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Publication Bias in Strategic Management Research

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ABSTRACT

This research explores the domain of strategic management for evidence of publication bias—the systematic suppression of research findings due to the magnitude, statistical significance, or generally accepted direction of effect sizes. We review why publication bias may exist in strategy research as well as report empirical findings regarding the influence of publication bias in the field. Overall, we found evidence consistent with the inference that publication bias affects many, but not all, topics in the strategic management research. Correlation inflation due to publication bias ranged from an average change in magnitude from .00 (no bias) to .19. These results serve to illustrate the robustness of some important empirical findings while also suggesting that caution should be exercised when interpreting other scientific conclusions in the field of strategic management. We discuss how publication bias can be addressed both philosophically and empirically in the domain of strategy.

KEY WORDS: publication bias, strategic management, trim and fill, cumulative meta-analysis, selection models

PUBLICATION BIAS IN STRATEGIC MANAGEMENT RESEARCH

Publication bias is the systematic suppression of research findings due to the magnitude, statistical significance, or generally accepted direction of effect sizes (Begg, 1994; Dickersin & Min, 1993). Put differently, publication bias occurs when the findings of published works differ systematically from the population of research on important relations of interest (Rothstein, Sutton, & Borenstein, 2005). Across multiple disciplines, such as education (Banks, Kepes, & Banks, 2012), general psychology (Ferguson & Brannick, 2012), political science and sociology (Gerber & Malhotra, 2008a; Gerber & Malhotra, 2008b) as well as medicine (Sterne et al., 2011), publication bias is considered a threat to the validity of empirical findings (Rothstein, Sutton, & Borenstein, 2005). Consequently, Pagell and Kristal (2011) speculate, "All academic fields are biased toward publishing research that has significant findings and/or confirms existing theory" (p. 3). The investigation of publication bias is necessary to reduce the science-practice gap (Briner & Rousseau, 2011).

That scientists tend to publicize their victories more so than their failures is not a new idea (e.g., Editors, 1909; Ferrier, 1792; Hall, 1965), but the assumption of favoritism towards "successful" research findings is not universally accepted and some argue that the problem of publication bias is overstated in the social sciences (e.g., Rosenthal, 1984). This assertion found some support in a study by Dalton, Aguinis, Dalton, Bosco, and Pierce (2012). They examined correlation matrices in published and unpublished studies in the organizational behavior, human resource management (OBHRM) and industrial-organizational (I-O) psychology literatures and found very little difference between the percentages of statistically significant variables in these studies.

Does publication bias exist nonetheless in the strategic management literature? It is an empirical question yet to be answered, but some such as Richard Bettis, a senior editor at *Strategic Management Journal* for many years, contends that publication bias in strategy is almost a certainty. Over two decades ago, he expressed concern that the field was falling into a pattern of constraining the discovery of knowledge by relying too much on research following narrow approaches and investigating well-worn issues (Bettis, 1991). Twenty years later Bettis (2012) echoed his previous concerns that publication bias exists in strategic management research. The concern is spreading, and recently the popular press asserted that strategic management research suffers from publication bias and that practitioners should be wary of the findings in the academic literature (Vermeulen, 2012). The inability to confirm or refute these accusations is problematic because if practitioners do not have faith in the published academic literature, then as an applied field our *raison d'être* is severely compromised (Briner & Rousseau, 2011).

The concerns are not unfounded. A recent evaluation of strategic management and organization theory identified the strong tendency not to publish findings that contradict widely accepted theory, and engage in a "positive test strategy" in which researchers persevere in modifying their procedures until supportive results are obtained (Miller & Tsang, 2011, p. 143; see also Hubbard, Vetter & Little, 1998). Miller and Tsang (2011) conclude that these practices "lead to inflated confidence in a theory's corroborating evidence and generalizability." Put differently, publication bias slows down the process of scientific discovery within strategy, misleads researchers and practitioners alike, and prevents the field from moving towards other explanations.

If publication bias is indeed a problem in the strategic management literature, then it is easy to understand how new insights for managers are becoming less common (Crook, et al., 2006). However, as no work has examined publication bias in the domain of strategy, we do not know if Bettis (1991; 2012), Vermeulen, (2012), and Miller and Tsang (2011) are correct or if their concerns are overstated. The purpose of our paper is to provide evidence around this issue.

We examine publication bias in strategic management research. Specifically, we assess the extent that publication bias affects the most common predictors of firm performance. Our purpose is not to confirm or deny a nebulous conclusion that the field of strategic management shows evidence of publication bias; rather the contribution lies in the specific relationships that warrant concern. An interesting aspect of a study of this nature, with so many topics under investigation, is the possibility of finding publication bias in some topic areas but not in others. One possible explanation for such a pattern is that particular ideas are more entrenched in some areas than in others.

We begin by discussing whether widely-held theories exist in strategic management that could provide the potential for reviewer and editorial biases during the journal review process. Next, we provide a systematic analysis of whether the published literature in specific content areas (e.g., corporate social responsibility, M&A) show indications of favoritism towards statistically significant findings that support the *zeitgeist.* We then conclude with specific suggestions for how the problem of publication bias might be addressed in order to aid in the building of knowledge within the strategy domain. We also offer suggestions to strategy researchers with regard to specific topics of investigation.

A Foundation for Publication Bias

Publication bias is a deleterious side effect of two fundamental and positive aspects of science; empirical testing and theoretical development. Researchers need theory to build hypotheses and they need empirical findings to test hypotheses. The intertwining of empirical testing and theoretical precision has allowed management to make tremendous gains in understanding the firm and its members in a relatively short time. These two aspects of our science create guidance and set expectations for effect direction and magnitude. It is only when access to research that does not meet expectations is systematically suppressed due to factors other than rigor and relevance that publication bias emerges. It is a reasonably safe statement that management research, including strategic management, relies strongly on empirical testing, but we pause to consider whether strategic management meets the second precondition of publication bias, a strong reliance on theoretical paradigms.

The field of strategic management is broad, both drawing from and contributing to a variety of business disciplines. Consequently, it is unlikely to have a single dominant paradigm (Schendel, 1994). Nonetheless, a few perspectives have become so popular in the strategic management literature that they might accurately be called theories, thus potentially biasing strategic management researchers in the manuscript preparation process and gatekeepers during the review process. Michael Hitt, past president of both the *Strategic Management Society* and the *Academy of Management*, identified a few of these in a 2005 article. They include industrial organization economics (i.e., Porter, 1980), agency theory (i.e., Jensen & Meckling, 1976), transactions cost economics (i.e., Williamson, 1975) and the resource-based view of the firm (i.e., Barney, 1991).

Consistent with what might be expected from an association of scholars, these perspectives have all been criticized in the strategy literature (i.e., Donaldson, 2012; Priem &

Butler, 2001; Short, Ketchen, Palmer, & Hult, 2007). Nonetheless, they are pervasive in strategic management research. For example, of 2,016 total articles and editorials published in *Strategic Management Journal* (*SMJ*) as of the first quarter of 2013, 553 (27%) mentioned resource-based theory, 414 (21%) mentioned transaction costs economics, 383 (19%) mentioned industrial organization economics, 227 (11%) mentioned agency theory, and 100 (5%) mentioned stakeholder theory. In sum, there is a strong reliance on theory, and although this has led to important discoveries and insights, it is possible that for some relationships, research that refutes established theoretical paradigms or even fails wholly to support them with statistically significant results will be less likely to be published.

Of the perspectives mentioned, the resource-based view is the most pervasive (Acedo, Barroso, and Galan, 2006). The resource-based perspective is based on the assumption that resources are heterogeneous among firms, and the simplest and most widely-cited view is that firms that possess resources that have market value (either to provide efficiency or differentiation), and are rare, nonsubstitutable, and difficult to imitate, provide firms a possible source of sustainable competitive advantage (Barney, 1991). Many types of resources have been investigated for their competitive potential. A number of meta-analyses have investigated this hypothesis through strategic resources (Crook et al., 2008), human capital (Crook et al., 2011), IT investments (Lim et al., 2011), marketing capabilities, R&D capabilities, and operations capabilities (the last three found in Krasnikov and Jayachandran, 2008). If we were to examine transactions costs economics or agency theory in a similar way, we would likewise find a variety of topics in which these perspectives have been applied. Ultimately, the pervasiveness of the resource-based view as well as the other aforementioned perspectives introduces the possibility that it is difficult to get work published if findings do not support the extant theoretical bases.

In our introduction we asserted that publication bias could also be inappropriately influencing practice through the dissemination of incorrect ideas or suppression of novel ideas. If this were the case, a telling sign would be evidence that practitioners made suboptimal decisions on the basis of widely embraced theory. Since the strategic management field is rather new relative to most of the other functional business disciplines, and since the ill effects of publication bias on practice tend to take considerable time to be realized and documented, this phenomenon would be, at present, a difficult thing to demonstrate. Nonetheless, we would like to submit an example for consideration, recognizing that in any academic field there are always multiple interpretations for a series of events.

As the field of strategic management was emerging, scholars were searching for credible theories upon which to base their research (Schendel & Hofer, 1979; Schendel, 1984), a necessary prerequisite to being recognized as a legitimate field (Hambrick & Chen, 2008). Diversification became a topic of interest to early strategy scholars, and researchers drew from more established academic fields such as finance for theory to explain this phenomenon (Hoskisson & Hitt, 1990). One of the most pervasive ideas about diversification in the 1960s and early 1970s, based on portfolio theory from finance, was that unrelated diversification could reduce risk and enhance returns in highly unrelated firms called conglomerates (Lewellen, 1971; Renshaw, 1968; Sharpe, 1964). Unrelated acquisitions became very popular during this period (Shleifer & Vishny, 1991; Ravenscraft & Scherer, 1987). Also, consistent with the widely-documented phenomenon that the stock market responds to announcements of events of importance to public corporations through adjustments in its evaluation of the worth of their securities (Fama, Fisher, Jensen, & Roll, 1969; McWilliams & Siegel, 1997), stock prices

increased during that period on the announcement of a diversifying acquisition (Matsusaka, 1993; Hubbard & Palia, 1999).

