



Note: The figure shows the distribution of short-term stock returns estimates surrounding shareholder activism that we collect from the primary studies. The vertical solid red line indicates the sample mean and the dashed blue line shows the median.

3. Publication Bias

3.1. Funnel Plot

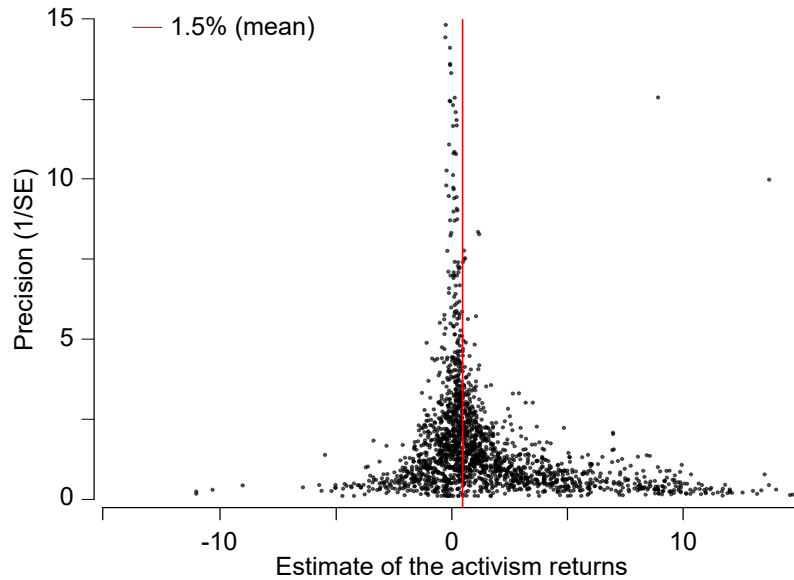
Publication selection bias is a phenomenon that arises when authors and editors have a conscious or subconscious tendency to publish estimates that are consistent with their *a priori* expectations about the nature of the examined relationship or with previously published results, and/or that are statistically significant (Ioannidis *et al.*, 2017). Especially research based on smaller datasets should sometimes generate counter-intuitive results simply because a given dataset may, purely by chance, happen not to be representative of the entire population. While it may be considered reasonable to discard results that seem implausible given the presumed relationship or in light of prior findings, doing so distorts the pool of

estimates published in the body of empirical research literature. Such a distortion may bias the perception of the overall strength of the studied relationship and lead to undue conclusions about the level of consistency of empirical evidence supporting it. This issue may be further compounded if researchers choose not to publish results that are inconsistent with prominent studies published in leading academic journals, that “set the tone” for the general understanding of the nature of the relationship. Prior research documents selective reporting of results in numerous research settings in economics (Blanco-Perez & Brodeur, 2020; Brown *et al.*, 2023; Campos *et al.*, 2019; Ugur *et al.*, 2018) and finance (Astakhov *et al.*, 2019; Gric *et al.*, 2021; Geyer-Klingeberg *et al.*, 2018; Zigraiova & Havranek, 2016). Ioannidis *et al.* (2017) concludes that the results published in research journals in economics tend to suggest a magnitude of a relationship that is, on average, twice as large as the true effect.

We find it plausible to expect selective publication in empirical research on the impact of shareholder activism because researchers may view it as a vital disciplining force to overcome agency problems and promote economic efficiency. Hence, they may be skeptical about results suggesting that shareholder activism is ineffective or even detrimental in performing this essential economic role. They may also be reluctant to submit articles that contain results inconsistent with key studies in the area (e.g., Cunat *et al.*, 2012; Gillan & Starks, 2000; Matsusaka *et al.*, 2019). Our meta-analysis allows us to assess how much the published empirical results are affected by selective reporting and to adjust the coefficients for the bias.

Following Egger *et al.* (1997), we start our analysis by examining a funnel plot depicted in Figure 4. The horizontal axis displays the value of the 1,973 reported price response estimates. The vertical axis displays the precision of these estimates defined as the inverse of their standard errors. The graph should have the shape of an inverted funnel because the most precise estimates should be centered around the sample mean, whereas the less precise estimates should be more dispersed. Absent any publication bias, it should be symmetric as less precise estimates that deviate from the sample mean should be equally likely published regardless of whether they are high or low or even negative. In contrast, under selective

Figure 4: Funnel plot suggests publication bias



Note: This figure shows a funnel plot of the short-term price responses to shareholder activism campaigns. On the horizontal axis, the funnel plot shows the value of the 1,973 reported price response estimates that constitute our sample. The vertical axis measures the precision of these estimates defined as the inverse of their standard errors ($1/SE$). Absent any publication selection bias, the observations should form a symmetric inverted funnel centered around the most precise estimates.

publication, some of the imprecise estimates that are low or negative get discarded, which skews the figure. An asymmetric funnel plot thus suggests that estimates are reported selectively in some primary studies and that their mean value constitutes a biased estimate of the true effect.

A visual examination of Figure 4 suggests that the funnel plot is positively skewed. This indicates that imprecise estimates are more likely to get reported if they are high rather than low or negative. This finding provides initial suggestive evidence consistent with the proposition that the pool of empirical results on value created by shareholder activism may be distorted by the absence of imprecise estimates that are low or negative. The mean value of these estimates may overstate the true impact activism has on enhancing company value.

3.2. Selectivity Tests

To formally test the proposition that empirical results on the impact of shareholder activism are published selectively, we follow Egger *et al.* (1997) and Stanley & Doucouliagos (2012) and estimate the following equation:

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

where \hat{x}_{ij} denotes the i -th estimate of price response to shareholder activism in the j -th study and $S\hat{E}_{i,j}$ denotes the corresponding standard error. Equation 1 is based on the assumption that over-reporting of either high or low results induces a linear association between the reported estimates (\hat{x}_{ij}) and their standard errors ($S\hat{E}_{i,j}$). If imprecise estimates that happen to be low or negative tend to get discarded, then high estimates should be more likely to have larger standard errors than low or negative estimates. Selective reporting of higher estimates thus implies a positive slope coefficient β_1 in Equation 1. The intercept term β_0 in turn represents the “true” effect corrected for potential publication bias.

To evaluate the robustness of our findings, in Panel A of Table 2 we present our results based on six different conventional ways of estimating Equation 1. First, we use ordinary least squares (OLS) estimation with two-way clustering at the study and country level (following Cameron *et al.*, 2011). The two-way clustering addresses the potential concentration of high or low estimates in specific countries or studies. Albeit commonly used in prior literature, OLS may produce spurious results when unobserved research design features are correlated with the reported estimates. Thus, we also run fixed effects (FE) and between effects (BE) regressions. Study-level FE absorb idiosyncratic study-level variation in research methodologies and data samples. In contrast, study-level BE account for the differences in the size of the 67 primary studies.

Furthermore, we follow Stanley & Doucouliagos (2012) and Astakhov *et al.* (2019) and estimate Equation 1 using techniques that weigh the observations by measures of study size

Table 2: Tests indicate publication bias

| <i>Panel A - Linear Estimation Methods</i> | | | | |
|--|---|---------------------------------|---|---|
| | OLS | FE | BE | IV |
| Effect beyond bias (β_0) | 0.590 ^{***} (0.202) [0.249, 0.956] | 1.256 ^{***} (0.296) | 1.473 ^{***} (0.318) | 0.657 ^{***} (0.208) |
| Publication bias (β_1) | 0.686 ^{***} (0.126) [0.456, 0.917] | 0.178 [*] (0.104) | 0.233 ^{**} (0.104) | 0.635 ^{***} (0.159) |
| #Observations | 1,973 | 1,973 | 1,973 | 1,973 |
| #Studies | 67 | 67 | 67 | 67 |
| | | | w(NOBS) | w(1/SE) |
| Effect beyond bias (β_0) | | | 0.713 ^{***} (0.239) [0.324, 1.100] | 0.008 (0.010) [-0.003, 0.095] |
| Publication bias (β_1) | | | 0.836 ^{***} (0.148) [0.597, 1.098] | 1.130 ^{***} (0.158) [0.851, 1.399] |
| #Observations | | | 1,973 | 1,973 |
| #Studies | | | 67 | 67 |
| <i>Panel B - Nonlinear Estimation Techniques</i> | | | | |
| | Top10 | Stem | Kinked | Selection |
| Effect beyond bias | 0.196 ^{**} (0.079) | 0.021 ^{***} (0.006) | 0.000 (0.001) | 1.062 ^{***} (0.024) |
| #Observations | 1,973 | 1,973 | 1,973 | 1,973 |
| #Studies | 67 | 67 | 67 | 67 |

Note: The uncorrected mean value creation by shareholder activism is 1.49%. The presented results are from regression $\hat{x}_{ij} = \beta_0 + \beta_1 S\hat{E}_{i,j} + e_{ij}$, where \hat{x}_{ij} denotes the i -th value creation estimated in the j -th study, and $\beta_1 S\hat{E}_{i,j}$ denotes the corresponding standard error. Panel A - OLS: the ordinary least squares estimation. FE: study-level fixed effects. BE: study-level between effects. w(NOBS): estimation that weights the individual estimates by the inverse number of observations reported in a given study. w(1/SE): estimation that weights the individual estimates by their precision, i.e. the inverse of their standard error, i.e. $1/SE(r_{ij})$. IV: estimation that uses the inverse of the square root of the number of observations as an instrument for the coefficient's standard error. This approach is also used by Astakhov *et al.* (2019) and Zigraiova & Havranek (2016) to address potential endogeneity between an estimate and its standard error (Havranek, 2015; Stanley, 2005). Panel B - Top10: estimates the "true effect" in the studied relationship based on the 10% most precise estimates (Stanley *et al.*, 2010). Stem: the stem-based model by Furukawa (2019) reflects the average of observations selected based on the optimization of the trade-off between bias and variance. Kinked: the endogenous kink model by Bom & Rachinger (2019). Selection: the selection model by Andrews & Kasy (2019) using clustered SEs. Standard errors reported in parentheses clustered at the level of studies and countries (Cameron *et al.*, 2011), and 90% confidence intervals obtained using wild bootstrap in square brackets (Roodman *et al.*, 2018). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

and by the precision of the estimates. In the next model (labeled “w(NOBS)”) we weigh the observations by the inverse number of estimates reported in a given study. This approach “levels the playing field” for studies that report more or fewer estimates and makes each of the 67 primary studies equally important in shaping our results. In another model (labeled “w(1/SE)”) we weigh the observations by their precision, i.e. by the inverse of their standard error, i.e. $1/SE(r_{ij})$. This approach assigns more weight to more precise estimates, which helps us adjust for the potential heteroskedasticity of our sample.

All the five tests discussed so far assume that selective reporting induces a linear association between \hat{x}_{ij} and $S\hat{E}_{i,j}$. While this assumption is likely plausible in most settings, in some cases, the relationship between the coefficients and their standard errors may be endogenously determined by specific study characteristics (Stanley, 2005; Havranek, 2015). To address this issue, Havranek *et al.* (2023) proposes to instrument $S\hat{E}_{i,j}$ by the inverse of the square root of the number of observations. This instrument is likely valid because, by construction, the number of observations is correlated with the standard error. At the same time, the number of observations is unlikely to be related to the methods used and other potential confounding study characteristics. Hence, it is reasonable to assume that the number of observations is quasi-randomly distributed across the primary studies. Thus, following Astakhov *et al.* (2019) and Zigraiova & Havranek (2016), in the last model of Panel A in Table 2 (labeled “IV”) we report our results from this estimation approach.

Panel A of Table 2 provides evidence of a publication selection bias in the empirical literature on shareholder activism. All six β_1 coefficients are positive, which is consistent with the under-reporting of low or negative price responses to shareholder activism campaigns. Five out of six of these results are statistically significant at the conventional 5% level and the FE estimate is significant at the 10% level (coef. 0.178, std. err. 0.104). Furthermore, Panel A of Table 2 also shows that regardless of the estimation approach, the intercept terms β_0 are positive. Again, five out of six of these results are statistically significant at the conventional 5% level. These results suggest that, on average, shareholder activism is associated with

positive price responses even after controlling for selective publication. Nevertheless, the magnitude of β_0 coefficients is lower than what is commonly suggested in the prior research literature, ranging from 0.008% to 1.473%.

