Online Appendix to "Does Shareholder Activism Create Value? A Meta-Analysis"

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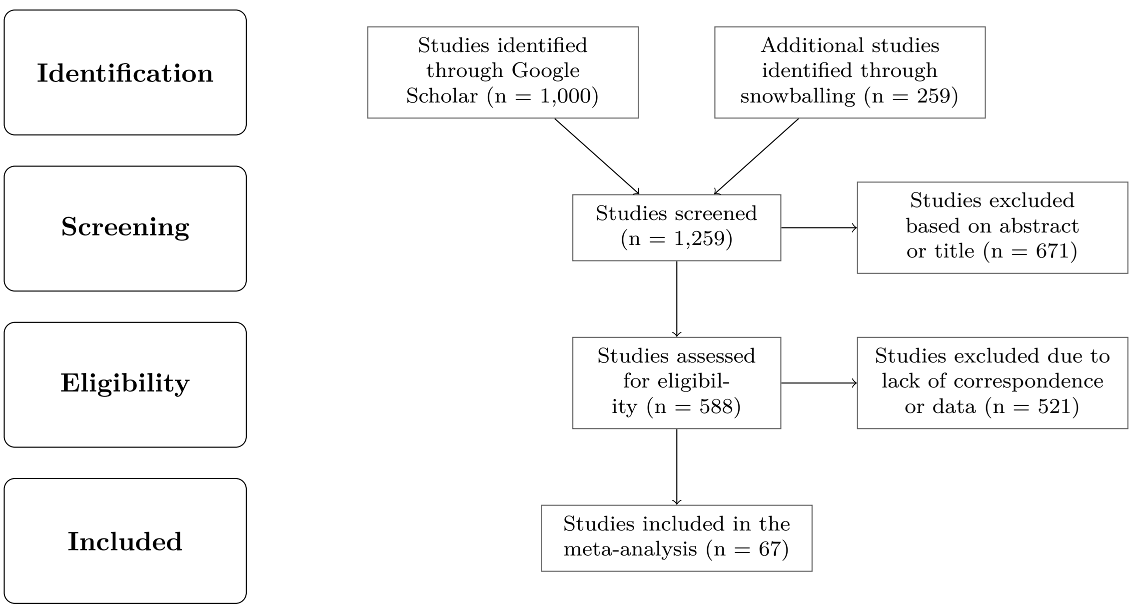
Data Collection

Figure A1: 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).

Definition of Variables

Table A1: Definition of Variables

Variable	Definition
Main variables	
Estimate	Estimate of the price response to shareholder activism.
SE	Standard error of the price response estimate.
Activism sponsors	
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.
Activism approaches	
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.
Activism objectives	
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.
Activisms success	
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.
Geographic regions	
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.
Institutional setting	
Antidirector_rights	Index of shareholder protection rights by La Porta *et al.* (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.
Event types	
Press_announcement	= 1 if the price response is measured around press announcements.

Continued on the next page

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

Variable	Definition
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.
Event Windows	
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.
Returns models	
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.
Index weightings	
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.
Estimation method	
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.
Sample characteristics	
Years_no	Length of the primary data set in years.
Midyear	Median year of the primary data set.
Publication Characteristics	
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.

Shareholder activism characteristics

In this Appendix, we comment on the descriptive statistics for the selected dummy variables representing different, mostly exclusive, subsamples of our data. Table A1 provides the definitions of the variables used in BMA. Table 2 in the main paper presents descriptive statistics for these variables. For continuous variables, we define additional indicators prefixed "Hi_" and "Lo_" to 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_".

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 while 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 experience. Sponsors differ in the strength of their incentives to enhance value and their sensitivity to the risk of failure. For instance, individual sponsors often retain much of their own wealth in targeted firms, internalizing 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 persistently than other investors.

Within institutional investors, hedge funds have strong incentives and flexibility to pursue aggressive activism campaigns. High-performance fees provide hedge fund managers with significant motivation to enhance firm value (Bebchuk *et al.*, 2020; Bessler *et al.*, 2015; Brav *et al.*, 2008; Klein & Zur, 2009; Krishnan *et al.*, 2016). The asymmetric nature of payoffs encourages them to take risks (Stulz, 2007). Light regulation and limited disclosure requirements allow hedge funds to accumulate larger ownership stakes and maintain flexibility in achieving their goals (Brav *et al.*, 2008). "Lock-up" periods further provide them with the maneuvering space to initiate and benefit from protracted campaigns. Hedge funds often coordinate with others in so-called "wolf packs" (Becht *et al.*, 2017; Coffee & Palia, 2016; Wong, 2020), making it plausible to expect hedge fund activism to generate significant shareholder value.

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).

The category "*Individual_investors*" typically refers to high net-worth individuals who frequently engage in activism campaigns, often using their own wealth to acquire significant ownership stakes in targeted companies (e.g., Carl Icahn, Victor Posner, Irwin Jacobs, and David Murdock). This category also includes other forms of individual activism, such as by founders. The category "*Institutional_investors*" includes all institutional investors other than pension funds, such as private equity investors and labor unions. We group these investors into a single category because prior empirical research reports a limited number of estimates for them. Without aggregation, inferences about their impact would rely on a small number of observations.