However, by the 1980s the view of market participants regarding conglomerates and diversifying acquisitions had apparently changed to a negative valuation, as documented in several event studies (Bhagat, Shleifer & Vishny, 1990; Berger and Ofek, 1995; Comment & Jarrell, 1995; Lang & Stulz, 1994). Also, considerable restructuring was observed during the 1980s involving divestiture of unrelated businesses by large corporations, and this restructuring is often interpreted as a correction by scholars in both finance and strategic management (Hoskisson & Hitt, 1994; Kaplan & Weisbach, 1992; Shleifer & Vishny, 1991; Ravenscraft & Scherer, 1987).

We have provided portfolio theory as one explanation for why stock market participants might have originally believed unrelated diversification would lead to positive outcomes among firms, although we recognize that there may be other explanations. The important point is that, regardless of why companies engaged in these sorts of acquisitions, the market evaluation was that they would improve corporate performance, and this evaluation proved to be incorrect, at least over the longer term (Burch & Nanda, 2003; Rumelt, 1982; Ravenscraft & Scherer, 1987; Kaplan & Weisbach, 1992). Importantly, it took over a decade for market participants to realize that something was wrong, and during this same period executives making decisions would have been experiencing reinforcement through observed increases in share prices. Furthermore, *the academic literature lagged the correction instead of instigating it*. This example illustrates the potential danger that exists when widely accepted, but incorrect, ideas lead to poor business decisions.

In the current work, we seek to determine if there is a bias held by authors, reviewers, and editors towards statistically significant and theoretically consistent findings. This question will be answered through a systematic examination of the published literature. Specifically, we will re-analyze the published studies included in existing systematic reviews (i.e., meta-analyses) and test whether the subset of published studies provides evidence consistent with the inference of publication bias. The question is important because preliminary research suggests there is not a problem with publication bias in other behaviorally oriented business disciplines (Dalton, et al., 2012) and some have argued that the problem is overstated (e.g., Rosenthal, 1984). However, others have warned about its existence and possible negative effects (Banks, Kepes, & McDaniel, 2012; Bettis, 1991, 2012; Landis & Rogelberg, 2013; Miller & Tsang, 2011; Schmidt & Hunter, in press). Following the guidance of Kepes et al. (2012), we employ the most up-to-date and sophisticated analyses available to test for a publication bias on a variety of strategic management research topics

METHODS

Choice of Publication Bias Technique

Broadly, management research has used two techniques to test for publication bias. The first was illustrated in Dalton et al. and involved aggregating and taking averages of all the correlations in a sample of published studies and compares them to a sample of correlations in unpublished studies. The means are compared and if the correlations in published studies are larger than the correlations in unpublished studies, then according to this technique, a conclusion of publication bias is supported. The Dalton et al. approach seems most appropriate for broad research questions concerning potential field-level or research bloc-level (e.g., test publishers) bias where every correlation in a matrix is believed to be equally influenced by publication bias.

The alternative is what we refer to as the triangulation approach (Kepes et al., 2011: Schmidt & Hunter, in press). This technique differs from the Dalton et al. technique in three key ways. First, it focuses on specific relationships (e.g., employee turnover and firm performance), not entire fields. As such, it does not treat every correlation in a matrix as equally impacted by author and gatekeeper expectations. This is an important distinction because strategy researchers often use a large number of control variables (CITE). Whereas the triangulation approach focuses only on those substantive relations germane to the research questions, the Dalton et al technique assumes that the significance and direction of control variables are just as important to manuscript evaluation as the significance and direction of relationships between substantive variables and outcomes.

The second key distinction is that the triangulation technique avoids the assumption that the sample of unpublished studies is representative of the entire population of unpublished studies (i.e., no second order sampling error), while the Dalton et al. technique requires it. In related fields such as general psychology, there is evidence that the population of unpublished studies is rarely if ever representative of the population of unpublished literature (Ferguson & Brannick, 2012). The reasons for non-representativeness are that authors are less likely to respond favorably to a request for unpublished work if the results are non-significant or counterintuitive, and unpublished studies with theory consistent, significant results are more likely to be identified (Ferguson & Brannick, 2012; Schmidt & Hunter, in press). For example, consider the *Academy of Management* annual conference. If reviewers do in fact favor significant results over non-significant results, then the papers accepted are more likely to have bigger effect sizes. Because they are accepted and their findings presented at the annual meeting or published in the proceedings, they are more easily acquired.

The final distinction is that the triangulation technique uses multiple procedures with differing assumptions and thresholds to "triangulate" on the possibility of publication bias. The triangulation approach encourages authors to employ multiple publication bias tests that rely on different assumptions and with the goal of increasing confidence in the results when there is agreement (Kepes et al., 2012; Schmidt & Hunter, in press). Further, the recommended procedures to be used in the triangulation approach are specifically designed and validated through simulation work to be sensitivity tests for publication bias, whereas the newness of the Dalton et al. approach has limited its use to the one occasion in I-O psychology.

We opted to use the triangulation technique because we contend that if strategic management suffers from publication bias, it will not be uniformly applied. That is, we expect that for some relations, the presence and amount of bias will be minimal and for others that are perhaps more theoretically entrenched, there will be greater bias. Second, we see no reason to suspect that the available unpublished literature in strategy will be any more representative of the population of research than that found in related disciplines (i.e., there is likely to be extensive second-order sampling error). Finally, the triangulation method allows for the use of multiple techniques that, when in agreement, provide greater confidence in the findings than a reliance on a single methodology.

Use of Existing Meta-Analyses as Study Repositories

The quest to understand the determinants of firm performance is at the core of the field of strategic management (Hambrick, 1980; Rumelt, Schendel, & Teece, 1994). Consequently, we examined the extent of publication bias in strategy by re-examining previous meta-analytic reviews that included the outcome of firm performance because they are likely to be of the most interest and relevance to strategy researchers and practitioners. We believe that limiting the focus

to a variable that is most often the central criterion provides a more sensitive test for publication bias. We reiterate that this is a study about the potential for publication bias in the strategic management literature, and we are simply using meta-analyses as sources of data. Put differently, the level of analysis is the primary study, not the systematic review (i.e., meta-analysis). The same technique could be applied to annotated bibliographies, narrative reviews, etc. However, the emphasis in meta-analysis on identifying the entirety of the empirical literature on a relation makes them ideal for studying publication bias.

There are several additional advantages to using existing meta-analytic reviews rather than conducting a new systematic review on a single predictor or relationship. First, using existing systematic reviews provides the ability to generalize findings across a large number of firm performance antecedents. Second, the appropriateness and feasibility of a meta-analysis is established by its theoretical relevance and an adequate number of primary studies. Thus, there is an implicit assumption that the predictors of performance examined in meta-analytic reviews are those that draw the greatest amount of interest because there is a history of studying the relation and an ongoing debate about the presence, magnitude, or direction of the relation (Hunter & Schmidt, 2004). Third, the use of existing reviews provides greater transparency and the opportunity to replicate findings. This helps to reduce biasing effects that could arise if our results were based exclusively on our own selection criteria and coding decisions (Orwin, 1994).

We worked to include, and examine, as many systematic reviews as possible. We used various online search combinations of the keywords “strategy,” “meta-analysis,” “firm performance,” as well as systematic searches of journal issues and review articles to identify the meta-analytic work related to strategic management. From an initial pool of over 50,000 potentially relevant citations, we found 43 articles in the domain of strategy.

These 43 articles included meta-analyses ranging in publication date from 1985 through 2012. In examining these articles, we noticed that the search terms we used cast a wide net that identified a few systematic reviews that might be considered more relevant to strategic human resource management (SHRM) than strategic management. We elected to include them because of the importance of human resources management in the strategic management literature (Kor & Leblebici, 2005; Li et al., 2008) and also because they may shed new light on previous findings (Dalton, et al., 2012).

Other inclusion criteria were as follows. First, for a meta-analysis to be included in the current work, it needed to be replicable. More specifically, we had to be able to reconstruct the original meta-analysis by identifying the samples included, coding rules, and effect size calculations. We made substantial efforts to contact authors who did not provide the necessary information in the original work. Second, each meta-analysis needed to have included 20 or more published samples. In some literatures such as medicine, true experiments and controlled trials allow for accurate publication bias results with as few as ten samples (Sterne et al., 2011), but the strategy literature more often relies on field surveys and archival sources (Snow & Thomas, 1994). These sources of data can introduce other forms of variance (i.e., substantive and methodological moderators) and increase the number of samples necessary to obtain a clear picture of the presence of a publication bias. As a conservative estimate, we opted to only include those meta-analytic reviews with greater than 20 published samples.

Within systematic reviews it is common to conduct multiple meta-analyses, and we included each separate analysis as long as it (a) met the 20-sample criterion, and (b) used firm performance as the outcome. For example, Combs et al. (2006) provided an overall relation between strategic HR practices and firm performance as well as a subgroup comparison of high

performance work systems versus individual practices. We included each meta-analysis in our tests. Of the 43 systematic reviews initially identified, 21 met all of our inclusion criteria—and, from these 21, we report the results of 89 meta-analyses.

We note the following additional details regarding the meta-analyses we excluded. As discussed previously, the analyses had to be replicable. In two cases, we could not obtain the reference lists or included articles (i.e., Capon, Farley, & Hoenig, 1990; Grinstein, 2008). In only three cases, the sample size did not meet our cutoff, greater than 20 published samples (Calantone, Harmancioglu, & Droge, 2010; Campbell-Hunt, 2000; Subramony, 2009). There were nine cases in which the dependent variable was outside the scope of our work, i.e., not firm performance (Daily, Certo, Dalton, & Roengpitya, 2003; Damanpour, 1991, Damanpour, 2010; Geyskens, Steenkamp, & Kumar, 2006; Kirca, Hult, Deligonul, Perryy, & Cavusgil, 2012; Lux, Crook. & Woehr, 2011; Meyer & Sinani, 2008; Miller, Glick, Wang, & Huber, 1991; Rosenzweig & Easton, 2010). In six cases, we omitted an article because a more recent article had been written on the same topic (Boyd, 1991; Dalton, Daily, Ellstrand, & Johnson, 1998; Dalton, Daily, Johnons, & Ellstrand, 1999; Orlitzky, Schmidt, & Rynes, 2003; Rhoades Rechner, & Sandaramurthy, 2000; Van Wijk, Jansen, & Lyles, 2008). We did this to try to limit the degree of overlap between meta-analyses when the topic was the same. For example, the Orlitzky et al. (2003) meta-analysis on corporate social responsibility included 52 studies. Several years later, Margolis, Elfenbein, and Walsh (2007) updated this meta-analysis with an additional 111 samples ($k = 163$). We chose to include the larger and more recent CSR meta-analysis and not include Orlitsky et al. because of the high degree of overlap between the two meta-analyses on an identical topic. Finally, in another case we deemed the article outside the domain of strategy

(i.e., Carney, Gedajlovic, Heugens, Van Essen, & Van Oosterhout, 2011) and in one more case the coding method was not replicable (i.e., vote counting; Horváthová, 2010).

