It's merely a matter of time: A meta-analysis of the causality between environmental performance and financial performance*

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Markus Hang

corresponding author, email: Markus.Hang@mrm.uni-augsburg.de

Jerome Geyer-Klingeberg

email: Jerome.Geyer-Klingeberg@mrm.uni-augsburg.de

Andreas W. Rathgeber

email: Andreas.Rathgeber@mrm.uni-augsburg.de

Author's affiliation
Institute of Materials Resource Management
Faculty of Mathematics, Natural Sciences, and Materials Engineering
University of Augsburg
Alter Postweg 101
86135 Augsburg, Germany
Tel: +49 821 598 - 3042

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Abstract

Research on the relationship between corporate environmental performance (CEP) and financial performance (CFP) continuously receives high attention in both general media and academic publications. One central issue concerns the causal effects between the two constructs. Since existing primary literature is characterized by its heterogeneous study designs and mixed empirical evidence, the aim of this paper is to explicitly shed light on the causality effects between CEP and CFP by means of a meta-analysis of 893 empirical estimates from 142 CEP-CFP studies. Our findings suggest that in the short run (one year), financial resources can increase a firm's environmental performance as proposed by the slack resources hypothesis; however, the effects disappear in the long run (after more than one year). Conversely, increasing environmental performance has no short-term effect on a corporate financial performance, while a firm significantly benefits in the long-term, which is in accordance with the Porter hypothesis. Overall, our results show that the causality between environmental performance and financial performance depends on the time horizon.

Keywords: corporate environmental performance, corporate financial performance, sustainable development, environmental policy, causality, meta-analysis

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1 Introduction

Over the past decades, empirical literature studying the relationship between corporate environmental performance (CEP) and financial performance (CFP) has grown rapidly. As a consequence, controversies arose concerning the sign of the relation, moderating and mediating factors as well as the causality of the effect (Feng *et al.*, 2018; Hartmann and Vachon, 2018). Due to these aspects, academics often refer to this bivariate relation as an overall heterogeneous and complex interaction (Endrikat *et al.*, 2014; Guenther and Hoppe, 2014). Although literature provides a vast number of comprehensive and meaningful primary studies (among others, Hart and Ahuja, 1996; King and Lenox, 2002; Konar and Cohen, 2001; Russo and Fouts, 1997; Tang *et al.*, 2018) and meta-analyses (among others, Albertini, 2013; Endrikat *et al.*, 2014; Guenther *et al.*, 2012; Hang *et al.*, 2018), the discussion of the causality is largely fragmented. Besides the prevalent mixed empirical evidence, existing primary studies are strongly characterized by their heterogeneous study designs, which hampers comparison. Overall, the questions whether CEP affects CFP, CFP affects CEP, or if there even exists a bidirectional relation are still unresolved, also the signs of the respective effects are ambigious.

The most common approach of addressing this issue in a primary study is the application of the Granger causality specification (Angrist and Pischke, 2009). Therein, CEP and CFP are analyzed in a regression framework, while the two constructs of interest are measured at different time lags. Results for the causal effects are then revealed by exchanging the dependent and independent variable or varying the time lag between them. Although, delayed effects are not identical to causality, they are at least accepted as a strong indicator (for example, Bono and McNamara, 2011; Mitchell and James, 2001; Wagner and Blom, 2011).

Most of the existing studies focus on a single causality between CEP and CFP. The majority of articles analyzes the causality from CEP to CFP, as for example the studies by Hart and Ahuja (1996), Hillman and Keim (2001), King and Lenox (2002), Konar and Cohen (2001), and Russo and Fouts (1997). In contrast, a smaller part of the literature investigates the causality from CFP to CEP. This includes, for example, the studies from Arora and Cason (1995), Berrone and Gomez-Mejia (2009), Cohen *et al.* (1995), Levy (1995), and Makni *et al.* (2009). As the concentration on a single direction of causality does not allow general conclusions, several primary studies contrast the two causal directions. Ameer and Othman (2012) and Makni *et al.* (2009) confirms a negative influence of CEP on CFP after one year. Horváthová (2012) extends this result by finding that CEP leads to a CFP decreases after one year, but firms profit from CEP after two years. Hart and Ahuja (1996) suggest that CEP has a positive effect after one year for accounting-based CFP measures and after two years for market-based CFP measures, since it takes some time until the market recognizes a firm's environmental performance. Using a one-year lag, Nakao *et al.* (2007) even finds significant effects for both causal directions. However, they add that CEP increases CFP only recently, but the positive effect from CFP to CEP exists longer. In contrast, CEP does not affect future CFP according to Alvarez (2012). Heras-Saizarbitoria *et al.* (2011) find in a longitudinal study that CEP does at least conditionally increase CFP. Their results imply that firms that are more profitable prefer investing in CEP, while there are also weak anticipation effects of investors. However, they state that firms do not profit from enhanced CEP in the future. In this context, Kim and Statman (2012) propose that the positive relation between CFP and CEP is greater in times of CFP increases in contrast to period of CFP decreases. In contrast, Levy (1995) reveals that the effect is insignificant for both causal directions using a one-year lag. Overall, primary studies show inconclusive results for the general question of the causality between CEP and CFP, which might especially stem from different variable measures, sample compositions and estimation methods (Guenther *et al.*, 2012; Guenther and Hoppe, 2014). Overall, empirical evidence from primary studies is fragmented and inconsistent.

Considering these circumstances, several meta-analyses review the cumulative results of the literature by quantitatively aggregating existing primary studies. Dixon-Fowler *et al.* (2013) examine the causality by comparing the reported results in the form of 202 Pearson correlation coefficients from 39 studies measuring CEP and CFP concurrently with measuring CFP one or more years ahead. However, they find no significant difference between the two groups. The causality from CFP to CEP is not analyzed due to a lack of data. Dixon-Fowler *et al.* (2013) encourage to also "examine this important relationship". Furthermore, Endrikat *et al.* (2014) analyze the causality between CEP and CFP based on 245 Pearson correlation coefficients from 149 primary studies and additionally take the reverse causality into account. Since they use multidimensional subgroups by splitting the sample according to the analyzed causality and the variable measurement, a general understanding of the causality is still not possible. Their results reveal statistically and economically significant effects from CEP to CFP for process-based CEP and subsequent accounting-based CFP as well as for outcome-based CEP and subsequent market-based CFP. Moreover, they find statistically and economically significant effects for all specifications measuring CEP and CFP concurrently. In contrast, they find no evidence for CFP affecting CEP. The meta-analyses by Albertini (2013), Guenther *et al.* (2012), and Horváthová (2010) do not analyze the causality issue at all. This summary demonstrates, that meta-analytical literature addressing this question is also characterized by mixed results and various study designs providing only insufficient answers to the question of causality.