In Panel B of Table 2 we complement the conventional approaches for testing publication selection bias with four recently developed techniques that do not require the assumptions of independence and linearity to be satisfied. The first method, labeled “Top10” and developed by Stanley *et al.* (2010) estimates the “true effect” based on the 10% most precise observations collected from primary studies, which are unlikely to be severely affected by selective reporting. The second method, the stem-based method (labeled “Stem”) by Furukawa (2019), builds on Stanley *et al.* (2010) but aims to limit the loss of sample variation by optimizing the trade-off between bias and variance. Instead of discarding 90% of less precise estimates, it discards only those estimates that do not add value in the light of this trade-off. The “true effect” is then computed as the average value based on the remaining estimates. The third method, the endogenous kink model (“Kinked”) by Bom & Rachinger (2019), assumes the existence of an endogenously determined threshold, at which the relationship between an estimate and its standard error changes, and aims to detect this “kink”. The fourth model, the selection model (“Selection”) by Andrews & Kasy (2019), assumes that the probability of publishing an estimate depends on its statistical significance. The model identifies the likelihood of an estimate falling into different intervals determined by critical values of the t -statistics. The model assigns more weight to intervals that are underrepresented.

These non-linear techniques lead us to similar conclusions. The estimated “true effect” ranges from 0.000% for the kink method to 1.062% for the selection model, which is broadly comparable to the interval of (0.008%, 1.473%) that we observe for the linear approaches. Furthermore, three out of four results reported in Panel B of Table 2 are statistically significant at the conventional 5% level. This suggests that our results based on the linear techniques are unlikely to be severely affected by the potential violation of the assumption of independence and a linear association between the price response estimates and their

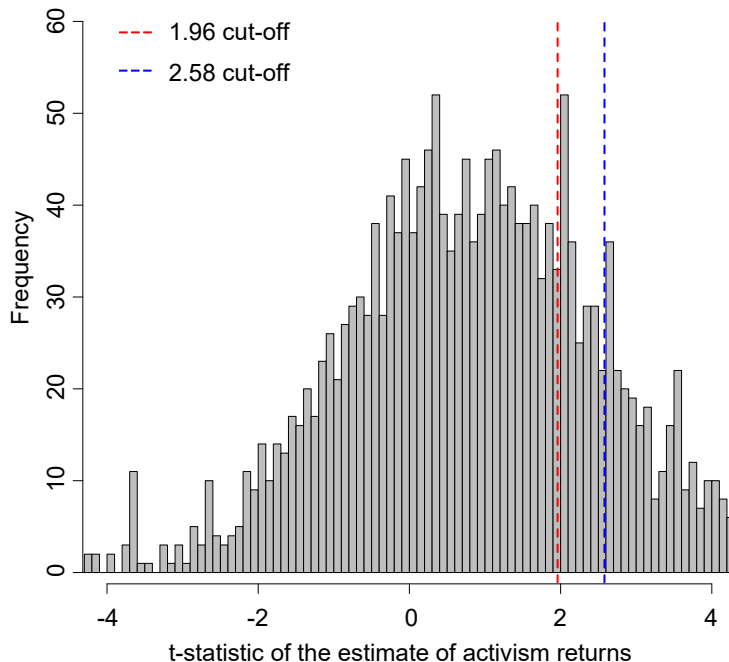
standard errors. Our results thus suggest that shareholder activism creates value but, after controlling for selective publication of empirical, its magnitude is more modest than commonly thought.

3.3. *P-Hacking*

In this section, we complement our analysis of selective publication by examining the role of statistical significance of empirical results (rather than their sign or magnitude). The tendency to selectively publish empirical tests that just surpass common benchmarks for statistical significance is often referred to as “*p*-hacking” (e.g., Harvey, 2017). Statistically significant results may be considered more “attractive” for publication because they provide fairly clear support for the relationship of interest, which is quite straightforward to interpret. In contrast, insignificant results may arise either due to the absence of the proposed relationship or insufficient power of statistical tests. Discriminating between the two explanations is challenging. Insignificant results are thus arguably less informative. Journal editors may prefer studies that include significant results, which may incentivize authors to select them for publication. Such a tendency would contribute to the bias in the pool of published estimates. Harvey (2017) argues that *p*-hacking is indeed rather prevalent in asset pricing literature.

P-hacking is observable in the distribution of published test statistics. Under selective reporting of significant results, *t*-statistics just exceeding the 5% level significant threshold at 1.96 and the 1% threshold at 2.58 should be over-represented relative to those just below these thresholds. Figure 5 shows a histogram of *t*-statistics that correspond to the price response estimates in our sample. As expected, the shape of the distribution resembles a normal distribution. Nevertheless, we observe discontinuities around two cut-off levels for statistical significance at 1.96 and 2.58. For both thresholds, the incidence of *t*-statistics just exceeding the threshold is more than 1.5 times greater than the number of *t*-statistics just below it. This pattern suggests that academic journals tend to over-report results that are

Figure 5: Visible jumps at critical t -statistic values

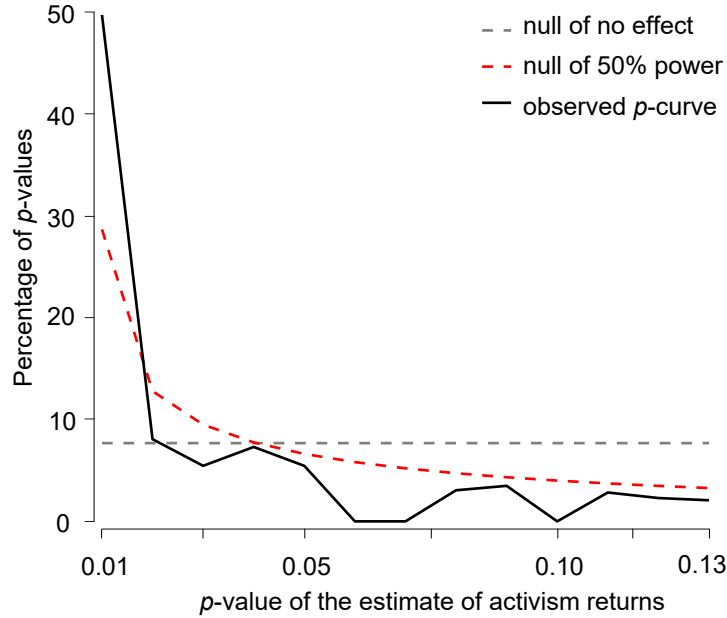


Note: The figure shows the distribution of t -statistics corresponding to the short-term stock returns surrounding shareholder activism campaigns. The vertical dashed lines indicate the boundary of 1.96, which corresponds to statistical significance at 5% level, and 2.58, which corresponds to statistical significance at 1% level.

statistically significant, which may distort readers' views about the strength and consistency of empirical evidence on the positive value created by shareholder activism.

Unsurprisingly, we reach similar conclusions when observing the p -value curve depicting the distribution of the levels of significance of reported value creation estimates (Simonsohn *et al.*, 2014b,a). Figure 6 shows that the estimates just below 5% level are under-represented. In contrast, estimates that would potentially be significant at 5% and higher are under-represented. It is merely a convention to consider results below the 5% thresholds significant. Thus, absent selective publication, there is no reason to expect p -values to be concentrated around this arbitrary threshold. The documented pattern is thus likely to be generated by deliberate choices to report significant results.

Figure 6: Estimates just below 0.05 and 0.1 p -values are slightly over-represented



Note: The figure depicts the p -curve based on Simonsohn *et al.* (2014b,a). The dashed curves show the expected uniform distribution of p -values under the null effect (the grey flat line, lighter in grayscale) and the expected right-skewed distribution with an effect of 50% power (the red dashed line, darker in grayscale). The solid line shows the observed p -curve representing the distribution of the levels of significance of value creation estimates collected from primary studies.

We use the caliper test (Bruns *et al.*, 2019; Gerber *et al.*, 2008; Gerber & Malhotra, 2008) to formally evaluate whether the patterns observed in Figure 5 and Figure 6 represent significant breaks in the distribution. The caliper test is based on a comparison of the proportion of results with corresponding p -values in narrow equal-sized intervals just above and just below the cut-off levels (referred to as “calipers”). In case of no “ p -hacking”, the incidence of reported coefficients with p -values in the narrow interval just above the threshold (“over caliper”) should be comparable to the incidence of reported coefficients with p -values just below the threshold (“under caliper”), i.e., the over-to-under caliper ratio is expected to be equal to 0.5 (50:50) (Clopper & Pearson, 1934). Ioannidis *et al.* (2017) argue that

to detect “ p -hacking” in economics and finance research, the cut-off level for over-to-under caliper ratio should be adjusted for the fact that empirical tests in these fields tend to be underpowered. We follow Bruns *et al.* (2019) who suggest the over-to-under ratio of 0.4.

Table 3 presents our results based on four caliper widths (0.05, 0.1, 0.15, and 0.2). We observe consistent evidence on p -hacking at H1: $C \leq 0.4$ for both the 1.96 and the 2.58 levels. The lower bounds of the 95% confidence interval for the over-to-under caliper ratio are all above the critical value of 0.4. Furthermore, for the narrowest caliper of 0.05, the entire 95% confidence interval is above the critical value of 0.5 even for the 2.58 significance level. These results suggest that estimates of price responses to shareholder activism that narrowly surpass the thresholds for statistical significance at 1.96 and 2.58 are more likely to be published compared to estimates that just fall short of the threshold. These results provide further support for the selective publication of results in the activism literature.

Table 3: Caliper tests corroborate some bias in reporting

| t -statistic | Caliper size | Caliper ratio | 5%CI |
|----------------|--------------|---------------|---------|
| 1.96 | 0.05 | 0.520 | (0.411) |
| | 0.10 | 0.491 | (0.417) |
| | 0.15 | 0.531 | (0.469) |
| | 0.20 | 0.522 | (0.467) |
| 2.58 | 0.05 | 0.633 | (0.512) |
| | 0.10 | 0.544 | (0.457) |
| | 0.15 | 0.520 | (0.447) |
| | 0.20 | 0.523 | (0.458) |

Note: The table shows the over-to-under caliper ratio for caliper sizes of 0.05, 0.1, 0.15, and 0.2 around significance thresholds at 1.96 and 2.58. The numbers in parentheses represent the lower bound of the 95% confidence intervals.

4. Activism Characteristics

In the second part of this paper, we exploit the heterogeneity of our sample and examine how different characteristics of individual estimates and research design choices influence the reported magnitude of the value shareholder activism creates. In Table A1 we provide definitions of these characteristics coded into explanatory variables used later in our analysis.

Table A2 provides the descriptive statistics for selected dummy variables that represent different, mostly exclusive, subsamples of our data. We comment on the descriptive statistics in the Appendix. For our regression analysis, we Winsorize all continuous variables at the top and bottom 1%.

4.1. *Bayesian Model Averaging*

In formally examining the importance of the explanatory variables, we face the “model uncertainty” problem. Prior literature does not provide clear guidance on a specific set of conceptually grounded determinants of the impact of shareholder activism on firm value. Including all potentially relevant variables in a single regression may be problematic due to multi-collinearity. To address this problem, we use Bayesian Model Averaging (BMA), which considers various combinations of potential determinants and evaluates how consistently they explain the variation in the dependent variable (e.g., Moral-Benito, 2015). BMA weighs alternative regression specifications by their posterior model probability (PMP), i.e., their “goodness of fit”¹. Relevance of “candidate” explanatory variables is evaluated based on their posterior inclusion probability (PIP), which represents the likelihood that a given variable is included in the “true” model. To interpret our results, we follow Jeffreys (1961) and Raftery (1995), who suggest that variables with PIP greater than 0.99 should be seen as “decisive” for explaining the variation in the dependent variable, those with PIP greater than 0.95 should be interpreted as having a “strong” effect, those with PIP greater than 0.75 should be seen as having a substantial effect, and those with PIP greater than 0.50 should be viewed as having a “weak” effect.