Publication Bias Tests

Three tests of publication bias were selected; trim and fill, cumulative meta-analysis, and selection models. There were several reasons these tests were selected. First, unlike other tests of publication bias, which may only identify the presence of publication bias (e.g., failsafe N, rank correlations), the three tests selected all provided estimates of the magnitude of publication bias. Second, all three tests attempt to estimate the potential magnitude of publication bias in different ways. Consequently, we are able to better triangulate the magnitude of publication bias as their combined results give us a range of potential estimates as opposed to a single point estimate (Kepes et al., 2012). Finally, each test makes certain assumptions and we can have greater confidence in our results when the different tests are in agreement.

Trim and Fill. Our first test for publication bias was the trim and fill method (Duval & Tweedie, 2000a, 2000b). An advanced and intuitive technique (Aguinis et al., 2011), the trim and fill method is based on the notion that samples may not be published because their effect sizes are small, counterintuitive, or statistically non-significant (Borenstein et al., 2009). Accordingly, the procedure plots all of the samples in a meta-analysis with the effect size on the x-axis and the precision (i.e., inversed standard error) of the samples on the y-axis. Precision is the proximity of the sample estimate to the population mean. In general, larger studies have less sampling error and smaller standard errors and as such show greater precision. This is evidenced when studies are plotted. The large sample studies will show less variability in effect sizes and tend to cluster closer to the population estimate than the smaller studies. Visually, this creates an inverted-V pattern with the large studies near the top and centered close to the mean estimate and the

smaller sample studies scattered at the base of the distribution. Because sampling error is randomly distributed, the plotted samples should make a symmetrical pattern with roughly half of the samples to the left of the mean effect size and half to the right (see Figure 1a for an example of a funnel plot with no detected bias). When publication bias is present, the funnel plot will be asymmetric and it will appear as if samples are missing on one side (Duval, 2005).

Asymmetry can be caused by missing samples on either side of the mean estimate, but to avoid capitalizing on chance we determined a priori to search for bias on the side of the distribution where effect sizes were smaller and/or non-significant (Stanley, 2005). All but seven of the antecedents (e.g., firm size, efficiency, sub-unit level, size assets, employee turnover, cultural distance, and mergers-acquiring firm) were found in the original articles to be positively related to financial performance. Thus, for all but those seven negative relations, we searched for publication bias to the left of the distribution (Peters, Sutton, Jones, Abrams, & Rushton, 2007). This tests for suppression among studies with weaker than expected results or a negative relation (Duval & Tweedie, 2000a). For the eight studies that reported a negative relation, we tested for bias to the right of the mean.

When asymmetry in the funnel plot was detected, the trim and fill technique first excluded or "trimmed" samples on the opposite side of the funnel plot to create symmetry and calculate an effect size ($r_{t\&f}$) (Duval, 2000a). The technique then reintroduces the trimmed samples and imputes or fills the missing samples needed to create a symmetrical distribution (see Figure 1b-d for examples of funnel plots). The extent of potential publication bias is established through the number of imputed samples necessary to create a symmetrical distribution (*ik*) and the difference in the observed effect size among published studies and the "trimmed' effect size (Sterne et al., 2005).

It is important to recognize that asymmetry can be caused by factors other than systematic suppression of studies. For example, second-order sampling error occurs when random properties of the available studies introduce the possibility of biased or attenuated estimates (Hunter & Schmidt, 2004). This not only can alter mean estimates, this can also introduce asymmetry that is not attributable to publication bias. Although the effects of second order sampling error and other statistical artifacts can never be fully accounted for when assessing publication bias, the recommended practice is to triangulate on potential bias through the use of multiple tests (Kepes et al., 2012).

Cumulative Meta-Analysis. Our second test of publication bias was the cumulative meta-analysis (CMA). CMA assesses the drift of effect sizes by precision (Borenstein et al., 2009; Kepes et al., 2012). Based on a cumulative estimate (i.e., an effect is generated on the most precise (i.e., largest) sample, then on the two most precise samples, and so on until all samples are included) a forest plot is generated. The forest plot can be examined for evidence of "drift" in the cumulative estimate (Borenstein et al., 2009). A positive drift is consistent with the typical case of publication bias as small magnitude effects from less precise samples are suppressed. Examples of two forest plots are found in Figures 2a and 2b. In Figure 2a (data from Richter, Dawson & West, 2011), the cumulative meta-analysis supports an inference of publication bias as the cumulative mean drifts from .03 (N_{cum}=1,805, k_{cum}=1) to .04 (N_{cum}=4,403, k_{cum}=5) to finally .12 (N_{cum}=10,604, k_{cum}=44) which is a change of 67 percent from the five most precise samples to the total distribution of 44. Conversely, in Figure 2b (data from Gooding & Wagner, 1985-subunit level), the cumulative meta-analysis does not support an inference of publication bias. The magnitude of publication bias was assessed by establishing an estimate ($r_{precise}$) of the

most precise samples in the top 10% of each distribution. Next, the difference between $r_{precise}$ and the final meta-analytic estimate was compared for the degree of change.

Selection Models. Our third and final test was the use of selection models (Hedges & Vevea, 2005; Vevea & Woods, 2005). The selection model approach takes into consideration the probability that certain samples are included in a meta-analysis due to specific characteristics (Hedges & Vevea, 2005; Vevea & Woods, 2005). While a typical meta-analytic model assumes that all samples have a 100 percent chance of being included, a selection model assigns probability weights to the samples that vary based on their characteristics (Kepes et al., 2012). In this case, the specific characteristic is the statistical significance of the effect size (a function of magnitude and sample size). In our study, we draw upon guidance from Vevea and Woods (2005) and conduct a selection model test that makes an assumption of moderate, one-tailed publication bias. That is, we expect that studies with larger *p*-values will be underrepresented in the published literature as compared to published studies (hence the one-tailed test), but that studies with larger, non-significant *p*-values will not be completely absent (hence the moderate).

If moderate publication bias were to exist in a pool of studies, one might expect that effect sizes with *p* values that exist between .000 and .005 could have a 100% probably (or given a weight of 1.0) of being included in the meta-analysis. Conversely, effect sizes with *p* values that are between .050 to .100, might have an 80% chance of being observed in a condition of moderate publication bias. In the event that effect sizes have *p* values between .750 to .900, one might believe that the study has a 50% chance of being included in the meta-analysis (Table 1 in Vevea and Woods, 2005 provides a full description of the proposed ranges).

Traditional approaches to conducting selection models were limited because of a need for 100 to 200 samples in order to achieve accurate estimates of publication bias (Hedges & Vevea,

2005). Because of this limitation, selection models have not been as commonly used as other publication bias tests. However, recent advances that use a priori selection models have allowed for their use to be extended to much smaller pools of studies (Hedges & Vevea, 2005; Vevea & Woods, 2005). The a priori selection model approach thus provides an illustration of how a meta-analytic estimate might be influenced by publication bias (Kepes et al., 2012). Finally, similar to the trim and fill approach, selection models provide an adjusted meta-analytic estimate that can be compared to the original meta-analytic estimate in order to determine the degree of change.

Triangulation and Meta-Analytic Procedure. In order to calculate an overall degree of publication bias, we examined the extent of the change from the original meta-analytic estimate to the trim and fill, CMA, and selection model adjusted estimates. In addition to calculating the degree of change observed within each sensitivity analysis, we also calculated the average degree of change across all publication bias tests. The trim and fill and CMA tests were conducted in the statistical software *Comprehensive Meta-Analysis* (Borenstein, Hedges, Higgins, & Rothstein, 2005). The selection model tests were conducted in the statistical software *R* using syntax by Vevea and Woods (2005) and adapted by Field and Gillett (2010) for use in *R.* All three publication bias tests use the meta-analytic technique that is derived from the Hedges and colleagues (Borenstein, Hedges, Higgins, & Rothstein, 2009; Hedges & Olkin, 1985) tradition of meta-analysis. There may be minor variations that can occur due to the formulaic differences between Hedges and Olkin (1985) and Hunter and Schmidt (2004). The relevant difference between the two techniques are in the weighting of the samples with Hedges and Olkin (1985) using the inverse of the standard error (i.e., precision) and Hunter and Schmidt (2004) using the sample size. However, the correlation between precision and sample size typically approaches

1.0 and results are virtually identical in all but the rarest instances (Banks, Bachelor, & McDaniel, 2010).

RESULTS

Table 1 provides a summary of our analyses, with the effects size (for each predictor) reported in the original paper (r_p) as well as what was recaptured here (r_o). The average difference between r_p and r_o was .01, and where there was still significant disparity we returned to the original articles and either recoded them or we reached out to the authors of the systematic reviews and asked for clarification[1]. Nevertheless, it is also possible that there were minor differences in coding for those meta-analyses that did not provide their data either in the article or through personal communication and thus required us to independently code the samples.

(Insert Table 1 about here)

Table 1 also reports the results of the publication bias tests for published studies described in the previous section, including the number of imputed samples necessary to create a symmetrical distribution (*ik*) and the re-estimated effect size ($r_{t\&f}$) and 95% confidence interval ($95\%CI_{t\&f}$) from the trim and fill analysis. Here, we also included the cumulative meta-analysis results. We report the effect sizes based on the most precise studies that fall in the top 10% of each individual distribution (r_{prec}). The final test of publication bias, selection models, is presented in the next column of Table 1 and displays the estimated effect sizes (r_{sm}). As described above, the three tests of publication bias; trim and fill, CMA, and selection models allowed us to triangulate on the possibility of bias for any particular correlate of firm performance. We report the differences between the original meta-analytic estimates (r_o) and the

[1] For meta-analyses that included unpublished studies, r_o was taken from Table 2.

trim and fill ($\Delta r_{t\&f}$), CMA (Δr_{cum}), and selection models adjusted estimates (Δr_{sm}) as well as the average difference in the last four columns (Δr_{avg}), respectively.