The final category, "*Sponsor_na*", includes estimates where the primary study does not specify the type of activist investors or where activism involves multiple investor types (e.g., Carleton *et al.*, 1998; Caton *et al.*, 2001; Wahal, 1996). We use this category as the reference group for benchmarking the value created by other, more specifically defined types of activism sponsors. Descriptive statistics show that both the simple mean (1.43%) and the weighted mean (1.74%) for the 837 price response estimates in this category are very similar to the full sample values. This suggests that this group does not represent any specific, unidentified 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 exceeds the median (0.98%), suggesting the presence of several highly positive estimates. Individual activists may be less consistent in enhancing firm value, but their successful campaigns often result in substantial price increases. Within institutional investors, hedge fund activism is associated with the most positive price responses (mn. = 2.42%, w.mn. = 3.10%). In contrast, estimates are lower for pension funds (mn. = 0.84%, w.mn. = 0.56%) and other institutional investors (mn. = 0.16%, w.mn. = 0.47%). This is notable given Smith (1996)'s comment about pension fund activism: "*[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 findings suggest that the stock market reacts more positively to activism by investors with stronger incentives to enhance firm value and fewer constraints from shorter investment horizons.

Activism approaches. Prior research suggests that the value created by shareholder activism depends on its method (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 awareness of pending activist campaigns increases the likelihood of pursuing and winning a proxy fight. Denes *et al.* (2017) report that proxy fights, one of the most confrontational methods used to overcome managerial resistance, generate value of 6.77%, 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 a separate category for 98 direct negotiations with firm management, which are less confrontational than proxy fights (e.g., Carleton *et al.*, 1998; Smith, 1996). We also form a category for 479 shareholder proposals that typically advocate specific policy changes (e.g., Prevost & Rao, 2000; Strickland *et al.*, 1996). Media pressure represents another activism method (e.g., Bessler *et al.*, 2015; Bassen *et al.*, 2019), while multiple methods are grouped into a separate category (e.g., Del Guercio & Hawkins, 1999; Yang *et al.*, 2012). Remaining observations, where the method is unspecified, are grouped into the final category (e.g., Venkiteshwaran *et al.*, 2010).

Table 2 in the main text suggests that more assertive activism approaches tend to induce greater price responses. Proxy fights (mn. = 2.37%, w.mn. = 2.57%) generate higher price responses than direct negotiations (mn. = 1.85%, w.mn. = 2.11%), which in turn surpass shareholder proposals (mn. = 0.36%, w.mn. = 0.29%). Moderate responses are observed for activism using media pressure (mn. = 1.12%, w.mn. = 3.20%). In contrast, activism combining multiple approaches is associated with weaker price responses (mn. = 0.20%, w.mn. = 0.12%). Interestingly, the reference category, where the activism approach is unspecified, exhibits the highest mean price response (mn. = 2.43%, w.mn. = 3.21%), comparable to proxy fights. This suggests that studies without explicitly defined activism methods 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). Classification is complex, as activists may pursue multiple interrelated goals. Corporate governance improvements or greater board representation can help activists enhance firm performance. We follow classifications commonly used in prior research (Brav *et al.*, 2008; 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) corporate governance improvements (Karpoff *et al.*, 1996; Mulherin & Poulsen, 1998). Additionally, Brav *et al.* (2008) and Greenwood & Schor (2009) examine activism aimed at generating value by forcing the firm to become an acquisition target.

We define a category for activism aimed at performance improvements, including proposed changes to business strategy. Second, we group activism aimed at corporate governance improvements, such as voting practice reforms and implementing constraints on defense tactics. Third, we classify activism aimed at obtaining greater board representation as a separate category, distinct from general corporate governance reforms. Another category groups activism targeting changes in executive remuneration. We define a further category for activism aimed at increasing financial leverage, which can enhance firm value by shielding income from taxes and disciplining management to reduce wasteful activities. A separate category covers activism aimed at forcing the company to become an acquisition target (Brav *et al.*, 2008; Greenwood & Schor, 2009). Finally, we define a reference category for cases where the objective of shareholder activism is unspecified or formulated in general terms.

Table 2 in the main text shows the largest price responses for activism aimed at forcing the company to become an acquisition target (mn. = 3.59%, w.mn. = 4.12%). Positive price responses are also observed for activism increasing financial leverage (mn. = 2.48%, w.mn. = 2.33%) and obtaining greater board representation (mn. = 2.48%, w.mn. = 2.07%). In contrast, weaker price responses are associated with activism aimed at performance improvements (mn. = 0.71%, w.mn. = 0.78%), corporate governance reforms (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 those observed in the reference category (mn. = 1.34%, w.mn. = 2.23%).

Activisms success. We further examine the importance of activisms success (e.g., Boyson *et al.*, 2017; Cunat *et al.*, 2012; Mulherin & Poulsen, 1998). If activism is beneficial, it is natural to expect greater value to be created when it succeeds in achieving its objectives (Boyson *et al.*, 2017). However, success may not always be essential for enhancing firm performance. Shareholder activism may create value simply by challenging inefficient managerial practices (Jensen & Meckling, 1976; Jensen, 1986). Even unsuccessful campaigns might discipline management and prompt performance improvements. Investigating the role of success thus provides additional insights into the underlying mechanisms.