Since it would be technically cumbersome to evaluate all possible combinations of potential explanatory variables, we employ the Markov Chain Monte Carlo process (MCMC) with Metropolis-Hastings algorithm (Zeugner *et al.*, 2015) to identify the most probable regression specifications. In our baseline specification, we follow Eicher *et al.* (2011) and

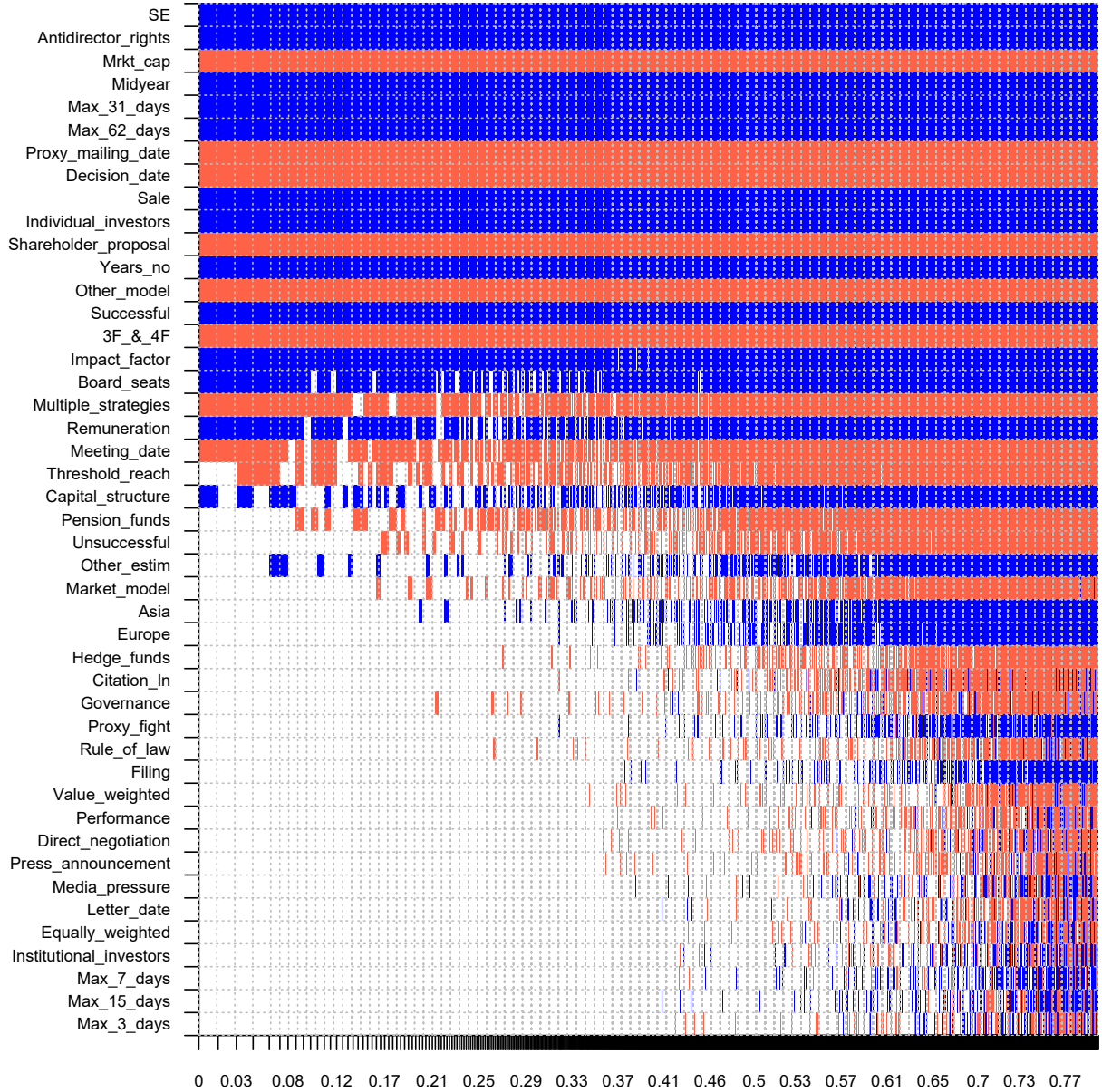
¹In Bayesian econometrics, PMP roughly corresponds to the R^2 measure in frequentist econometrics.

employ the unit information g-prior, which sets all the regression coefficients to zero and attributes to them the weight of one data point, which implicitly assumes the absence of *a priori* knowledge about the importance of the individual characteristics. We use the dilution model prior proposed by George (2010) because it gives the models with highly collinear variables less weight in the overall evaluation. We examine the robustness of our findings to these parameters in subsection 4.2.

Figure 7 provides a visualization of our BMA results. The alternative regression specifications in columns are ordered based on their PMP represented by each column's width. The individual explanatory variables in rows are sorted based on their PIP with the most relevant variables listed at the top. Blue cells (darker in grayscale) represent a positive association between a given explanatory variable and the dependent variable and red cells (lighter in grayscale) denote a negative association. Figure 7 shows that about one-half of the variables we consider are actually included in the regression models with the best fit. BMA identifies four models with PMP considerably above the remaining models. These four models share 20 explanatory variables. It is reaffirming to observe that the associations between all of these 20 variables and the dependent variable are consistent across all the models considered in the BMA. Such consistency implies that the inclusion of other variables does not affect the nature of the identified association.

To assess the importance of individual explanatory variables we tabulate our BMA results in Table 4. The left panel of the table shows PIP for individual explanatory variables, as well as their posterior mean (P. mean) and posterior standard deviation (P. SD). The latter two measures are computed from the distribution of slope coefficients from various regression specifications considered in BMA. The posterior mean represents the typical value a particular coefficient has and its standard deviation shows how the estimated coefficients vary in different combinations of explanatory variables.

Figure 7: Bayesian Model Averaging visualization



Note: This figure provides a visualization of our BMA results. On the vertical axis the explanatory variables are sorted on their PIP from the highest at the top to the lowest at the bottom. On the horizontal axis the individual regression models are ordered based their PMP with the models with the best fit on the left. A blue color (darker in the gray scale) means that the corresponding explanatory variable has a positive effect on the dependent variable in a given regression specification. A red color (lighter in the gray scale) means that the variable has a negative effect. No color means that the variable is not included in the model. We report the corresponding numerical results in Table 4. We provide the definition of variables in Table A1.

Table 4: Why the activism returns vary

| | Bayesian Model Averaging | | | Frequentist Check (OLS) | | |
|------------------------------|--------------------------|-------|-------|-------------------------|-------|---------|
| | P. mean | P. SD | PIP | Coef. | SE | p-value |
| Constant | -5.181 | NA | 1.000 | -5.133 | 1.508 | 0.001 |
| SE (Publication bias) | 0.264 | 0.043 | 1.000 | 0.270 | 0.098 | 0.007 |
| <i>Activism sponsors</i> | | | | | | |
| Hedge_funds | -0.068 | 0.198 | 0.131 | | | |
| Pension_funds | -0.237 | 0.320 | 0.407 | | | |
| Institutional_investors | 0.003 | 0.045 | 0.017 | | | |
| Individual_investors | 1.729 | 0.338 | 1.000 | 1.761 | 0.928 | 0.062 |
| <i>Activism approaches</i> | | | | | | |
| Shareholder_proposal | -1.319 | 0.225 | 1.000 | -1.241 | 0.299 | 0.000 |
| Direct_negotiation | -0.001 | 0.041 | 0.016 | | | |
| Proxy_fight | 0.010 | 0.070 | 0.033 | | | |
| Multiple_strategies | -0.575 | 0.338 | 0.811 | -0.735 | 0.249 | 0.004 |
| Media_pressure | 0.003 | 0.051 | 0.018 | | | |
| <i>Activism objectives</i> | | | | | | |
| Performance | -0.002 | 0.035 | 0.019 | | | |
| Governance | -0.005 | 0.058 | 0.028 | | | |
| Board_seats | 0.522 | 0.262 | 0.870 | 0.577 | 0.359 | 0.112 |
| Remuneration | 0.681 | 0.495 | 0.726 | | | |
| Capital_structure | 0.367 | 0.511 | 0.390 | | | |
| Sale | 1.612 | 0.258 | 1.000 | 1.545 | 0.621 | 0.015 |
| <i>Activism's success</i> | | | | | | |
| Successful | 0.803 | 0.269 | 0.964 | 0.840 | 0.529 | 0.117 |
| Unsuccessful | -0.214 | 0.324 | 0.353 | | | |
| <i>Geographic regions</i> | | | | | | |
| Asia | 0.050 | 0.201 | 0.084 | | | |
| Europe | 0.023 | 0.138 | 0.043 | | | |
| <i>Institutional setting</i> | | | | | | |
| Antidirector_rights | 0.552 | 0.096 | 1.000 | 0.528 | 0.169 | 0.003 |
| Rule_of_law | -0.016 | 0.119 | 0.038 | | | |
| Mrkt_cap | -0.020 | 0.003 | 1.000 | -0.018 | 0.004 | 0.000 |
| <i>Event Types</i> | | | | | | |
| Press_announcement | -0.002 | 0.031 | 0.018 | | | |
| Proxy_mailing_date | -1.615 | 0.317 | 1.000 | -1.333 | 0.625 | 0.037 |
| Meeting_date | -0.515 | 0.440 | 0.647 | | | |
| Filing | 0.003 | 0.039 | 0.018 | | | |
| Decision_date | -1.675 | 0.238 | 1.000 | -1.616 | 0.505 | 0.002 |
| Letter_date | -0.002 | 0.044 | 0.016 | | | |
| Threshold_reach | -0.487 | 0.450 | 0.602 | | | |
| <i>Event Windows</i> | | | | | | |
| Max_3_days | 0.0005 | 0.016 | 0.013 | | | |

(continued on next page)

Table 4: Why the activism returns vary (continued)

| | Bayesian Model Averaging | | | Frequentist Check (OLS) | | |
|------------------------------------|--------------------------|-------|-------|-------------------------|-------|---------|
| | P. mean | P. SD | PIP | Coef. | SE | p-value |
| Max_7_days | 0.0003 | 0.022 | 0.013 | | | |
| Max_15_days | -0.00003 | 0.018 | 0.013 | | | |
| Max_31_days | 0.859 | 0.176 | 1.000 | 0.829 | 0.568 | 0.149 |
| Max_62_days | 3.026 | 0.237 | 1.000 | 3.106 | 0.570 | 0.000 |
| <i>Returns models</i> | | | | | | |
| Market_adjusted | 1.281 | 0.215 | 0.997 | 1.240 | 0.348 | 0.001 |
| Market_model | 0.929 | 0.204 | 0.997 | 0.916 | 0.353 | 0.012 |
| 3F_&_4F | -0.010 | 0.079 | 0.030 | | | |
| <i>Index weighting</i> | | | | | | |
| Equally_weighted | 0.0001 | 0.019 | 0.014 | | | |
| Value_weighted | -0.002 | 0.024 | 0.017 | | | |
| <i>Estimation method</i> | | | | | | |
| Other_estim | 0.300 | 0.446 | 0.356 | | | |
| <i>Sample characteristics</i> | | | | | | |
| Years_no | 0.072 | 0.017 | 0.999 | 0.071 | 0.037 | 0.062 |
| Midyear | 0.122 | 0.017 | 1.000 | 0.114 | 0.024 | 0.000 |
| <i>Publication Characteristics</i> | | | | | | |
| Impact_factor | 0.210 | 0.095 | 0.910 | 0.200 | 0.124 | 0.112 |
| Citation_ln | 0.000 | 0.034 | 0.044 | | | |
| #Observations | | 1,973 | | | 1,973 | |
| #Studies | | 67 | | | 67 | |

Notes: The table shows the results of the multivariate analysis of value creation determinants. The dependent variable is the price response to shareholder activism campaigns collected from 67 primary studies. The left part of the table includes results based on the BMA estimation. BMA employs uniform model prior (Eicher *et al.*, 2011) and dilution prior suggested by George (2010), which accounts for potential multi-collinearity among variables. The right part presents the results of the “frequentist check” based on an OLS regression that includes the 20 explanatory variables that BMA identifies as most relevant for explaining the variation in the dependent variable that have PIP higher than 0.5. *PIP* denotes the posterior inclusion probability of a given variable in the “true” explanatory model. *P. mean* shows the posterior mean of the distribution of regression coefficients. *P. SD* represents the posterior standard deviation of the distribution of regression coefficients. *Coef.* denotes the slope coefficient based on the OLS estimation. *SE* shows the standard error of the slope coefficient in the OLS regression model. The *p-value* show the probability of obtaining the result for a given explanatory variable under the assumption that the variable has no explanatory power (i.e. the null hypothesis is correct). Definition of all variables in Table A1.

We interpret our findings primarily based on the BMA estimates. Nevertheless, for the sake of comparison with frequentist econometric approaches, the right panel of Table 4 also shows OLS results from a regression model that includes all variables with PIP greater than 0.75 (denoting a “substantial” effect based on Jeffreys, 1961, and Raftery, 1995). We report

slope coefficients (Coef.), their standard errors (SE), and the corresponding p -values.

Publication bias. Consistent with our univariate statistics reported in Table 2, the multivariate results in Table 4 also provide strong evidence on a positive association between the price response estimates and their standard error (SE), which is consistent with selective reporting of empirical results in this stream of literature. As apparent from the ordering of the individual variables in Figure 7, SE has the highest PIP out of all considered explanatory variables. Table 4 shows that its posterior mean is positive 0.264%, and its PIP approaches 1.000. This result is corroborated by the OLS model where the slope coefficient is equal to 0.270% (which is very close to the estimate based on BMA) and statistically significant at a lower than 1% level (p -value 0.007). Thus, even in a multivariate setting, we observe strong support for the proposition that reporting of empirical results on the value created by shareholder activism suffers from the publication selection bias. In other words, our earlier results on selective publication are robust to the inclusion of various activism characteristics. This bias leads to a distortion in the pool of published results, which may affect the established understanding of the impact of shareholder activism on firm value.