This study aims to provide the first complete analysis of the CEP-CFP causality by means of meta-analysis using a sample of 893 existing results drawn from 142 empirical primary studies and contributes to prior primary studies as well as meta-analyses in the following ways. We thoroughly analyze the following three relations: CEP affecting CFP, CFP affecting CEP, and the bidirectional impact. Furthermore, we investigate the temporal development of the effects by including yearly lagged effects up to 5 years, which has yet not been done in previous literature. This approach allows more general and fine-grained conclusions regarding CEP-CFP causality. In contrast to prior reviews, we use the partial coefficient derived from regression coefficients to measure the bivariate relationship. As advantages over traditional Pearson correlation coefficients as used in the majority of previous literature, disruptive effects are filtered out in order to isolate the effect of interest. Moreover, primary studies mostly report multiple results in their regression analysis (for different time periods, model specifications and other subgroups) but only few Pearson correlation. Accordingly, the use to partial correlations computed from regression results maximized the sample of primary studies and effect sizes to be included in the meta-analysis. Moreover, we also test for the potential presence of publication bias. In general, publication selection bias refers to the phenomenon that certain estimates are systematically underrepresented in empirical literature (Rosenthal, 1979). In other words, publication selection bias exists when researchers prefer statistically significant results or results that are consistent with the theory and previous research outcomes (Stanley, 2005). One potential source of publication bias might be the selective reporting of results depending on the number of lagged years (Bruns and Stern, 2018). Finally, we explore the heterogeneity of results by applying a meta-regression analysis incorporating differences in measurement, study quality, regions, time, industry, data and estimation procedures of the primary studies. This procedure should reveal the main drivers of the variation among the primary study results.

The remainder of this paper is structured as follows. Section 2 sums up the theoretical literature for the different causalities between CEP and CFP. The data set and the applied meta-analytical procedures are described in section 3. Subsequently, section 4 presents the empirical results, while section 5 concludes the paper.

2 Theory of the CEP-CFP relation

For the interaction between the two dimensions, three causal directions are plausible: CEP influences CFP, CFP affects CEP and a bidirectional relationship. Besides the direction of the effect, literature is also inconsistent about the sign of the relation, which might either be negative, neutral or positive. In this section, we briefly present the theoretical considerations, each pointing to a certain causality between CEP and CFP, as they also underlie the primary studies included in our meta-analytical data set. For the categorization of the existing theoretical constructs, we follow the structure by Preston and O'Bannon (1997) and Waddock and Graves (1997), which is summarized in Figure 1 and briefly outlined below.

Insert Figure 1 about here

2.1 CEP affects CFP

Concerning the first causal sequence, the most antiquated argument for the potential impact of CEP on CFP is known as the tradeoff hypothesis, indicating a negative influence as formulated by Levitt (1958). Thus, environmental engagement requires financial investments by the firm, which are not completely compensated by financial returns from environmental activities. These negative effects might especially occur in the short-term, when the costs are realized. Since firms not investing in CEP do not have to bear these costs, following Aupperle *et al.* (1985) and Vance (1975) these unfair costs might be a danger to free market economy. As the generation of financial returns constitute the primary goal of a firm, CEP stands in a competing relationship with CFP. Research by Bragdon, Jr. and Marlin (1972) complement that there is only a choice between investing in a profitable firm or a responsible firm. Conversely, the creation of additional value is the single social corporate responsibility (Friedman, 2002).

The supply and demand model, a theoretical framework modeling CFP independently from CEP and vice-versa, is developed by McWilliams and Siegel (2001). It proposes the existence of an optimal investment in environmental engagement, which can be determined by cost-benefit analysis. Accordingly, the decision for environmental investments should be based on the same principles as any other investment (Barnett, 2007; McWilliams and Siegel, 2001). This leads to a synthesis of interests, as not only the maximization of profitability demanded by shareholders is considered, but also the claims of stakeholders for environmental responsibility, such as those of customers, employees and communities (McWilliams and Siegel, 2001). However, firms following this procedure do not exhibit higher profitability than those who do not invest in environmental activities. Assuming that all firms take optimal decisions and they are within the optimum between supply and demand, they are equally profitable. As soon as one firm has a higher return on investment, the competing company would change its product strategy (McWilliams and Siegel, 2001).

A positive impact of CEP on CFP is motivated by the Porter hypothesis. Consequently, environmental regulation might induce innovations in order to increase a firm's efficiency and competitiveness (Esty and Porter, 1998). Since pollution as an outcome of CEP can be seen as economic waste, pollution reduction contributes to a firm's profitability (Porter and Linde, 1995). We conclude, that while innovations take time for development, firms profit from them in the long run.

Furthermore, the assumption of a positive influence of CEP on CFP by the natural resource-based view (NRBV) is based on the resource-based view proposed by Barney (1991), Hart (1995), and Wernerfelt (1984). Hence, the strategic advantages of a firm can be reduced to the access to strategically valuable resources in a firm's individual and hard to duplicate resource bundle, for example the particular mix of management skills, business processes, routines or knowledge, as well as a superior utilization of the available resources (Barney *et al.*, 2011). However, in consideration of technological developments and changes in the environment, the concentration on core competences loses its effect, which justifies the relevance of the NRBV (Kraaijenbrink *et al.*, 2010; Tushman and Anderson, 1986). Accordingly, the competitive advantage of a firm is directly linked to how it deals with the natural environment, as responsible behavior enables a firm to gather further capabilities and new resources, such as knowledge or enhanced corporate culture (Branco and Rodrigues, 2006). Hart (1995) complements that strategy and competitive advantages are strongly influenced by the environmental performance of a firm, while pollution prevention, product stewardship, and sustainable development are the core drivers. Following Davis (1973), environmental responsibility consequently leads to a long-term profit maximization followed by a better community and society.

The NRBV is supported by the instrumental stakeholder theory (Davis, 1973; Donaldson and Preston, 1995; Jones, 1995; Orlitzky *et al.*, 2003). Thus, each firm is surrounded by a network of expectations from contractual relationships, for example from suppliers, employees or customers. Managing these stakeholder interests leads to increasing profitability, stability and growth (Damak-Ayadi and Pesqueux, 2005). As Jones (1995) points out, trusting and cooperative behavior solves problems related to opportunistic behavior. Since environmental engagement can be seen as an effort to meet these stakeholder expectations, a firm has to meet these requirements to achieve financial advantages, although such behavior may seem to be economically irrational or altruistic (Buysse and Verbeke, 2003; Jones, 1995). For example, economic advantages might be greater customer loyalty, long-term supply relationships, better reputation, product differentiation, and higher selling prices. We hypothesize, that this effect might especially hold long-term.

In this context, literature often refers to the social impact hypothesis coined by Latané (1981). Accordingly, a firm must not only meet explicit expectations of stakeholders as shown above, but also implicit expectations like quality service or environmental responsibility (Cornell and Shapiro, 1987). If such expectations are not fulfilled, fears and risk on the market increase and such firms may be faced with additional and more costly explicit agreements in the future, for example, as parties like the government pass more stringent rules to ensure more environmentally conscious behavior.

Since changes in the trust and contractual conditions of stakeholders are not assumed to arise in the short-term, we expect these negative effects to occur in the medium to long run. On the contrary, environmentally conscious firms have more low-cost implicit claims, resulting in higher financial performance (McGuire *et al.*, 1988). Hence, environmental consciousness is a means to retain freedom in decision making (Davis, 1973).