Several studies distinguish between successful and unsuccessful campaigns or focus only on successful ones in their analysis (e.g., Alexander *et al.*, 2010; Becht *et al.*, 2009; Bizjak & Marquette, 1998). Other research provides information on the proportion of events considered successful, such as proposals passed or board seats won (e.g., Bassen *et al.*, 2019; Carleton *et al.*, 1998; Caton *et al.*, 2001). Although the combined estimates for successful and unsuccessful activism (178 and 150, respectively) account for only 16% of our sample, they allow us to investigate whether achieving activism's intended goals significantly impacts value creation. Our reference category includes estimates that do not distinguish between successful and unsuccessful activism (e.g., Alexander *et al.*, 2010; Becht *et al.*, 2009; Bizjak & Marquette, 1998).

Table 2 in the main text shows a positive simple mean and weighted mean for all three categories reflecting activism's success, which may indicate an indirect disciplining effect. The descriptive statistics reveal that the average price response for unsuccessful campaigns (mn. = 1.68%, w.mn. = 2.32%) is slightly higher than that of the reference category (mn. = 1.30%, w.mn. = 1.61%). However, successful campaigns are associated with even more positive price responses (mn. = 3.14%, w.mn. = 3.07%). These findings align with the view that shareholder activism benefits firm value.

Geographic regions. We also investigate the relevance of geographic regions where activism occurs. Traditionally, most shareholder activism research has focused on data from the United States (U.S.), where shareholder activism is well established (e.g., Barber, 2009; Holderness & Sheehan, 1985; Morgan & Poulsen, 2001). Recently, however, an increasing number of studies have used 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 regulations may influence both the tools available to shareholder activists and their incentives. Only a limited number of prior studies have explored these regional differences (e.g., Bassen *et al.*, 2019; Becht *et al.*, 2017; Cziraki *et al.*, 2010; Maffett *et al.*, 2022). Therefore, examining these differences is worthwhile. Conducting a meta-analysis enables us to investigate this effect while controlling for various additional factors that systematically differ across regions and may also influence the value created by activism.

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, with 16 observations from Canada compared to 1,361 from the U.S. Due to the economic proximity of Canada and the U.S., we include these observations in a single category. Our European sample includes 317 estimates from Germany, 97 from the United Kingdom (U.K.), and 43 from other European countries. The Asian category comprises 64 observations from Malaysia, 39 from Japan, 20 from South Korea, and 16 based on other Asian data.

Table 2 in the main text shows only modest differences in price responses to shareholder activism across geographic regions. Price responses appear slightly higher in Europe (mn. = 1.80%, w.mn. = 2.78%) compared to North America (mn. = 1.41%, w.mn. = 1.70%) and Asia (mn. = 1.23%, w.mn. = 1.21%). However, these differences are fairly small. Additionally, value creation estimates are slightly more concentrated in Asia, with an interquartile range of (0.07%, 1.71%), compared to North America (-0.17%, 2.00%) and Europe (-0.01%, 3.26%). Thus, our univariate statistics do not indicate major differences in the effectiveness of shareholder activism across geographic regions.

Institutional setting. Beyond comparing the value created by shareholder activism across regions, we examine the impact of institutional differences across countries. A better institutional framework may enhance the value created by shareholder activism by empowering activists and helping them achieve their goals. It may also promote corporate transparency, enabling activists to better identify suitable target companies. Prior meta-analyses in finance have successfully leveraged cross-country differences to assess the importance of institutional frameworks (e.g., Holderness, 2018).

We consider several measures of institutional setting quality. First, we use the antidirector rights index (*Antidirector_rights*) developed by La Porta *et al.* (1997, 1998), which reflects the strength of shareholder protection in negotiations with management. The index aggregates six indicators capturing various aspects of shareholder rights protection, including the ability to mail proxy votes to the firm, the absence of a requirement to deposit shares prior to the general meeting, the possibility for cumulative voting or proportional representation of minority shareholders on the board of directors, the presence of mechanisms to protect minority shareholders, a relatively low threshold for aggregate ownership needed to call for an extraordinary meeting, and the existence of shareholders preemptive rights. These measures collectively strengthen shareholders' bargaining position relative to the company and enhance their ability to influence its management. Accordingly, we expect shareholder activism to be more effective in enhancing firm value in settings with higher-quality institutional frameworks.

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

Table 2 in the main text shows that the descriptive statistics do not differ dramatically between high and low shareholder protection countries (mn. = 1.47%, w.mn. = 1.70%, relative to mn. = 1.54%, w.mn. = 2.42%), nor between settings where stock markets are 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%). However, 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%) compared to those with a weak rule of law (mn. = 1.16%, w.mn. = 1.41%). Nonetheless, we caution against over-interpreting these univariate results, as the quality of a country's institutional setting may correlate with other characteristics. Therefore, it is essential to investigate the explanatory power of these determinants in combination with other potentially relevant variables, which we do in Section 4.1 of the main text.