Table 4 also identifies 13 additional variables with PIP greater than 0.99 (conventionally interpreted as “decisive” for explaining the variation in the dependent variable, see e.g. Jeffreys 1961 or Raftery, 1995). These relate to the type of activism sponsors, their objectives, the approaches they use to achieve their goals, and the nature of the institutional setting within which activism takes place. We also find that the reported estimates are affected by research design choices and data set characteristics, such as the types of events around which the impact of activism is measured, the length of the event windows, the choice of a model for normal returns, as well as the length and recency of the data sample used in the primary studies. Below, we discuss these results.

Activism sponsors. Consistent with our univariate results, Table 4 shows that stock prices respond more favorably to announcements of activism conducted by individual rather than institutional investors. The posterior mean for *Individual_investors* of 1.729 is nu-

merically rather close to the slope coefficient in the OLS regression of 1.761. This suggests that the price response to announcements of individual investors' activism is, on average, by more than 1.5 percentage points higher. The result generated by BMA is strongly statistically significant with PIP approaching 1.000. The slope coefficient based on OLS approaches the conventional level of statistical significance at 5% (p -value 0.062). These findings are consistent with our earlier proposition that individual investors have strong incentives to engage in value-enhancing changes and they may be better positioned to withstand any temporary increases in stock price volatility that the conflict with the firm's management may involve. This result is also consistent with prior research by Bassen *et al.* (2019) who discuss the prominent role of individual shareholder activists, but it differs from Filatotchev & Dotsenko (2015) who report little or negative value creation by the activism of individual shareholders.

Interestingly, stock price responses to activism by the three remaining types of sponsors, i.e., hedge funds, pension funds, and other institutional investors, do not seem to differ from the reference group, for which the sponsor type is not specified. We observe PIP below 0.5 for all three variables that represent these investor types. This result is particularly interesting for hedge funds. Much of prior research suggests that hedge funds tend to be strongly incentivized and better equipped to conduct effective activism campaigns. Becht *et al.* (2017) and Denes *et al.* (2017) quantify the value created by hedge fund activism around 5% to 6%.

Our conditional descriptive statistics reported in Table A2 indeed show fairly high unconditional mean price responses to hedge fund activism. Nevertheless, in our multivariate analysis, which takes into consideration other characteristics of shareholder activism, we observe a slightly negative posterior mean of -0.068 and a negligible PIP of 0.131. Our evidence thus suggests that the value generated by hedge fund activism does not substantially differ from the value generated by activism sponsored by other types of institutional investors. This finding underscores the importance of considering the multitude of activism character-

istics and adjusting for potential publication selection bias. Even though unconditionally, the stock price response to hedge fund activism is rather positive, hedge funds seem to engage in the type of activism that would be rewarded comparably if it were performed by other types of institutional investors.

Activism approaches. We next examine how price responses to shareholder activism differ across the individual approaches used by the sponsors. Bhandari *et al.* (2021) suggest that conflicts and frictions between the activists, other shareholders, and managers limit the effectiveness of shareholder proposals as an activism tool. We indeed observe more attenuated price responses to shareholder proposals. The posterior mean of -1.319 resulting from BMA is somewhat more negative than the slope coefficient of -1.241 based on OLS. Nevertheless, both of these results are clearly statistically significant with PIP approaching 1.000 and the p -value approaching 0.000. It thus seems that due to the non-confrontational nature of shareholder proposals and the uncertainty over whether they will ultimately be approved the stock price response to this type of activism is less positive. This finding is consistent with Denes *et al.* (2017) who report a weak impact of shareholder proposals and strong market responses to announcements of proxy fights.

Our results also provide weaker evidence on less positive price responses to activism using multiple strategies. Both the posterior mean in BMA -0.575 and the slope coefficient in OLS of -0.735 are smaller in magnitude than the corresponding figures for shareholder proposals. Furthermore, its PIP of 0.811 surpasses only the 0.75 threshold for a “substantial” effect. In OLS, the numerically smaller slope coefficient is still strongly statistically significant with the p -value of 0.004. The individual phases of these multiple strategies may be initiated at different points in time after the preceding attempts failed to deliver the desired results. We conjecture that the gradual release of information about the individual steps in the multiple strategies may attenuate the price response to any single announcement.

Prior research documents strong price responses to announcements of proxy fights (e.g., Mulherin & Poulsen, 1998; Boyson *et al.*, 2017). Our univariate results presented in Table A2

also show fairly large positive price responses to announcements of proxy fights. Nevertheless, contrary to the univariate results, we do not observe stronger price responses to proxy fights after controlling for other characteristics. The posterior mean of 0.010 is small in magnitude and its PIP of 0.033 is well below all the cut-offs for relevant variables. Hence, we conclude that price responses to announcements of proxy fights, as well as those related to direct negotiations and media pressure, do not substantially differ from price responses observed for the generic category of activism approaches. We presume that this generic category likely includes many of the more aggressive shareholder activism approaches, which may explain why we do not observe a statistically important difference between the two groups.

Overall, our results are broadly consistent with prior research that suggests that the more confrontational approaches are more likely to affect firm value than the less assertive approaches (e.g., shareholder proposals or direct negotiations as in Cunat *et al.*, 2012; Denes *et al.*, 2017; Filatotchev & Dotsenko, 2015; Karpoff *et al.*, 1996; Prevost *et al.*, 2012; Wahal, 1996). Most notably, we observe weaker price responses for shareholder proposals.

Activism objectives. Prior research also suggests that value created by shareholder activism varies with its objectives (Brav *et al.*, 2008a; Denes *et al.*, 2017; Greenwood & Schor, 2009; Mulherin & Poulsen, 1998). Consistent with the descriptive statistics presented in Table A2, we observe more positive price responses to activism intended to make the company a takeover target. Again, the magnitude of the posterior mean in BMA of 1.612 is very close the OLS regression coefficient of 1.545. Both findings are statistically significant with PIP approaching 1.000 and the p -value of 0.015. These findings are consistent with Greenwood & Schor (2009) who single out potential prospective takeovers as the main underlying reason for the positive price response to activism announcements.

We also observe weaker evidence on stronger price responses to activism aimed at obtaining greater board representation (*Board_seats*). Both the BMA posterior mean of 0.522 and the OLS slope coefficient of 0.577 are smaller than those for *Sale*. Although the simple OLS estimation with p -value of 0.112 deems the result insignificant, in BMA estimation, PIP

consistently above 0.8 suggests the effect is “substantial”.

We find only weak evidence on differential price responses to announcements of activism that pursues other goals. PIP of none of the remaining indicator variables categorizing activism objectives exceeds the cut-off level of 0.75. Thus we conclude that our multivariate analysis does not identify differential price responses to shareholder activism pursuing objectives other than company sales and greater board representation.

Activism success. Our conditional descriptive statistics reported in Table A2 also distinguish between activism campaigns that are identified in the primary studies as successful in achieving their stated objectives and those that are not. Prior research that examines the importance of activism’s success typically concludes that success indeed matters for the value that activism creates (e.g., Mulherin & Poulsen, 1998; Cunat *et al.*, 2012; Boyson *et al.*, 2017). Our multivariate analysis supports this conjecture. As expected, the posterior mean for the indicator variable denoting successful activism is positive (0.803) and the one representing unsuccessful activism is (small but) negative (-0.214). PIP of the variable for successful activism is 0.964, which is slightly below PIP of the most relevant variables that we consider in our analysis, but still well above the threshold of 0.95 commonly used to denote a “strong” effect. In the OLS estimation, the slope coefficient for successful activism of 0.840 is also positive and it approaches statistical significance with the corresponding p -value of 0.117. On the other hand, the slope coefficient for the variable representing unsuccessful campaigns is negative but its PIP of 0.353 is below the thresholds for the variable’s relevance. Hence, in our sample, we do not observe a significant difference in value creation by unsuccessful campaigns and those that are not classified in terms of their success.

These results are consistent with our expectations. They also convey a clearer message than the pattern in the descriptive statistics that we report in Table A2. Controlling for other activism characteristics leads to an intuitive order of results where the price response is most positive to successful activism, less positive for activism for which the level of success isn’t classified, and the weakest for activism identified as unsuccessful. Thus, we conclude

that our evidence suggests that success in achieving activism’s goals is positively associated with the value it creates for shareholders.

Geographic regions. The heterogeneity of our data set also allows us to compare the impact of shareholder activism across various regions. Our conditional descriptive statistics reported in Table A2 show only minor differences across regions. Consistent with our univariate results, Table 4 shows no significant differences between Asia and Europe relative to the U.S. The posterior means for both *Asia* and *Europe* are positive but very small in magnitude (0.050 and 0.023 respectively). As expected, these results do not pass the conventional cut-offs used to evaluate the relevance of these variables. PIP for *Asia* is equal to 0.084 and the one for *Europe* is 0.043, both of which are below the lowest threshold of 0.5 for a “weak” effect. We conclude that our evidence does not identify major differences in price responses to shareholder activism across geographic regions.

Institutional setting. We document that the quality of the institutional setting, in which shareholder activism takes place, matters for how much value it creates. Table 4 shows that the price response to activism is stronger when shareholder rights are better protected. The posterior mean for the anti-director rights index (*Antidirector_rights*) of 0.552 is numerically very close to the slope coefficient based on the OLS regression of 0.528. Both of these results are strongly statistically significant, with PIP approaching 1.000 and the p -value of 0.003.

We consider this finding remarkable for several reasons. First, it underscores the importance of institutional framework quality for the effectiveness of corporate governance mechanisms. Local regulatory authorities have considerable discretion in how they shape the institutional framework. Our results are thus highly relevant for policymakers as they provide evidence of the merits of the country-level regulatory framework quality. Specifically, our study documents that firm-level application of one of the key corporate governance mechanisms, i.e., shareholder activism, may be more or less effective in enhancing firm value depending on the quality of the country-level regulatory framework.

Second, we note that it would be impossible to obtain this result in a study based on a data set from a single institutional setting. Our meta-analysis thus goes beyond synthesizing prior empirical results and adjusting them for publication selection bias. Our methodological approach allows us to reach new conclusions based on comparing a diverse pool of estimates reported in relatively many primary studies. Our approach is similar to Holderness (2018) who performs a meta-analysis of price responses to firms' announcements of new equity issues. He exploits the variation in country-level requirements for mandatory shareholder approval and documents their importance for explaining how much value stock issues create.

Our research complements a recent paper by Maffett *et al.* (2022) who examine shareholder activism with the use of a large international sample and exploit differences in the institutional framework. They construct an index that captures the transparency of firm-level governance processes and document a higher incidence of shareholder activism in settings where it is high. They also show that changes in this measure affect outcomes in firms that are not themselves targeted by activists but that may be threatened by activism in the future. We complement these findings by providing additional evidence in support of the proposition on the importance of regulatory framework quality using a different methodological approach and examining price responses to shareholder activism rather than its incidence. In particular, we show that after controlling for a wide range of potentially relevant explanatory factors, better shareholder protection makes activism more effective (rather than more frequent) even in firms that are targeted by activists (rather than being threatened by potential future activism). Our findings thus support and extend the empirical evidence presented in Maffett *et al.* (2022).

The second measure that we observe to be systematically related to the value created by shareholder activism is the aggregate stock market capitalization relative to a country's GDP (*Mrkt_Cap*). Table 4 shows that *ceteris paribus* shareholder activism is more effective in settings where stock markets are smaller relative to the size of the economy. Again, we clearly observe this result both using the BMA and OLS estimation. The posterior mean of -0.020 is

very close to the OLS regression slope coefficient of -0.018. Both of these results are strongly statistically significant with PIP approaching 1.000 and the p -value approaching 0.000. The correlation between the two institutional setting measures is 0.436 (not tabulated), which implies that these results are not driven by multi-collinearity between them.

Controlling for the shareholder protection quality, larger stock markets typically imply better benchmarking possibilities across individual companies. In larger stock markets, best practices may more easily spread from the stronger to the weaker firms. Therefore, greater inefficiencies may arise in markets that are smaller. Addressing these inefficiencies by shareholder activism campaigns may have a greater potential to enhance firm value, which may explain why it is associated with particularly large price responses. Furthermore, it is also possible that investors spend more resources monitoring potential targets in large and liquid markets where they have better possibilities to accumulate a sufficiently large ownership stake, which may be important to make their activism campaign successful. Hence, in smaller markets greater inefficiencies may be needed to trigger activists' action.