We will work through an example of the three techniques for detecting publication bias based on Lee and Madhavan (2010), which is the largest included systematic review (k = 637). They reported an overall relation between divesture and firm performance of .11 (see r_p in Table 1). We replicated their coding and arrived at an estimate of r_o = .12 and then proceeded to test for publication bias using the three techniques. The trim and fill procedure calculated the necessary samples that would be needed to the left of the mean in order to achieve symmetry (i_k = 146). The trim and fill process calculated an estimate of the overall effect size based on the symmetrical distribution ($r_{t\&f}$ = .00). Figure 1, panel c presents the graphic of these imputed studies.

For the CMA of Lee and Madhavan, we ran individual meta-analyses starting with the largest sample, then the largest and second largest samples, and so on until all 637 samples were included. The estimate based on the 64 largest studies (the top 10% of the distribution) was .11. Hence, the cumulative meta-analysis test does not appear to indicate evidence of bias. For the divesture-firm performance relation, the selection model estimate under conditions of moderate bias was noticeably smaller (r_{sm} = .04) than the overall estimate of the relation (r_o = .12). In sum, the trim and fill, the CMA, and the selection model tests resulted in adjustments to the original estimates of .12, .01, and .08, respectively (an average change of .07). These results led us to a conclusion of likely publication bias in the divesture-firm performance relationship, with adjustments ranging from very small (CMA) to very large (Trim and Fill).

Overall, across distributions the mean difference between the original meta-analytic estimate and the trim and fill, CMA, and selection model adjusted estimates were .04, .06, and

.04, respectively. The average difference across all three tests was .05. The range in the adjusted differences was between .00 and .19. There were 28 out of 65 distributions with a magnitude change of at least .05, but only 5 of the 65 distributions changed by at least .10. A total of 13 out of the 65 distributions changed by .01 or less. For these 13 relationships, the point estimates and correlations established in prior systematic reviews can be taken at face value. On the other hand, there were a number of relations (e.g., divesture-firm performance) where publication bias needs to be considered a real and substantial threat.

Inclusion of Unpublished Studies

Another important consideration is whether meta-analyses that included unpublished and published studies would lead to different conclusions regarding publication bias as opposed to meta-analyses that only included published studies. Essentially, the question is whether the inclusion of unpublished studies (e.g., conference papers, technical reports) in systematic reviews mitigates the effects of publication bias in strategic management research. Consequently, we specifically examined those 24 analyses that included unpublished studies. These results are listed in Table 2. We found that the inclusion of unpublished studies did little to alleviate the effects of publication bias. While the average adjustment across the trim and fill, CMA, and selection model tests was .05 when only published samples were used, the average adjustment was actually .06 after the inclusion of unpublished samples. However, unpublished studies made up less than 10 percent of the included studies within these 24 meta-analyses, which we attribute to the difficulty researchers have in locating unpublished work. Hence, our results lend support for the assertion of some that unpublished samples that are identifiable may be systematically different from those that are not as easily retrieved (Ferguson & Brannick, 2012; Schmidt &

Hunter, in press). Such an explanation is offered as one possible reason that the inclusion of unpublished samples did not mitigate publication bias concerns.

DISCUSSION

The underlying assumption of publication bias is that authors, reviewers, and editors more readily submit, favorably review, and accept articles when hypotheses are supported with statistically significant results that conform to the extant theoretical base (Borenstein, Hedges, Higgins, & Rothstein, 2009). Accordingly, the present research asks whether effect sizes in the published literature in strategic management show evidence of publication bias. This study provides the first systematic investigation of this topic specific to strategic management.

Our analyses support the inference that publication bias is a concern in strategic management research in some topic areas but not all. This is as we expected and consistent with other fields in which publication bias is perceived to be a threat, but is not ubiquitous. Nevertheless, consistent with the warnings of Bettis (1991, 2012), Hubbard et al. (1998), and Miller and Tsang (2011), we found evidence of some level of publication bias for the majority of the topics we examined. In sum, a general overestimation of effect sizes in the published literature seems likely. Because we also included an evaluation of studies that tend to straddle strategic management and human resource management, we were also able to confirm some previous findings by Dalton et al. (2012). That is, we found little or no evidence of publication bias in the literatures on work practices; however, we did find evidence of bias in studies regarding team structure.

Implications of Findings and General Recommendations for Researchers

Strategic management research, like research in other behavioral sciences, suffers from a variety of methodological challenges (Bergh & Holbein, 1997; Boyd, Gove, & Hitt, 2005;

Ketchen, Boyd, & Bergh, 2008; Shook, Ketchen, Hult, & Kacmar, 2004). For example, it is difficult to measure many of the most relevant constructs in the field (Boyd et al., 2005), and particular sampling practices have also been criticized (Short, Ketchen, & Palmer, 2002). Recognition of shortcomings is vital to improving research in a field. In this paper, we have added to this criticism by offering evidence of publication bias in several topic areas important to strategic management research. This evidence partially confirms what Bettis (1991, 2012) and Vermeulen (2012), and others have been saying, but runs counter to Rosenthal's (1984) claim of unilateral study suppression. Some topic areas should are of more concern than others. In this section, in addition to general recommendations, we will offer suggestions for researchers based on our specific results.

One implication of our findings is that strategy researchers should use greater caution before embracing widely accepted theories and ideas based on the understanding that empirical support in the published literature may be inflated. Instead of accepting theories at face value, researchers should actively seek out the boundary conditions and limitations of prominent theories and examine phenomena through less dominant paradigms. For example, McCarthy and Puffer (2008) applied social contracts theory as an alternative to agency theory in examining corporate governance decisions in less mature market-oriented economies such as Russia. Older paradigms can also be combined with other theories to supplement previously held views. One example is Geletkanycz and Boyd (2011), who proposed and tested a contingency model that combines agency and embeddedness perspectives to explain the influence of outside directorships on firm performance. Another example is Kacperczyk (2009), who combined agency theory and stakeholder theory to explain how takeover protection leads to increased attention to stakeholders, which is associated with higher shareholder value. The critical message

here is that science in the area of strategic management will be advanced more rapidly if researchers are a little more skeptical of widely accepted theories and more open to alternative explanations. This is especially true in strategic management because of rapid changes in the global business environment that are likely to require new theories and perspectives (Hitt, Haynes and Serpa, 2010).

There are several additional ways that publication bias might be addressed in the field of strategic management. Consistent with actions in other disciplines (e.g., American Psychological Association's *Meta-Analysis Reporting Standards*; Cooper, 2010), we suggest that editorial staff members (editors, reviewers) in strategy require appropriate publication bias tests (and consistent reporting) for all meta-analyses. In addition, instead of being discouraged, replication studies should be embraced by journal editors, reviewers, and those who train doctoral students (Bettis, 2012; Hubbard et al., 1998; Tsang & Kwan, 1999). One example of encouraging replication is found in an upcoming special research forum in *Strategic Management Journal* called "Replication in Strategic Management" (Bettis is one of the co-editors). If during the replication process widely-accepted theories or ideas are not supported, a paper can be made more attractive for publication by offering and testing a competing perspective (i.e., Bowen, 2007; Ketchen & Palmer, 1999; Le Breton-Miller & Miller, 2009) or examining potential moderating variables (i.e., St. John & Harrison, 1999).

Intermediary Steps to Mitigate Publication Bias

The call for better reporting practices in primary studies (Hunter & Schmidt, 2004), less rigid adherence to dominant theory (Hambrick, 2007), and more replications are not new calls to action (Smith, 1970). However, at present we have seen infrequent execution of these recommendations. Rather than simply echo scholars from past and present, we offer the

following intermediary steps that may help in the transition to mitigating publication bias. Our recommendations target those individuals that set policy and establish the norms in the field (i.e., editors and senior leaders) as well as those most responsible for summarizing the extant literature (i.e., meta-analysts).

First, replications will only be undertaken if they rewarded. At present, if a study succeeds in replicating a finding it is dismissed as old news. If it fails to replicate a result, then it faces the challenge of publishing non-significant results. Therefore, editors need to create rewards and the best reward, at least for junior faculty, is publication in premier outlets. Replication studies take up very little journal space (a brief synopsis of the original study followed by results may use as little as two journal pages) and we recommend that a fixed amount of journal space annually be allocated exclusively for replication studies.

Second, not all recommendations to mitigate publication bias generalize to strategy. For example, some (e.g., Banks & McDaniel, 2011; Banks, Kepes, & McDaniel, 2012; Schmidt & Landers, 2013) have called for data registries similar to the ones used in medicine. However, much of our research uses existing databases such as Compustat. Asking researchers to submit their protocol before accessing a publicly available database seems unlikely to yield high conformance. However, if SMS and AoM made all accepted conference papers available online in their entirety (not just the abbreviated versions of best paper proceedings and not just the top 10% of papers), then this would immediately increase access to the unpublished or "grey" literature for those conducting systematic reviews (e.g., meta-analysts). It is not as revolutionary a step as data registries, but it would be easy to implement, as it would only take a few gigabytes of space on a server.

We echo the call to include more unpublished studies in meta-analyses, but meta-analyses were simply the tools to identify our primary studies and this paper is not a critique of existing meta-analyses. Once again, the level of analysis was the primary study. Rather than retread the reasons for including unpublished studies we offer the briefest of tutorial on ways to maximize the inclusion of unpublished studies. First, those seeking unpublished work should use multiple databases that include conference paper repositories such as AllAcademic.com. Second, non-English studies are less likely to be published (Hopewell, Clark, & Mallett, 2005) so search for keywords in multiple languages. Third, most search engines (e.g., Google Scholar) cap the number of results to 1000 and order hits by relevance. Published studies are more likely to be in the first 1000 so to acquire unpublished studies, limit the search by year. This allows for maximum coverage, but prevents coders from overlapping searches. Fourth, if the constructs have commonly used measures or scales, meta-analysts can use the "cited by" function in Google Scholar to conduct a legacy search. Even articles written in less common languages generally cite work in English. Fifth and finally, scholars increasingly post their curriculum vitae on personal websites. The vitas generally have a "works in progress" section. Coders could search these works in progress for potentially relevant material. Although anecdotal, the authors of this work have found far greater response to specific requests for unpublished works than general posting to listservs.