We further consider several characteristics related to the methodology and data samples used in the primary studies. If research design choices impact 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. Similarly, 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 consider these differences in their research design (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 estimates in our sample (609) are based on the first announcement of shareholder activism, grouped 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. Another category comprises estimates based on the press announcement date, which likely attracts more investor attention, potentially leading to a stronger price response. The next category includes observations based on dates when a certain ownership threshold is reached, typically making the activists "blockholders" in a target company. We also define categories for "letter day", when activists inform the company they are targeting it; proxy mailing dates, which provide detailed information about the activism campaign; meeting dates, when proposals are discussed; and decision dates, when uncertainties are resolved, and potential changes become approved plans.

We observe positive average price responses on the first announcement days (mn. = 2.39%, w.mn. = 2.91%), regulatory filing days (mn. = 1.87%, w.mn. = 3.98%), and decision dates (mn. = 1.87%, w.mn. = 0.51%). Interestingly, for regulatory filings, the weighted mean exceeds the simple mean. A similar but more pronounced pattern exists for threshold days (mn. = 0.96%, w.mn. = 4.52%), implying that a few high estimates influence these results. Modest average responses are observed for press announcements (mn. = 1.08%, w.mn. = 1.11%), while responses are weaker for 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. The length of the event window over which the price response is measured captures researchers' choices to optimize the signal-to-noise ratio. Shorter windows focus on specific campaigns and avoid confounding events, while longer windows better capture potential run-ups from rumors or prolonged market processing. It is not *a priori* clear whether shorter or longer windows are preferable. While abnormal returns are adjusted for expected returns, rendering window length theoretically irrelevant, prior studies (e.g., Gillan & Starks, 2000; Wahal, 1996) find little difference across window lengths. Nonetheless, event window length varies systematically across studies, making meta-analysis essential for identifying patterns.

We define indicator variables for event window lengths, starting with one-day windows, followed by windows of up to 3 days, 7 days, 15 days, 31 days, and 62 days. With the exception of slightly lower responses for three-day windows (mn. = 0.68%, w.mn. = 1.27%), we observe a monotonic increase in price responses with window length, from the lowest for one-day windows (mn. = 0.92%, w.mn. = 1.51%) to the highest for the longest windows (mn. = 5.52%, w.mn. = 4.83%). This variation highlights how differences in event window lengths may contribute to inconsistencies in empirical results.

Returns models. We also code variables for the normal return models used in the primary studies. Most employ simple models, likely due to the short event windows. Our sample includes 663 estimates based on market-adjusted returns, 837 estimates using the market model, 186 based on factor pricing models (e.g., three- or four-factor models), and a final category for more sophisticated risk adjustments.

Studies using simple market-adjusted returns report the most positive estimates (mn. =

2.23%, w.mn. = 1.99%). Other categories show mean values ranging from 1.10% to 1.21% and weighted means from 1.47% to 1.90%. This suggests simple models may inflate estimates, but we defer stronger conclusions until considering all factors in multivariate analysis.

Index weighting. We differentiate between studies using equally-weighted (690 estimates) and value-weighted indices (596 estimates). Minor differences are observed, with mean values of 1.18% (w.mn. = 1.42%) for equally-weighted indices and 1.26% (w.mn. = 1.63%) for value-weighted indices. The remaining category (mn. = 1.99%, w.mn. = 2.42%) comprises estimates where index weighting is unknown or irrelevant.

Estimation method. The majority of estimates in our sample (1,850) use OLS, while 122 use WLS, fixed effects, or instrumental variables. Mean values for OLS (mn. = 1.52%, w.mn. = 1.85%) align closely with the full sample, while alternative methods show slightly lower averages (mn. = 1.11%, w.mn. = 1.49%).

Sample characteristics. We also examine the length and recency of the data sample. Studies using longer samples report higher price responses (mn. = 2.10%, w.mn. = 2.20%) compared to shorter samples (mn. = 0.86%, w.mn. = 1.40%). Similarly, more recent samples yield higher responses (mn. = 1.77%, w.mn. = 2.40%) than older ones (mn. = 1.15%, w.mn. = 1.27%). These findings suggest that longer and more recent data sets contribute to greater estimates of value creation.

Publication characteristics. Lastly, we consider publication quality proxies, including journal impact factor and normalized citations. Studies published in more influential journals report slightly lower estimates (mn. = 1.48%, w.mn. = 1.64%) compared to others (mn. = 1.51%, w.mn. = 2.03%). Highly cited articles report somewhat higher responses (mn. = 1.70%, w.mn. = 2.08%) than less-cited ones (mn. = 1.28%, w.mn. = 1.61%). Thus, publication characteristics appear to play a limited role in influencing reported price responses.

Selective Reporting Methodology

0.1. 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:

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

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 selective reporting.