Our BMA analysis also identifies several additional characteristics related to the research design, data set, and publication quality relevant to the documented price responses. These findings help researchers compare and contrast prior empirical results and inform them about the likely impact of their own research design choices.

Event types. Table 4 shows that the magnitude of the reported price responses is affected by the type of events around which they are measured. Descriptive statistics reported in Table A2 show that the reported results are most positive when measured around the first announcement day, which constitutes the reference category in our BMA analysis. The posterior means are negative for all event types with the exception of the regulatory filing date (for which we observe a posterior mean very close to zero, i.e., 0.003). The posterior mean for the *Proxy_mailing_date* is -1.615, and the one for the *Decision_date* is -1.675. In both cases PIP approaches 1.000 and the results are statistically significant at the conventional 5% level also in the OLS estimation. The latter result is somewhat surprising in the light

of prior literature that suggests substantial value creation at the decision date (Bizjak & Marquette, 1998; Karpoff *et al.*, 1996).

Event windows. We also observe that the reported magnitude of value created by shareholder activism increases with the length of the estimation window. Using the 1-day window as a reference category in our BMA analysis, we document more positive price responses for the two longest windows, namely, the one up to 31 days and the one up to 62 days. For the longest event window category up to 62 days we observe the posterior mean of 3.026, which is again very close to the slope coefficient from the OLS regression of 3.106. Both of these results are strongly statistically significant with PIP approaching 1.000 and the p -value approaching 0.000. By comparison, for the second-longest window of up to 31 days, we observe a posterior mean of 0.859 and an OLS slope coefficient of 0.829, both of which are smaller in magnitude. Furthermore, only the result based on the BMA estimation is statistically significant with PIP approaching 1.000. The slope coefficient based on the OLS estimation has a p -value of 0.149. In contrast to the more positive price responses that we observe for the two longest event windows we consider, PIP for the remaining three windows (up to 3, 7, and 15 days) are all close to zero. This indicates that after controlling for activism characteristics the reported estimates based on these windows do not materially differ from the estimates measured over the default 1-day window.

Returns models and other estimation characteristics. Our results also show that researchers' choices of models used to measure the normal rate of return affect the reported estimates. Specifically, price response estimates based on the three-factor (3F) and the four-factor (4F) models do not materially differ from the estimates in our reference category, which typically also involves more sophisticated ways of adjusting for systematic risk. In contrast, we observe more positive estimates of price response to shareholder activism in the studies based on market-adjusted returns and on the market model. For the market-adjusted returns, we observe the posterior mean of 1.281 and the slope coefficient in our OLS regression of 1.240. Both of these results are strongly statistically significant, with PIP of

0.997 and the p -value of the regression coefficient in OLS of 0.001. Similarly for the market model, we observe only a slightly lower posterior mean of 0.929 and the OLS regression slope coefficient of 0.916. Again, both of these results are statistically significant, with PIP of 0.997 and the p -value of 0.012.

Nelson (2006) suggests that the choice of the returns models is unlikely to substantially affect the results. In contrast, our results suggest that empirical studies that use simpler ways of adjusting for the normal rate of return are more likely to report stronger price responses to shareholder activism. We find this result intuitive since the market-adjusted returns and the market model might not fully control for the differences in the exposure of the targeted companies to systematic risk. Results reported in the studies may thus overstate the *true effect* due to insufficient risk adjustment. Indeed, reported results based on more sophisticated returns models are likely more convincing than those that use simpler ways of adjusting for the normal rate of return.

In contrast to the importance of the returns models, we do not observe any meaningful differences between estimates that use equally-weighted and value-weighted market returns. This finding is consistent with Denis & Serrano (1996); Nelson (2006); Chen *et al.* (2020). Similarly, we do not observe meaningful differences between estimates based on various estimation methods.

Sample characteristics. Our results also show that data sample characteristics matter for the magnitude of the reported estimates. Table 4 shows that primary studies that use longer and more recent data samples report more positive price responses to shareholder activism. The posterior mean of *Years_no* of 0.072 is positive and its PIP of 0.999 indicates that data sample length is highly relevant for explaining the variation in price responses to shareholder activism. We observe a similar, albeit statistically slightly weaker, result based on OLS. The slope coefficient of 0.071 is numerically very similar to the posterior mean based on BMA and with the p -value of 0.062 it approaches statistical significance at the 5% level. We also observe more positive price responses to shareholder activism in studies that

are based on more recent data sets. The posterior mean *Midyear* is equal to 0.122, and the slope coefficient in our OLS estimation is equal to 0.114. Both of these results are strongly statistically significant, with PIP approaching 1.000 and the p -value approaching 0.000. We verify that these two results are distinct and they are not driven by the correlation between the two variables (which is actually slightly negative, -0.205, not tabulated). Given that both the length of the sample period and data set recency may be seen as proxies of data quality, our results suggest that prior studies based on higher-quality data report larger estimates of value created by shareholder activism.

Publication characteristics. Lastly, we consider proxies for publication quality, namely the impact factor of the journal where the study is published and the normalized number citations. We observe that studies published in more influential journals report more positive estimates. The posterior mean of *Impact_factor* is equal to 0.210 and its corresponding PIP of 0.910 indicates that the journal's impact factor has a *substantial* effect for explaining the variation in price responses to shareholder activism. In OLS we observe a numerically similar slope coefficient of 0.200. Nevertheless, the p -value of 0.112 implies that this result is just below the 10% level of statistical significance. In contrast to the documented relevance of academic journal quality, the insignificant results for *Citation_ln* suggest that the estimates of value generated by shareholder activism reported in the primary studies are not systematically related to the number of citations in Google Scholar.

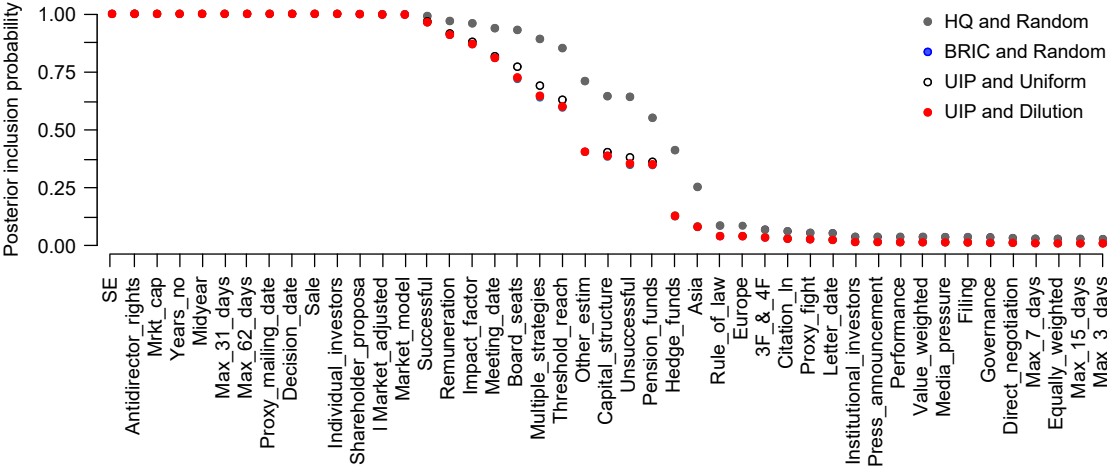
4.2. *Sensitivity Analysis*

We use Bayesian approaches to obtain our main results because the Bayesian framework has important advantages in the analysis of heterogeneity. It allows us to consider a wide range of potentially relevant explanatory variables and avoid issues of multi-collinearity. However, it may be affected by the priors used as a point of departure. Given that the size of our sample is quite large, we do not expect the choice of these priors to have a dramatic impact on our results. Nevertheless, in this subsection, we examine the sensitivity of our results to the

priors we use. Specifically, we recompute our results using several different priors proposed in prior literature, and we examine whether these modifications affect our inferences about the explanatory power of the individual variables.

We visualize the results from our sensitivity analysis in Figure 8. Our baseline model (UIP and dilution) follows George (2010). This approach modifies the model probabilities by multiplying them by the determinant of the correlation matrix of all the explanatory variables. Matrix determinants for models with low multi-collinearity between the explanatory variables will be close to one, and so the models will receive a high weight. In contrast, models that include highly multi-collinear variables have determinants close to zero, and accordingly, they receive little weight. As a robustness check, we use three additional sets of priors that reflect various combinations of *a priori* expectations.

Figure 8: Sensitivity of the results to different priors



Note: This figure summarizes the PIPs of the considered explanatory variables depending on the various g-priors and model priors used in BMA. In our baseline model, we follow Eicher *et al.* (2011) and use a unit information g-prior and a uniform model prior (UIP and Uniform) that *a priori* remains agnostic about the relevance of the individual explanatory variables. As a robustness check, we use the dilution model prior (George, 2010), which accounts for potential multi-collinearity between the considered explanatory variables. We also use a combination of the Hannan-Quinn (HQ) g-prior and random model prior (HQ and Random) that adjusts data quality. Finally, we use a combination of the BRIC g-prior and random model prior (BRIC and Random) that minimizes the prior effect on the results.

Following Eicher *et al.* (2011) we use the uniform prior on models. The uniform prior gives each estimated model the same weight. Furthermore, we apply the unit information g-

prior (UIP) on coefficients that is based on the assumption that all the regression coefficients are zero. UIP, on the other hand, has the same weight as one observation in our data. Both priors reflect the absence of any *a priori* expectations about the relevance of the individual model or any individual explanatory variable, which is typical for meta-analysis. In addition, we use the BRIC g-prior with the random model prior proposed by Fernandez *et al.* (2001), and the Hannan-Quinn (HQ) g-prior with the random model prior (Fernandez *et al.*, 2001; Ley & Steel, 2009).

Figure 8 depicts PIPs for the individual explanatory variables based on different priors. Variables are sorted based on their estimated relevance in our main test. We note that “UIP and Dilution”, “UIP and Uniform”, and “BRIC and Random” priors yield virtually identical estimates. Our baseline prior “UIP and Dilution” mostly produces slightly lower PIP than the remaining two priors, which suggests that our baseline results tend to be conservative. In comparison, “HQ and Random” indicate somewhat higher PIP for variables in the middle of the relevance spectrum. However, the order of importance of the individual explanatory variables remains mostly unchanged. Furthermore, all variables that we identify as relevant using our baseline prior are also considered such when estimated with the use of the “HQ and Random” priors. “HQ and Random” suggests greater relevance of *Board_seats*, *Multiple_strategies*, *Remuneration*, *Meeting_date*. Overall, these findings suggest that, consistent with our expectations, our inferences about the explanatory power of the individual variables are robust to the choice of priors we use in BMA.

5. Conclusions

Shareholder activism may be beneficial by curbing economic inefficiency. It may also harm firms by stifling innovation and capital-intensive projects that enhance firm value in a long run. An optimal regulatory framework should be based on an understanding of how much value activism creates and of the relevant conditioning characteristics. A systematic ex-

amination of this question is complicated by the fragmentation of the underlying data and by the use of a multitude of methodological approaches that limit the comparability of reported results. We perform a meta-analysis of 1,973 estimates of price responses to activism campaigns reported in 67 research articles, which allows us to correct the results for the publication selection bias and to examine the importance of relevant activism and research design characteristics.

We find that the pool of reported estimates overstates the “true effect” of shareholder activism. Adjusting for the publication selection bias, the price response estimates range from 0% to 1.5%. In addition, we observe that stock prices respond more positively to activism exercised by individual investors, conducted by more confrontational approaches, aimed at selling the target company, and successful in achieving its objectives. Estimates based on longer measurement periods, simpler approaches to risk adjustment, more recent and longer data sets, and published in more reputable academic journals tend to be higher. Our results provide valuable insights for regulators in designing an optimal framework for regulating investor activism, for researchers in interpreting prior results, and in making research design choices in future studies.

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Appendix

In this Appendix, we comment on the descriptive statistics for the selected dummy variables that represent different, mostly exclusive, subsamples of our data. Table A1 shows the definitions of the variables we use in BMA. Table A2 provides the descriptive statistics for these variables that represent different, mostly exclusive, subsamples of our data. For the continuous variables, we define additional indicators prefixed “Hi_” and “Lo_” that represent observations above and below the median of the full sample. Similarly, we define variables prefixed “Long_” and “Short_”, as well as “Older_” and “Recent_”.