2.2 CFP affects CEP

For the second causal sequence, literature provides the managerial opportunism hypothesis, which suggests a negative impact of CFP on CEP (Preston and O'Bannon, 1997). This position is grounded on the assumption that managers follow their own targets, which may not be in the best interest of the shareholders (Alkhafaji, 1989; Posner and Schmidt, 1992; Weidenbaum and Vogt, 1987). This circumstance directly leads to inefficiencies in the operating firm, which may result from wages linked to short-term profits and stock prices (Preston and O'Bannon, 1997; Weidenbaum and Vogt, 1987). Thus, especially when a firm performs well, managers tend to increase their own income by reducing environmental investments, which we expect to affect CEP in the long run. Apart from that, managers may expand corporate environmental expenditure to compensate bad corporate performance (Preston and O'Bannon, 1997). These effects might especially occur in the short-term, if a firm suffers an unexpected bad business results. This managerial behavior is especially driven by a firm's compensation system. According to Preston and O'Bannon (1997), opportunistic activities are reinforced if manager salaries are linked to short-term profits of a firm and the improvement of environmental performance is postponed for the benefit of private bonus payments. One potential solution might be the linkage of executive compensation to corporate environmental performance (Cordeiro and Sarkis, 2008).

Representatives of the slack resources theory suggest a positive impact of CFP on CEP. Hence, slack resources generated from good financial performance enable companies to invest in environmental programs (Kraft and Hage, 1990). Analogous to the NRBV, investing in environmental activities is connected with enhancing internal resources, new capabilities, and comparative advantages of the firm, as well as possibilities for differentiation through innovative and eco-friendly developments (Bourgeois, 1981). Thus, investing slack resources also allows firms to adapt to their external environment for long-term profitability. Although firms often want to act environmentally friendly and socially, they might be restricted by limited availability of financial resources (Preston and O'Bannon, 1997). If financial slack is only available in one year, there might be too little time to develop proactive environmental strategies and a firm instead invests in ecological one-off actions.

2.3 Bidirectional relationship between CEP and CFP

Waddock and Graves (1997) reconciled the two previously explained causal relations, proposing the "virtuous circle" – a concurrent relationship between CEP and CFP (Waddock and Graves, 1997). Following this hypothesis, superior CEP initiates better CFP, which again allows companies to reinvest in CEP. This leads to a two-way causality for the two constructs. The process might either begin with available financial slack or with an initial investment in environmental performance. This mutually supportive process might, however, also arise as a negative synergy (Allouche and Laroche, 2005).

3 Empirical analysis

This section describes the search for empirical studies and the subsequent preparation of data. Second, the statistical approaches of Hedges and Olkin-type meta-analysis and publication bias test are presented.

3.1 Data search and preparation

Meta-analysis starts with the collection of available CEP-CFP studies. First, we gathered the sample of 149 primary studies used by Endrikat *et al.* (2014). In the next step, we extended their sample by studies published after 2012 using the same search strategy to search major databases[1] for published research articles and grey literature using a comprehensive search term incorporating

[1] The screened databases are ABI/Inform Complete, Business Source Complete, EconLit, GreenFILE, ScienceDirect and Social Science Research Network.

various keywords for CEP, CFP and applied empirical methodology. At the end, the literature search left us with 198 relevant publications.

To identify the studies that are eligible to be included in our analysis, we adopt the following set of selection criteria on the sample of relevant work:

(1) Studies examining disclosure data as proxy for CEP are dropped, since recent empirical analyses show an inconsistent and contradictory behavior in contrast to other frequently used measures as, for example, intensity of emissions (among others, Aragón-Correa *et al.*, 2016; Hughes *et al.*, 2001; Patten, 2002).

(2) Because statistical aggregation of empirical results of many studies requires that the effect sizes are comparable across studies (Stanley and Doucouliagos, 2012), event studies and findings from probit/logit models are excluded.

(3) Each study has to report appropriate statistical results from regression analysis including regression coefficients, the corresponding standard error (or *t*-statistics) and the underlying sample size. This is necessary to extract the empirical effect between CEP and CFP in form of partial correlation coefficients.

After employing the selection criteria, the final sample is composed of 142 empirical primary studies. All studies are included and highlighted in the reference list.

As effect size to be synthesized across studies, we use the partial correlation coefficient, which is directly calculated from the studies' regression results. The partial correlation coefficient (r) is defined as follows

$$r = \frac{t}{\sqrt{t^2 + df}}, \quad (1)$$

where t is the t-statistic of the regression estimate collected from the primary studies and df are the corresponding degrees of freedom. The variance of the partial correlations is computed by

$$v(r) = \frac{(1 - r^2)}{df}. \quad (2)$$

The partial correlation coefficient from equation (1) measures the intensity of the CEP-CFP relation, while keeping constant all other variables in model.[2] This effect size is preferable for our analysis due to several aspects. First, compared to the Pearson correlations, the effect of interest is corrected for spurious influences. Second, the use of the Pearson correlations would lead to a smaller set of studies that could be included. Furthermore, using partial correlations allows to collect multiple estimates from each study as most authors report empirical results for several regression models.

After inspecting the appropriateness of each regression analysis in the collected primary studies, the sample of 142 primary studies provides a database of 893 partial correlation coefficients, which are obtained from a total sum of 757,154 firm-year observations[3].

3.2 Hedges and Olkin-type meta-analysis

For the aggregation of reported effect estimates, we apply Hedges and Olkin-type meta-analysis (HOMA; Hedges and Olkin, 1985) in order to calculate mean effect sizes. Consequently, the mean effect size for a bivariate relation and its standard deviation are computed by

$$\bar{r} = \frac{\sum(w_i \times r_i)}{\sum w_i} \text{ and } w_i = \frac{1}{v_i + \tau^2}, \tag{3}$$

where w_i is the effect size specific weight and v_i is the variance of the effect size as calculated by equation (2). Up to now, the primary studies, effect sizes respectively, are assumed to share one common population effect size and variation in the effect sizes only stems from a study-specific sampling error covered by the variance of the effect size v_i. However, it could be argued that the population effect size follows a normal distribution due to variation induced by random effects across the primary studies and effect estimates respectively. In order to account for such unobserved heterogeneity in the effect sizes, a random effects model is more appropriate incorporating a random effects component τ^2 as shown in equation 3. The latter is estimated by the restricted maximum likelihood estimator. This approach produces random effects weights calculated by the inverse sum

[2] Due to different definitions of CEP in primary studies for the measurement of the CEP-CFP relation, the sign of the estimated impact of CEP on CFP might differ (Albertini, 2013). For example, CEP measured by "the total amount of waste" should produce the inverse sign compared to "the amount of reduced emissions". As a consequence, the sign of the effect size is unified across studies so that higher values of a certain variable are associated with higher CEP.