To evaluate the robustness of our findings, in Panel A of Table A2, we present our results based on six different conventional ways of estimating Equation 1. We use ordinary least squares (OLS), fixed effects (FE) a between effects (BE), IV, and weighted OLS (wOLS) with two ways of weighting. In Panel B of Table A2 we complement the conventional approaches for testing selective reporting with four recently developed techniques that do not require the assumptions of independence and linearity to be satisfied. Namely we use "Top10" (Stanley *et al.*, 2010), "Stem" (Furukawa, 2019), "Kinked" (Bom & Rachinger, 2019) and "Selection" (Andrews & Kasy, 2019). We report the corresponding results in Table A2.

Panel A of Table A2 provides evidence of selective reporting in the empirical literature on shareholder activism. All six β_1 coefficients are positive, consistent with the under-reporting

Table A2: Tests indicate selective reporting

Panel A - Linear Estimation Methods				
	OLS	FE	BE	IV
Effect beyond bias (β_0)	0.590***	1.256***	1.473***	0.657***
	(0.202)	(0.296)	(0.318)	(0.208)
	[0.249, 0.956]			
Selective reporting (β_1)	0.686***	0.178*	0.233**	0.635***
	(0.126)	(0.104)	(0.104)	(0.159)
	[0.456, 0.917]			
#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***	0.008
			(0.239)	(0.010)
			[0.324, 1.100]	[-0.003, 0.095]
Selective reporting (β_1)			0.836***	1.130***
			(0.148)	(0.158)
			[0.597, 1.098]	[0.851, 1.399]
#Observations			1,973	1,973
#Studies			67	67

Panel B - Nonlinear Estimation Techniques				
	Top10	Stem	Kinked	Selection
Effect beyond bias	0.196**	0.021***	0.000	1.062***
	(0.079)	(0.006)	(0.001)	(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). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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, while the FE estimate is significant at the 10% level (coef. 0.178, std. err. 0.104). Furthermore, Panel A of Table A2 also shows that, regardless of the estimation approach, the intercept terms β_0 are positive. Five out of six of these results are statistically significant at the conventional 5% level. These findings suggest that, on average, shareholder activism is associated with positive price responses even after controlling for selective publication. However, the magnitude of β_0 coefficients is lower than what is commonly suggested in prior research, ranging from 0.008% to 1.473%.

The non-linear techniques summarized in Panel B lead to similar conclusions. The estimated "true effect" ranges from 0.000% for the kink method to 1.062% for the selection model, which aligns with the interval of (0.008%, 1.473%) observed for the linear approaches. Moreover, three out of four results reported in Panel B of Table A2 are statistically significant at the conventional 5% level. This consistency suggests that the results based on linear techniques are unlikely to be severely affected by potential violations of the assumptions of independence and linearity between the price response estimates and their standard errors. Overall, these findings indicate that shareholder activism creates value, but after adjusting for selective reporting in prior literature, its magnitude is more modest than commonly perceived.

0.2. Working Papers

As a robustness check aimed at further strengthening our confidence in the broad generalizability of our results, we collect empirical estimates from 10 working papers, following the same procedure used to gather our main sample of estimates from published journal articles, and we re-estimate our results using 388 coefficients collected from working papers. We report these results in Table A5. Consistent with expectations, we observe comparable findings between our main analysis based on published journal articles and the supplementary analysis using the working paper sample. For the working paper sample, the estimates of selective reporting β_1 range from 2.160% to 2.652%. These are larger in magnitude than the corresponding coefficients for our main sample and are strongly statistically significant at the 1% level. This indicates that evidence of selective reporting is at least as strong in the working paper sample as in the main sample.

Furthermore, after correcting for bias due to selective reporting, the range of estimates for the "true effect" (β_0) is similar for both published articles and working papers. For the working paper sample, β_0 ranges from -0.414% to 1.538%, which aligns closely with the corresponding range of 0.000% to 1.473% in the main analysis. The overlap between the two sets of results is even more pronounced for non-linear techniques, which do not rely on some of the assumptions underpinning linear techniques and may yield more robust inferences, especially for smaller samples. For the non-linear techniques, the "true effect" for the working paper sample ranges from 0.011% for the Stem to 1.538% for the selection model, closely matching the interval of 0.008% to 1.473% observed in the main analysis. These findings are consistent with prior research suggesting that selective reporting of empirical results affects both working papers and published articles similarly.