An additional step that may be used to prevent publication bias at the primary study level is the implementation of a two-stage review process. Research shows that reviewers are more likely to recommend a study be published when results are statistically significant (Emerson et al., 2008). Furthermore, reviewers are more likely to be critical of research methods and to find purposely planted errors when results are non-significant. To combat such biases, strategy

journals could provide reviewers with access to introduction and methods sections of original submissions. Reviewers would be given the opportunity to evaluate the relevance of the research questions, the strength of the theory development, as well as the rigor of the methodological approach. Immediately after submitting these ratings reviewers would gain access to the results and discussion sections. Although reviewers would by no means be bound to their initial ratings as the results may shed additional insight on the methods, editors would still have the reviewers' original and unbiased perspective.

Specific Suggestions for Strategic Management Researchers

Topic areas central to strategic management that demonstrate a high likelihood of publication bias include IT investment (Δr_{avg} of .10), as well as strategic resources, such as human capital (Δr_{avg} of .08), R&D activities (Δr_{avg} of .11), and recoverable slack (Δr_{avg} of .12). Crook, et al. (2008) included five meta-analyses based on different firm resources, with average magnitude changes between .03 and .06. The literatures underlying all these studies are dominated by resource-based theory. Because of the widespread use of the theory across the strategy field, this is perhaps the biggest problem identified through our analyses. As we mentioned in the introduction, the perspective found in Barney's (1991) paper has tended to stimulate the most interest. This view has been criticized from a number of angles (i.e., Lockett, Thompson & Morgenstern, 2009; Priem and Butler, 2001a, b). However, its popularity may be discouraging the publication of empirical research that contradicts it. We recommend that researchers interested in a resource-based perspective revisit and build upon other perspectives (i.e., Wernerfelt, 1984) or develop new theory on the subject.

With regard to the corporate social responsibility literature, where we also found evidence of bias (Δr_{avg} of .06), we are encouraged by a recent call for papers by *Academy of*

Management Review, a response perhaps to a special issue calling for new theories of organization in which the editors found very little that was actually new (Suddaby, Hardy and Huy, 2011). The call is titled "Management Theory and Social Welfare." The editors specifically encourage ideas that deal with important social welfare issues even if there is not much in the way of current theory to support them: "Theories depending on reasonable changes in existing institutions and those based on values other than those that underpin existing theory will be considered as well." Later, they state: "Submitted manuscripts could differ from more conventional journal articles by: 1) challenging received wisdom; 2) relaxing the assumptions that underpin existing theories in order to make them more realistic; and 3) explicitly addressing values and their effects on existing theory" The editor group, which is large, has agreed to work directly with authors and reviewers to nurture, rather than reject, novel ideas. These sorts of research forums would seem to be very useful in developing new ways of thinking about strategic management topics in that the articles produced can then be used to support novel empirical research. That is, empirical researchers of necessity draw from the existing theoretical literature to justify their hypotheses, and research efforts such as this one hold the potential to provide new theory. *Strategic Management Journal* also recently announced a special issue devoted to the development of novel strategic management theory. This is very useful in light of the fact that findings regarding the performance implications of strategic planning itself may be tainted by a publication bias ($\mathbf{\Delta r}_{avg}$ of .11).

CEO compensation is another potential problem topic ($\mathbf{\Delta r}_{avg}$ of .10). Without going into too much detail, we will state what is probably obvious to those familiar with this literature. That is, this literature is strongly influenced by an agency theory perspective (i.e., Tosi & Gomez-Mejia, 1989). Stewardship theory (Davis, Schoorman, & Donaldson, 1997) is an alternative

perspective to agency theory that is given much less attention in this literature. We encourage researchers to use more stewardship theory in developing theory to explain the relationship between CEO compensation and firm performance. In addition to the topics we have discussed in this section, researchers and gatekeepers can use Table 1 as a tool in identifying other areas that indicate a potential bias.

We should add before leaving this section that the very high magnitude change (Δr_{avg} of .19) found for organizational configurations is probably largely a function of the fact that the researchers combined various organizational configuration typologies (i.e., Miles and Snow, Porter) into one meta-analysis (Ketchen, et al., 1997). Nonetheless, this finding may still indicate the need for additional research on this currently less popular topic.

Limitations

Our findings should be considered in light of the limitations of our approach. First, it should be noted that each publication bias technique has limitations and draws upon various assumptions (for a complete discussion see Kepes et al., 2012). For example, the trim and fill technique can only account for publication bias in one direction—if there were a systematic suppression of articles such that both very small effects and very large effects were missing, then the trim and fill technique may not detect publication bias. On the other hand, asymmetry can appear when publication bias is not present if meaningful moderators exist (Weinhandl & Duval, 2012). To address this limitation, previous research has recommended that the trim and fill be conducted within more homogeneous sub-groups (Kepes et al., 2012; Schmidt & Hunter, in press). Thus, the sub-groups implemented by the original meta-analytic authors were recreated in this study and the trim and fill analyses were completed within these sub-groups. However, the reader should exercise caution when interpreting the sub-groups as they are more vulnerable to

second-order sampling error. Nonetheless, we only report and interpret distributions where at least 20 samples exist and that this is a more conservative approach than previous researchers (e.g., Sterne et al., 2011; Kepes et al., 2012b). The use of cumulative meta-analysis and selection models to complement trim and fill results (as has been suggested by Banks et al., 2012b; Kepes et al., 2012) helps to reduce, but never completely eliminate, this problem.

Similarly, the cumulative meta-analysis has the limitation that clear guidelines for its interpretation have not been established (Kepes et al., 2012). Better guidance for its interpretation is needed. The selection model approach also has a limitation due to the fact that the selection process is proposed a priori and therefore, begins with the assumption that publication bias may exist to some extent. Consequently, comparisons of the a priori selection models to other publication bias methods should take this into considerations. However, some have argued that the selection models are less affected by unexplained heterogeneity compared to other tests such as the trim and fill (Vevea & Woods, 2005; Kepes et al., 2012). Overall, it is nonetheless useful to use publication bias detection techniques that rely on different assumptions as one may have greater confidence in the results when there is agreement (Kepes et al., 2012; Schmidt & Hunter, in press).

The exclusion of small meta-analyses could be considered a limitation as well. We took what we believe is a conservative strategy in our inclusion criterion that a meta-analysis needed twenty (20) samples. This was based on Sterne et al.'s (2011) recommendation of 10 within the medical field that commonly relies on fixed effects estimates. Fixed effects models assume a universal effect and variability across studies is entirely attributable to sampling error. This is a more plausible model for medicine as controlled trials and true experiments greatly reduce confounding and moderating influences. All things equal across experiments and non-

experiments (including sample size and degree of publication bias), if there was a single non-zero population value (i.e., fixed effects model), then non-experiments would have more variance and as such more studies would be suppressed. If true, publication bias in strategy research should be easier to detect than it is in fields that rely more on experimental studies. However, we contend that a fixed effect model is not a tenable assumption in strategic management research. Instead, a random effects model that assumes variability in the population effect sizes is more applicable to our field. This is not only because of the lack of experimental research, it is also because of the way the outcome of interest, firm performance, is typically measured. Combs, Crook, and Shook (2005) meta-analyzed the various measures of firm performance and found that there was little agreement between accounting, marketing, growth, and hybrid measures. This introduces additional variance beyond sampling error and when coupled with other measurement, design, and analysis imprecisions (e.g., non-response bias, subjective ratings, non-normality of the firm performance), the variability in population values hampers the signal to noise ratio and reduces the ability to detect publication bias. As such, we felt justified in requiring more than 10 samples in our inclusion criteria, but more research is needed to justify or adjust the specific value of 20 as this limitation could have changed our results, at least for those topic areas we included in this study.

An additional limitation is that some of the systematic reviews included were published quite some time ago (Datta & Narayanan, 1989; Godding & Wagner, 1985) and although we found no evidence that the issue of publication bias has changed over time, we still recommend that many of these reviews be updated in the near future. Some of the older reviews likely did not have access to the internet, which severely limits the ability to identify potentially relevant published and unpublished works. Further, a number of new practices (e.g., meta-regression,

meta-SEM) allow for tests of continuous moderators and complex theoretical models. Nonetheless, because the majority of the meta-analyses reviewed in this study are recent, we believe that our results largely reflect the current state of the strategic management literature.

Conclusion

We found evidence consistent with the inference that some topic areas in strategic management research suffer from publication bias while others appear to be robust to its effects. For those literature areas that appear to be free from bias, this is an encouraging result and researchers and practitioners can now have greater confidence in the results. For those studies in which publication bias appears to be problematic, we encourage greater caution when interpreting those results.

We based our study on data reported in meta-analytic reviews; however, the greatest harm caused by publication bias begins long before a systematic review. From the time a variable or relationship is introduced in the literature to the first meta-analysis may be decades. During this period, researchers and practitioners may be working off false assumptions. The increasingly advanced techniques to detect publication bias and the greater access to dissertations and conference paper databases may help meta-analysts achieve the most accurate estimates, but this is little consolation for researchers who design their studies and calculate statistical power based on inflated effect size estimates found in the published literature. We hope the present work cautions researchers to be vigilant against the effects of publication bias by more vigorously questioning the universality of commonly held beliefs (paradigms) and by more deliberately searching for alternative explanations of important phenomena.