Table A1: Definition of Variables

| Variable | Definition |
|----------------------------|---|
| <i>Main variables</i> | |
| Estimate | Estimate of the price response to shareholder activism. |
| SE | Standard error of the price response estimate. |
| <i>Activism sponsors</i> | |
| Hedge_Funds | = 1 if the activist is a hedge fund. |
| Pension_Funds | = 1 if the activist is a pension fund (e.g., Calpers, CALSTRS). |
| Institutional_investors | = 1 if the activist is an institutional investor other than a pension fund. |
| Individual_investors | = 1 if the activist is an individual investor. |
| Sponsor_na (*) | = 1 if the activist is not specified. |
| <i>Activism approaches</i> | |
| Shareholder_proposal | = 1 if the activism is conducted via shareholder proposals. |
| Direct_negotiation | = 1 if the activism is conducted via direct negotiations with managers. |
| Proxy_fight | = 1 if the activism is conducted via proxy fights. |
| Multiple_strategies | = 1 if the activism is conducted via a combination of approaches. |
| Media_pressure | = 1 if the activism is conducted via media pressure. |
| Activism_approach_na (*) | = 1 if the activism approach is not specified. |
| <i>Activism objectives</i> | |
| Performance | = 1 if the objective is the target firm’s performance improvement. |
| Governance | = 1 if the objective is the target firm’s governance improvement. |
| Board_seats | = 1 if the objective is to obtain a greater board representation. |
| Remuneration | = 1 if the objective is the target firm’s executive compensation changes. |
| Capital_structure | = 1 if the objective is the target firm’s capital structure changes. |
| Sale | = 1 if the objective is the sale of the company. |
| Objective_general (*) | = 1 if the objective is not specified. |
| <i>Activism’s success</i> | |
| Successful | = 1 if the activism reached its stated goals (at least partially). |
| Unsuccessful | = 1 if the activism did not reach its goal. |
| Outcome_na (*) | = 1 if the activism’s success is not specified. |
| <i>Geographic regions</i> | |

Continued on the next page

Table A1: Description and summary statistics of the regression variables (continued)

| Variable | Definition |
|-------------------------------|---|
| Europe | = 1 if the country of activism is from Europe. |
| Asia | = 1 if the country of activism is from Asia. |
| North_America (*) | = 1 if the country of activism is from North America. |
| <i>Institutional setting</i> | |
| Antidirector_rights | Index of shareholder protection rights by La Porta <i>et al.</i> (1997, 1998). |
| Rule_of_law | Index of law and order tradition by the World Bank (WB). |
| Mrkt_Cap | A country's aggregate stock market capitalization scaled by GDP. |
| <i>Event types</i> | |
| Press_announcement | = 1 if the price response is measured around press announcements. |
| Proxy_mailing_date | = 1 if the price response is measured around proxy mailing dates. |
| Meeting_date | = 1 if the price response is measured around shareholder meeting dates. |
| Filing | = 1 if the price response is measured around the 13D filing dates. |
| Decision_date | = 1 if the price response is measured around the dates of the decision. |
| Letter_day | = 1 if the price response is measured around the date when the letter was sent to the target firm. |
| Threshold_reach | = 1 if the price response is measured around the date when the notification threshold was reached. |
| First_announcement (*) | = 1 if the price response is measured around the earliest date when activism was announced to investors. |
| <i>Event Windows</i> | |
| The_day (*) | = 1 if the measurement window includes 1 day. |
| Max_3_days | = 1 if the measurement window is between 2 and 3 calendar days long. |
| Max_7_days | = 1 if the measurement window is between 4 and 7 calendar days long. |
| Max_15_days | = 1 if the measurement window is between 8 and 15 calendar days long. |
| Max_31_days | = 1 if the measurement window is between 16 and 31 days long. |
| Max_62_days | = 1 if the measurement window is between 32 and 62 days long. |
| <i>Returns models</i> | |
| Market_model | = 1 if the price response is computed based on the market model. |
| Market_adjusted | = 1 if the market-adjusted returns are used for the price response. |
| 3F_&_4F | = 1 if the price response is computed based on the three-factor model (3-F) (Fama & French, 1995, 1996) or the four-factor model (4-F) (Carhart, 1997). |
| Other_model (*) | = 1 if the price response is computed based on a different model. |
| <i>Index weightings</i> | |
| Equally_weighted | = 1 if an equally-weighted index is used to compute stock returns. |
| Value_weighted | = 1 if a value-weighted index is used to compute stock returns. |
| Not_eq_nor_value (*) | = 1 if the index for computing stock returns is not specified. |
| <i>Estimation method</i> | |
| OLS (*) | = 1 if an OLS estimator is used for the estimation of stock returns. |
| Other_estim | = 1 if an estimator other than OLS is used. |
| <i>Sample characteristics</i> | |
| Years_no | Length of the primary data set in years. |
| Midyear | Median year of the primary data set. |

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Table A1: Description and summary statistics of the regression variables (continued)

| Variable | Definition |
|------------------------------------|--|
| <i>Publication Characteristics</i> | |
| Impact | Recursive discounted impact factor from the RePEc database. |
| Citation_ln | Natural logarithm of the number of Google Scholar citations normalized by the number of years since posting the first draft. |

Note: The table shows the definition of variables. Asterix (*) denotes the reference category for our regression analysis.

Table A2: Descriptive statistics for different subsamples

| Variable | Nobs | Unweighted statistics | | Weighted statistics | |
|----------------------------|-------|-----------------------|------|---------------------|------|
| | | Mean | SD | W.Mean | W.SD |
| All | 1,973 | 1.49 | 3.04 | 1.83 | 3.42 |
| <i>Activism sponsors</i> | | | | | |
| Hedge_funds | 467 | 2.42 | 3.72 | 3.10 | 3.51 |
| Pension_funds | 474 | 0.84 | 1.93 | 0.56 | 1.71 |
| Institutional_investors | 129 | 0.16 | 2.72 | 0.47 | 3.19 |
| Individual_investors | 66 | 2.99 | 4.21 | 4.17 | 4.69 |
| Sponsor_na (*) | 837 | 1.43 | 2.88 | 1.74 | 3.32 |
| <i>Activism approaches</i> | | | | | |
| Shareholder_proposal | 479 | 0.35 | 1.90 | 0.29 | 1.69 |
| Direct_negotiation | 98 | 1.85 | 3.06 | 2.11 | 3.48 |
| Proxy_fight | 432 | 2.37 | 3.53 | 2.57 | 4 |
| Multiple_strategies | 287 | 0.20 | 0.68 | 0.12 | 0.78 |
| Media_pressure | 103 | 1.12 | 2.64 | 3.20 | 4.46 |
| Approach_na (*) | 574 | 2.43 | 3.60 | 3.21 | 3.71 |
| <i>Activism objectives</i> | | | | | |
| Performance | 302 | 0.71 | 1.92 | 0.78 | 2.04 |
| Governance | 350 | 0.25 | 1.75 | 0.28 | 1.93 |
| Board_seats | 360 | 2.48 | 3.62 | 2.07 | 3.72 |
| Remuneration | 117 | 0.65 | 1.93 | 1.32 | 2.58 |
| Capital_structure | 54 | 2.48 | 3.18 | 2.33 | 2.67 |
| Sale | 210 | 3.59 | 4.25 | 4.12 | 4.35 |
| Objective_na (*) | 580 | 1.34 | 2.78 | 2.23 | 3.52 |
| <i>Activism's success</i> | | | | | |
| Successful | 178 | 3.14 | 3.99 | 3.07 | 4.26 |
| Unsuccessful | 150 | 1.68 | 3.63 | 2.32 | 4.42 |
| Outcome_na (*) | 1,645 | 1.30 | 2.80 | 1.61 | 3.12 |
| <i>Geographic regions</i> | | | | | |
| Europe | 457 | 1.80 | 3.16 | 2.78 | 3.73 |
| Asia | 139 | 1.23 | 2.22 | 1.21 | 1.93 |
| North_America (*) | 1377 | 1.41 | 3.07 | 1.70 | 3.42 |

Continued on next page

Table A2: Descriptive statistics for subsamples (continued)

| Variable | Nobs | Unweighted statistics | | Weighted statistics | |
|------------------------------------|-------|-----------------------|------|---------------------|------|
| | | Mean | SD | W.Mean | W.SD |
| <i>Institutional setting</i> | | | | | |
| Lo_antidirector_rights | 499 | 1.54 | 3.04 | 2.42 | 3.49 |
| Hi_antidirector_rights | 1,474 | 1.47 | 3.04 | 1.7 | 3.39 |
| Lo_rule_of_law | 787 | 1.16 | 2.55 | 1.41 | 3.05 |
| Hi_rule_of_law | 1,186 | 1.71 | 3.31 | 2.13 | 3.63 |
| Lo_mrkt_cap | 980 | 1.48 | 3.13 | 1.98 | 3.56 |
| Hi_mrkt_cap | 993 | 1.5 | 2.95 | 1.71 | 3.3 |
| <i>Event types</i> | | | | | |
| Press_announcement | 294 | 1.08 | 2.41 | 1.11 | 2.59 |
| Proxy_mailing_date | 134 | -0.45 | 1.33 | -0.27 | 1.35 |
| Meeting_date | 186 | 0.44 | 1.42 | 0.24 | 1.62 |
| Filing | 300 | 1.87 | 3.17 | 3.38 | 3.98 |
| Decision_date | 227 | 1.87 | 3.80 | 0.51 | 2.84 |
| Letter_day | 109 | 0.48 | 0.94 | 0.52 | 1.01 |
| Threshold_reach | 114 | 0.96 | 3.28 | 4.52 | 4.59 |
| First_announcement (*) | 609 | 2.39 | 3.36 | 2.91 | 3.60 |
| <i>Event windows</i> | | | | | |
| Max_3_days | 692 | 0.68 | 1.80 | 1.27 | 2.80 |
| Max_7_days | 203 | 1.02 | 2.28 | 0.63 | 2.28 |
| Max_15_days | 444 | 1.07 | 2.65 | 1.29 | 3.23 |
| Max_31_days | 293 | 2.17 | 3.92 | 2.61 | 4.29 |
| Max_62_days | 182 | 5.52 | 4.17 | 4.83 | 3.95 |
| The_day (*) | 159 | 0.92 | 1.81 | 1.51 | 2.21 |
| <i>Returns models</i> | | | | | |
| Market_adjusted | 663 | 2.23 | 3.54 | 1.99 | 3.35 |
| Market_model | 837 | 1.10 | 2.62 | 1.90 | 3.57 |
| 3F_ & 4F | 186 | 1.21 | 2.28 | 1.74 | 2.12 |
| Other_model (*) | 287 | 1.10 | 3.07 | 1.47 | 3.53 |
| <i>Index weighting</i> | | | | | |
| Equally_weighted | 690 | 1.18 | 2.91 | 1.42 | 3.28 |
| Value_weighted | 596 | 1.26 | 2.61 | 1.63 | 3.05 |
| Weighting_na (*) | 687 | 1.99 | 3.43 | 2.42 | 3.75 |
| <i>Estimation method</i> | | | | | |
| Other_estim | 123 | 1.11 | 2.43 | 1.49 | 2.97 |
| OLS (*) | 1,850 | 1.52 | 3.08 | 1.85 | 3.44 |
| <i>Sample characteristics</i> | | | | | |
| Short_sample | 974 | 0.86 | 2.41 | 1.40 | 3.06 |
| Long_sample | 999 | 2.10 | 3.45 | 2.20 | 3.66 |
| Older_sample | 889 | 1.15 | 2.78 | 1.27 | 3.08 |
| Recent_sample | 1,084 | 1.77 | 3.21 | 2.40 | 3.64 |
| <i>Publication characteristics</i> | | | | | |
| Lo_imp_factor | 808 | 1.51 | 3.20 | 2.03 | 3.61 |
| Hi_imp_factor | 1,165 | 1.48 | 2.93 | 1.64 | 3.21 |
| Lo_cited | 982 | 1.28 | 2.97 | 1.61 | 3.52 |

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Table A2: Descriptive statistics for subsamples (continued)

| Variable | Nobs | Unweighted statistics | | Weighted statistics | |
|----------|------|-----------------------|------|---------------------|------|
| | | Mean | SD | W.Mean | W.SD |
| Hi_cited | 991 | 1.70 | 3.10 | 2.08 | 3.28 |

Notes: The table presents descriptive statistics for different subsamples of data, which are defined based on the shareholder activism characteristics we expect to affect the value activism creates. Nobs: number of estimates within each subsample. Mean and SD refer to the mean and standard deviation; W.Mean and W.SD refer to the mean and standard deviation weighted by the inverse of the number of estimates reported in each study. The asterisk (*) denotes a reference category for different groups of subsamples (dummy variables omitted from our later analyses). For a detailed description of all the variables see Table A1.