0.3. P-*Hacking*

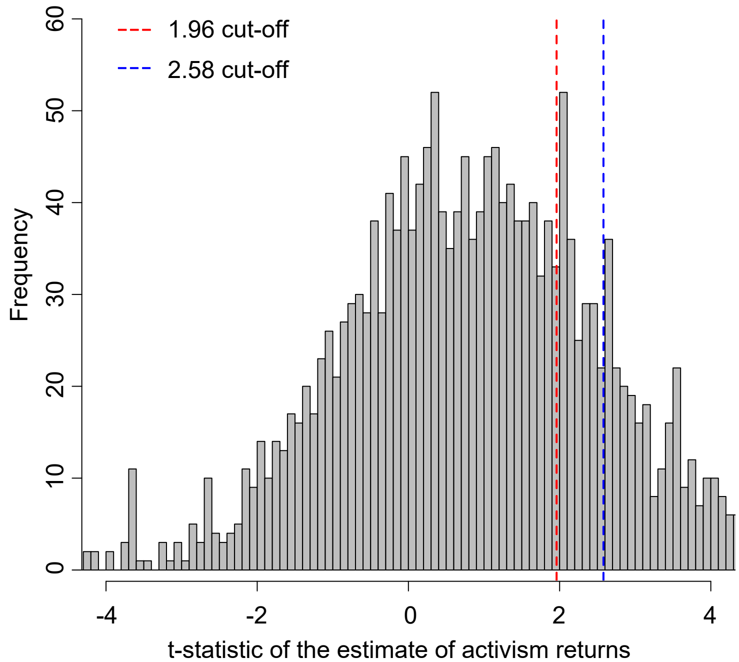
In this section, we complement our analysis of selective publication by examining the role of the statistical significance of empirical results (rather than their sign or magnitude). The tendency to selectively publish empirical tests that just surpass common benchmarks for statistical significance is often referred to as "*p*-hacking" (e.g., Harvey, 2017). Statistically significant results may be deemed more "attractive" for publication because they provide clearer support for the relationship of interest, which is more straightforward to interpret. In contrast, insignificant results may reflect either the absence of the proposed relationship or insufficient power of statistical tests. Discriminating between these explanations is challenging. Insignificant results are thus arguably less informative. Journal editors may prefer studies that include significant results, incentivizing authors to select these results for publication. Such tendencies contribute to bias in the pool of published estimates. Harvey (2017) argues that p-hacking is indeed prevalent in the 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% significance threshold at 1.96 and the 1% threshold at 2.58 should be over-represented relative to those just below these thresholds. Figure A2 shows a histogram of t-statistics corresponding to the price response estimates in our sample. As expected, the shape resembles a normal distribution. However, we observe discontinuities around the 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 statistically significant results, potentially distorting 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 A3 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.

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 A2 and Figure A3 represent significant breaks in the distribution. The caliper test compares the proportion of results with corresponding p-values in narrow equal-sized intervals just above and below the cut-off levels (referred to as "calipers"). In the absence of "p-hacking", the incidence of reported

Figure A2: Visible jumps at critical t-statistic values

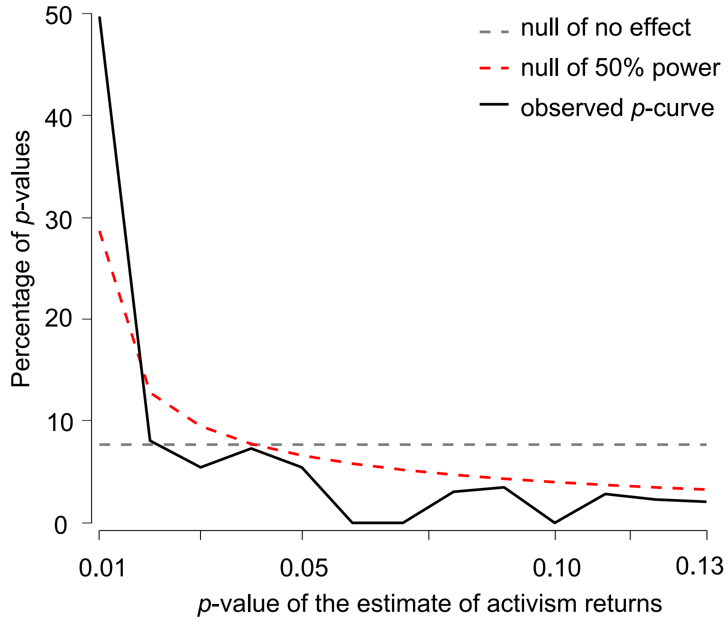


Note: The figure shows the distribution of t-statistics corresponding to the short-term stock returns surrounding shareholder activism campaigns. The vertical dashed lines indicate the boundary of 1.96, which corresponds to statistical significance at 5% level, and 2.58, which corresponds to statistical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

coefficients with p-values in the narrow interval just above the threshold ("over caliper") should be comparable to the incidence of those just below the threshold ("under caliper"). That is, the over-to-under caliper ratio is expected to equal 0.5 (50:50) (Clopper & Pearson, 1934). Ioannidis *et al.* (2017) argue that to detect "p-hacking" in economics and finance research, this ratio should be adjusted to account for the tendency of empirical tests in these fields to be underpowered. Following Bruns *et al.* (2019), we adopt an adjusted critical ratio of 0.4.

Table A3 presents our results based on four caliper widths (0.05, 0.1, 0.15, and 0.2). We find consistent evidence of p-hacking at H1: $C \leq 0.4$ for both the 1.96 and 2.58 significance levels. The lower bounds of the 95% confidence interval for the over-to-under caliper ratio

Figure A3: 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.

exceed the critical value of 0.4 across all tested calipers. Furthermore, for the narrowest caliper width of 0.05, the entire 95% confidence interval is above the critical value of 0.5 even at 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 those that just fall short. This provides further support for the existence of selective publication of results in the shareholder activism literature.