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Table 1. Results of re-analyses and tests of publication bias for strategy specific areas

Article	Effect	r_p	k	Σn	r_o	95CI	ik	$r_{t\&f}$	95CI$_{t\&f}$	r_{prec}	r_{sm}	$\Delta r_{t\&f}$	Δr_{cum}	Δr_{sm}	Δr_{avg}
Bausch & Krist, 2007	*internalization*	*.06*	*40*	*8309*	*.10*	*.06; .15*	*5*	*.07*	*.02; .11*	*.07*	*.07*	*.03*	*.03*	*.03*	*.03*
Bausch & Krist, 2007	*older firms*	*.04*	*35*	*7547*	*.09*	*.05; .14*	*3*	*.06*	*.01; 11*	*.08*	*.06*	*.03*	*.01*	*.03*	*.02*
Bausch & Krist, 2007	*US firms*	*.13*	*22*	*4063*	*.11*	*.05; .17*	*0*	*x*	*x*	*.05*	*.08*	*.00*	*.06*	*.03*	*.03*
Brinckmann et al., 2010	*strategic planning*	*.10*	*46*	*10589*	*.11*	*.07; .14*	*3*	*.09*	*.06; .13*	*.09*	*.08*	*.02*	*.02*	*.03*	*.02*
Brinckmann et al., 2010	*established firms*	*.16*	*32*	*6994*	*.13*	*.08; .18*	*0*	*x*	*x*	*.17*	*.10*	*.00*	*.00*	*.03*	*.01*
Brinckmann et al., 2010	*growth outcomes*	*.12*	*31*	*5166*	*.12*	*.07; .17*	*0*	*x*	*x*	*.09*	*.09*	*.00*	*.03*	*.03*	*.02*
Brinckmann et al., 2010	*US firms*	*.13*	*25*	*3690*	*.13*	*.07; .19*	*0*	*x*	*x*	*.21*	*.10*	*.00*	*.00*	*.03*	*.01*
Combs et al., 2006	*work practices*	*.15*	*74*	*15731*	*.17*	*.14; .20*	*0*	*x*	*x*	*.22*	*.15*	*.00*	*.00*	*.02*	*.01*
Combs et al., 2006	*HPWP*	*.21*	*35*	*8351*	*.22*	*.18; .27*	*0*	*x*	*x*	*.29*	*.21*	*.00*	*.00*	*.01*	*.00*
Combs et al., 2006	*indiv practices*	*.11*	*46*	*8604*	*.12*	*.09; .14*	*2*	*.11*	*.09; .14*	*.12*	*.11*	*.01*	*.00*	*.01*	*.01*
Combs et al., 2006	*manufacturing*	*.24*	*23*	*3457*	*.24*	*.16; .32*	*0*	*x*	*x*	*.37*	*.15*	*.00*	*.00*	*.09*	*.03*
Crook et al, 2008	*strategic resources*	*.17*	*127*	*29657*	*.20*	*.17; .22*	*32*	*.13*	*.10; .16*	*.17*	*.17*	*.07*	*.03*	*.03*	*.04*
Crook et al, 2008	*meets RBT criteria*	*.20*	*89*	*17333*	*.23*	*.19; .26*	*17*	*.17*	*.14; .21*	*.19*	*.21*	*.06*	*.04*	*.02*	*.04*
Crook et al, 2008	*imitable resources*	*.09*	*48*	*11680*	*.11*	*.04; .18*	*11*	*.03*	*-.04; .10*	*.07*	*.04*	*.08*	*.04*	*.07*	*.06*
Crook et al, 2008	*appropriation*	*.16*	*55*	*11570*	*.18*	*.15; .22*	*7*	*.14*	*.10; .19*	*.15*	*.16*	*.04*	*.03*	*.02*	*.03*
Crook et al, 2008	*no appropriation*	*.23*	*28*	*5590*	*.26*	*.21; .31*	*10*	*.18*	*.12; .24*	*.20*	*.25*	*.08*	*.06*	*.01*	*.05*
Crook et al, 2011	*human capital*	*.18*	*44*	*8159*	*.21*	*.16; .25*	*12*	*.12*	*.07; .17*	*.10*	*.18*	*.09*	*.11*	*.03*	*.08*
Crook et al, 2011	*general human capital*	*.14*	*33*	*8135*	*.15*	*.10; .20*	*0*	*x*	*x*	*.20*	*.12*	*.00*	*.00*	*.03*	*.01*
Crook et al, 2011	*global perf.*	*.15*	*24*	*5921*	*.17*	*.012; .23*	*9*	*.08*	*.01; .14*	*.05*	*.15*	*.09*	*.12*	*.02*	*.08*
Daniel et al., 2004	*potential slack*	*.28*	*36*	*41434*	*.19*	*.16; .22*	*4*	*.17*	*.13; .20*	*.20*	*.18*	*.02*	*.00*	*.01*	*.01*
Daniel et al., 2004	*recoverable slack*	*.05*	*27*	*37356*	*.07*	*-.04;.17*	*10*	*-.05*	*-.13; .05*	*-.11*	*.01*	*.12*	*.18*	*.06*	*.12*
Datta & Narayanan, 1989	*concentration*	*.29*	*45*	*3762*	*.31*	*.26; .36*	*5*	*.28*	*.23; .33*	*.32*	*.14*	*.03*	*.00*	*.17*	*.07*
Datta & Narayanan, 1989	*4 firm concentration*	*.28*	*35*	*3097*	*.29*	*.24; .35*	*3*	*.27*	*.21; .33*	*.32*	*.27*	*.02*	*.00*	*.02*	*.01*
Datta & Narayanan, 1989	*pre-1960 samples*	*.37*	*26*	*1133*	*.40*	*.33; .47*	*7*	*.33*	*.24; .41*	*.38*	*.38*	*.07*	*.02*	*.02*	*.04*
Datta & Narayanan, 1989	*4-digit SIC code*	*.27*	*25*	*2552*	*.27*	*.20; .33*	*0*	*x*	*x*	*.29*	*.24*	*.00*	*.00*	*.03*	*.01*
Datta & Narayanan, 1989	*ROA measures*	*.33*	*24*	*1373*	*.37*	*.27; .45*	*5*	*.29*	*.19; .39*	*.45*	*.33*	*.08*	*.00*	*.04*	*.04*
Ellis, 2006	*market orientation*	*.26*	*38*	*8242*	*.29*	*.25; .32*	*7*	*.25*	*.21; .29*	*.22*	*.28*	*.04*	*.07*	*.01*	*.04*
Ellis, 2006	*subjective perf.*	*.27*	*25*	*4580*	*.31*	*.26; .35*	*0*	*x*	*x*	*.31*	*.30*	*.00*	*.00*	*.01*	*.00*
Ellis, 2006	*Western firms*	*.27*	*25*	*6237*	*.30*	*.25; .34*	*5*	*.26*	*.21; .31*	*.25*	*.29*	*.04*	*.05*	*.01*	*.03*
Ellis, 2006	*small businesses*	*.22*	*20*	*4820*	*.30*	*.24; .36*	*5*	*.25*	*.19; .31*	*.23*	*.29*	*.05*	*.07*	*.01*	*.04*
Gooding & Wagner, 1985	*firm size*	*-.02*	*90*	*6970*	*-.05*	*-.12; .03*	*13*	*.03*	*-.04; .10*	*-.03*	*.06*	*.08*	*.02*	*.11*	*.07*
Gooding & Wagner, 1985	*perf. (efficiency)*	*-.09*	*72*	*5781*	*-.13*	*-.18; -.07*	*2*	*-.11*	*-.16; -.05*	*-.09*	*-.05*	*.02*	*.04*	*.08*	*.05*
Gooding & Wagner, 1985	*perf. (productivity)*	*.25*	*22*	*1389*	*.15*	*-.07; .34*	*0*	*x*	*x*	*.21*	*.04*	*.00*	*.00*	*.11*	*.04*
Gooding & Wagner, 1985	*organizational level*	*.03*	*67*	*5635*	*.01*	*-.07; .09*	*0*	*x*	*x*	*.00*	*-.06*	*.00*	*.01*	*.07*	*.03*
Gooding & Wagner, 1985	*subunit level*	*-.25*	*23*	*1335*	*-.22*	*-.33; -.10*	*0*	*x*	*x*	*-.23*	*-.18*	*.00*	*.01*	*.04*	*.02*
Gooding & Wagner, 1985	*size-assets*	*-.01*	*51*	*4391*	*-.06*	*-.13; .01*	*0*	*x*	*x*	*.10*	*.02*	*.00*	*.16*	*.08*	*.08*
Hancock et al., 2007	*overall*	*-.03*	*47*	*24830*	*-.07*	*-.09; -.04*	*0*	*x*	*x*	*.00*	*-.05*	*.00*	*.07*	*.02*	*.03*
Heugens et al., 2009	*owner concentration*	*.04*	*221*	*374252*	*.05*	*.03; .06*	*0*	*x*	*x*	*.06*	*.01*	*.00*	*.00*	*.04*	*.01*