[3] The number of firm-year observations is calculated as the sum of the number of firms times the corresponding number of observed years (balanced panel) across studies. In the case of an unbalanced panel, the exact number of firm observations over the research period is used.

of these two variance components. By including τ^2 in the weighting scheme, the analysis accounts for typical drivers of heterogeneity in this field of research, such as measurement difference, regional differences, temporal effects, differences in study quality, study characteristics and data characteristics. For the estimation of τ^2 and further explanations of the HOMA procedure, please refer to Borenstein (2009), Carney *et al.* (2011), and Essen *et al.* (2015). The standard error of the mean effect size is given by

$$SE(\bar{r}) = \sqrt{\frac{1}{\sum w_i}}. \tag{4}$$

Moreover, we use Fisher's z-transformation in a robustness test in order to correct for potential skewness in r_i and to achieve normally-distributed effect sizes. Consequently, the z-transformed effect sizes and their standard error are calculated by

$$z_i = 0.5 * ln\left(\frac{1+r_i}{1-r_i}\right) \text{ and } SE(z_i) = \frac{1}{\sqrt{n_i - 3}}, \tag{5}$$

where n_i is the number of firms related to a certain effect size. The transformed values are then re-transformed into the correlation metric for interpretation.

3.3 Publication bias test

As typically applied in meta-analysis, we investigate the presence of selective reporting of research results. Publication selection bias exists if specific estimates are systematically overrepresented in empirical literature (Rosenthal, 1979). This means that researchers favor statistically significant results or results that are in line with theory and previous research outcomes (Stanley, 2005). If publication selection bias is present in literature, the overall picture across the available literature will be distorted (Card and Krueger, 1995; Doucouliagos and Stanley, 2013).

The statistical analysis of publication bias is carried out by analyzing the relation between the observed effect sizes and their standard errors. Accordingly, the model can be formulated as (Card and Krueger, 1995):

$$r_i = \beta_0 + \beta_1 SE(r_i) + \varepsilon_i, \quad \varepsilon_i \sim N(0; SE(r_i)^2). \tag{6}$$

The dependent variable r_i is the *i*-th partial correlation coefficient, $\mathrm{SE}(r_i)$ is the standard error of the partial correlation, and ε_i is the error term.

As proposed by the Egger-test (Egger *et al.*, 1997), the *t*-test of the regression coefficient β_1 in this model investigates publication selection bias. If $\beta_1 = 0$, it can be reasoned that literature is unbiased. Hence, the probability of measuring the true population effect increases with the precision of the estimates and the reported effect estimates in the primary studies are normally (symmetrically) distributed around the true population effect. If there is statistically significant evidence that $\beta_1 \neq 0$, certain results are overrepresented, and the presence of publication bias would be confirmed.

While performing the publication bias test, the following aspects are considered in the model specification. (1) The errors of the regression might be heteroskedastic due to the usual great variation of the standard errors of the reported estimates across the primary studies. Therefore, a weighted least squares approach is conducted using the inverse standard errors of the effect sizes as weights. Accordingly, studies reporting lower standard errors, more precise results respectively, get larger weights in the MRA estimation (Hedges and Olkin, 1985). (2) Multiple estimates per study are integrated in our meta-analysis. For this reason, potential within-study correlation among the effect sizes obtained from the same study have to be taken into account. Thus, standard errors are clustered at the level of each individual study (Hedges *et al.*, 2010). (3) We perform a random effects model to account for residual heterogeneity. The latter might, for example, come from deviations of the effect sizes due to unobserved heterogeneity on the firm-level (like management quality).

4 Presentation of meta-analytical results

4.1 Results of Hedges and Olkin-type meta-analysis

For the analysis of the causality between CEP and CFP, we conduct Hedges and Olkin-type meta-analysis measuring the effect between CEP and CFP in the form of a random effects mean effect size. The adequacy of the random effects model is especially motivated by the results from heterogeneity test. Table 1 sums up the results given by the *Q*-statistic (Cochran’s heterogeneity statistic), I^2 (percentage of total variation across studies, which stems from heterogeneity rather than chance), and τ^2 (variance of the effect size parameters across the population of studies) (Higgins *et al.*, 2003). In general, the statistics confirm that a statistically and economically significant part of the variation of effect sizes stems from heterogeneity. For this reason, the assumption of random effects seems appropriate.

The random effects model is applied to various subsamples of effect sizes measuring the relation between CEP and CFP depending on the number of lagged years between the two constructs as reported in primary studies. Following the common practice introduced by Cohen (1992), we assess mean effect sizes as economically significant, if they are greater than 0.10. The results are displayed in Table 1.

Insert Table 1 about here

Starting with the full sample, the results show a mean effect size of 0.072, which is statistically significant at any common level. The same holds for the subsample of effect sizes measuring CEP and CFP concurrently with a mean effect size of 0.077. However, according to Cohen (1992), these effects are not economically significant.

Continuing with the causality from CEP to CFP, the full subsample has a mean effect size of 0.056, which is statistically significant. Hence, on average, the effect seems to be slightly lower compared to the full sample. If CEP and CFP are lagged by one year, the effect even decreases to just 0.030, statistically significant at 5%. Since the two previous values do not exceed the threshold of 0.10, we do not share the opinion of Ameer and Othman (2012), Hart and Ahuja (1996), Horváthová (2010), Makni *et al.* (2009), Nakao *et al.* (2007), and Rassier and Earnhart (2011), that CEP really affects CFP in the following year. However, the effect increases when the time lag is extended to two years as proposed by Hart and Ahuja (1996), and Horváthová (2010). In this case, the mean effect size is 0.110, which is statistically and the mean effect size also lies above the threshold by Cohen (1992). Hence, this effect is assessed as economically significant, which allows the conclusion that increasing CEP leads to financial benefits after two years. As an extension of existing literature contrasting the different causalities, we continue with the effects for three-year and five-year lags. As found for the two-year lag, the statistically and economically significant effect holds for the time lag of three years. Here, the mean effect size even increases to a statistically significant value of 0.158. For a time lag of five years, the mean effect size again drops to 0.117, which is still statistically and economically significant.

For the causality from CFP to CEP, the mean effect size for the full subsample is 0.100, which is statistically and also economically significant. For a more comprehensive analysis, the mean effect sizes are again calculated for the different number of lagged years. At a time lag of one year, the mean effect size measures 0.104, which is statistically and economically significant. This result confirms the conclusions by Heras-Saizarbitoria *et al.* (2011) and Nakao *et al.* (2007). For a time lag of two years, the mean effect size even gets negative with an insignificant value of -0.058. This means that increasing CEP, which stems from the availability of additional financial resources of a firm, only remains for one year. After two years, no effect is observable anymore. Furthermore, the effect remains insignificant for a time lag of three years with a mean effect size of 0.055.