Full sample. The first row of Table A1 reports the descriptive statistics for the full sample of 1,973 observations collected from 67 studies. As already discussed the distribution of estimates is positively skewed and the mean value of 1.49% is higher than the median value of 0.50%. The skewness of the distribution can also be readily observed by comparing the distance between the median and the corresponding lower and upper percentiles. For example, the 5-th percentile is 2.39 percentage points below the median, whereas the 95-th percentile is 8.15 percentage points above the median. We observe a similar pattern for most of the individual subsamples, which suggests that the positive skewness is not restricted to specific subgroups of our observations. In addition to the simple mean, we also report in Table A1 the mean value weighted by the inverse of the number of estimates reported in individual studies, which gives all studies an equal impact on our results. For the full sample, the weighted mean (W.Mean) of 1.83% exceeds the simple mean of 1.49% implying that reported estimates tend to be slightly higher in smaller studies. However, the difference is modest, so it is unlikely our results are driven by a few large studies.

Activism sponsors. Prior literature suggests that the effectiveness of inducing value-enhancing changes varies across activism sponsors (e.g., Becht *et al.*, 2017; Denes *et al.*, 2017; Filatotchev & Dotsenko, 2015). Boyson *et al.* (2022) argue that even though prior research on hedge fund activism typically classifies activists as a single group, various types of activists are likely to exhibit different skills depending on their past experience. Sponsors differ in the strength of their incentives to enhance value and sensitivity to the risk of potential failure. For instance, individual sponsors typically keep much of their own wealth in targeted firms and so they internalize much of the value potentially created by successful activism campaigns (e.g., Bassen *et al.*, 2019; Holderness & Sheehan, 1985; Venkiteshwaran *et al.*, 2010). Hence, they may pursue their goals more tenaciously than other investors. Within the institutional investors, hedge funds have both the incentives and the flexibility to pursue aggressive activism campaigns. High-performance fees give hedge fund managers

strong incentives to enhance firm value (Bebchuk *et al.*, 2020; Bessler *et al.*, 2015; Brav *et al.*, 2008a; Klein & Zur, 2009; Krishnan *et al.*, 2016). The asymmetric nature of pay-offs encourages them to take risk (Stulz, 2007; Yang *et al.*, 2022). Light regulation and limited disclosure requirements help hedge funds accumulate larger ownership stakes and maintain flexibility in pursuing their goals (Brav *et al.*, 2008a). The “lock-up” periods may give them greater maneuvering space to launch and reap the benefits. Hedge funds also frequently coordinate activism campaigns supported by several rather than one hedge fund (in a so-called “wolf pack”) (Becht *et al.*, 2017; Coffee & Palia, 2016; Wong, 2020). Thus, they may be more effective activists.

Several prior studies distinguish between various activism sponsors (e.g., Filatotchev & Dotsenko, 2015; Renneboog & Szilagyi, 2011). Following this research, we categorize activists into individual investors and their coalitions, hedge funds (e.g., Brav *et al.*, 2010; Weber & Zimmermann, 2013), and pension funds (e.g., Carleton *et al.*, 1998; Del Guercio & Hawkins, 1999; English *et al.*, 2004; Nelson, 2006). We group the remaining observations into the fourth category (e.g., Carleton *et al.*, 1998; Caton *et al.*, 2001; Wahal, 1996). Table A2 shows that both the simple mean of 1.43% and the weighted mean of 1.74% (the median of 0.53% as well) for the 837 price response estimates in this category are very similar to the full sample values, which suggests that this group is unlikely to represent some specific unknown sponsor type.

Consistent with our expectations, the mean price response estimates are higher for individual investors (mn. = 2.99%, w.mn. = 4.17%). In this group, the sample mean is higher than the median of 0.98%, which suggests that it includes several highly positive estimates. Individual activists may thus be less consistent in enhancing firm value, but when they succeed their activism may lead to very large price increases. Within institutional investors, hedge funds activism seems to be associated with the most positive price responses (mn. = 2.42%, w.mn. = 3.10%). In contrast, the estimates are lower for pension funds (mn. = 0.84%, w.mn. = 0.56%) and for other institutional investors (mn. = 0.16%, w.mn. = 0.47%). This finding is remarkable in the context of Smith (1996) who comment on one of the pension funds: “[s]ince CalPERS is a leader in activism, if significant results are not found, results are not likely to be found for other activists” (p. 228). These results suggest that the stock market reacts more positively to activism by investors who have stronger incentives to enhance firm value and who are less constrained by shorter investment horizons.

Activism approaches. Prior research also suggests that the value created by shareholder activism depends on how it is conducted (e.g., Cunat *et al.*, 2012; Denes *et al.*, 2017; Ferri & Sandino, 2009; Filatotchev & Dotsenko, 2015; Karpoff *et al.*, 1996; Prevost *et al.*, 2012; Wahal, 1996). More confrontational approaches may have a greater impact on firm

value. Flugum *et al.* (2022) show that outside investors’ knowledge of pending activist campaigns increases activists’ likelihood of pursuing and winning a proxy fight. Denes *et al.* (2017) report value created by proxy fights (arguably one of the most confrontational methods often intended to overcome managerial resistance to proposed changes) of 6.77%, which can be compared to much lower estimates for direct negotiations (0.26%), and shareholder proposals (0.06%). Our sample contains 432 shareholder proxy fights (e.g., Boyson *et al.*, 2017; Mulherin & Poulsen, 1998). We define another category for 98 direct negotiations with the firm management, which are arguably less confrontational than a proxy fight (e.g., Carleton *et al.*, 1998; Smith, 1996). We also form a separate category 479 shareholder proposals that typically advocate specific policy changes (e.g., Prevost & Rao, 2000; Strickland *et al.*, 1996). Furthermore, we categorize media pressure as another method of shareholder activism (e.g., Bessler *et al.*, 2015; Bassen *et al.*, 2019). We set up another category for activism involving multiple methods (e.g., Del Guercio & Hawkins, 1999; Yang *et al.*, 2012). We group the remaining observations, for which the method of shareholder activism is not specified into the last category (e.g., Venkiteshwaran *et al.*, 2010).

Table A2 indeed suggests that more assertive activism approaches tend to induce greater price responses. The price response associated with proxy fights (mn. = 2.37%, w.mn. = 2.57%) tends to be higher than the one related to direct negotiation (mn. = 1.85%, w.mn. = 2.11%), which is in turn on average higher than the price response on shareholder proposals (mn. = 0.36%, w.mn. = 0.29%). We also observe moderate price responses to activism conducted using media pressure (mn. = 1.12%, w.mn. = 3.20%). In comparison, activism that combines various approaches is associated with weak price responses (mn. = 0.20%, w.mn. = 0.12%). Interestingly, the reference category, for which the activism approach is not defined, exhibits the highest mean price response (mn. = 2.43%, w.mn. = 3.21%) that is comparable to the proxy fights. These findings suggest that studies that do not explicitly specify the method of conducting the campaign likely examine campaigns that materially challenge firm management.

Activism objectives. The impact of shareholder activism may also depend on its objectives (Denes *et al.*, 2017). Their classification is not trivial because activists may pursue multiple inter-related objectives. Corporate governance improvements and/or greater board representation may help the activists improve firm performance. We follow the classification commonly used in prior research (Brav *et al.*, 2008a; Greenwood & Schor, 2009; Rose & Sharfman, 2014). Rose & Sharfman (2014) propose two primary objectives: (i) business strategy changes aimed at improving firm performance (Bebchuk *et al.*, 2020; Bessler *et al.*, 2015; Krishnan *et al.*, 2016), and (ii) activism aimed at corporate governance improvements (Karpoff *et al.*, 1996; Mulherin & Poulsen, 1998). In addition, Brav *et al.* (2008a) and

Greenwood & Schor (2009) study activism intended to generate value by forcing the firm to become an acquisition target.

We define a category for activism aimed at performance improvements, which includes proposed changes to business strategy. Second, we group together activism aimed at corporate governance improvements, e.g., voting practices reforms, and implementing constraints on defense tactics. Third, we classify activism aimed at obtaining greater board representation as a separate category, which we view as distinct from reforming corporate governance mechanisms as such. We group requested changes in executive remuneration into another category. The following category includes activism aimed at increasing financial leverage, which can enhance firm value by shielding some of the firm's income from taxes and by disciplining the management and incentivizing them to cut down on wasteful activities. We define a separate category from activism aimed at forcing the company to become an acquisition target (Brav *et al.*, 2008a; Greenwood & Schor, 2009). Finally, we again define a reference category for cases when the objective of shareholder activism is not specified in the primary study or it is formulated in general terms.

Table A2 shows the largest price responses for shareholder activism aimed at forcing the company to become an acquisition target (mn. = 3.59%, w.mn. = 4.12%). We also observe positive price response to activism aimed at increasing financial leverage (mn. = 2.48%, w.mn. = 2.33%) and at obtaining a greater representation in a firm's corporate board (mn. = 2.48%, w.mn. = 2.07%). In contrast, we observe fairly weak price responses to activism aimed at performance improvements (mn. = 0.71%, w.mn. = 0.78%), corporate governance improvements (mn. = 0.25%, w.mn. = 0.28%), and changes to executive compensation (mn. = 0.65%, w.mn. = 1.32%). The average price responses for these three categories are lower than the typical price response to activism in our reference category (mn. = 1.34%, w.mn. = 2.23%).

Activism's success. We further examine the importance of activism's success (e.g., Boyson *et al.*, 2017; Cunat *et al.*, 2012; Mulherin & Poulsen, 1998). If activism is beneficial it is natural to expect more value to be created when it succeeds in achieving its objectives (Boyson *et al.*, 2017). Nevertheless, it is not obvious that success is essential for enhancing firm performance. If shareholder activism creates value by challenging inefficient managerial practices (Jensen & Meckling, 1976; Jensen, 1986) even unsuccessful campaigns may suffice to discipline the management and prompt performance improvements. Hence, investigating the importance of activism's success offers additional insights into the nature of the underlying mechanism.

Several prior studies distinguish between successful and unsuccessful campaigns or they consider only successful campaigns in their empirical analysis (e.g., Alexander *et al.*, 2010;

Becht *et al.*, 2009; Bizjak & Marquette, 1998). Other studies provide information about the proportion of events considered successful, i.e., the proposal passed or board seats won (e.g., Bassen *et al.*, 2019; Carleton *et al.*, 1998; Caton *et al.*, 2001). Even though the combined number of estimates in the successful and unsuccessful activism categories (178 and 150 respectively) constitute only 16% of our sample they allow us to investigate whether and how much achieving the activism’s intended goals matters. Our reference category comprises estimates that do not distinguish between successful and unsuccessful activism (e.g., Alexander *et al.*, 2010; Becht *et al.*, 2009; Bizjak & Marquette, 1998).

Table A2 shows a positive simple mean and weighted mean for all three categories reflecting activism’s success, which may reflect the indirect disciplining effect. The descriptive statistics also show that the average price response for unsuccessful campaigns (mn. = 1.68%, w.mn. = 2.32%) is slightly higher than the one in the reference category (mn. = 1.30%, w.mn. = 1.61%). Nevertheless, successful campaigns are associated with more positive price responses (mn. = 3.14%, w.mn. = 3.07%). These findings are consistent with shareholder activism being beneficial for firm value.

Geographic regions. We also investigate the relevance of geographic regions where activism takes place. Traditionally, most shareholder activism research was based on data from the United States of America (U.S.), where shareholder activism is well established (e.g., Barber, 2009; Holderness & Sheehan, 1985; Morgan & Poulsen, 2001). Nevertheless, more recently an increasing number of studies use data from Europe (e.g., Bassen *et al.*, 2016; Becht *et al.*, 2009; Bessler *et al.*, 2015; Filatotchev & Dotsenko, 2015), and Asia (e.g., Azizan & Ameer, 2012; Becht *et al.*, 2017; Hamao & Matos, 2018; Yeh, 2014). Differences in institutional settings and stock market regulation may affect both the tools available to shareholder activists and their incentives. Only a limited number of prior studies examine the differences across geographic regions (e.g., Bassen *et al.*, 2019; Becht *et al.*, 2017; Cziraki *et al.*, 2010; Maffett *et al.*, 2022). Hence, we find it worthwhile to consider these differences. Conducting a meta-analysis allows us to examine this effect while controlling for a number of additional factors that may systematically differ across regions and that may also affect the value activism creates.

We define three categories that mirror the three continents where most shareholder activism takes place: North America, Europe, and Asia. Our results for North America are dominated by estimates based on U.S. data. In our sample, 16 observations are based on Canadian data, relative to 1,361 based on U.S. data. Due to the economic proximity of Canada and the U.S., we include these observations in one category. Our European sample further includes 317 estimates from Germany, 97 estimates from the United Kingdom (U.K.), and 43 additional estimates based on European data. Our Asian category comprises 64 ob-

servations from Malaysia, 39 observations from Japan, 20 observations from South Korea, and 16 observations based on other Asian data.