Since publication bias and p-hacking are distinct phenomena, which nevertheless cause distortions in the reported literature (and correlation between reported estimates and stan-

Table A3: 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.

dard errors), we employ the newly developed Meta-Analysis Instrumental Variable Estimator (MAIVE) (Irsova *et al.*, 2023). MAIVE serves as a tool for spurious precision correction by using the square root of the inverse of the sample size as an instrument for reported standard errors. Spurious precision arises as a consequence of certain forms of p-hacking, making MAIVE more robust than conventional publication bias techniques.

Under publication bias, estimates are individually unbiased, but those that are statistically significant are more likely to be reported (published), resulting in a biased observed literature on average. Under p-hacking, researchers implicitly or explicitly attempt to achieve statistical significance by modifying their specifications (e.g., by changing control variables), potentially introducing bias into individual estimates and creating spurious precision. While traditional meta-analysis techniques address publication bias, they do not adequately correct for p-hacking. This is particularly challenging because, under extreme p-hacking, nearly any result is possible in the literature. Recent evidence suggests that p-hacking may be more important than publication bias in economics and related disciplines (Brodeur *et al.*, 2023).

Consistent with this observation, one would not assume that working papers produce results that are less biased than those published in journals, as most working papers are prepared with the intention of eventual publication. Using a large sample of empirical estimates in economics and related fields, Brodeur *et al.* (2020) demonstrate that working papers and journal articles exhibit similar levels of bunching in t-statistics (indicative of both publication bias and p-hacking), suggesting comparable biases in both. MAIVE controls for both selective reporting and p-hacking, providing corrected estimates. We summarize the results in Table A4. The MAIVE coefficient is 0.324, which is comparable to the results obtained using other methods reported in Table A2.

Table A4: MAIVE estimator confirms the bias in reporting

Parameter	Value
MAIVE coefficient	0.324
MAIVE standard error	0.274
F-test of first step in IV	21.332
Hausman-type test	1.4
Critical Value of $Chi^2(1)$	3.841
AR Confidence interval	NA

Note: The table displays results for Meta-Analysis Instrumental Variable Estimator (MAIVE) estimator that serves as a tool for spurious precision detection. The spurious precision is detected via p-hacking and the MAIVE corrects for that. Maive should be used when meta-analysis is suspected to have spurious precision and the difference between MAIVE and unadjusted estimators measures it. We use the MAIVE estimator in its PET-PEESE default form with no weights.

First, we use ordinary least squares (OLS) estimation with two-way clustering at the study and country levels (following Cameron *et al.*, 2011). Two-way clustering addresses the potential concentration of high or low estimates in specific countries or studies. Although 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 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 observations by measures of study size 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, making 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., the inverse of their standard error, $1/SE(r_{ij})$. This approach assigns more weight to precise estimates, which helps adjust for the potential heteroskedasticity of our sample.

All five tests discussed so far assume that selective reporting induces a linear association between $\hat{x}_{ij}$ and $S\hat{E}_{ij}$. While this assumption is 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.* (2024) propose instrumenting $S\hat{E}_{ij}$ by the inverse of the square root of the number of observations. This instrument is 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 primary studies. Following Astakhov *et al.* (2019) and Zigraiova & Havranek (2016), in the last model of Panel A in Table A2 (labeled "IV"), we report results from this estimation approach.

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 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 it 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. This model identifies the likelihood of an estimate falling into different intervals determined by critical values of the t-statistics and assigns more weight to intervals that are underrepresented.

Bayesian Model Averaging Methodology

BMA weighs alternative regression specifications by their posterior model probability (PMP), i.e., their "goodness of fit". In Bayesian econometrics, PMP roughly corresponds to the R^2 measure in frequentist econometrics. The 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. Variables with PIP greater than 0.95 should be interpreted as having a "strong" effect, those with PIP greater than 0.75 as having a "substantial" effect, and those with PIP greater than 0.50 as having a "weak" effect.

Since evaluating all possible combinations of potential explanatory variables would be technically cumbersome, we employ the Markov Chain Monte Carlo (MCMC) process with the 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 employ the unit information g-prior, which sets all the regression coefficients to zero and attributes to them the weight of one data point, implicitly assuming the absence of *a priori* knowledge about the importance of individual characteristics. We use the dilution model prior proposed by George (2010), which assigns less weight to models with highly collinear variables in the overall evaluation.

Table A5: Tests indicate selective reporting for working papers

Panel A - Linear Estimation Methods				
	OLS	FE	BE	IV
Effect beyond bias (β_0)	-0.027	0.252	0.151	-0.220
	(0.164)	(0.280)	(0.354)	(0.262)
	[-0.456, 0.312]			
Selective reporting (β_1)	2.369***	2.160***	2.237***	2.513***
	(0.315)	(0.207)	(0.272)	(0.278)
	[2.004, 3.261]			
#Observations	388	388	388	388
#Studies	10	10	10	10
			w(NOBS)	w(1/SE)
Effect beyond bias (β_0)			-0.414*	-0.017
			(0.186)	(0.023)
Selective reporting (β_1)			2.652***	2.361***
			(0.117)	(0.090)
#Observations			388	388
#Studies			10	10

Panel B - Nonlinear Estimation Techniques				
	Top10	Stem	Kinked	Selection
Effect beyond bias	0.011**	0.011	0.020 ***	1.538***
	(0.004)	(0.014)	(0.004)	(0.009)
#Observations	388	388	388	388
#Studies	10	10	10	10