Overall, the results suggest that increasing CEP as a consequence of additional financial resources only has a short-term effect, which lasts one year. This finding is in line with the slack resources hypothesis (Kraft and Hage, 1990). Accordingly, financial slack is especially invested in eco-friendly one-off actions. Hence, following our results it would be desirable from the perspective of the stakeholders, but also of the firm itself, to invest financial slack more wisely. In contrast, our results confirm that if a firm proactively increases CEP, it may achieve long-term economic benefits starting after two years, as proposed by the Porter hypothesis (Porter and Linde, 1995). Due to the time lag resulting from the development and realization of environmental innovations induced by environmental regulation, the positive financial effects delay. In the same way, firm might profit in the long-term from additional knowledge and resources as proposed by the NRBV (Hart, 1995) as well as from enhanced stakeholder relations following the instrumental stakeholder theory (Davis, 1973) and the social impact hypothesis (Latané, 1981). As a robustness test, all HOMA results are recalculated using *z*-transformed effect sizes. As presented in Table 2, the results remain stable.

Insert Table 2 about here

4.2 Publication bias test

In order to examine the robustness of our results, we perform a publication bias test as routinely employed in meta-analysis. As a first graphical impression, we consult the so-called funnel plots. Therein, the effect sizes (partial correlations r) are plotted against their precision (1/SE(r)). As an example, Figure 2 shows the funnel plots of the effect sizes measuring the relation between CEP and CFP for the different major (sub-)samples. An unbiased sample should lead to a symmetric-inverted funnel, indicating that the deviations of the single effect sizes from their mean value decrease with an increasing precision of their estimation. Figure 2 tends not to reject this hypothesis, as effect sizes are quite symmetrically distributed around the mean effect sizes. Solely for the last subsample of effect sizes measuring CFP as lagged independent variable, effect sizes are slightly underrepresented on the left side. However, this might be reasoned by the small sample size but could also be an indicator of publication bias.

Insert Figure 2 about here

For a more objective test of publication bias, we perform the Egger-test (Egger *et al.*, 1997) for all subsamples of effect sizes, for which a mean effect size is calculated. The results are displayed in Table 3. Accordingly, there are no significant effects, which point to the presence of publication bias. Solely for full subsample of effect sizes measuring CEP and lagged independent variable, the publication bias test reveals an estimate of 1.142, which is weakly significant at the 10% level. Hence, the choice of time lag is no means for selective reporting of results. Overall, the analysis provides no evidence for publication selection bias.

Insert Table 3 about here

As a robustness test, all publication bias tests are recalculated using *z*-transformed effect sizes. As presented in Table 4, the results remain stable.

Insert Table 4 about here

4.3 Robustness test and of heterogeneity

As already noted, hitherto unobserved heterogeneity (as incorporated in the random effects component τ^2 in previous analyses) is present in the field of the CEP-CFP relation. This is also empirically confirmed by the statistically significant heterogeneity test statistics displayed in Table 1 and Table 2. The subsequent analysis of heterogeneity sheds light on the main reasons of differences across studies. Therefore, we first derive various moderating variables based on information from primary studies, which might cause the heterogeneity among the effect sizes. For the choice and design of variables, we follow prior meta-analysis (Albertini, 2013; Dixon-Fowler *et al.*, 2013; Endrikat *et al.*, 2014; Guenther *et al.*, 2012; Horváthová, 2010). The set of variables is listed in Table 5 together with their descriptive statistics. After selecting the relevant moderating variables, these are included as additional explanatory variables in a meta-regression analysis as an extension of equation (6).

Insert Table 5 about here

Based on the primary studies in our sample, we designed the following moderating factors including measurement differences, study quality characteristics, temporal differences, regional differences, industrial differences, data characteristics, and estimation characteristics. Following previous meta-analyses, among the measurement differences we classify financial performance measures in market-based (Tobin's Q, stock return, or market value of a firm) and accounting-based (return on assets, return on equity, or return on sales). The two measures especially differ in terms of their forward-looking properties, which are more present in market-based measures compared to the backward-looking properties of accounting-based measures. *Market-based CFP* takes on the value one if a study uses a market-based CFP measure and zero otherwise. Furthermore, the CEP measurement is categorized by its strategic level and quantifiability. Process-based measures refer to CEP on a management or process level. These measures cover management practices, environmental policies, or environmental innovation. On the other hand, outcome-based measures refer to real impacts of these efforts by measuring the amount of emissions, the ratio of recycled waste to total waste, or energy consumption. *Process-based CEP* takes on the value one if a study uses a process-based CEP measure and zero otherwise. Additionally, proactive measures refer to pollution prevention through such as green process design, special capabilities, or resource combinations of the firm (Walls *et al.*, 2011). On the contrary, "end-of-pipe solutions" such as air filters or water clearers to comply with regulations and laws in order to minimize costs, risks, and liabilities are classified as reactive measures. *Proactive CEP* and *reactive CEP* take on the value one for the corresponding measures. The *number of citations* as derived from Google Scholar serve as a proxy for study quality. In order to capture temporal effects, the mean year of the observation period in a primary study is included, while 1950 is used as base year (*Mean sample year - 1950*). Since a major part of the literature analyzes US firms and EU firms, we incorporate two corresponding dummy variables to capture regional differences (*US data* and *EU data*). Similarly, large parts of the literature examine manufacturing and service firms in order to reveal industrial differences. The dummy variables *manufacturing sector* and *service sector* take on the value one for the corresponding studies as suggested by Fujii *et al.* (2013). The same also holds for small firms compared to large companies (*small firms*).[4] In order to cover potential differences in the effects due to estimation differences, *OLS estimation* distinguishes simple OLS techniques (=1) from more sophisticated approaches (=0).

[4] If the mean market capitalization is less than 1 billion dollars or a firm has less than 1,000 employees, we classify a firm as small. Approximately, these are the lower limits of the S&P 500 constituents.

Finally, the dummy variable *endogeneity considered* indicates if the used estimation procedure considers potential endogeneity between CEP and CFP.

For the selection of the moderating variables we face the problem that there is no underlying theory to derive the best set of variables. Thus, we collect a broad set of variables. If we would include all explanatory variables in the same regression model, we would probably face two problems: multicollinearity and high model uncertainty. To address these issues, we follow recent developments in meta-regression research and employ Bayesian model averaging (BMA) (see, for example, Babecky and Havranek, 2014; Zigraiova and Havranek, 2016). Instead of selecting just one of the possible regression specifications, the general idea behind BMA is to run regressions with different subsets of possible combinations of explanatory variables. Thus, BMA can be thought of a robustness check with many different subsets of explanatory variables. As a full enumeration of all possible subsets of explanatory variables would require too much computing capacity, a Monte Carlo chain algorithm is applied to consider the most promising models. The distribution of the model parameters over the individual models is captured by the posterior means and standard deviations. Furthermore, we can compute the posterior inclusion probability (PIP) for each explanatory variable, which is the sum of the posterior probabilities across all regression specifications including this variable. The PIP denotes the probability that a variable is included in the 'true' regression model. Table 5 reports the numerical results for the BMA. The posterior mean, standard deviation, and the PIP are shown in the first three columns.[5] In the next step, we add all moderator variables with a PIP greater than 0.2 into the multiple WLS model following equation (6), as these variables are identified to have explanatory power for heterogeneity. The results from the WLS regression are presented in the right part of Table 5.