Article	Effect	r_p	*k*	Σn	r_o	95CI	*ik*	$r_{t\&f}$	$95CI_{t\&f}$	r_{prec}	r_{sm}	$\Delta r_{t\&f}$	Δr_{cum}	Δr_{sm}	Δr_{avg}
Ketchen et al., 1997	*configuration*	*.53*	*35*	*4166*	*.49*	*.41; .56*	*16*	*.25*	*.15; .35*	*.17*	*.47*	*.24*	*.32*	*.02*	*.19*
King et al., 2004	*M&A-acquiring firms*	*-.07*	*61*	*8814*	*.02*	*-.03; .08*	*0*	*x*	*x*	*.08*	*-.04*	*.00*	*.00*	*.06*	*.02*
King et al., 2004	*M&A-acquired firms*	*.70*	*34*	*3131*	*.74*	*.66; .81*	*0*	*x*	*x*	*.62*	*.72*	*.00*	*.12*	*.02*	*.05*
Krasinkov & Jayachandran	*marketing activity*	*.23*	*109*	*27303*	*.27*	*.24; .31*	*27*	*.19*	*.15; .23*	*.12*	*.25*	*.08*	*.15*	*.02*	*.08*
Krasinkov & Jayachandran	*R&D activity*	*.22*	*61*	*14350*	*.28*	*.23; .33*	*16*	*.17*	*.11; .23*	*.11*	*.24*	*.11*	*.17*	*.04*	*.11*
Krasinkov & Jayachandran	*operations activity*	*.16*	*45*	*12394*	*.22*	*.17; .27*	*13*	*.12*	*.07; .18*	*.13*	*.19*	*.10*	*.09*	*.03*	*.07*
Krasinkov & Jayachandran	*market measures*	*.30*	*64*	*16891*	*.33*	*.28; .37*	*3*	*.30*	*.25; .35*	*.22*	*.30*	*.03*	*.11*	*.03*	*.06*
Krasinkov & Jayachandran	*objective perf.*	*.18*	*33*	*8847*	*.22*	*.14; .29*	*6*	*.15*	*.08; .23*	*.15*	*.17*	*.07*	*.07*	*.05*	*.06*
Krasinkov & Jayachandran	*subjective perf.*	*.25*	*77*	*17911*	*.30*	*.26; .34*	*21*	*.20*	*.16; .25*	*.23*	*.28*	*.10*	*.07*	*.02*	*.06*
Lee & Madhavan, 2010	*divesture*	*.11*	*637*	*101643*	*.12*	*.10; .14*	*146*	*.00*	*-.02; .02*	*.11*	*.04*	*.12*	*.01*	*.08*	*.07*
Lim et al., 2011	*IT investment*	*.18*	*38*	*23057*	*.18*	*.13; .24*	*10*	*.07*	*.00; .14*	*.04*	*.14*	*.11*	*.14*	*.04*	*.10*
Lim et al., 2011	*spending*	*.14*	*20*	*9593*	*.20*	*.09; .31*	*0*	*x*	*x*	*.11*	*.15*	*.00*	*.09*	*.05*	*.05*
Margolis et al., 2007	*CSR*	*.13*	*163*	*27097*	*.14*	*.11; .16*	*36*	*.06*	*.03; .09*	*.06*	*.11*	*.08*	*.08*	*.03*	*.06*
Margolis et al., 2007	*market measures*	*.11*	*100*	*17593*	*.12*	*.08; .16*	*11*	*.07*	*.03; .11*	*.06*	*.07*	*.05*	*.06*	*.05*	*.05*
Margolis et al., 2007	*accounting measures*	*.15*	*71*	*9830*	*.16*	*.12; .20*	*19*	*.08*	*.03; .13*	*.12*	*.12*	*.08*	*.04*	*.04*	*.05*
Margolis et al., 2007	*cross sectional design*	*.11*	*83*	*16163*	*.13*	*.09; .17*	*0*	*x*	*x*	*.07*	*.09*	*.00*	*.06*	*.04*	*.03*
Margolis et al., 2007	*longitudinal design*	*.15*	*66*	*8816*	*.12*	*.08; .16*	*12*	*.07*	*.02; .11*	*.04*	*.07*	*.05*	*.08*	*.05*	*.06*
Richter et al., 2011	*teams*	*.09*	*44*	*10846*	*.12*	*.08; .16*	*13*	*.06*	*.02; .10*	*.03*	*.10*	*.06*	*.09*	*.02*	*.06*
Richter et al., 2011	*healthcare teams*	*.06*	*25*	*5841*	*.09*	*.05; .14*	*7*	*.05*	*.00; .10*	*.03*	*.07*	*.04*	*.06*	*.02*	*.06*
Richter et al., 2011	*HR teams*	*.07*	*29*	*6329*	*.10*	*.06; .14*	*9*	*.05*	*.01; .10*	*.03*	*.08*	*.05*	*.07*	*.02*	*.05*
Richter et al., 2011	*technical teams*	*.06*	*26*	*6009*	*.09*	*.05; .14*	*7*	*.05*	*.01; .10*	*.03*	*.07*	*.04*	*.06*	*.02*	*.04*
Tihanyi et al., 2005	*cultural distance*	*-.04*	*35*	*11628*	*-.05*	*-.09; -.00*	*8*	*.01*	*-.03; .06*	*.01*	*-.01*	*.06*	*.06*	*.04*	*.05*
Tosi et al., 2000	*CEO compensation*	*.08*	*24*	*9225*	*.18*	*.13; .24*	*12*	*.07*	*.02; .13*	*.01*	*.16*	*.11*	*.17*	*.02*	*.10*
Van Essen et al., 2012	*board size*	*.03*	*103*	*63711*	*.02*	*.00; .04*	*0*	*x*	*x*	*.10*	*-.01*	*.00*	*.00*	*.03*	*.01*
Van Essen et al., 2012	*accounting measures*	*.04*	*44*	*26735*	*.01*	*-.01; .03*	*4*	*.00*	*-.02; .02*	*.02*	*.00*	*.01*	*.00*	*.01*	*.01*
Van Essen et al., 2012	*market measures*	*.00*	*27*	*20008*	*.03*	*-.02; .07*	*0*	*x*	*x*	*.09*	*.00*	*.00*	*.06*	*.03*	*.03*

NOTE: r_p = effect size reported in original publication, *k:* number of published samples in meta-analysis, Σn = total sample size, r_o = weighted mean correlation of published samples in meta-analysis, 95CI = 95 percent confidence interval, *ik* = number of trim and fill imputed samples, $r_{t\&f}$ = trim and fill adjusted observed mean correlation, $95CI_{t\&f}$ = 95 percent confidence interval around the adjusted observed mean correlation, r_{prec} = weighted mean correlation of the 10% most precise (i.e., largest samples) in meta-analysis; r_{sm} = moderate selection model, Δr *t&f*=Absolute change in expected direction from the observed mean analytic correlation and the trim and fill adjusted correlation, Δr cum= Absolute change in expected direction from most precise 10% of samples to the observed mean analytic correlation, Δr *sm*= Absolute change in expected direction from the observed mean analytic correlation to the moderate selection model adjusted correlation, Δr *avg*= average absolute change in expected direction across all three publication bias adjusted estimates.

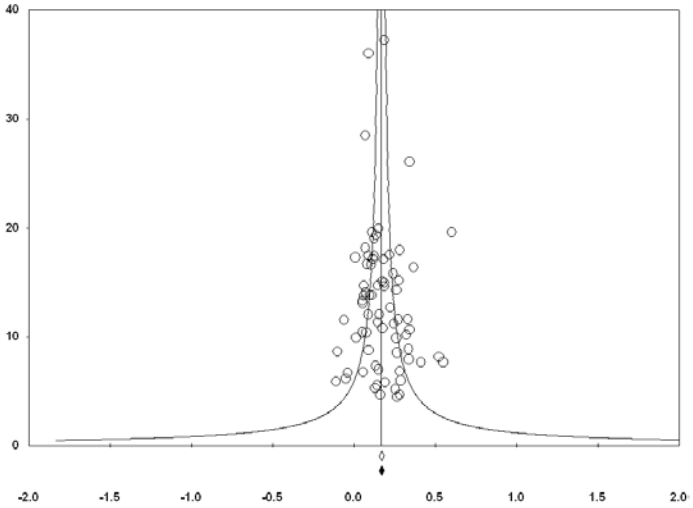
Table 2. Results of re-analyses and tests of publication bias with both published and unpublished studies

Article	effect	k_{unp}	k	Σn	r_o	95CI	Ur_o	ik	$r_{t\&f}$	$95CI_{t\&f}$	r_{prec}	r_{sm}	$\Delta r_{t\&f}$	Δr_{cum}	Δr_{sm}	Δr_{avg}
Brinckmann et al., 2010	*strategic planning*	*5*	*51*	*11046*	*.11*	*.21; .39*	*.20*	*0*	*x*	*x*	*.19*	*.09*	*.00*	*.12*	*.22*	*.11*
Brinckmann et al., 2010	*established firms*	*4*	*36*	*7333*	*.14*	*.09; .19*	*.18*	*0*	*x*	*x*	*.14*	*.10*	*.00*	*.00*	*.04*	*.01*
Brinckmann et al., 2010	*growth outcomes*	*5*	*36*	*5623*	*.13*	*.08; .18*	*.20*	*1*	*.12*	*.07; .17*	*.04*	*.10*	*.01*	*.09*	*.03*	*.04*
Brinckmann et al., 2010	*US firms*	*3*	*28*	*3960*	*.13*	*.07; .18*	*.11*	*0*	*x*	*x*	*.16*	*.09*	*.00*	*.00*	*.04*	*.01*
Combs et al., 2006	*work practices*	*18*	*92*	*19319*	*.16*	*.14; .19*	*.12*	*1*	*.16*	*.13; .19*	*.19*	*.14*	*.00*	*.00*	*.02*	*.01*
Combs et al., 2006	*HPWP*	*3*	*38*	*8615*	*.23*	*.18; .27*	*.27*	*6*	*.19*	*.14; .24*	*.29*	*.21*	*.04*	*.00*	*.02*	*.02*
Combs et al., 2006	*indiv practices*	*15*	*61*	*11928*	*.11*	*.09; .14*	*.09*	*1*	*.11*	*.09; .14*	*.10*	*.10*	*.00*	*.01*	*.01*	*.01*
Combs et al., 2006	*manufacturing*	*6*	*29*	*3989*	*.23*	*.16; .29*	*.18*	*0*	*x*	*x*	*.36*	*.19*	*.00*	*.00*	*.04*	*.01*
Ellis, 2006	*market orientation*	*1*	*39*	*8587*	*.29*	*.25; .32*	*Na*	*8*	*.25*	*.21; .29*	*.22*	*.28*	*.04*	*.07*	*.01*	*.04*
Ellis, 2006	*subjective perf.*	*1*	*26*	*4925*	*.30*	*.25; .34*	*Na*	*6*	*.26*	*.21; .31*	*.23*	*.29*	*.04*	*.07*	*.01*	*.04*
Hancock et al., 2007	*overall turnover*	*1*	*48*	*24944*	*-.08*	*-.10; -.05*	*Na*	*13*	*-.04*	*-.06; -.01*	*-.01*	*.02*	*.04*	*.07*	*.02*	*.04*
Hancock et al., 2007	*total turnover*	*1*	*38*	*20961*	*-.08*	*-.12; -.04*	*Na*	*10*	*-.03*	*-.07; .01*	*.00*	*.05*	*.05*	*.08*	*.13*	*.09*
Heugens et al., 2009	*owner concentration*	*16*	*237*	*397983*	*.04*	*.03; .06*	*.03*	*0*	*x*	*x*	*.07*	*.01*	*.00*	*.00*	*.03*	*.01*
Ketchen et al., 1997	*configuration*	*3*	*38*	*4830*	*.46*	*.39; .53*	*.27*	*17*	*.25*	*.16; .34*	*.18*	*.45*	*.21*	*.28*	*.01*	*.17*
Lee & Madhavan, 2010	*divesture*	*13*	*650*	*103299*	*.12*	*.10; .15*	*.21*	*146*	*.01*	*-.01; .03*	*.10*	*.04*	*.11*	*.02*	*.08*	*.07*
Lim et al., 2011	*IT investment*	*2*	*40*	*23891*	*.18*	*.12; .23*	*Na*	*10*	*.07*	*.00; .14*	*.04*	*.15*	*.11*	*.14*	*.03*	*.09*
Lim et al., 2011	*spending*	*1*	*21*	*10216*	*.20*	*.09; .30*	*Na*	*0*	*x*	*x*	*.11*	*.15*	*.00*	*.09*	*.05*	*.05*
Margolis et al., 2007	*CSR*	*4*	*167*	*28090*	*.13*	*.11; .16*	*.13*	*37*	*.06*	*.03; .09*	*.05*	*.28*	*.07*	*.08*	*.15*	*.10*
Margolis et al., 2007	*market measures*	*1*	*101*	*18009*	*.12*	*.08; .15*	*Na*	*11*	*.07*	*.03; .11*	*.06*	*.07*	*.05*	*.06*	*.05*	*.05*
Margolis et al., 2007	*accounting measures*	*2*	*73*	*10101*	*.16*	*.12; .20*	*Na*	*18*	*.09*	*.04; .13*	*.13*	*.12*	*.07*	*.03*	*.04*	*.05*
Margolis et al., 2007	*cross-sectional*	*4*	*87*	*17156*	*.13*	*.09; .17*	*.13*	*2*	*.12*	*.08; .16*	*.06*	*.09*	*.01*	*.07*	*.04*	*.04*
Richter et al., 2011	*teams*	*4*	*48*	*11108*	*.31*	*.23; .38*	*.14*	*13*	*.14*	*.05; .23*	*.08*	*.10*	*.17*	*.23*	*.21*	*.20*
Richter et al., 2011	*technical teams*	*4*	*30*	*6271*	*.10*	*.06; .14*	*.14*	*8*	*.06*	*.01; .10*	*.03*	*.08*	*.04*	*.07*	*.02*	*.04*
Tosi et al., 2000	*CEO compensation*	*13*	*37*	*10195*	*.18*	*.14; .23*	*.20*	*15*	*.08*	*.03; .13*	*.09*	*.16*	*.01*	*.09*	*.02*	*.07*