Table A2 shows only modest differences in price response to shareholder activism across geographic regions. It seems to be slightly higher in Europe (mn. = 1.80%, w.mn. = 2.78%) relative to North America (mn. = 1.41%, w.mn. = 1.70%) and Asia (mn. = 1.23%, w.mn. = 1.21%). Nevertheless, these differences are fairly small. Furthermore, the value creation estimates are slightly more concentrated for Asia with the interquartile range of (0.07%, 1.71%) relative to North America (-0.17%, 2.00%) and Europe (-0.01%, 3.26%). Thus, our univariate statistics do not point toward major differences in the effectiveness of shareholder activism across various geographic regions.

Institutional setting. Besides comparing the value created by shareholder activism across geographic regions, we explore the impact of differences in the institutional setting across countries. We suggest that a better institutional framework may affect the value created by shareholder activism because it may empower activists and help them achieve their goals. It may also promote corporate transparency and help activists better assess what companies constitute suitable activism targets. Prior meta-analyses in finance successfully exploit cross-country differences to analyze the importance of the institutional framework (e.g., Holderness, 2018).

We consider several institutional setting quality measures. First, we use the anti-director rights index (*Antidirector_rights*) developed by La Porta *et al.* (1997, 1998) that reflects the strength of shareholder protection in negotiation with the management. The index is based on an aggregation of six indicators that capture various aspects of shareholder rights protection. These include shareholders' ability to mail their proxy vote to the firm, the absence of the requirement to deposit their shares prior to the general meeting, the possibility for cumulative voting or proportional representation of minority shareholders in the board of directors, the presence of mechanisms to protect minority shareholders, a relatively low threshold for aggregate ownership that is needed to call for an extraordinary meeting, and the existence of shareholders' preemptive rights. All these measures strengthen shareholders' bargaining position vis-à-vis the company and improve their ability to influence how the company is run. We thus expect shareholder activism to be more effective in enhancing firm value in settings with a higher quality of the institutional framework.

Second, we use an index of law and order tradition (*Rule_of_law*), as specified in the Worldwide Governance Indicators (WGI) provided by the World Bank (WB). Third, following Djankov *et al.* (2008) and Holderness (2018), we use the ratio of the stock market capitalization of publicly listed companies to the country's gross domestic product (GDP) (*Mrkt_Cap*) as specified in the World Federation of Exchanges database provided by WB.

This measure captures the relative importance of stock markets in a given economy.

Table A2 shows that our descriptive statistics do not dramatically differ between the high and low shareholder protection countries (mn. = 1.47%, w.mn. = 1.70%, relative to mn. = 1.54%, w.mn. = 2.42%) and also between settings where the stock market is large relative to GDP (mn. = 1.50%, w.mn. = 1.71%) and those where they are relatively small (mn. = 1.48%, w.mn. = 1.98%). We observe some differences based on the law and order tradition with more impactful activism in settings with a strong rule of law (mn. = 1.71%, w.mn. = 2.13%) relative to those with a weak rule of law (mn. = 1.16%, w.mn. = 1.41%). However, we caution against over-interpreting these univariate results as the quality of the country’s institutional setting may be related to other characteristics. Thus, it is essential to investigate the explanatory power of various determinants in combination with the other potentially relevant variables, which we do in subsection 4.1.

We further consider several characteristics related to the methodology and data samples used in the primary studies. If research design choices impact on the magnitude of the published estimates their magnitude may not be directly comparable, which may distort the interpretation of the overall message conveyed in prior literature. For example, Wahal (1996) compares the impact of shareholder activism around various event dates and documents substantial differences between the “letter day”, the proxy mailing date and the press announcement date. In a similar vein, Karpoff *et al.* (1996) and Bizjak & Marquette (1998) document significant differences between the decision date, the press announcement date, and the proxy mailing date. Some studies explicitly take into consideration these differences in their research design choices (e.g., Nelson, 2006).

Event types. We consider the type of event used to identify when the information on shareholder activism reaches the stock market. Most of the estimates in our sample (609) are based on the first announcement of shareholder activism, which we group into the “First_announcement” category. We define a separate category for estimates measured around the date activist investors register with the regulator their intention to pursue significant changes in the target company. We form another category for estimates based on the press announcement date. Press announcements likely attract more investors’ attention and so may result in a stronger price response. Our next category includes observations based on dates when a certain ownership threshold is reached, which typically makes the activists “blockholders” in a target company. Our next category comprises estimates measured around the “letter day” when activists inform the company that it is targeted. We form another category for proxy mailing dates when a specific shareholder proposal is sent out. This event informs investors in greater detail about the intended activism campaign. We also group together estimates based on the meeting dates when activism proposals are

discussed. Price changes at meeting dates may be attenuated because the markets already know about activists' intentions. Uncertainty about the outcome of an activism campaign gets resolved at the decision date as the decision turns potential changes into an approved plan. We group these dates into our final category.

We observe on average positive price responses on the first announcement days (mn. = 2.39%, w.mn. = 2.91%), the regulatory filing days (mn. = 1.87%, w.mn. = 3.98%), and the decision dates (mn. = 1.87%, w.mn. = 0.51%). Interestingly, for regulatory filings, the weighted mean is higher than the simple mean. We observe an even larger difference between the simple mean and the weighted mean for the regulatory threshold days (mn. = 0.96%, w.mn. = 4.52%). These findings imply that our sample comprises studies that report few estimates that are very high. These results might be affected by research design choices in these studies. We explicitly consider this possibility in our regression analysis below. Furthermore, we observe modest average stock price responses to press announcements (mn. = 1.08%, w.mn. = 1.11%) and weak price reactions around letter dates (mn. = 0.48%, w.mn. = 0.52%), proxy mailing dates (mn. = -0.45%, w.mn. = -0.27%), and meeting dates (mn. = 0.44%, w.mn. = 0.24%).

Event windows. Our next set of variables captures the length of the event window, over which the price response is measured. We expect the event window length choices in the primary studies to optimize the signal-to-noise ratio. Shorter windows may be better focused on the specific campaigns and less contaminated by potential confounding events. In contrast, longer windows may better capture potential price run-ups resulting from rumors reaching the market and better cover the time when the markets process the information. Thus, it is not *a priori* obvious whether shorter or longer windows are preferable. Shareholder activism research routinely reports returns adjusted for the normal (or expected) rate of return, which implies that the length of the event window *per se* may not matter. Indeed, some studies like Gillan & Starks (2000) and Wahal (1996) observe little differences in stock returns measured over shorter and longer event windows. However, choices of event window lengths may systematically vary across the primary studies. Performing a meta-analysis allows us to identify general patterns while controlling for a multitude of other characteristics that may affect this research design choice.

We define a set of indicator variables that capture various event window lengths. The shortest event window only consists of the announcement day. Our next category includes estimates that are measured over a maximum of 3 days, which typically include the day before the announcement day and the day after it. We define additional categories for maximum window lengths of 7 days, 15 days, 31 days, and 62 days. Interestingly, we observe event window length matters for the reported price responses. With the exception of somewhat

lower stock returns measured over three-day windows (mn. = 0.68%, w.mn. = 1.27%), we observe a monotonic increase in stock returns across the window length ranging from the lowest price responses for the one-day windows (mn. = 0.92%, w.mn. = 1.51%) to the largest average price responses for the longest windows (mn. = 5.52%, w.mn. = 4.83%). These findings suggest that the variation in prior empirical results may be affected by differences in the event window lengths. Individual estimates may not be directly comparable, which may lead to seemingly conflicting results on the impact of shareholder activism.

Returns models. We also code variables that characterize the model of normal returns used in the primary studies. We observe that most of the primary studies use fairly simple models perhaps because the event windows are typically fairly short, which limits the impact of any particular risk-adjustments in computing the price response. Our sample comprises 663 estimates based on market-adjusted returns, i.e., a simple difference between the stock returns and market returns (e.g., Becht *et al.*, 2017; Boyson *et al.*, 2017; Matsusaka *et al.*, 2019). We include these estimates in one category. We define another category for 837 estimates based on the market model where the abnormal return is represented by the intercept term “alpha” in a regression of the asset returns on the market returns (e.g., Del Guercio & Hawkins, 1999; Smith, 1996). Our next category comprises 186 estimates (e.g., Cai & Walkling, 2011; Hamao & Matos, 2018; Matsusaka *et al.*, 2019) based on factor pricing models, such as the three-factor model (3-F) (Fama & French, 1995, 1996) and the four-factor model (4-F) (Carhart, 1997). We group the remaining estimates into the final category. Estimates included in this category typically involve more sophisticated ways of adjusting for systematic risk.

We observe the most positive price response estimates based on market adjusted-returns (mn. = 2.23%, w.mn. = 1.99%). The mean values for the other categories range from 1.10% to 1.21% and the weighted means from 1.47% to 1.90%. These results suggest that studies using simple returns models typically produce higher estimates. Thus, the returns model choices do not seem to play an important role in this stream of research. However, we delay drawing stronger conclusions till after we observe the relevance of all categories in combination.

Index weighting. We further differentiate between primary studies that use an equally-weighted and value-weighted index in computing the abnormal returns. Prior research produces conflicting findings on the importance of this choice. On the one hand, Denis & Serrano (1996), Nelson (2006), and Chen *et al.* (2020) detect no meaningful differences in empirical results based on the two indices. On the other hand, Brown & Warner (1985) and Campbell & Wesley (1993) argue that the equally-weighted index leads to more precise detection of abnormal returns in event studies. Ikenberry & Lakonishok (1993) conclude

that price response estimates based on equally-weighted index tend to be lower than those based on value-weighted index. Lee & Park (2009) suggests that the differences are mainly driven by small stock returns.

We define an indicator variable for 690 price response estimates that use equally-weighted index and another indicator variable for 596 estimates based on value-weighted index. We group the remaining 687 observations into the last category, for which the index choice is unknown or irrelevant. We observe only minor differences in both the simple means and the weighted means for the equally-weighted (mn. = 1.18%, w.mn. = 1.42%) and the value-weighted index (mn. = 1.26%, w.mn. = 1.63%). We also observe slightly higher price responses for the third category (mn. = 1.99%, w.mn. = 2.42%).

Estimation method. For the sake of completeness, we also classify the estimation methods. Prior research shows that the estimation methods may have an impact on the magnitude of reported coefficients (Bajzik *et al.*, 2020; Ehrenbergerova *et al.*, 2022). Most of the estimates in our sample, specifically 1,850 out of the total 1,973, are estimated based on OLS. We classify these coefficients in one category. We group the remaining 122 observations based on the weighted least squares (WLS), fixed effects (FE), and instrumental variables (IV) into another category. Consistent with our expectations, we do not observe dramatic differences between the two subsamples. Due to its prominence, the mean values for the OLS estimation (mn. = 1.52%, w.mn. = 1.85%) are very close to the ones for the entire sample. Mean price responses estimated using other estimation methods are slightly lower (mn. = 1.11%, w.mn. = 1.49%).

Sample characteristics. We further consider two data set characteristics: the sample period length in years and its midpoint year that proxies for its recency. Both of these measures can be seen as proxies for data set quality. We observe higher price responses in longer samples (mn. = 2.10%, w.mn. = 2.20%) relative to shorter samples (mn. = 0.86%, w.mn. = 1.40%). We also observe somewhat higher price responses based on more recent samples (mn. = 1.77%, w.mn. = 2.40%) relative to older samples (mn. = 1.15%, w.mn. = 1.27%). Our results thus suggest that primary studies based on data sets that cover longer time periods and that are more recent report greater value created by shareholder activism.

Publication characteristics. Finally, we consider two characteristics related to publication quality: the impact factor of the journal where the article is published and the number of citations. Specifically, we use the discounted recursive journal impact factor provided by the Research Papers in Economics (RePEc). Furthermore, we use the natural logarithm of the number of citations in Google Scholar normalized by the number of years since the first version of the study appeared on Google Scholar. Some prior studies suggest that publication quality proxies matter for reported results, (e.g., Gric *et al.*, 2021; Bajzik *et al.*, 2020),

while other observe few differences (e.g., Cazachevici *et al.*, 2020; Matousek *et al.*, 2021). We observe only minor differences. Unconditionally, price response estimates published in more influential journals are slightly lower (mn. = 1.48%, w.mn. = 1.64%) than those published in other journals (mn. = 1.51%, w.mn. = 2.03%). In addition, highly cited articles report somewhat higher price response estimates (mn. = 1.70%, w.mn. = 2.08%) than those that are less cited (mn. = 1.28%, w.mn. = 1.61%). Thus, we do not find strong evidence in support of the importance of publication quality for the reported price responses to shareholder activism.