The BMA results show that mean sample year crucially affects the CEP-CFP relation with the highest PIP of 1.00 (posterior mean = -0.006). Moreover, the analysis reveals a strong impact for reactive CEP measures (PIP = 0.91, posterior mean = -0.082). All other moderator variables do not even reach a weak level. In the subsequent WLS model, mean sample year and reactive CEP also

[5] Thereby we follow the classification by Eicher *et al.* (2011) and categorize an effect as 'weak' if the PIP is between 0.5 and 0.75, 'substantial' if the PIP is between 0.75 and 0.95, 'strong' for values between 0.95 and 0.99, and 'crucial' for values above 0.99.

show the most striking effects with coefficients of -0.005 and -0.091, which are significant at any level. This means that the CEP-CFP relation decreases over time and that there is a weaker dependency between reactive investments in environmental activities and a firm's financial performance. The latter stands in opposition with the result from Cordeiro and Sarkis (1997), who document that financial analysts expect lower earnings-per-share especially for environmentally proactive investments. Additionally, the results show a weakly significant effect for small firms with a coefficient of 0.050. Accordingly, for small firms the CEP-CFP relation shows higher values.

Overall, Table 5 indicates that the sign and the size of the regression coefficients from the WLS model are consistent with the posterior means from the BMA results. Variables with a high PIP are in most cases statistically significant. As BMA does not allow clustering standard errors, we can conclude from the WLS estimation that the findings are robust to error-clustering.

Finally, we split our sample of effect sizes according to the most significant moderator variables of the MRA r*eactive CEP,* and *mean sample year – 1950,* while simultaneously distinguishing between concurrent and lagged effects, in order to calculate the mean effects for the related subsamples. The results of this subgroup analysis in Table 6 show that, compared to the mean CEP-CFP effect of the full sample (0.072), the concurrent ($j = 0$) and lagged ($j \neq 0$) effects between CEP and CFP are especially small and insignificant for reactive investments ($j = 0$: 0.039; $j \neq 0$: 0.021). Furthermore, the effect is slightly greater than the overall mean and statistically significant for non-reactive investments ($j = 0$: 0.080; $j \neq 0$: 0.069). However, the difference between reactive and non-reactive investments is fairly the same for the concurrent and the lagged effect. Continuing with the temporal differences, the mean effects are slightly above the overall mean for the period until the year 2000 ($j = 0$: 0.098; $j \neq 0$: 0.103). After the year 2000, the effects seem to decrease with a mean concurrent effect of 0.067 and a mean lagged effect of 0.030. Here, the temporal difference is slightly greater for the period after 2000. Overall, we can conclude that our main results concerning the temporal structure of the CEP-CFP causality are robust to differences in the CEP strategy and temporal effects, since the differences in the mean concurrent and lagged effects are relatively small between the two pairs of subsamples.

Insert Table 6 about here

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5 Conclusion

Extending existing meta-studies on the relation between CEP and CFP (Albertini, 2013; Dixon-Fowler *et al.*, 2013; Endrikat *et al.*, 2014; Guenther *et al.*, 2012; Hang *et al.*, 2018; Horváthová, 2010), the aim of this paper is to shed light on the reverse causality between both constructs by applying meta-analaysis on a sample of 893 effect sizes.

Our findings suggest that in the short-term (one year), financial resources can increase a firm's environmental performance as proposed by the slack resources hypothesis; however, the effects disappear in the long-term (more than one year). Conversely, increasing environmental performance has no short-term effect on a firm's financial performance, while a firm significantly benefits in the long-term following the Porter hypothesis. In contrast, the concurrent/synergetic effect is significantly positive but economically insignificant. Overall, our results imply that the causality between environmental performance and financial performance depends on the time horizon. This result is not affected by publication bias. However, meta-regression analysis reveals that the CEP-CFP relation decreases over time and is significantly smaller for reactive environmental investments. To sum up, our results should encourage managers to stick to a proactive environmental policy and not to abandon the investments if the financial success is not immediately visible.

Future research might especially point to the temporal structure of causal effects for different environmental practices on a fine-grained level. For a more detailed understanding (also on a meta-level), additional studies examining long-term effects are also needed. Moreover, future studies might also investigate the causality between CEP and CFP assuming non-linear relations (Fujii *et al.*, 2013).

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**The references marked with an asterisk are included in the meta-analytic data set.*

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Tables

Table 1. Results of Hedges and Olkin-type meta-analysis

Subsample	*n*	*k*	Random effects mean effect size	Standard error	Results of heterogeneity test		
					Q statistic (*df*)	I^2	τ
Full sample							
Total	142	893	0.072***	0.006	5217.99*** (892)	85.68%	0.144
$CEP_t \leftrightarrow CFP_t$							
Total	106	569	0.077***	0.007	2705.32*** (568)	82.32%	0.134
$CEP_t \rightarrow CFP_{t+j}$							
Total	46	260	0.056***	0.010	1553.49*** (259)	89.40%	0.150
j = 1	42	186	0.030**	0.012	1112.07*** (185)	88.10%	0.146
j = 2	9	54	**0.110*****	0.024	376.06*** (53)	93.40%	0.161
j = 3	5	18	**0.158*****	0.022	18.65 (17)	0.02%	0.001
j = 5	1	2	**0.117*****	0.031	0.05 (1)	0.00%	0.000
$CFP_{t-j} \rightarrow CEP_t$							
Total	12	64	**0.100*****	0.020	672.76*** (63)	88.23%	0.146
j = 1	10	59	**0.104*****	0.021	667.68*** (58)	89.35%	0.150
j = 2†	1	1	-0.058	0.118	-	-	-
j = 3†	1	4	0.055	0.041	-	-	-
j = 5†	0	0	-	-	-	-	-

This table shows the results from Hedges and Olkin-type meta-analysis for the CEP-CFP relation. Data are split according to the number of lagged years *j* between the two constructs. *n* is the number of studies, *k* is the number of effect sizes. Besides, the random effects mean effect sizes and their standard errors are shown. Since mean correlation coefficients are accepted as remarkable in meta-analysis when they exceed 0.10 (Cohen, 1992), mean correlation coefficients are highlighted in a bold font when they are equal to or larger than 0.10. *, ** and *** indicate a 10%, 5%, or 1% significance level, respectively.

† Due to the small number of observed effect estimates, the calculations cannot be performed completely for this subgroup.