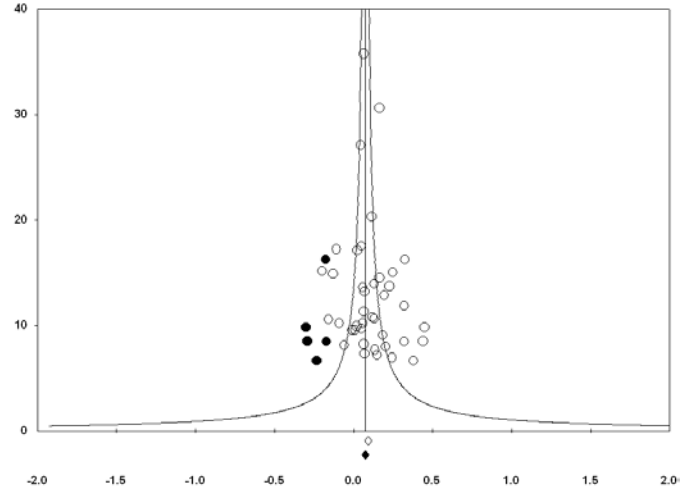
NOTE: r_p = effect size reported in original publication, *k:* number of published samples in meta-analysis, Σn = total sample size, r_o = weighted mean correlation of published samples in meta-analysis, 95CI = 95 percent confidence interval, *Ur_o = weighted mean correlation of unpublished samples in meta-analysis when $k \geq 3$, ik* = number of trim and fill imputed samples, *ik* = number of trim and fill imputed samples, $r_{t\&f}$ = trim and fill adjusted observed mean correlation, $95CI_{t\&f}$ = 95 percent confidence interval around the adjusted observed mean correlation, r_{prec} = weighted mean correlation of the 10% most precise (i.e., largest samples) in meta-analysis; r_{sm} = moderate selection model, Δr *t&f*=Absolute change in expected direction from the observed mean analytic correlation and the trim and fill adjusted correlation, Δr cum= Absolute change in expected direction from most precise 10% of samples to the observed mean analytic correlation, Δr *sm*= Absolute change in expected direction from the observed mean analytic correlation to the moderate selection model adjusted correlation, Δr *avg*= average absolute change in expected direction across all three publication bias adjusted estimates.

Figure 1: Funnel plots for selected distributions. The x-axis is the effect size and the y-axis is the precision (1/standard error). White circles are observed samples and black circles are imputed samples. Panel a shows no bias, panel b shows slight bias, and panels c and d show significant bias.

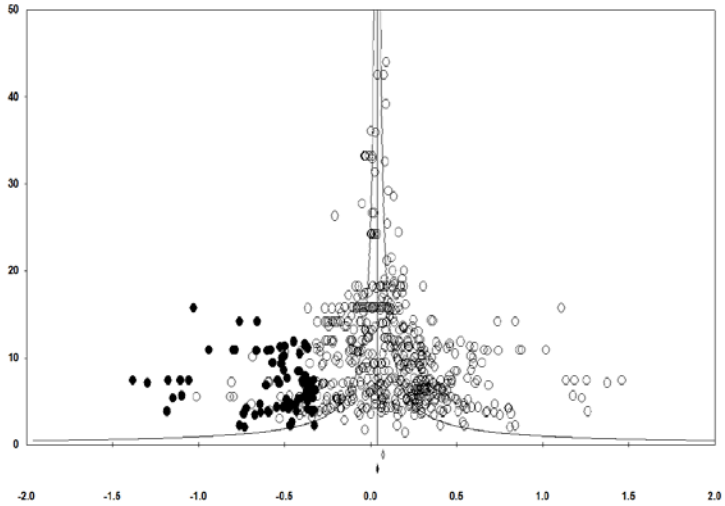
Panel a.



Panel b.



Panel c.



Panel d.

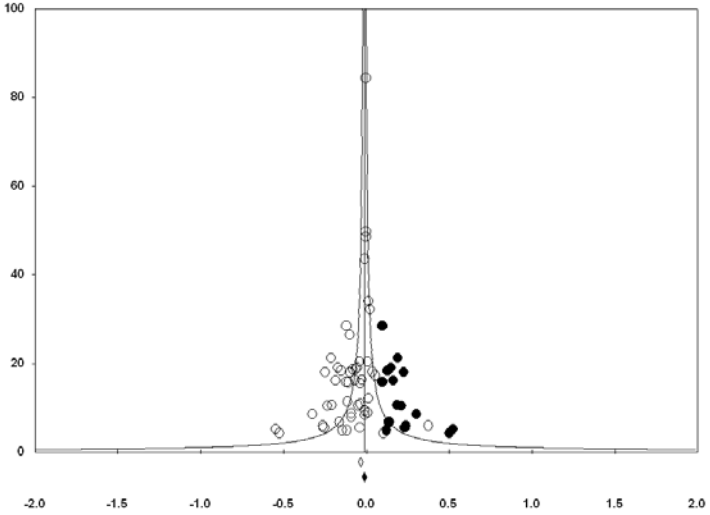
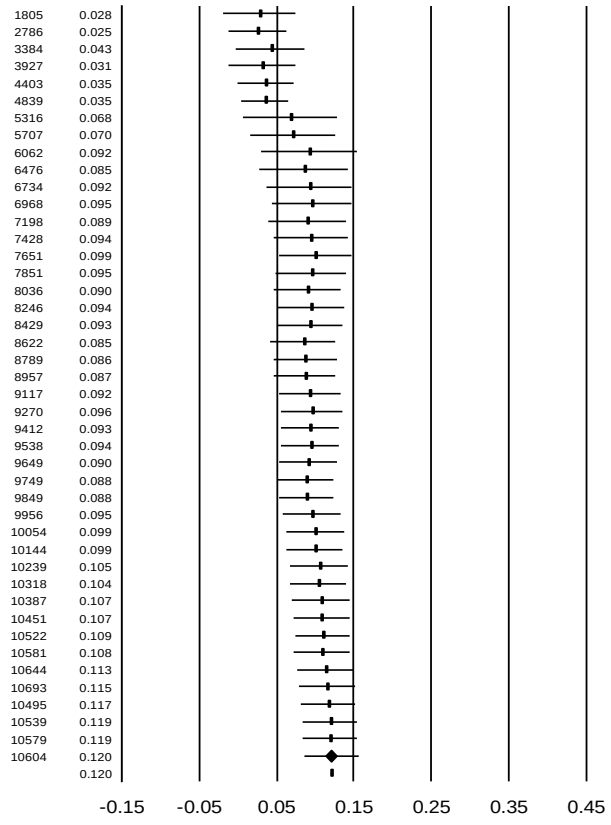
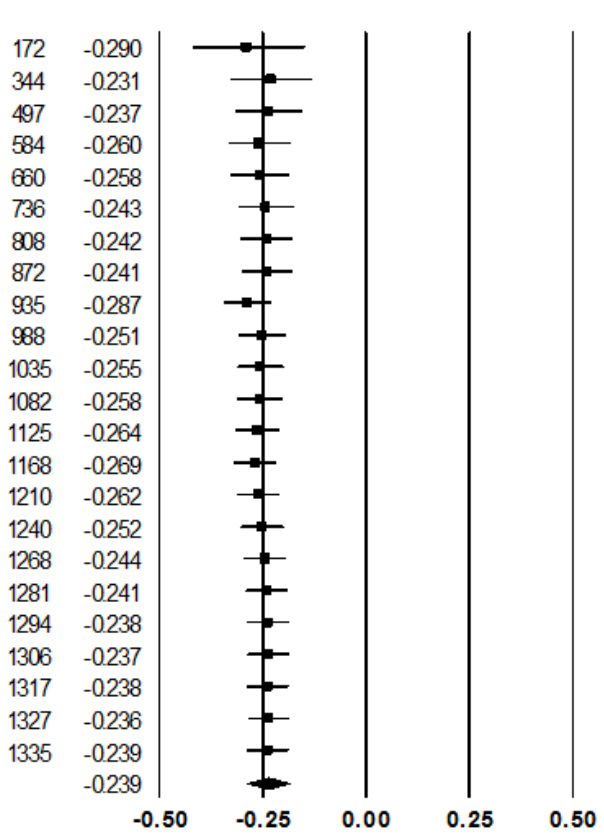


Figure 2:
Examples of Two Forest Plots Illustrating Cumulative Meta-Analysis by Precision

(a) Forest Plot Illustrating Drift (data from Richter et al. *2011*)



(b) Forest Plot Illustrating No Drift (data from Gooding & Wagner, 1985)



Note: for both plots the samples sizes and *r* estimate in each row are cumulative; N_{cum} = Cumulative sample size