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). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

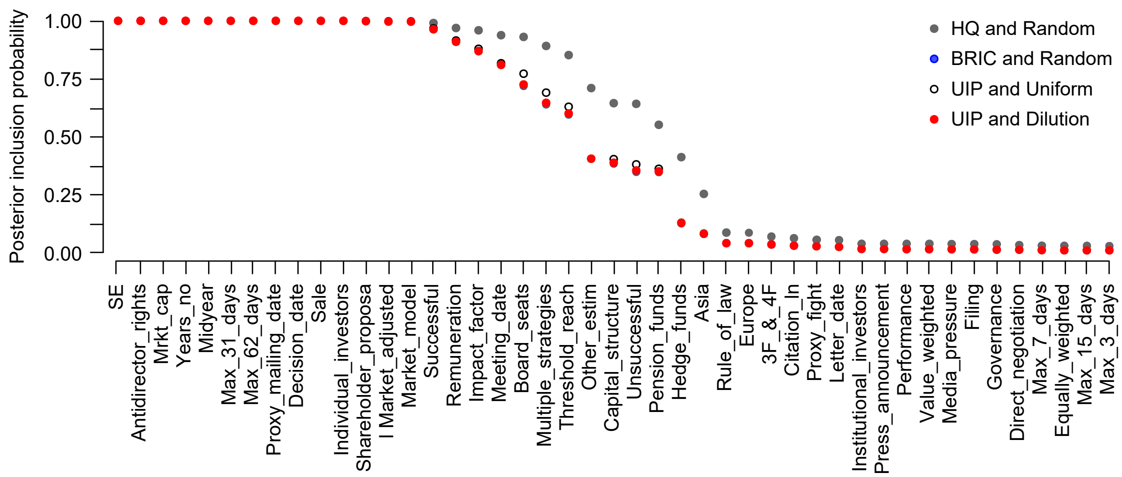
Sensitivity Analysis

We use Bayesian approaches to obtain our main results because the Bayesian framework offers important advantages in the analysis of heterogeneity. It allows us to consider a wide range of potentially relevant explanatory variables while avoiding issues of multi-collinearity. However, for testing the robustness of our results, we also employ a simple OLS frequentist check based on variables with PIP above 0.5, as well as diverse sets of Bayesian priors. This approach ensures that our results are not unduly influenced by the priors used as a starting point. Given the relatively large size of our sample, we do not expect the choice of priors to have a dramatic impact on our results. Nevertheless, in this subsection, we examine the sensitivity of our results to different priors proposed in prior literature and assess whether these modifications affect the explanatory power of individual variables.

Regarding the results of the OLS analysis, Table 3 in the main text shows that the frequentist OLS estimates are broadly similar in magnitude to our baseline BMA results. However, some variables with lower PIPs are statistically insignificant in the OLS framework (*Board_seats*, *Successful*, *Max_31_days*, *Impact_factor*). For the different sets of BMA priors, we visualize the results of our sensitivity analysis in Figure A4. Our baseline model (UIP and dilution) follows George (2010). This approach adjusts model probabilities by multiplying them by the determinant of the correlation matrix of all explanatory variables. Models with low multi-collinearity have determinants close to one and receive a high weight, while models with high multi-collinearity have determinants close to zero and receive little weight. As a robustness check, we use three additional sets of priors that reflect various combinations of *a priori* expectations.

Following Eicher *et al.* (2011), we use the uniform prior on models, which assigns equal weight to each estimated model. Additionally, we apply the unit information g-prior (UIP) on coefficients, which assumes that all regression coefficients are zero. UIP assigns the same weight as one observation in our data. Both priors reflect the absence of any *a priori*

Figure A4: 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.

expectations about the relevance of any individual model or explanatory variable, a standard approach in meta-analysis.

We also incorporate the BRIC g-prior with the random model prior proposed by Fernandez *et al.* (2001), as well as the Hannan-Quinn (HQ) g-prior with the random model prior (Fernandez *et al.*, 2001; Ley & Steel, 2009). These alternative priors allow us to test the robustness of our findings to changes in prior assumptions.

Figure A4 presents PIPs for individual explanatory variables under different priors. The variables are sorted based on their estimated relevance in our main test. We observe that the "UIP and Dilution", "UIP and Uniform", and "BRIC and Random" priors yield nearly identical estimates. Our baseline prior, "UIP and Dilution", generally produces slightly lower PIPs compared to the other two, indicating that our baseline results are conservative. In contrast, the "HQ and Random" prior suggests somewhat higher PIPs for variables in the middle of the relevance spectrum. However, the ranking of the individual explanatory variables remains largely unchanged across priors.

Furthermore, all variables identified as relevant under our baseline prior are also considered relevant under the “HQ and Random” prior. Notably, “HQ and Random” suggests greater relevance for *Board_seats*, *Multiple_strategies*, *Remuneration*, and *Meeting_date*. Overall, these findings indicate that our inferences about the explanatory power of individual variables are robust to the choice of priors used in BMA.

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