Table 2. Results of Hedges and Olkin-type meta-analysis using *z*-transformed effect sizes

Subsample	*n*	*k*	Random effects mean effect size	Standard error	Results of heterogeneity test		
					Q statistic (*df*)	I^2	τ
Full sample							
Total	142	893	0.073***	0.006	4852.16*** (892)	85.52%	0.146
$CEP_t \leftrightarrow CFP_t$							
Total	106	569	0.078***	0.007	2521.21*** (568)	81.54%	0.138
$CEP_t \rightarrow CFP_{t+j}$							
Total	46	260	0.055***	0.011	1453.18*** (259)	89.18%	0.150
$j = 1$	42	186	0.030**	0.012	1046.58*** (185)	87.92%	0.146
$j = 2$	9	54	**0.111*** **	0.025	348.94*** (53)	93.55%	0.165
$j = 3$	5	18	**0.155*** **	0.022	15.19 (17)	0.03%	0.002
$j = 5$	1	2	**0.117*** **	0.031	0.05 (1)	0.00%	0.000
$CFP_{t-j} \rightarrow CEP_t$							
Total	12	64	**0.104*** **	0.021	623.50*** (63)	89.62%	0.160
$j = 1$	10	59	**0.109*** **	0.023	619.60*** (58)	90.64%	0.166
$j = 2$†	1	1	-0.058	0.120	-	-	-
$j = 3$†	1	4	0.055	0.041	-	-	-
$j = 5$†	0	0	-	-	-	-	-

This table shows the results from Hedges and Olkin-type meta-analysis for the CEP-CFP relation. Data are split according to the number of lagged years *j* between the two constructs. *n* is the number of studies, *k* is the number of effect sizes. Besides, the random effects mean effect sizes and their standard errors are shown. Since mean correlation coefficients are accepted as remarkable in meta-analysis when they exceed 0.10 (Cohen, 1992), mean correlation coefficients are highlighted in a bold font when they are equal to or larger than 0.10. *, ** and *** indicate a 10%, 5%, or 1% significance level, respectively.

† Due to the small number of observed effect estimates, the calculations cannot be performed completely for this subgroup.

Table 3. Results of publication bias test

Subsample	*n*	*k*	β_0	SE(β_0)	Results of publication bias test	
					β_1	SE(β_1)
Full sample						
Total	142	893	0.065*	0.032	0.105	0.416
$CEP_t \leftrightarrow CFP_t$						
Total	106	569	0.114**	0.038	-0.422	0.488
$CEP_t \rightarrow CFP_{t+j}$						
Total	46	260	-0.022	0.017	1.142*	0.377
$j = 1$	42	186	0.003	0.028	0.420	0.693
$j = 2$	9	54	-0.087	0.330	3.165	3.520
$j = 3$	5	18	0.086	0.033	0.891	0.368
$j = 5^{\dagger}$	1	2	-	-	-	-
$CFP_{t-j} \rightarrow CEP_t$						
Total	12	64	0.357	0.393	-4.562	5.395
$j = 1$	10	59	0.397	0.444	-5.395	6.403
$j = 2^{\dagger}$	1	1	-	-	-	-
$j = 3^{\dagger}$	1	4	-	-	-	-
$j = 5^{\dagger}$	0	0	-	-	-	-

This table shows the results from the publication bias test for the CEP-CFP relation. Data are split according to the number of lagged years *j* between the two constructs. n is the number of studies, *k* is the number of effect sizes. Besides, the estimates and their standard errors are shown for β_0 and β_1. The observations are weighted by inverse standard errors. Standard errors of the meta-analysis are clustered at the study level. *, ** and *** indicate a 10%, 5%, or 1% significance level, respectively.

† Due to the small number of observed effect estimates, the calculations cannot be performed completely for this subgroup.

Table 4. Results of publication bias test using *z*-transformed effect sizes

Subsample	*n*	*k*	β_0	SE(β_0)	Results of publication bias test	
					β_1	SE(β_1)
Full sample						
Total	142	893	0.057*	0.030	0.219	0.382
$CEP_t \leftrightarrow CFP_t$						
Total	106	569	0.106**	0.037	-0.305	0.460
$CEP_t \rightarrow CFP_{t+j}$						
Total	46	260	-0.021	0.016	1.087*	0.353
j = 1	42	186	0.004	0.029	0.386	0.697
j = 2	9	54	-0.084	0.272	2.997	2.834
j = 3	5	18	0.086	0.037	0.839	0.343
j = 5†	1	2	-	-	-	-
$CFP_{t-j} \rightarrow CEP_t$						
Total	12	64	0.174	0.418	-1.184	5.946
j = 1	10	59	0.155	0.507	-0.796	7.898
j = 2†	1	1	-	-	-	-
j = 3†	1	4	-	-	-	-
j = 5†	0	0	-	-	-	-

This table shows the results from the publication bias test for the CEP-CFP relation. Data are split according to the number of lagged years *j* between the two constructs. *n* is the number of studies, *k* is the number of effect sizes. Besides, the estimates and their standard errors are shown for β_0 and β_1. The observations are weighted by inverse standard errors. Standard errors of the meta-analysis are clustered at the study level. *, ** and *** indicate a 10%, 5%, or 1% significance level, respectively.

† Due to the small number of observed effect estimates, the calculations cannot be performed completely for this subgroup.

Table 5. Results of meta-regression analysis

Dependent variable: partial correlation coefficient of CEP-CFP *r*			Bayesian model averaging			WLS		
Independent variables	Mean	Std. dev.	Post. mean	Post. std. dev.	PIP	Coefficient	Std. error	*t*-value
Standard error of r	0.091	0.054	-0.011	0.057	0.07			
Process-based CEP	0.432	0.495	-0.007	0.014	0.24	-0.023	0.021	-1.069
Market-based CFP	0.308	0.462	0.011	0.018	0.32	0.031	0.020	1.556
Proactive CEP	0.408	0.491	0.000	0.004	0.05			
Reactive CEP	0.068	0.252	-0.082	0.036	0.91	-0.091***	0.027	-3.359
Number of citations	155.046	321.549	0.000	0.000	0.14			
Mean sample year - 1950	50.232	7.453	-0.006	0.001	1.00	-0.005***	0.002	-3.021
US data	0.340	0.474	0.015	0.024	0.33	0.032	0.032	0.986
EU data	0.199	0.399	-0.007	0.017	0.20	-0.017	0.029	-0.570
Manufacturing sector	0.402	0.490	0.001	0.006	0.07			
Service sector	0.382	0.486	-0.005	0.013	0.20	-0.023	0.022	-1.049
Small firms	0.233	0.423	0.019	0.024	0.44	0.050*	0.027	1.876
OLS estimation	0.321	0.467	0.004	0.011	0.17			
Endogeneity considered	0.429	0.495	0.000	0.003	0.04			
Constant	0.074	0.186	0.351	NA	1.00	0.319***	0.085	3.750
Observations	893			893			893	

Besides the explanatory (moderator) variables and their descriptive statistics, this table presents the results from Bayesian model averaging and meta-regression analysis via WLS. In the WLS regression, we only include explanatory variables with PIP > 0.2. Standard errors are clustered at the study level. *, ** and *** indicate a 10%, 5%, or 1% significance level, respectively.

Table 6. Results of subgroup analysis

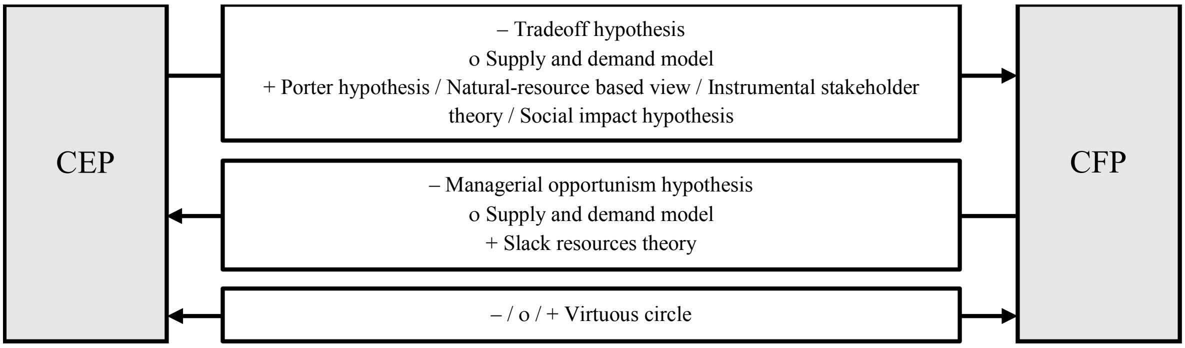
Subsample	*n*	*k*	Random effects mean effect size	Standard error	Results of heterogeneity test			
					Q statistic (*df*)		I^2	τ
Full sample								
Total	142	893	0.072***	0.006	5217.99***	(892)	85.68%	0.144
CEP strategy								
Reactive								
$j = 0$	20	39	0.039*	0.021	115.57***	(38)	66.24%	0.089
$j \neq 0$	6	22	0.021	0.014	42.23***	(21)	37.40%	0.039
Non-reactive								
$j = 0$	103	530	0.080***	0.007	2527.71***	(529)	82.70%	0.142
$j \neq 0$	52	302	0.069***	0.010	2333.61***	(301)	89.86%	0.155
Time								
Mean sample year ≤ 2000								
$j = 0$	51	203	0.098***	0.013	974.81***	(202)	82.56%	0.154
$j \neq 0$	31	157	0.103***	0.012	1001.39***	(156)	82.97%	0.127
Mean sample year ≥ 2001								
$j = 0$	56	366	0.067***	0.008	1721.47***	(365)	81.51%	0.130
$j \neq 0$	23	167	0.030**	0.014	1223.30***	(166)	92.04%	0.163

This table shows the results from Hedges and Olkin-type meta-analysis for the CEP-CFP relation. Data are split according to the the CEP strategy applied and the mean sample year of the analyzed data, while simultaneously splitting the sample according to the number of lagged years *j* between the two constructs. *n* is the number of studies, *k* is the number of effect sizes. Besides, the random effects mean effect sizes and their standard errors are shown. *, ** and *** indicate a 10%, 5%, or 1% significance level, respectively.

† Due to the small number of observed effect estimates, the calculations cannot be performed completely for this subgroup.

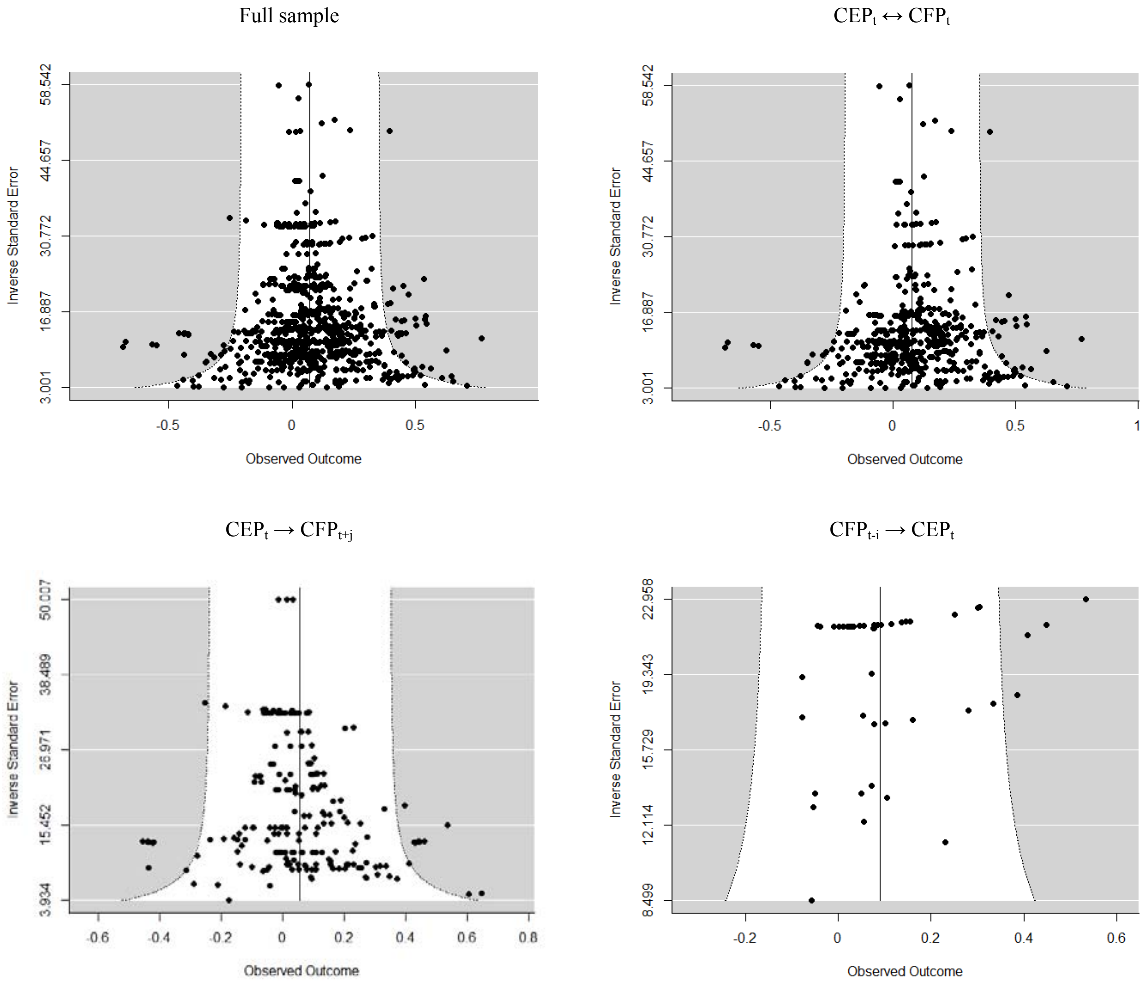
Figures

Figure 1. Typology of theoretical CEP-CFP relations



This figure shows a classification of the theoretical considerations for the relation between CEP and CFP. The causal sequence, which is supported by the specific theory is indicated by arrows. The sign of each effect ("–" for negative, "o" for neutral, and "+" for positive) is given beside each theory.

Figure 2. Funnel plots



This figure shows the funnel plots for the effect sizes measuring the relation between CEP and CFP. The plots show the individual observed effect sizes (partial correlation coefficients) on the horizontal axis against the corresponding random effects standard errors on the vertical axis. Based on the random effects results, the vertical line indicates the estimate, while the 95% confidence intervals are displayed by dashed boundaries.